Gunnery and the Struggle for the New Science (1537-1687)

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The candidate confirms that the work submitted is her own and that appropriate credit has been given where reference has been made to the work of others.

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Abstract

This thesis re-examines the contribution of ballistics and gunnery to the emergence of modern science. It seeks to answer the question that inevitably emerges from A. R. Hall's seminal *Ballistics in the Seventeenth Century* (1952): Why did early modern scientists and writers on gunnery include theoretical treatments of the trajectory of a gun in their works, despite the fact that it could be of no use to the practice of gunnery?

Hall's response to this perplexing question was simply that ballistic theory provided a scientific 'veneer' in support of attempts to gain patronage from rulers and military leaders who were anxious to gain an advantage in the new cannon warfare that played a crucial role in the development of the emerging European nation states from the end of the fifteenth century.

Recent historiography, which has emphasised the role of etiquette and rhetoric in patronage relationships, has only served to bolster the credibility of Hall's explanation, leading to an attenuation of the programme of the early modern writers who attempted to solve the mystery of the trajectory ('the gunners' question').

My thesis contends that, pace Hall, the struggle for the solution to the gunners' question is paradigmatic for the resolution of unsolved issues in the history of science, and would aid substantially in delineating the role of mathematics and quantification not only in ballistics but in the transformation of natural enquiry into a recognisably modern enterprise.

Whilst retaining the long-term chronological approach of Hall, my thesis re-examines in detail a number of central figures in the history of ballistics as historical actors, rather than focusing narrowly on theoretical results. This brings to the

forefront their struggle to unite theory with practice and to persuade their audience of the necessity for a new approach to natural enquiry. Through a re-examination of key texts, the thesis attempts to uncover their wider programmatic aims. They all had in common a self-perception that they were involved in building a new science of motion that would lay certain foundations for practice, they sought commonalities in all the diverse domains of the natural and artificial world, and they recognised that this was the only route to new and certain knowledge.

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I have retained the original spelling in quotations from primary texts. Where translations have been available I have compared them to the original, and provided references to the original. Where I have had to provide my own translation, I have indicated this in the footnotes and provided the original text.

Gunnery and the Struggle for The New Science: 1537-1687

> Tous ces obstacles qui s'oppose à l'intégration de la pensée théorique et de l'action expérimentale se reflètent dans le dialogue sur la trajectoire parcourue par un boulet de canon.[1]

Introduction

Anyone who writes on gunnery in the Early Modern period does so in the shadow of A. R. Hall's *Ballistics in the Seventeenth Century* (1952). It will remain the seminal work of reference on gunnery and science, and my aim is not to replace it but to offer an alternative narrative and historiographical approach.

Unlike Hall, who focused on the question of whether science was any use to gunnery in the early modern period, I have sought to use the relationship between developments in gunnery and the theory of motion as a prism, through which it may be possible to gain a deeper insight into how science developed over the period from the appearance of Tartaglia's *Nova Scientia* (1537) to the publication of Newton's *Principia* (1687).

The origin of the modern science of ballistics can be historically pinpointed in the technological developments in metallurgy and cannon warfare of the fifteenth century. These developments led to a recognition that there was a measurable relationship between the range of a gun and its angle of elevation. The search for this relationship and its quantification, the 'gunner's question', was the stimulus for the quest for the discovery of the trajectory of a cannonball. This problem sits at the core of the science of motion and played a crucial epistemological role in the transition from pre-classical to classical mechanics. Furthermore, whether its interests were

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[1] Serge Moscovici, *L'Expérience du mouvement: Jean-Baptiste Baliani disciple et critique de Galilée* (Paris: Hermann, 1967), 190: 'All these obstacles to the integration of theoretical thought and experimental action are reflected in the dialogue on the trajectory travelled by the bullet of a canon.' My translation.

attack or defence, the enormous benefit to any nation that was able to find the secret of aiming a cannon with perfect accuracy ensured that this became one of the major unresolved technical issues of the day.[2]

I have taken the position that it is precisely the long-term theoretical and practical challenge that the development of cannon warfare posed that makes it a fruitful subject for close historical scrutiny. During the early modern period, the cannon and its trajectory played a key epistemological role as a 'challenging object'. As Jürgen Renn and Peter Damerow explain, before the onset of the experimental method, challenging objects:

> … were a key source of empirical knowledge which accounts for one aspect of their fundamental role in the conceptual reorganization of mechanical knowledge in the sixteenth and seventeenth centuries. Thus, Galileo's new science of motion, for instance, can be conceived as resulting from a struggle with the challenges represented by the pendulum and the motion of a projectile, both addressed on the basis of attempts to understand another challenging object, the inclined plane.[3]

Important developments in cannon technology and warfare dovetailed with the emergence of the humanist movement and the increasing availability of classical texts to which artist-engineers such as Leon Battista Alberti, Brunelleschi and Leonardo da Vinci could turn for guidance in finding solutions to problems of motion and mechanics.[4] This turn to classical texts for the solution to practical problems of motion was explicitly articulated by Niccolò Tartaglia in the dedication of his *Nova*

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[2] Stephen Johnston, 'Mathematical Practitioners and Instruments in Elizabethan England', *Annals of Science* 48 (1991), 319-344, on 323.

[3] Jürgen Renn and Peter Damerow, *The Equilibrium Controversy* (Berlin: Max Planck Research Library for the History and Development of Knowledge: Edition Open Access, 2012), 13. For a summary of the approach of historical epistemology and an explanation of key concepts such as 'mental model', 'challenging object' and 'shared knowledge' used in this historiographical methodology see, for example, the forward by Jürgen Renn to Matthias Schemmel's *The English Galileo: Thomas Harriot's Work on Motion as an Example of Preclassical Mechanics* (Dordrecht; Heidelberg; London; New York: Springer, 2008).

[4] For example, Leonardo da Vinci:'Science is the captain, practice the soldiers', quoted by Henrik Grossman in 'The Social Foundations of the Mechanistic Philosophy and Manufacture' translated in *The Social and Economic Roots of the Scientific Revolution: Texts by Boris Hessen and Henryk Grossmann,* ed. Gideon Freudenthal and Peter McLaughlin (Dordrecht; Heidelberg; London; New York: Springer, 2009), 132.

Scientia (1537):

> …as I entered [the investigation of] this matter by that time, I decided to investigate further. I started (not without reason) to investigate the kinds of motions that take place when a heavy body is involved. I found that there are two kinds [of motion]; the natural and the violent.[5]

Thus the conception of ballistics and mechanics of Tartaglia and his successors cannot be divorced from the pursuit of the causal understanding of matter and motion. The dialectical struggle to reconcile the mathematics of Euclid and Archimedes and pre-classical theories of motion with the challenge posed by cannon warfare led to a new synthesis in the form of classical mechanics.

All the subjects of my case studies were conscious that the study of the causes of motion promised enormous practical rewards. The pursuit of causes, a philosophical end in itself, in the early modern period became interventionist, and was seen as the only route to inventions and discoveries on a scale that would far outstrip those hitherto discovered by chance or craft ingenuity.

In a narrow sense, Hall's major contention that science was useless to the practice of gunnery until the nineteenth century was true; and it has generally been accepted as the last word on the subject, as was its corollary that gunnery writers only included theory for rhetorical purposes, or, as Hall put it, to provide a 'scientific veneer'.[6] He also dismissed the arguments of social historians who had been

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[5] Nicolò Tartaglia, *La nova scientia de Nicolò Tartaglia con una gionta al terzo libro* (Venetia: per Curtio Troiano, 1558), fol. 3v, unnumbered, translated by Matteo Valleriani in *Metallurgy, Ballistics and Epistemic Instruments: The Nova Scientia of Niccolò Tartaglia. A New Edition,* (Berlin: Max Planck Institute for the History and Development of Knowledge, Edition Open Access, 2013), 73. Valleriani provides parallel text with the original, as well as a facsimile of the original. He has used the 1558 edition, but has indicated where it differs from the original 1537 edition. In the 1537 edition, *ratiocinare* was used instead of *investigare* and *videlicet* is inserted, referring to *moti* (motions). An alternative translation may be found in Stillman Drake and I.E. Drabkin (trans.)*, Mechanics in Sixteenth-Century Italy: Selections from Tartaglia, Benedetti, Guido Ubaldo, & Galileo* (Madison, Milwaukee, & London: The University of Wisconsin Press, 1969), 65, which is based on the 1537 edition, with original page sequence shown in the running heads (for this extract, given as 8-9), which includes the engraved title page and blank pages.

[6] A. R. Hall, *Ballistics in the Seventeenth Century* (Cambridge: Cambridge University Press, 2009; first published 1952), 5.The best treatment of the historiographical context for Hall's thesis, and why he chose to attack a view

influenced by the 'seventeenth-century literature of apology for the new philosophy, in which, from the time of Bacon onwards, utility is put forward as a principal reason for the study of natural science'.[7]

In contrast to those who promote the rhetorical explanation, or who argue that theory was just a ruse to promote self-image, my thesis argues that causal enquiry was not a ploy but an epistemological insight encapsulated in the Baconian programme to effect a 'marriage' between theory and practice.

The seductive Kuhnian heuristic that segregated the mathematical and the Baconian/experimental traditions in the historiography of early modern science has acted as a block to further investigation of a commonality of programme between Bacon and mathematical physicists such as Galileo.[8] The link between them is their method of causal enquiry into matter and motion, and its materialist foundation. Kuhn's view shows evidence of the influence of a traditional, but still current, historiography that asserted Bacon's ignorance of mathematics and his lack of appreciation of the importance of mathematics to physical science.

Though Bacon was suspicious of mathematical speculation without firm grounding in the real (matter) or things (*res*), the importance of quantitative and mathematical reasoning for the Baconian programme is apparent throughout his writings. [9] Quantification is linked to the foundational work of compilation of natural

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that no historian of the period appears to have held, may be found in Gideon Freudenthal and Peter McLaughlin's introduction to *The Social and Economic Roots of the Scientific Revolution.*

[7] A. R. Hall, *Ballistics,* 163.

[8] Thomas Kuhn, 'Mathematical versus Experimental Traditions in the Development of Physical Science' *Journal of Interdisciplinary History,* 7 (1976) 1-31, *passim*. This article is also available as chapter three of Kuhn's *The Essential Tension* (Chicago: University of Chicago Press, 1977).

[9] See Graham Rees, 'Mathematics and Francis Bacon's natural Philosophy' *Revue Internationale de Philosophie* 159 (1986) 399-426 on 399 and Graham Rees, 'Quantitative Reasoning in Francis Bacon's Natural Philosophy', *Nouvelles de la Republique des Lettres* (1985) 27-48. See also Peter Urbach, *Francis Bacon's Philosophy of Science: An account and a Reappraisal* (La Salle, Illinois: Open Court, 1993, first published, 1987) esp.134–143 Paolo Rossi's thought-provoking article, 'Galileo e Bacone', *Saggi su Galileo Galilei* ed. Carlo Maccagni (Firenze: G Barbera, 1972), 248-296, esp. 286 and 289, contrasts Bacon's image of nature as a labyrinth with Galileo's platonic view of a rationally-structured world governed by mathematical principles characterized by simplicity and economy. Though he considers Bacon's opposition to mathematics as a major limitation of his method, he nevertheless admits that certain passages of Descartes and Galileo are reminiscent of Bacon, and

histories. In the *Novum Organum* Bacon complains that in traditional natural history we find nothing:

> … duly examined, verified, counted, weighed and measured. But loose and vague observation yields unreliable and untrustworthy information… And these two kinds of history differ in many respects but above all in this: that the first takes in the variety of natural species but not the experiments of the mechanical arts.[10]

Of Bacon's 'Instances of Special Powers' there were four mathematical instances, which he called 'Mathematical Instances' and 'Instances of Measure'.[11] In the *De Augmentis,* he promotes mathematics from being, under Aristotle, only productive of 'Practice or Mechanic' to a branch of metaphysics and mechanics, because it is one of the essential forms of things (ideoque inter *Formas Essentiales* numeranda est). [12] This situates mathematics at the top of the pyramid amongst the most general of axioms of metaphysics, and he considers mathematics to be a crucial component of causal enquiry. Of all the forms it is the one most easily abstracted and separable from matter:

> … which has likewise been the cause why it has been the more carefully laboured and more acutely inquired into than any of the other forms, which are all the more immersed in matter. For it being plainly the nature of the human mind, certainly to the extreme prejudice of knowledge, to delight in the open plains (as it were) of generalities rather than in the woods and inclosures of particulars, the mathematics of all other knowledge were the goodliest fields to satisfy that appetite for expatiation and meditation. But though this be true, regarding as do not only truth and order but also the advantage and order of mankind, I have thought it better to designate Mathematics, seeing that they are of so much importance both in Physics and Metaphysics, and Mechanics and Magic, as appendices and auxiliaries to them all. Which indeed I am in a manner compelled to do, by reason of the daintiness and pride of mathematicians, who will needs have this science almost domineer over Physic. For it has come to pass, I know not how, that Mathematic and Logic, which ought to be but the handmaids of Physic, nevertheless presume on the

comments that the greatest 'example' that Bacon provided of his method (the nature of heat), not only bore strong similarities to Galileo's views, but could be conceived as a hypothesis or 'anticipation of nature'.

[10] Francis Bacon, *Novum Organum,* in *The Oxford Francis Bacon* XI, ed. Graham Rees with Maria Wakeley (Oxford: Clarendon Press, 2004), Aphorism 98, 157.

[11] *ibid*., 367.

[12] Bacon, *De Augmentis,* in *The Works of Francis Bacon* IV, ed. James Spedding, Robert Leslie Ellis and Douglas Denin Heath (London: Longman and Co, 1858), book III, chapter VI, 369-70. Latin original in Spedding, *Works,* I, 575.

> strength of the certainty which they possess to exercise dominion over it. But the place and dignity of this science is less importance…[13]

In Book V chapter II of the *De Augmentis,* Bacon provides a striking illustration, crucial to the development of the law of fall and the break with Aristotelianism, of this necessity to subordinate mathematics to physics:

> For men believe that if the quantity be increased or multiplied, the power and virtue is increased or multiplied proportionately. And this they postulate and suppose as if it had a kind of mathematical certainty; which is utterly false. A leaden ball of a pound weight dropped from a tower reaches the ground in (say) ten seconds: will a ball of two pounds weight (in which the force of natural motion, as they call it, ought to be doubled) reach the ground in five seconds? No, but it will take almost the same time in falling, and will not be accelerated in proportion to the increase in quantity.[14]

Galileo would have agreed wholeheartedly! Bacon's 'Articles of Inquiry concerning Heavy and Light' is a research programme consisting of nineteen questions including questions on the speed of bodies under the force of gravity (seventh article of enquiry) and 'the motion of gravity as compared with other motions; what motions it overcomes, and what overcome it. As in violent motion (as it is called) the motion of gravity is overpowered for a time…' [15] (fourteenth article of enquiry). This is the gunner's question, abstracted from its context.

Bacon regretted that more progress had not been made in the development of mathematics, and showed remarkable prescience in identifying particular areas of untapped potential.[16] He articulated in exquisite detail the transformation of natural philosophy that was taking place in the course of the early modern period. Fundamental to his method was the production of a natural history that involved the

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[13] *ibid.,*370. Original Latin in Spedding et al., *Works* I, 576-577. Bacon's designation of mathematics as auxiliary attests to its universal applicability. Similarly, Descartes saw in his synthesis of algebra and geometry a universal philosophical method or instrument of fixed, mechanical and easily applicable general rules that could be applied to all natural phenomena. See Henryk Grossman 'Descartes and the Social Origins of the Mechanistic Concept' in *The Social and Economic Roots of the Scientific Revolution* ed. Freudenthal and McLauchlin, 183-4.

[14] Bacon, *De Augmentis,* book V, chapter II in *The Works of Francis Bacon* IV, ed. Spedding, Ellis and Heath, 414. Original Latin in Spedding et al., *Works,* I, 625.

[15] Bacon, *De Augmentis*, book V, chapter III, in Spedding et al., *Works,* IV, 424-27. Original Latin in Spedding, *Works*, I, 636.

[16] Bacon, *De Augmentis,* book III, chapter VI, in Spedding et al., *Works,* IV, 370-1. Original latin in Spedding, *Works,* I, 577.

collection and organisation of 'colossal bodies of scrupulously verified empirical data' which was to be obtained both from the mechanical arts and nature: '[I]n effect, this concept of natural history abolished the ancient distinction between the artifacts of man and the works of nature.'[17]

As Matthias Schemmel, Matteo Valleriani and others have shown, those who worked on the solution to the gunners' question drew on the 'shared knowledge' of practitioners as an essential foundation for their theoretical research. This shared knowledge of gunnery can be conceived as serving as a Baconian natural history of gunnery, though gunnery literature could also be viewed as a 'partial digestion' of the Baconian 'mother' history. As Sophie Weeks has explained, Bacon's tables 'digest the mother history *before* the intellect gets to work on it…The intellect, in Bacon's view, is not capable of digesting "a farrago and mass" of mother history: the tables must therefore perform a "first digestion."'[18] Quantification, tabulation, and the search for patterns and rules abound in gunnery manuals from the sixteenth century on. For example, thanks to the research of Matthias Schemmel (see chapter three, below), we have evidence from Thomas Harriot's manuscripts of his use of the tables and rules in gunnery manuals as his 'natural history' on which he based his research into the nature of the trajectory.

Tartaglia, when he promised range tables that would ensure accuracy in firing at all angles, was applying his mathematical and physical knowledge to the experience of bombardiers who, from the fifteenth century, informally recorded their own data for personal use relating to a specific piece of artillery. These informal

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[17] Graham Rees, *Quantitative Reasoning,* 32.
[18] Sophie Weeks, *Francis Bacon's Science of Magic* (Unpublished PhD Thesis, University of Leeds, 2007), 183-4.

annotations represent the 'beginning of a codified written recording of the experience of the bombardier and his practical knowledge in general'.[19]

However, a tension existed between mathematical truth and physical truth, a tension that is linked to the Baconian concern over premature systematisation and the 'daintiness and pride' of mathematicians. The study of the motion of a projectile posed particular problems for the subjects of my case studies because one is dealing here with what Ken Alder has called 'thick' objects:

> …the term 'thick things' alludes to the inability of mathematical theory to fully encompass the behaviour of material objects…This is what François Blondel, the foremost ballistic theorist of the late seventeenth century called 'the resistance and obstinacy of matter'. Overcoming this resistance, Blondel noted, would be possible only through a new amalgam of theoretical and practical knowledge.[20]

But what links the protagonists of my chapters with Francis Bacon was his insistence that the knowledge of practical effects (*scientia operativa)* are part of the process of enquiry into causes, and are the criterion of truth:

> Science also must be known by works. It is by the witness of works, rather than by logic or even observation, that truth is revealed and established. Whence it follows that the improvement of man's mind and the improvement of his lot are one and the same thing.[21]

As Sophie Weeks has shown, Bacon's cybernetic procedure for discovery of forms has an inherent feedback system by constantly moving between axioms and works, so that the connection between *mens* and *res* is always retained.[22] This vital connection is broken in the case of the development of the theory of the trajectory because truths are discovered and demonstrated from axioms on the basis of mathematico-deductive reasoning alone. Because mathematics abstracts from matter, the feedback provided

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[19] Matteo Valleriani, *Metallurgy, Ballistics and Epistemic Instruments,* 43.
[20] Ken Alder, *Engineering the Revolution: Arms and Enlightenment in France 1763-1815* (Princeton N.J.: Princeton University Press, 1977), 13.
[21] Bacon, *Cogitata et visa,* quoted and translated by Sophie Weeks in 'The Role of Mechanics in Francis Bacon's Great Instauration', *Philosophies of Technology: Francis Bacon and his Contemporaries* (Leiden, Boston: Brill, 2008), 133-196, on 182.
[22] Weeks, 'The Role of Mechanics',180.

by experiment and observation in the inquisitional process becomes more problematic. The study of the trajectory came from a problem in practical gunnery and thus the hope of those who studied it was to be able to use it to improve gunnery practice. But the gap between mathematical ideal and its applicability in practice was enormous. A means had to be found to bridge this gap between mathematical truth and the material world. Galileo's and Harriot's experiments, for example, (see chapter four), and Torricelli's instructions for gun experiments (chapter five), are an attempt to do precisely this.

My aim has been to identify the commonalities of programme and approach to the investigation of nature that embody the development of the new science. Despite their differences, the figures considered in my case studies are united by their pursuit of what Bacon called the 'marriage' between theory and practice. The key to uniting theory with concrete reality was to sustain the union between the mind (*mens*) and things (*res*). The mind's connection with the concrete behaviour of moving bodies cannot be broken, hence there is the necessity of constant intercourse between experiment and the production of axioms. Praxis (practice guided by theory) is the *sine qua non* in terms of discovering the causes of motion. Continual engagement of the mind in material processes would make mechanics philosophical. The marriage of *res* and *mens* corresponds to what Weeks calls Bacon's 'philosophical mechanics'.[23] The Baconian marriage protects/restrains the mind, always ready to relapse into imaginary explanations divorcing itself from reality and consequently from the truth of things themselves.

Thus in spite of the diversity of context, the perception of a practice-theory union is the historical thread uniting the main characters in this study. In their diverse

[23] Weeks, 'The Role of Mechanics', 134. Weeks uses the term 'philosophical mechanics', which depends on knowledge of physical causes, to avoid confusion with the two other types of Baconian mechanics, artisanal mechanics and *experientia literata.*

approaches to the study of motion, they shared an understanding that geometrical axioms describing motion must approximate closer and closer to the behaviour of projectiles in concrete reality. Mechanics reduces concrete reality to an ideal, stripped of the baffling complexity of phenomenal impediments. Thus unless it is constrained by experiment, the mind is always in danger of being misled by its ideal formulations. To bring the science of motion to geometrical exactness, experiment was seen to be the key unlocking the door leading to ever increasing approximation to the ideal.

In mechanics there must be a dialectic between theorems/axioms (in the mind) and the behaviour, qualities and causes of concrete bodies (in nature). Something is generating the motion of a specific projectile: we are dealing with causes and impediments. It is necessary to find the way to link the mind to the causes and at the same time overcome the impediments to unfettered motions. Hall's great oversight was that he focused on the ideal and its failure to be of use, but none of the subjects of my case studies was that naïve. They fully understood the relationship between mechanics as a science of ideal motions and its need for immersion in actuality.

Chapter one looks at the work of Tartaglia, not because he was the first to theorise about motion or ballistics, but because he was the first to articulate programmatically the necessity to study and apply the theory of motion and the mathematics of Euclid to solve practical gunnery questions. This programme is encapsulated in the iconic frontispiece of his *Nova Scientia,* where the gate of Euclid provides the only route to knowledge of the sciences. Furthermore, he was the first to do this within the vernacular tradition of artisans and practitioners rather than the learned tradition of scholars. The artist-engineers of Renaissance Italy had long been looking to the classics to assist them in perfecting their arts, but Tartaglia was the first to present this attempt to a wide vernacular audience and in the form of a new axiomatically-based science of ballistics.

Thus Tartaglia was a pioneer and an evangelist because his whole endeavour was aimed at convincing his readership and patrons that the only route to the perfection of practice, the discovery of useful knowledge, was through the study and application of mathematics to natural phenomena. The central problematic for him, as for my other subjects of study, was that a science of practice comes from and can only ultimately be confirmed through practice. Yet because matter is more complex than theoretical abstractions it may not always be possible to achieve exact compliance between the two:

> … the cause of this contradiction stems simply from matter; for things constructed or fabricated thereof can never be made as perfectly as they can be imagined apart from matter, which sometimes may cause in them effects quite contrary to reason.[24]

The same dilemma was articulated some hundred and fifty years later by Newton in his preface to the *Principia*, despite his having immensely more powerful theoretical tools at his disposal:

> But as artificers do not work with perfect accuracy, it comes to pass that mechanics is so distinguished from geometry that what is perfectly accurate is called geometrical; and what is less so, is called mechanical. However, the errors are not in the art, but in the artificers. He that works with less accuracy is an imperfect mechanic; and if any could work with perfect accuracy, he would be the most perfect mechanic of all…[25]

Mechanics and the science of motion aimed at approximating the perfect mechanic in so far as it was possible; the 'perfect mechanic' motif underpins the entire history of the problem of the trajectory.

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[24] Nicolò Tartaglia *Quesiti et inventioni diverse de Nicolo Tartalea Brisciano* (Venetia: Rufinelli, 1546) book 7, 78v, translated in Stillman Drake and I.E. Drabkin *Mechanics,* 106. The *Quesiti was* reprinted, with additions to Book VI at Venice, 1554, 1562, and in the Opere, 1606. Drake and Drablin, *Mechanics,* 401, provides a full list of Tartaglia's publications, including all the editions and translations.
[25] Isaac Newton, *Mathematical Principles of Natural Philosophy,* Newton's Preface to the First Edition, trans. Andrew Motte (1729), revised and annotated by Florian Cajori (Berkeley, California: University of California Press, 1947), xvii.

Galileo had great hopes for the practical application to gunnery of his demonstration of the law of fall and the parabolic trajectory. He provided a challenge to his successors that related to both aspects of his programme, the practical and philosophical. This ushered in a European-wide quest to determine the effect of air resistance on the trajectory, both mathematically and by experimental means.

However, this did not mean that the possibility of making use of the parabolic theory was abandoned; if bodies did not conform to the parabolic path then the practical problem became one of overcoming, through experimental approximation, whatever material factors were impeding the achievement of that ideal.

By Edmond Halley's time (see chapter seven), the level of experimental sophistication and theoretical input had developed significantly, yet there remains a remarkable continuity of approach. His recognition that the resistance of air was an impediment to the practical applicability of the parabolic trajectory led to his encouragement of Newton and John Wallis to develop mathematical models that accounted for the effect of air resistance. Nevertheless, this did not stop him from carrying out experiments on guns that would enable him to determine the conditions that would most accurately correspond to the ideal, not only to be able to shoot with accuracy, but with the maximum efficiency of powder and other resources. Thus the parabolic curve served as the ideal in the determination of his experimental iterations, serving an economical function in every sense of the term. The mathematical ideal was both the measure and the model that rendered material reality intelligible and optimally utilitarian, as well as providing the criteria to search in the same material reality for the conditions of applicability of that demonstrative mathematical certainty.

Bacon reflected the recognition, evident in Tartaglia, of the requirement to reorient one's audience, and most importantly one's potential patrons, towards causal enquiry as the only route to perfection of practice and new inventions. The patronage relationship privileged immediate results and it was Bacon who articulated most coherently and programmatically the immense benefits that deferred gratification and state investment in causal enquiry could bring, a message that resonated throughout Europe. The writings of people like Tartaglia and the rise of Baconianism challenge A. R. Hall's thesis, which attempted to draw a strict line of demarcation between causal enquiry and utilitarian ends. Practice provided a self-correcting mechanism for the new science. It provided the challenge, the guide and the aim, whilst the knowledge of causes was both means and end of this iterative process.

Hall's emphasis on the uselessness of gunnery theory to practice has acted as a smokescreen obscuring a crucial aspect of the development of early modern science, which is the close relationship between these early scientist-engineers and the highest echelons of the emerging early modern states in their various and changing forms, to which these pioneers of the science of motion had a thoroughgoing and unquestioning ideological commitment. In the era of the emerging nation states and the drive for imperial expansion, the development of scientific knowledge and nationalism were, in the mind of the early modern scientists, seen as one and the same.

As well as direct patronage by rulers, recognition of the benefits of expertise led to sponsorship of training and research, such as the bombardiers schools of the Venetian republic, to train practitioners and perfect their art. The Venice Arsenal was 'perhaps the most developed industrial organisation in the sixteenth century world. Its technological marvels were a state secret, hidden behind massive walls, but inside, its crews could reportedly build and rig a ship within a single day.'[26] Indeed, on the occasion of the visit of the French King Henry III in 1574, a galley apparently had to

[26] Ingrid Rowland, *Giordano Bruno: Philosopher/Heretic* (New York: Farrar, Strauss and Giroux, 2008), 94.

be built, launched, and completely armed in the space of an hour for his royal entertainment.[27] As Renn and Valleriani note:

> Galileo's evocation of the Arsenal [in the opening lines of the *Two New Sciences*] is generally considered as a literary *topos* which provides an appealing setting for the ensuing dialogues. And yet this explanation remains unsatisfactory in the light of Galileo's intriguingly concise reference to encounters with the workmen of the Venetian Arsenal.[28]

The practical problems involved in military technology were inextricably linked to a more general demand for expertise in the early modern period that led to the transformation of mechanics from a low-status art to a noble science and its appropriation as an integral component of the struggle for political and economic power during the early modern period.

The status of mathematicians rose during this period precisely because, as experts and 'expert mediators', they were able to provide engineering and mathematical know-how in both civil and military engineering projects. [29] For mathematical practitioners who wrote on gunnery, from William Bourne in the sixteenth century to Sir Jonas Moore in Restoration England, many of whom worked as Masters of the Ordinance, work on gunnery was just one stage in careers that spanned a wide range of activities to which they applied their mathematical knowledge, the only connection being their utility, particularly in navigation, civil engineering and military projects.

For example, Sir Jonas Moore had a long career as mathematician masterminding engineering projects before obtaining a position at the Ordinance. It was Moore who persuaded the King to agree to set up the Royal Observatory, and he

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[27] Jurgen Renn and Matteo Valleriani 'Galileo and the Challenge of the Arsenal' *Nuncius* 16 (2001), 481-503, on 492.

[28] *ibid*., 484.

[29] See Eric Ash, *Power, knowledge and Expertise in Elizabethan England* (Baltimore; London: John Hopkins University Press, 2004), *passim*. Ash introduces the term 'expert mediator' to describe someone who ideally combined learning with technical expertise. In Early Modern Europe, expert mediators were increasingly sought out by patrons to advise on and manage engineering projects.

facilitated the appointment of John Flamsteed as Royal Astronomer. In the early days of the Royal Society, because of his position as Master of the Ordinance, he was part of the working group on gunnery initiated by Lord Brouncker and including Robert Hooke.

It seems likely that it was the promise of improvements in gunnery that helped William Brouncker to induce Charles II to grant a Royal charter to the Royal Society, which helped to legitimise the new science and detach it from association with the vestiges of suspect republican and free-thinking associations under which it had gestated in the previous period. This possibility is supported by the fact that experiments on guns and gunpowder, such as those that William Brouncker carried out on the effects of recoil in the Tiltyard at Whitehall in front of the King and his brother in 1661, represented a major focus of interest for the early Royal Society.[30]

The main criterion for the choice of subjects for my case studies has been their programmatic self-consciousness. Crucially they all wrote some or all of their works in the vernacular and in many cases they explicitly articulated their reasons for doing so. The development of the vernacular tradition was crucial to their conception that this was a science that was useful and not simply for contemplation by scholars; thus it was a mark of each individual's commitment to the progress of their own state. It was also part of the construction of national identity that expressed itself in the increasing vernacular literary confidence and independence of these emerging nations.

Each case study in my thesis focuses on one or more key vernacular texts; Gabriel M Spiegel has suggested that modern sociolinguistics has taught us that

[30] In addition one of the earliest Baconian histories prepared for the Royal Society was 'The History of Saltpetre and Gunpowder', written by Thomas Henshaw in 1662 and printed in Thomas Sprat's *History of the Royal Society* (London: J. Martyn, 1667). This was followed in 1670 by William Clarke's *The Natural History of Nitre: or, A Philosophical Discourse of the Nature, Generation, Place, and Artificial Extraction of Nitre with its Vertues and Uses* (London: E Okes for Nathaniel Brook, 1670).

'social groups most affected by changes in status tend to be most conscious of alternative modes of discursive behaviour, they are, in other words, most sensitive to the power of language to register social transformations.'[31] All of my protagonists were historical actors who to some degree or another were participants in the enormous transformatory struggles that were taking place in early modern society, in which the struggle for the new science was one facet. How they wrote is thus as significant as what they wrote, and I hope to convey the personality and individual voice of each of my protagonists.

The prosperity of the state was explicitly linked to the freedom to philosophise. This is particularly apparent in the cases of Thomas Digges and Galileo; Galileo articulated his fears that Italy would fall behind the rest of Europe because of antagonism to new ideas, something which has been ignored in much recent historiography that suggests Galileo's downfall was due to hubris, arrogance, or errors in political judgement and social etiquette. If it were just about ideas then Galileo's behaviour would be inexplicable – why bother to bring all that trouble down on himself? But the importance of the new science lay in its promise of new discoveries based on sound theoretical foundations, in contrast to the barrenness of Aristotelianism. Galileo was not alone in this view that Italy would not only suffer ridicule but would suffer practically from censorship; Milton, in his speech to Parliament for the liberty of unlicensed printing (*Areopagitica*, 1644) astutely invoked the fear of Papal repression, adding sure-fire rhetorical power to his case:

> I could recount what I have seen and heard in other countries, where this kind of inquisition tyrannizes; when I have sat among their learned men, for that honour I had, and been counted happy to be born in such a place of philosophic freedom, as they supposed England was, while themselves did nothing but bemoan the servile condition into which learning amongst them was brought: that this was it which had damped the glory of Italian wits: that

[31] Gabrielle M. Spiegel 'History, Historicism, and the Social Logic of the Text in the Middle Ages', *Speculum,* 65 (1990), 59-86, on 79.

> nothing had been written now these many years but flattery and fustian. There it was that I found and visited the famous Galileo, grown old a prisoner to the Inquisition, for thinking in astronomy otherwise than the Franciscan and Dominican licensers thought.[32]

As Ingrid Rowland notes, one of the reasons Venice kept the Holy Office at a distance was that the city's economy depended significantly on publishing.[33] Nevertheless, over time Italy lost out to other centres at least partly due to the tortuous processes of censorship that resulted in inefficiency and loss of competitive edge in the printing industry – another one of the unintended consequences of censorship that Milton details with relish. Italy's loss was the gain of England and the Netherlands. Censorship and religious persecution were bad for business as well as science.

The variety of interests and activities of those who pursued natural enquiry during the early modern period has posed a problem for historians of science. There has been a tendency to classify individuals according to the interests of the historian rather than their subject, a methodology that runs counter to the spirit of the time. Bacon articulated the danger inherent in the compartmentalisation of knowledge:

> And generally let this be a rule; that all division of knowledges be accepted and used rather for lines to mark or distinguish, than sections to divide and separate them; in order that solution of continuity in sciences may always be avoided. For the contrary hereof has made particular sciences to become barren, shallow, and erroneous; not being nourished and maintained and kept right by the common fountain and aliment.[34]

In each case study, I have attempted to use ballistics as a vehicle to gain an understanding of the programmatic aims behind diverse interests and activities. It is not just a matter of putting their science in historical context but recognising the subjects in my case studies as significant participants in momentous historical events and struggles.

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[32] John Milton, *Prose Writings* (Everyman, 1974), 170.
[33] Ingrid Rowland, *Giordano Bruno,* 93.
[34] Bacon, *De Augmentis,* book IV, chapter I, in Spedding et al., *Works*, IV, 373. Original Latin in Spedding et al., *Works*, I, 580.

This goal of uncovering an underlying programmatic coherence has meant that my thesis has sometimes developed in unexpected directions, so that the question of the defence of Copernicanism came to play an increasing role in my examination of the relationship between gunnery and science. The key to this programmatic coherence is motion, both its causes and epistemology, which are grounded in the union of axioms with practice. Thomas Digges (discussed in chapter two) may have been the first to recognise or at least to articulate a link between motion on earth and motion in the heavens; what that meant was that it was impossible to deal with the trajectory of the cannon in isolation because it had become an essential cognitive step towards understanding motion universally.

Whilst Hall emphasised the problems that Galileo's theory encountered when they were put to the test in gun experiments, from a historical perspective this has to be dealt with within the wider context of the attacks on Copernicanism and Galileo's theory of fall and of the parabolic trajectory, since these had become connected in the minds of both opponents and supporters of Copernicanism. My short case-study on the gun experiments of the Accademia del Cimento in chapter five is just one example of how the political and religious background have to be taken into consideration in understanding the Cimento's attempt to break down the cognitive barriers to the acceptance of Copernicanism through the means of experiments on the trajectory of a gun.

It has been recognised that whilst gunnery was a major stimulus to scientific enquiry in Italy, in England the improvement of techniques in navigation was the prime motive. But the continuing observations of the satellites of Jupiter (the Medici satellites) made by Galileo and his successors were motivated by Galileo's conviction that they could be used for the solution to the problem of longitude. Motion in the heavens and the motion of the trajectory are closely connected both epistemologically and historically. Early modern scientists increasingly sought commonalities between them, since they recognised that the causes of motion are universal, and therefore the epistemology is the same. A full synthesis was achieved with the publication of Newton's *Principia.* What started with Tartaglia as *a* new science had become *the* new science. It was *the* new science because of its universality and because, in Baconian terms, it was immensely powerful for the production of new knowledge and works. This was what Halley understood, and what all the subjects in my case studies had struggled to achieve.

Chapter one situates Tartaglia's writings on ballistics within the context of the military culture of the Italian city states of the sixteenth century, and the technological changes that opened up the cannon as an object for scientific enquiry. It offers a reassessment of Tartaglia's scientific contribution by means of a close examination of the relationship between his theoretical results and his practical claims.

Chapter two examines the ballistics of Thomas Digges. Unlike Tartaglia, Thomas Digges was a gentleman of substantial means who was not dependent for his living on the attraction of patronage. Digges's interest in the military arts was directly related to his involvement in the political and religious turbulence of Elizabethan England. While Tartaglia boasted he had never fired a gun, for Digges his theoretical work on the trajectory was integral to his wider programme for reform of the military, and he was prepared to put his ideas into practice by personal participation in military action in defence of his religion and country.

Chapter three gives an account of the discovery of the law of fall and the parabolic trajectory by Thomas Harriot and Galileo. It draws heavily on Matthias Schemmel's research into the manuscripts of Thomas Harriot, and shows how the two scientists drew on similar conceptual tools and practitioner's knowledge that had become available in this period. But they also came to their conclusions via different routes, showing how, as Schemmel puts it, their shared knowledge 'defined a space for alternative solutions'.[35]

Chapter four follows the efforts of Galileo's disciple Evangelista Torricelli as he attempted to defend Galileo's theory of motion, and its applicability to the practice of gunnery. In the process it tackles fundamental epistemological questions regarding the attitude of Galileo and Torricelli towards mathematical certainty.

In chapter five I show how under the patronage of the Medici Princes, the Accademia del Cimento used experiments on guns as a proxy to challenge traditional conceptions of motion and thus to attempt to subvert the ban on the teaching of Copernicanism.

Chapter six focuses on Francois Blondel's *L'Art de Jetter les Bombes* (1683). Blondel's work has so far been treated as an overly theoretical and ultimately unsuccessful attempt to apply Galileo's theory of the trajectory to the practice of gunnery. In contrast I argue that *L'Art* was a vehicle for the defence of Galilean science and for its propagation as part Colbert's programme of modernisation, for which the reform of technical and mathematical education was central. The significance of Blondel's work is that it inaugurated a tradition in France where the study of mathematics, engineering and science were integrated into an elite state-funded military education.

Though the story of ballistics does not end with the publication of Newton's *Principia*, the story of the struggle for the new science does. Chapter seven focuses not on Newton, however, but on Edmond Halley. Halley's crucial role in the

[35] Matthias Schemmel, *The English Galileo*, 235.

publication of the *Principia* is well known, but his subsequent role in promotion of the *Principia* for its value as a means of solving important practical problems, particularly in navigation and gunnery, is less appreciated. Encouraged by rivalry with the French, who had a reputation for their scientific approach to war, Halley sought to refine the theory of the trajectory as a foundation for instruments and rules accommodated for use by practitioners. At the same time, he carried out practical experiments that he hoped would improve the reliability and efficiency of the guns themselves. These efforts laid the basis for the ground-breaking work on ballistics by Benjamin Robins in the eighteenth century.

Chapter one

Niccolò Tartaglia: evangelist for a new science

> It is apparent that the success of his ballistic basic knowledge as well as the problematic of the theory of motion attracted this man, as he made the first attempt to bring to completion a generally useful combination of empirical knowledge and theory.[1]

Introduction

Charles VIII's invasion of Italy in 1494 led to the swift realization that the Italian cities and small states could only last a half-day under his cannon bombardment. It was a 'political and cultural trauma (well described by Machiavelli) that led to the discovery around 1500 of the bastion, the cannon-proof system of fortification.'[2] In 1494 the Italians 'saw gun-carriages for the first time. They saw gunners who had been trained in special schools, who were assisted by numerous personnel. And whom their fellow-soldiers held in honour. Instead of the iron guns firing stone or leaden balls to which they were accustomed they saw huge bronze 'cannoni' firing iron balls the size of a man's head.'[3] The Italian states learnt quickly; the importance of military culture to the Venetian state is shown by its establishment of *scuola dei bombardieri* where, by the time of the publication of Tartaglia's *Nova Scientia* (1537), bombardiers had been developing mathematical skills and instruments for some time.

The destructive effects of the rivalry between France and Spain for control of the Italian peninsula had a personal impact on the young Niccolò Tartaglia that he would carry with him for his whole life. He was brutally attacked by a soldier and left for dead at the age of twelve in the siege of Brescia (1512). Tartaglia's mother had

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[1] Leonardo Olschki, *Geschichte der Neusprachliche Wissenschaftlichen Literatur*, Book 3 *Galileo und seine Zeit* (Halle: Max Niemeyer Verlag, 1927), 78: 'Es ist offenbar, dass sowohl der Erfolg seiner ballistischen Grunderkenntnis wie die Problematik der Bewegungslehre diesen Mann anzogen, als er den ersten Versuch gemacht hatte, eine für alle Teile nützliche Verbindung von Empirie und Theorie zu vollziehen.' My translation.
[2] Mario Biagioli 'The Social Status of Italian Mathematicians 1450-1600' *History of Science* 27 (1989), 41-95, on 44.
[3] F.L. Taylor, *The Art of War in Italy* (London: Cambridge University Press, 1921), 89.

hidden him and his sister in the cathedral, but the French soldiers, fired up by the desire for vengeance because the city had refused to surrender, entered and attacked him, leaving him for dead. He received three blows to the back of the head (from which the 'brain cream' was visible), as well as being slashed twice on his face, one blow splitting his jaw and palate so he could not speak.[4] The family was not able to afford a doctor, and his survival was solely due to the exceptional maternal care that he received. As a result, however, he was left with a permanent stammer, which led to him being given the name of Tartaglia, which means the stammerer, and which he adopted as a badge of honour.

This horrifying experience instilled a hatred of war in Tartaglia which he expressed eloquently and movingly in the dedication to the *Nova Scientia*, and it may be the reason that he claimed never to have personally fired a gun. But military culture permeated Italian life and as a gifted mathematician Tartaglia could not resist the challenge that the new cannon technology opened up for mathematical investigation. Karin Ekholm notes that it was only at the end of the fifteenth century that cannon 'were placed on wheeled carriages, and founders began casting barrels with trunnions, the mounting lugs that allow barrels to pivot so that their inclination is adjustable'.[5] It was only with this innovation that the analysis of the angle to range relationship became important and this posed the challenge of determining why the range varies with the elevation of the cannon – 'the gunners' question'. This was indeed new and potentially powerful knowledge and explains why Tartaglia was so enthusiastic about explaining its importance to the Duke of Urban in his dedication to his *Nova Scientia.*

[4] Tartaglia, *Quesiti*, book 6 (1554), 69v: *in cadauna la panna del cervello si vedeua.*
[5] Karin J. Ekholm, 'Tartaglia's *ragioni*: A maestro d'abaco's mixed approach to the bombardier's problem' *British Journal for the History of Science* (2010), 181-207, on 202. See also MatteoValleriani, *Metallurgy, Ballistics and Epitemic Instruments,* 35-44.

Tartaglia was the first to describe mathematically the trajectory of a cannonball, and to insist that the whole trajectory is curved, contrary to the common opinion of the time. He proved that the maximum range is achieved at an angle of elevation of 45 degrees, though the proof was less than rigorous and the result could be roughly ascertained by practical trials.

Tartaglia's more important contribution to a science of motion is his vision of what *could* be achieved and *how* it could be achieved. He was the first to attempt to build a science of motion on systematic, axiomatic, mathematical foundations as the only basis for precision in gunnery practice, and in his first definition, of an equally heavy body (*corpo equalemente grave*), he attempted to abstract air resistance from motion, thus breaking decisively from the Aristotelian milieu theory which asserts that a projectile is kept in motion by the air.[6] The separation of motion from its impediments was the crucial prerequisite for its mathematical analysis.

Assured of his place in the history of mathematics both for his discovery of a general solution to the cubic equation, and for his dramatic and acrimonious dispute with the famous mathematician, physician, astrologer to the pope and compulsive gambler, Girolamo Cardano, until relatively recently Tartaglia has not been a prominent figure in the history of science, at least as far as English language scholarly publications are concerned.[7] Since beginning to write this chapter, however, two important works have appeared that not only make a highly significant contribution to

[6] Henryk Grossman directly links this key conceptual break, which ultimately led to the discovery of the law of fall, to the development of firearms where bombardiers experienced directly the effect of air resistance from repeated observations of cannon fire. See Henrk Grossman, 'Descartes and the Social Origins of the Mechanistic Concept' in Freudenthal and McLaughlin, *Social and Economic Roots,* 215.

[7] Gerhard Arend's *Die Mechanik des Niccolò Tartaglia: Im Kontext der zeitgenössischen Erkenntnis- und Wissenschaftstheorie* (München: Institut für Geschichte der Natürwissencschaften, 1998) situates Tartaglia within the intellectual and practical culture of sixteenth century Italy and is a comprehensive and systematic treatment of Tartaglia's mechanics and mathematics, but there has been nothing of this scope written about Tartaglia in the English language.

our understanding of Tartaglia's writings and his historical significance, but also provide an important source for future scholarly research.

The first is Matteo Valleriani's *Metallurgy, Ballistics and Epistemic Instruments: The Nova Scientia of Niccolò Tartaglia, A New Edition* (2013).[8] This is the first full English translation of Tartaglia's *Nova Scientia* (1537) with an introduction and extensive commentary. The second is *The Equilibrium Controversy* (2012) by Jűrgen Renn and Peter Damerow. The equilibrium controversy was a sixteenth century debate amongst mathematician-mechanics about whether a balance in equilibrium, after being deflected, will return to its original position. Through this controversy, an understanding of the positional effect of a force was clarified. Renn and Damerow provide a commentary on Tartaglia's exposition of the science of weights that he included in his second book, the *Quesiti et Inventioni Diverse* (1546), and they highlight his importance as one of the key protagonists to contribute to the spread of both ancient and medieval sources that provided alternative conceptual frameworks for understanding motion: the static Archimedian tradition, based on the concept of centre of gravity, favoured by Guidobaldo del Monte, and the alternative Medieval tradition of Jordanus' science of weights, based on the concept of positional heaviness, presented by Tartaglia in the *Quesiti*. It was from these conflicting conceptual frameworks that a new synthesis would emerge.[9]

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[8] Before Valleriani's translation, the first two books of the *Nova Scientia* had been translated by Stillman Drake & I. E. Drabkin in *Mechanics in Sixteenth-Century Italy*. Drake focussed solely on Tartaglia's mechanics and thus did not translate the third book on practical measurement, which, as Valleriani notes, prevents the reader from appreciating fully Tartaglia's integrative endeavour.

[9] Renn and Damerow, *The Equilibrium Controversy*, 71. For texts and editorial comment on the medieval science of weights see Earnest Moody and Marshall Claggett, *The Medieval Science of Weights* (Madison: University of Wisconsin Press, 1952); Joseph E Brown, 'The Science of Weights' in David C Lindberg, *Science in the Middle Ages* (Chicago; London: University of Chicago Press, 1978) gives an account of the history of the science of weights, its achievements, uncertain synthesis, and a presentation of key theorems; see also Pierre Duhem, *Les Origines de la Statique* (Paris: Librairie Scientifique A Hermann, 1905), especially chapter five on Tartaglia and the Jordanus school. On pre-classical mechanics as well as Drake and Drabkin (1969), see Paul Lawrence Rose and Stillman Drake 'The Pseudo-Aristotelian Questions of Mechanics in Renaissance Culture' *Studies in the Renaissance,* 18 (1971), 65-104; W R Laird 'The Scope of Renaissance Mechanics' *Osiris* 2 (1986), 43-68; W. R. Laird, in "Renaissance Mechanics and the New Science of Motion", in José Montesinos and Carlos Solís Santos, eds., *Largo campo di filosofare* (La Orotava: Fundacíon Canaria

Prior to the appearance of these works, recent reappraisal of the importance of the development of military culture to an account of the origins of early modern science had led to an increased interest in Tartaglia and significant new research. In his study of the intricate interplay between social class and social power in a changing society where old hierarchies had to adjust to new conditions, Mario Biagioli's 'The Status of Italian Mathematicians, 1450-1600' (1989), focuses on the role of patronage and class distinctions in Tartaglia's relationships with his interlocutors in his *Quesiti*. Serafina Cuomo's 'Shooting by the Book: Notes on Niccolo Tartaglia's Nova Scientia' (1997),[10] analyses the use of rhetoric in the *Nova Scientia* to illustrate the relationship between mathematics, mechanics and power.

Mary-Jo Henniger-Voss's 'How the "New Science" of Cannons Shook up the Aristotelian Cosmos' (2002), and her PhD thesis, brings a rich knowledge of Renaissance history and culture to bear in analysing how military demands brought about a transformation of science. She shows how Tartaglia's attempt to use Aristotelian physics to analyse the motion of a projectile subverted traditional conceptions of motion, creating a 'fertile confusion' from which the new science was able to emerge.[11] Karin Ekholm's 'Tartaglia's *ragioni*: A *maestro d'abaco*'s mixed approach to the bombardier's problem' (2010) links Tartaglia's different modes of reasoning in his *Nova Scientia* to the mathematical methods practised by the abacus

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Orotava de Historia de la Ciencia, 2001), 255-267, discusses how Galileo adopted the Aristotelian mechanical principle and used it to extend Archimedian mechanics; Egidio Festa and Sophie Roux 'The Enigma of the Inclined Plane from Heron to Galileo' in W R Laird and Sophie Roux, *Mechanics and Natural Philosophy before the Scientific Revolution* (Dordrecht: Springer, 2008) 195-222; Christiane Vilain, 'Circular and Rectilinear Motion in the *Mechanica* and in the 16th Century' in Laird and Roux, *Mechanics,* 149-172. For a discussion of problems related to disciplinary taxonomies of mechanics in the early modern period (machine mechanics versus the causal enquiry into motion) see Alan Gabbey 'Between *ars and Philosophia naturalis*: *reflections on the historiography of early modern mechanics'* in J.V. Field and Frank A. J. L. James (eds.) *Renaissance and Revolution: Humanists, Scholars, Craftsmen and Natural Philosophers in Early Modern Europe* (Cambridge: Cambridge University Press, 1993). 133-46.

10 Serafina Cuomo, 'Shooting by the Book: Notes on Niccolò Tartaglia's *Nova Scientia*', *History of Science* 35 (1997), 155-188.

11 M Henniger Voss 'How the "New Science" of Cannons Shook up the Aristotelian Cosmos', *Journal of the History of Ideas* 63 (2002), 371-397 on 374. See also: *Between the Cannon and the Book: Mathematicians and Military Culture in Sixteenth-Century Italy* (unpublished doctoral thesis, The John Hopkins University, 1995).

school teachers (*maestro d'abaco*), who addressed the growing demand for practical mathematical instruction from merchants and engineers.

Whilst these contributions have all been immensely important for widening our knowledge and understanding of Tartaglia and the cultural and political milieu from which he emerged, they have sidestepped Tartaglia's crucial evangelical role in propagandising for a programme for the integration of the theory of motion with its practical applications. Indeed, it is Tartaglia's very evangelism that appears to have encouraged an emphasis on rhetoric and personal ambition, and even intimations of charlatanism, in relation to the difficulties that he encountered in the pursuit of this programme.

This chapter examines the *Nova Scientia* and the *Quesiti* in the context of Tartaglia's programmatic aims; these aims emerge from his social position as someone who, because of his unique ability to cross social boundaries, was able in a very concrete sense to bring together the world of the bombardier and the engineer with that of the scholar and, indeed, those at the highest levels of power in society. In this sense he was the social embodiment of the endeavour to merge theory and practice, and he had the difficult task of negotiating the terrain between the necessity for immediate practical solutions to military problems and his belief that the only route to new inventions was through causal enquiry. The important technological changes in military technology enabled the cannon to become a crucial stimulus to philosophical and mathematical enquiry, and also a space within which an attempt to integrate theory and practice took place.

Consideration of the difficulties Tartaglia encountered in his mission to direct his readership to an understanding of how theory underlies successful practice and provides the key to the discovery of new inventions, have led me to reassess the relationship between the practical claims he made and how they relate to his theoretical results. A refusal to accept that Tartaglia simply used smoke and mirror techniques to cover over the inconsistencies in his ballistic theory has led me to question three assumptions that have been made by a number of historians. These assumptions have arisen from misconceptions of Tartagia's practical aims in relation to the means at his disposal. Firstly, I argue that there has been an underestimation of the importance of effect on a target in Tartaglia's ballistic theory. Secondly I examine Tartaglia's claim to have devised range tables that would enable the bombardier to predict ranges at any angle for any type of gun from one trial shot. Thirdly, I cast doubt on the contention of Voss, Ekholm, Cuomo and others that in response to criticism Tartaglia modified his views between the two works in relation to the nature of the trajectory and the question of mixed motion.[12] These examples are important if we are to gain an understanding of Tartaglia's integrative endeavour. Ekholm describes Tartaglia's use of different 'ragioni', or types of reasoning, as a 'patchwork'.[13] This is a useful metaphor that illustrates the creative inspiration he drew from diverse sources, but it might be more helpful to see this as part of a synthesising and integrating programme, the causal foundation of which was the science of weights, but which always had the integration of theory and practice as its goal.

Tartaglia's presentation of the science of weights in the *Quesiti* is crucial to an appreciation of the programmatic coherence of his two works and provides the link between them. He saw the science of weights as having the potential to provide a

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[12] The earliest source for this that I have found is René Dugas, '*A History of Mechanics'* translated by J.R. Maddox (London; Routledge & Kegan Paul Ltd., 1955), 96. It can also be found in: Alexander Koyré, 'La Dynamique de Niccolò Tartaglia' in *Études D'Histoire de Pensée Scientifique* (Paris: Presses Universitaires de France, 1966), 111; in Serafina Cuomo *Shooting by the Book,* on 165, note 30, and Paul Lawrence Rose, *The Italian Renaissance of Mathematics* (Geneva:Librairie Dross, 1975), 152: 'It is only in books I and II of the *Quesiti* that he overcomes his objection to mixed motion and arrives at the idea of a completely curved trajectory.'

[13] Ekholm, 'Tartaglia's *ragioni',* 200.

more robust causal theoretical framework for his new science than the one presented in the *Nova Scientia*, which relied largely on Euclid's mathematics and Aristotle's physics. Thus his different works should be seen in the context of his own quest for a deeper understanding of motion and its causes, and his use of his 'inventions' as persuasive illustrations of the practical benefits of his method.

In contrast to the emphasis on inconsistency and discrepancies between the two works, I show that Tartaglia's application of the science of weights to explain the angle to range relationship, and his insistence on the separation of violent and natural motion, complemented and supported his theoretical results in the *Nova Scientia.* Furthermore, his insistence that violent and natural motion cannot be mixed, points towards Galileo's solution to the problem based on the independence of the two separate components of the motion of the trajectory.

In contrast to A. R. Hall's view of the historical separation between the interests of practical gunnery and the theoretical study of motion, Tartaglia's whole endeavour can be seen as an attempt to integrate the disparate domains of the craftsman and the scholar. Moreover, he is important given current conceptions of the fusion of theoretical natural philosophy and practical/ technical abilities as the fundamental reorientation in natural enquiry that defines the emergence of modern science.

Tartaglia was obsessed with the new. Only new inventions interested him. He proudly proclaimed his new science, so it is appropriate to first interrogate what was meant by science in this period, and what Tartaglia really meant when he presented his new science of ballistics to the late Renaissance world.

What was Tartaglia's new science?

In the *Nova Scientia,* Tartaglia presents his new science, rather than defining it. If all five books of his proposed *Nova Scientia* are considered, the scope of his new science covered the whole practice of gunnery. [14] What he saw as new was not necessarily a specific scientific discovery, as was the case with Galileo's law of fall and the derivation of the parabolic trajectory, but his attempt to combine experience with mathematical reasoning and Aristotelian physics in a systematic axiomatic way for the perfection of military practice. This nuanced distinction may be at the heart of misconceptions of what Tartaglia's new science was claiming.

In the fifteenth and sixteenth century the term *scientia* referred to a multitude of disciplines, including the occult sciences. Tartaglia's iconic frontispiece (see Figure 1) illustrates the scope of the term. Tartaglia leads a chorus of classical female personifications, comprising *Arithmetica*, *Hydromantia, Geomantia, Architectura, Astrologia, Cosmographia, Necromantia, Presti[di]g[itat]io,* and *Sortilegio*. While Tartaglia did not specifically define his new science, in book eight of the *Quesiti,* devoted to the science of weights, he explains to Ambassador Mendoza that there are only seven 'simple' sciences, the 'seven liberal arts', and that the science of weights is a 'subordinate science or discipline' partly derived from geometry and partly from natural philosophy. [15] He continues that:

> by the power of this science, it is possible to know and to measure by reason the force and strength of all those mechanical instruments that were discovered by the ancients to augment the strength of a man for raising, carrying, or driving forward all the heavy weights… to know and measure by reason the force of a man…how to augment this infinitely… and thus it is possible to know the order and proportion of such augmentation in any manner…[16]

[14] The first two books contained his axiomatically based theoretical presentation. The third book was on measuring distances. In the fourth he had intended to give rules for the calculation of lengths of all shots made by a given cannon from the result of a given shot. In the fifth he had intended to set forth detailed information concerning the manufacture of gunpowder.

[15] The seven liberal arts make up the *quadrivium* and *trivium.*

[16] Tartaglia *Quesiti* (1554) 82v-83r, trans. Drake & Drabkin, *Mechanics in Sixteenth Century Italy,*111.

Figure 1. Frontispiece of the *Nova Scientia*

As W. R. Laird has noted, by ‘science of weights’ Tartaglia means theoretical, mathematical mechanics. Despite his familiarity with the works of Archimedes, it was the science of weights that had the most influence on Tartaglia’s mechanics. For example, although he was familiar with Archimedes’ statical proof of the law of the lever based on centres of gravity, he preferred the Aristotelian-Jordanus proof involving velocities and displacements. Laird suggests that perhaps because he found the latter more promising for a science of motion, and he was not able to reconcile the two traditions, which was achieved by Galileo.[17]

Deborah Harkness found that the word 'science' was widely used in Elizabethan England by vernacular writers on natural history, medicine, mathematics, instrumentation, mechanics and chemistry, both as a collective term and to denote individual sciences. *Science* denoted the study and manipulation of the natural world, and she distinguishes it from natural philosophy, which was:

> '… an elite set of interests, founded in Aristotelian and anti-Aristotelian currents, informed by the new scholarship of humanism, and practiced by gentlemen and scholars with free time and material resources required to contemplate nature.'[18]

Mary J. Voss favours the use of the term 'science' to describe the development of mechanics in the sixteenth century on the grounds that it cannot be categorised satisfactorily as natural philosophy, technology or mathematics. She argues that prominent exponents of mechanics such as Guidobaldo del Monte, who published the most important treatise on mechanics of the sixteenth century, reconfigured mechanics from an art into a science, by connecting philosophical concerns with concrete objects. [19]

What was understood as science therefore was undergoing substantial redirection and transformation. In his frontispiece, Tartaglia appears to be putting forward his new science of ballistics as an addition to the list of mixed sciences such as astromomy and architecture. But he consciously places himself at the head of the traditional sciences. This, combined with his emphasis on a generalised method and

[17] W R Laird, 'The Scope of Renaissance Mechanics', 53.
[18] Deborah Harkness, *The Jewel House* (New Haven: Yale University Press, 2007), xv.
[19] Mary J. Voss 'Working Machines and Noble Mechanics: Guidobaldo del Monte and the Translation of Knowledge' *Isis* 91 (2000), 234, note 1.

the bringing together of the theoretical with the use of instruments, the empirical, and the experimental, suggests that Tartaglia was consciously not just adding one new science to the list, but that this was about the emergence of science in a more general, modern sense; the *new* science was a science because it pursued causes as the necessary condition for real world solutions to mechanical and physical problems.

The impact of Tartaglia's new science

Tartaglia's works on gunnery were a unique hybrid that opened up the practical secrets and know-how of gunners to natural philosophical and mathematical investigation and they remained classics for the next century. Galileo had a copy of the *Nova Scientia* and the *Quesiti et invention diverse,* the latter heavily annotated, and there is evidence that Thomas Harriot read Tartaglia as part of his theoretical work on trajectories.[20] Tartaglia's social position as low-status, self-taught abacus school mathematics teacher (*maestro d'abaco*) who, through his renown and abilities, associated with the whole spectrum of the society of the Venetian republic, enabled him to cross traditional social and knowledge boundaries bringing together the separate domains of the classical works of mathematics, natural philosophy with the practical experience of the bombardier and the engineer.[21]

Tartaglia was a master of self-promotion and he recognised a gap in the market – the demand for theory, and it seems that his works were snapped up by an eager public. [22] His unique selling point was his ability to bring the mathematician's

[20] Jean-Jacques Brioist, and Pascal Brioist, 'Harriot, lecteur d'Alvarus Thomas et de Tartaglia' in Joel Baird and Sabine Rommevaux (editors) *Histoire des Sciences*, (Villeneuve d'Ascq: Septentrion Presses Universitaires, 2008), *passim*.

[21] He learnt to write the alphabet from A to K then his mother ran out of money to pay the teacher and he had to teach himself. See Niccolò Tartaglia, *Quesiti* (1554), 69v.

[22] For example Cuomo 'Shooting by the Book', 169-70 comments that ' "General public" is a convenient label to indicate the people who allowed *Nova Scientia* to go through six editions, the people who justified two posthumous editions of Tartaglia's works, the "great many" people who Giovann Battista Manassi describes as "laudably importune" in their eager requests for Tartaglia's works.' Galileo, Jacopo Foscarini, Giorgio Vasari and Giovann Battista Alleotti had copies.

and theoretician's eye to problems that were beyond the solution of the narrow view of the practitioner. Indeed he often boasted his lack of the need for or interest in the practitioner's direct experience, though his works actually provide evidence of considerable practical and empirical knowledge. He made the gunners' quadrant (*squadra)* famous with his clear drawings and detailed explanations of its use.[23] His works were selectively plagiarized and translated all over Europe both for their practical content as well for stimulating theoretical speculation.

Tartaglia's aim was the discovery of new practical knowledge and inventions. The study of motion through gunnery promised a huge payoff and Tartaglia's life output, including his hugely important publication of vernacular translations and commentaries on Euclid and Archimedes, can be seen as a personal campaign to show that the way to new inventions was through the study and emulation of these axiomatically-based classical works. Stillman Drake notes the importance of his publication of Euclid in the vernacular to the science of mechanics. Literacy at the time in Italy was very high and for the first time engineers and artisans had access to this 'principal treasury of mathematical reasoning'. Tartaglia's publication for the first time in Latin of Archimedes' *On Bodies in Water* and *On Plane Equilibrium* were of even more importance to the history of mechanics.[24]

Tartaglia responded to the interest and controversy aroused by his *Nova Scientia* with the publication of his *Quesiti et inventioni diverse.* The latter is

[23] Valleriani, *Metallurgy, Ballistics and Epistemic Instruments,* 9 and 43, provides conclusive evidence that the gunners' quadrant had been in use some 120 years before the *Nova Scientia* was published, and that gunners would have noted data on ranges at different angles on an *ad hoc* basis which would only have been of use for a particular gun. This provides the context for Tartaglia's claim to have discovered range tables that could be adapted for use with any type of gun.

[24] Drake & Drabkin *Mechanics*, 21. Tartaglia's publications were the *Nova Scientia* (1537); *Euclide Megarense* (1543); *Archimedes; Opera Archimedis Syracvsani philosophi et mathematici ingeniosissimi* (1543); *Quesiti et Inventioni* (1546, 1554), of which the first three books (on gunnery) were translated into English by Cyprian Lucar in 1588*; Regola generale da sulevare con ragione e misura no solamete ogni affondata nave intitolata la travagliata inventione* (1551), on raising sunken ships, which included the first Italian translation of the first book of Archimedes' *On bodies in Water,* translated into English by Thomas Salusbury in his *Mathematical Collections,* (1661)*; Trattato di Numeri et Misure,* which included an Italian translation of the first book of Archimedes' *On the Sphere and the Cylinder* (1556, 1560).

effectively a diary, written in dialogue form over a period of about twenty years cataloguing his responses to questions from a wide range of interlocutors ranging from the plain bombardier to the illustrious Francesco Maria I della Rovere, the Duke of Urbino (1490-1538), and the Spanish Ambassador Don Diego Hurtado de Mendoza (1503-1575). The Duke of Urbino was considered the leading expert in military matters in Italy at the time, with a wide knowledge and experience in mathematics, military architecture and engineering.

At the time that he wrote the *Nova Scientia* in 1537, Tartaglia had become famous for his discovery, in 1535, of the general solution to the cubic equation. Yet rather than go on to make further discoveries in the solution of algebraic equations, as Cardano did after he had persuaded Tartaglia to reveal his secret method, he turned to research into ballistics and then to his vernacular translation of Euclid. By his own account in his dedication to the Duke of Urbino in the *Nova Scientia,* Tartaglia's interest in ballistics had been prompted some years previously by a question posed to him by a bombardier friend who worked in the Castel Vecchio in Verona, in 1531, but he threw away all his work on this subject when he realized that 'working toward the perfection of such an art, harmful to the neighbor or even destructive for the human species and especially for the Christians because of their continuous wars, was a reproachful, vituperative and cruel thing, worthy of heavy punishment by God and by human beings.'[25]

The changing political situation was the reason Tartaglia gave for his change of heart. In 1538 the configuration of state forces took the form of a Holy Alliance between Venice, allied to Charles V and the Papacy, against France and the Ottoman Empire. A special bond of mutual esteem had developed between the Emperor and

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[25] Tartaglia, *Nova Scientia,* fol.4r (unnumbered). Translation in Valleriani, *Metallurgy, Ballistics and Epistemic Instruments*, 79.

the Duke of Urbino and as Gerhard Arend recounts, the Duke, already well-known as a Turk-hater and vassal of the pope, nurtured a bold battle plan that was designed to repeat the triumphant campaign of Charles V in 1535 against Algiers with the ultimate aim of winning back Constantinople for Christendom. As part of his personal preparations he sought technical discussions with all available military experts including Tartaglia. This was the immediate cause of Tartaglia's writing the *Nova Scientia* and of its dedication to the Duke.[26] Tartaglia's reference to the threat of the 'wolf at the door' would have appealed to the Duke and provided justification for overriding his moral objections to the use of science for such destructive ends.

Unfortunately for the Duke, neither the Emperor, nor his appointed commander of the imperial fleet, Andreas Doria, a Genoese, were unequivocally enthusiastic about the idea of Venice having unchallenged hegemony over the eastern Mediterranean, and the ensuing battle at Prevesa was pursued with little enthusiasm, ending in defeat for the Holy Alliance. The Sultan made repeated peace offers to the Venetians, and the Duke was finally forced to advise the Venetian Senate to agree to discuss terms. The Duke's last stay in Venice gave him the opportunity to have the final conversation with Tartaglia that was recorded in the *Quesiti,* where he ends by saying: 'You have defended well your reasons, and have sayd enough for this tyme, and at my returne from *Pesaro* I will cause your said Inventions to be proved.'[27] However, on his return from the Adriatic to Venice in September 1538 he was taken ill, probably due to poisoning, and died soon afterwards. On hearing the news, Sulliman II ordered celebrations of joy to mark the event.[28]

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[26] Arend, *Die Mechanik*, 103-106.
[27] Tartaglia, *Quesiti* (1554), 12v. Translated by Cyprian Lucar, *Three books of colloquies concerning the the arte of shooting* (London: John Harrison,1588),14.
[28] Arend, *Die Mechanik,*106.

Though both the Duke of Urban and Don Diego Hurtado de Mendoza (Spanish ambassador to Venice from 1539 to 1546) were loyal vassals of Charles V, in character and inclination there could not have been more contrast between them. An illustration of this difference is that when peace was finally concluded Suleiman II sent Mendoza six boxes of Greek manuscripts and grain supplies for the starving Venetians in gratitude for Mendoza's return of a captured prisoner.

Mendoza had realised that the war was disastrous for the Venetian republic, which suffered an unprecedented famine as a result of being cut off from the grain of its colonies and trade with the Turkish market. Yet he was instructed by the Emperor to do all in his power keep the Venetian republic in the alliance. Mendoza's loyalty to the Emperor, who valued, but did not always heed, his ambassador's advice, often left him both morally compromised and in danger of his life.

The cost of bribes and bodyguards, together with the expense of building his vast manuscript library, left Mendoza in such debt that his health began to suffer from the pressure, so it seems likely he was not able to pay Tartaglia particularly highly for his mathematics tuition. But far more important for Tartaglia was Mendoza's generosity in providing scholars with the benefit of access to his vast manuscript library, from which Tartaglia obtained the Moerbeke Archimedes that he published in 1543, and the manuscript of Jordanus's *De ratione ponderis,* which formed the basis for his exposition of the science of weights in the *Quesiti,* and which was published in full posthumously.[29]

Mendoza had been educated in Rome in the 1520s and he had come under the influence of a secular Aristotelianism studied there since the fourteenth century under

[29] Drake and Drabkin, *Mechanics in Sixteenth-Century Italy,* 23.

the guidelines of the commentaries of Averroes.[30] He later translated the Pseudo-Aristotlian *Mechanical Questions* into Spanish, and Tartaglia's account of their conversation in which Mendoza asks his opinion of this work, which I describe below, thus has a ring of authenticity. Mendoza's preference was for reading and study and his discussions with Tartaglia must have provided him with a welcome respite from his cloak and dagger political manoeuvrings and official business.

The importance of his relationship to Mendoza leads one to doubt Biagioli's suggestion that the real reason that Tartaglia entered into his dispute with Girolamo Cardano was because Cardano promised to introduce him to a rich patron in exchange for his secret of the solution to the cubic equation, which he failed to do, thus breaking the rules of 'gift exchange'.[31] Cardano broke his word of honour to Tartaglia not to publish the solution to the cubic equation or tell anyone. The dispute was about honour not patronage, and honour *was* important to Tartaglia. If it had been about an exchange of 'gifts', Tartaglia would have owed Cardano a big favour, since it seems it was he who introduced Tartaglia to Mendoza, noted in one of his letters published in the *Quesiti.*[32]

It is these two patrons, Mendoza and the Duke of Urbino, that link the *Nova Scientia* to the *Quesiti* through three key themes, the importance of the *squadra* as an instrument for investigation of the range to angle relationship, which Tartaglia describes to the Duke in the dedication to the *Nova Scientia*, the science of weights that he discusses with Mendoza in books seven and eight of the *Quesiti* and his explanation to the Duke in book one of the *Quesiti* of how the science of weights provides a causal explanation of the angle to range relationship.

[30] See Erika Spivakovsky's engaging biography of Mendoza, *Son of the Alhambra* (Texas: University of Texas Press, 1970), 40.
[31] Biagioli, 'Social Status', 55.
[32] Tartaglia *Quesiti* (1954) Book 9, 123r Letter from Cardano to Tartaglia 4 August, 1539 'Ve aviso anchora qualmente io indrizzai da voi il signor Don Diego de Mendocia Ambasciatore della maesta dell'Imperiore, qual se diletta di queste scientie, qual penso non vi sara inutile, & gli dissi dell'altezza delle virtu vostre, come meritati.'

The cognitive importance of the range to angle relationship

Jim Bennett's work has drawn attention to the importance of the development of instrumentation for its dual practical and natural-philosophical value, and the role of the mathematical practitioner in this important fusion of functions.[33] Tartaglia was the first to recognise the importance of the role of the gunners' quadrant (*squadra*) in opening the way to the theoretical treatment of the trajectory. It was the *squadra* that Tartaglia prioritised as the key to his new science, both in the *Nova Scientia* and in the *Quesiti.*

It is easy with hindsight to take for granted what was new in the *Nova Scientia*. The cognitive significance of recognising the relationship between the two variables of range and angle and the way range first increases and then decreases continuously against angle is easy to overlook. And the discovery of this relationship goes hand in hand with an understanding of the necessity for holding other variables such as velocity and air resistance constant to analyse these effects.

Voss astutely notes that projectiles thrown by hand or by a bow depended on the strength of the men involved, whereas a machine demonstrates a certain amount of regularity and thus, I would add, the promise of controllability.[34] This created a dream of precision, and illustrates the crucial importance of the impact of cannon technology. Now, in theory at least, all the variables were measurable and controllable. No matter how difficult it proved in practice, the dream remained.

In order to achieve this dream, those who wished to improve precision in gunnery were forced to consider the question in terms of essential variables that had to be controlled or abstracted in order to isolate the action of angle upon range. This

[33] Bennett, J. A. 'The Mechanics' Philosophy and the Mechanical Philosophy' *History of Science, 24* (1986),1-28.
[34] Voss, 'How the New Science of Cannons shook up the Aristotelian Cosmos', 384.

abstraction was crucial to the development of the science of mechanics. To his surprise and delight, from his reasoning Tartaglia realised that the same range could be reached at two different angles of elevation, as he explained to the Duke of Urbino in the preface to the *Nova Scientia*:

> Besides this, I found with a very evident argument that a piece of artillery can hit one place along two different paths (or at two different elevations) and I found the method of how to execute this in reality, a subject never heard or conceived by anyone else, ancient or modern. [35]

Tartaglia's proof of this discovery, with its complement that the maximum range is achieved at an angle of forty-five degrees has led Serafina Cuomo to complain that Tartaglia:

> …really seems to choose whatever most fits his purpose, regardless of an epistemologically 'coherent' vision of mechanics…
> The point is that Tartaglia's mechanics is less about challenging, or adding to mainstream natural philosophy than about claiming a role - for the discipline itself and obviously for its author… It is in fact he who decides, case by case, which mental faculty should be listened to, or which stool the reader should sit on.[36]

What Cuomo sees as evidence of Tartaglia's attempt to 'claim a role', I see as evidence of his struggling with important and difficult epistemological questions.[37] Cuomo discusses a number of examples of this, including Tartaglia's admittedly lengthy and tortuous argumentation in proposition VIII of book 2 of the *Nova Scientia*, which forms the basis of his crucial theoretical proof that the maximum range is at 45 degrees. He bases his argument about the angle of maximum trajectory on the 'physical argument' that anything that passes from the less to the greater, and through everything in between, necessarily passes through the equal. [38] Recognising

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[35] Tartaglia, *Nova Scientia* (1558) fol.3v (unnumbered), translated by Valleriani, *Metallurgy, Ballistics and Epistemic Instruments*, 75.
[36] Cuomo 'Shooting by the Book', 166.
[37] Ekholm 'Tartaglia's *ragioni'*, 205.
[38] Ekholm, 'Tartaglia's *ragioni',* 188, suggests that 'physical and geometrical reasoning' entails knowledge acquired through the senses that can be measured and compared' She adds this suggestion to that of Mary J Voss that 'physical reasoning' was 'sometimes educated guesses, sometimes sharp analysis of common sense, and

that the validity of this argumentation is problematic, he discusses cases where the argument he uses is valid or invalid.[39] That he is making such considerations is, I would argue, of more consequence than any flaw in his argument. Critics such as Guidobaldo del Monte who attacked Tartaglia's 'barbaric proofs' missed what was daring and new about Tartaglia's insights.

Another example of Tartaglia's arbitrariness cited by Cuomo is when he takes part of the trajectory as straight even though he admits it is 'insensibly' curved, which will be discussed in a later section. She continues that:

> …in a sense, *Nova Scientia* … is *itself* an instance of control over phenomena, in that the author has faculty [sic] to choose between different explanations with no apparent fixed guiding principle apart from that of reducing or eliminating the factor of untamed chance in finding the best inclination.[40]

The whole purpose of the exercise was the elimination of chance in aiming, yet Cuomo implies there is something wrong with this. She concludes:

> Thus, the significance of the book on the one hand lies in its claim that it can actually help the leader manage his troops, or the gunner his shots, while on the other the internal dynamics of the text itself offer a model of control and an image, as it were, of leadership.[41]

Cuomo portrays Tartaglia as a manipulating charlatan whose main aim is his own self-aggrandisement. Excessive focus on apparently contradictory aspects of Tartaglia's theoretical results has diverted attention from his overall mission and his ongoing search for the causes of motion. However, as Voss has noted, Tartaglia pursued research into precisely those questions that would prove to be central aspects of Galileo's research programme – the problems of motion opened up by cannon

sometimes the concepts taken over from natural philosophers' (Voss, 'How the New Science of Cannon Shook up the Aristotelian Cosmos', 382).

[39] The two examples involve discussions of an intricate geometrical question involving 'horned' angles and a discussion of the existence of a point of equality between the length of day and night during the year.

[40] Cuomo, 'Shooting by the Book', 166.

[41] *ibid.,* 166.

warfare, balances, inclined planes, and hydrostatics.[42] Despite this insight, Voss's assessment of Tartaglia echoes that of Cuomo. She notes his

> muddled use of his sources, his redefinitions, and his extravagant claims for the ability of mathematics to deal with the quintessential problems of artillery warfare (knowing how best to aim a gun), make sense only in the world from which Tartaglia came: a world of cannons and of intellectuals who vied with their books and mathematical practices to gain some entry to the patronage of those leaders that orchestrated the use of cannons… All that makes his work coherent is its consistency with the goals of an artillery science. The ambitious mathematics teacher brought the terms of "resistance" and "weight " into a particularly complex state of confusion…Tartaglia's new science was much more self-serving than coherent… [43]

Voss, in the end, like Cuomo and others, reduces Tartaglia's pioneering efforts to apply theory to practice as evidence of his clumsy attempts to gain patronage.

The suggestion that Tartaglia, like Galileo, was desperate to promote himself by claiming that his theoretical input validated his professional aspirations is a plausible but not sufficient explanation either of Tartaglia's social context or his scientific goals. Tartaglia can easily be placed in the context of the Renaissance struggle by the talented and skilled to improve their social standing by allying themselves with classical revivalism, as evidenced by Tartaglia's editions of classical texts. Yet in doing so we risk losing sight of the fact that he was a scholar and exceptionally talented mechanic as well as a lowly, but gifted, mathematician. Whatever Tartaglia's professional aspirations, they neither undermine his quest to unite theory with practice, nor his genuine abilities, skills and knowledge, that gained him patronage and a ready public. Tartaglia seems to have found a niche or gap in which he could construct a distinct category for his talents similar to those artist engineers such as Brunelleschi and Leon Battista Alberti who had successfully introduced the theory of proportions and perspective to create a 'scientific conception

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[42] Voss 'How the "New Science" of Cannons Shook up the Aristotelian Cosmos', 393.
[43] *ibid.*, 389.

of art'.[44]

The fact that Tartaglia managed to promote his skills successfully is not a sufficient reason to single out his self-publicising and his need for support for his scientific programme. Rather we should see him as one who embodies the interplay of skills, instruments, understanding, and successful patronage, an interplay that has significant historical importance for the whole issue of delineating the characteristics of modern science; to eliminate or dismiss one of these would weaken our understanding of a crucial historical juncture that is illustrated admirably in Tartaglia's new science. Tartaglia was struggling to unite distinct domains using the as yet inadequate cognitive tools available to him. That is why it is necessary to examine carefully how he related his practical claims to his theoretical means.

Unifying disparate domains: the structure of the *Nova Scientia* and Tartaglia's central dilemma

An understanding of how the theoretical relates to the practical in the *Nova Scientia* is essential to working through a number of misconceptions and questions about Tartaglia's theoretical results. The letter of dedication to the Duke of Urbino is unusual in that it serves a pedagogical as well as a dedicatory purpose. It is as much an initial chapter as a dedication, giving an illustrated explanation of the use of the *squadra* and its military significance in opening the way to a scientific approach to more accurate shooting. But in addition it serves to prepare and to persuade the reader of the benefit of the deeper theoretical knowledge provided by books one and two. The third book is then devoted to measurement of distances and heights, to provide a more integrated approach to theory and practice. As Tartaglia explains:

[44] Paolo Rossi *Philosophy, Technology and the Arts in the Early Modern Era*, trans. Salvator Attanasio (New York: Harper & Row, 1970), 19.

> Therefore there are two fundamental subjects necessary to the real bombardier (if he does not want to shoot casually, but with cognition) and one subject without the other is not really useful (I say this concerning long shots). The first thing is that he has to be able to find out and investigate (by sight) the distance to the place he needs to shoot. The second is that he needs to know the quantities of the shots of his artillery according to the various elevations.[45]

The structure of the *Nova Scientia* illustrates the central dilemma that Tartaglia had to negotiate. Valleriani notes the particular difficulty that Tartaglia experienced in the third book as he grappled with the linguistic and explanatory demands both of providing written instructions for complex practical activities, including the construction of measuring instruments, and simultaneously attempting to integrate his theoretical explanations, with direct references to the relevant sections of Euclid, all without losing the attention of the unlearned among his intended readership. At one point he actually gives up and says he could provide simpler methods but that he has decided to only provide them verbally because it would be too complicated to try to put then in written form.[46]

Tartaglia's worry about the abstract nature of both his mathematical approach (in the *Nova Scientia*) and his mechanics (the science of weights, in the *Quesiti*) and the over-empirical interests and requests of his interlocutors is the central methodological problem for him. He has to reprogramme his interlocutor's wishes to bring to the surface what they need; this is in keeping with the message of the frontispiece, a visual representation of his programme that the only route to knowledge is first through Euclid, and at a higher level, Aristotle and Plato.[47] There are no short cuts, as is indicated by the man with the short ladder who is trying to scale the wall of the citadel of knowledge rather than pass through the gate of Euclid.

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[45] Tartaglia, *Nova Scientia* (1558), fol.4r (unnumbered), Trans. Valleriani, *Metallurgy, Ballistics and Epistemic Instruments,* 77. Valleriani notes that to 'know the quantities' means to calculate transits and ranges (the transit is the interval along a straight line from the beginning to the end of the violent motion). Alternative translation, Drake & Drabkin *Mechanics in Sixteenth Century Italy,* 67.
[46] See Valleriani, *Metallurgy, Ballistics and Epistemic Instruments,* 23-29, for an analysis of the third book.
[47] See Figure 2.

Tartaglia's conviction on this matter and its centrality to his programme is undermined by interpretations that focus on other rhetorical purposes.[48]

His own story of his quest indicates to the reader the path that must be taken; after he has solved the bombardier's problem, he says

> I started (not without reason) to investigate the kinds of motions that take place when a heavy body is involved. I found there are two kinds [of motion], the natural and the violent. I also found that, in reference to their accidents, they are completely contrary to each other because of their contrary effects.[49]

The contrary actions are explained by Aristotle's theory of motion: violent motion starts fast and gradually diminishes, while natural motion starts from zero and continuously increases. It should be noted that when Tartaglia talks about Aristotelian motion it is in relation to the different effects of the two types of motion, not to the solution to the bombardier's question about the relationship between the angle and range of the cannon. The importance of effect on a target and its relationship with the Aristotelian distinction between violent and natural motion, has been ignored by most historians but it is crucial for understanding what Tartaglia saw as the practical payoff of his theoretical work.

The trajectory, proportion and eeffects

Most historians from A. R. Hall's *Ballistics* onwards have considered Tartaglia's trajectory solely in terms of the problem of determining the range from his trajectory, but close reading of the *Nova Scientia* shows that his actual claims were more modest and specific. The title page of the *Nova Scientia* explains that the first book demonstrates theoretically 'the nature and effects of equally heavy bodies in the two contrary motions that may occur in them, and their contrary effects.' The second

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[48] See, for example, Cuomo 'Shooting by the Book', 157ff.
[49] Tartaglia, *Nova Scientia* (1558) fol.3v (unnumbered), trans. Valleriani, *Metallurgy, Ballistics and Epistemic Instruments,* 73. Valleriani notes that according to Medieval Aristotelian terminology 'accidents' refer to 'qualities'. See also Drake & Drabkin, *Mechanics in Sixteenth Century Italy,* 65.

geometrically proves and demonstrates 'the similarity and proportionality of their trajectories in the various ways that they can be ejected or thrown forcibly through the air, and likewise the [proportionality] of their distances…'. Thus Tartaglia makes clear that he has two specific goals in mind: the results relating to proportionality of shots, and the effect on a target.

In the dedication, after referring to his result on the proportionality of shots (proposition VII) he goes on to explain the purpose of his final result, the culmination of Book II of the *Nova Scientia*. This is proposition IX and in the dedication he makes it clear that the result of this proposition - that the straight part of the trajectory at 45 degrees is about four times point blank range - is about effects. Based on his discovery by 'natural arguments'[50] that a cannon at 45 degrees shoots about ten times further than at point blank, he explains:

> On the basis of this evidence, Magnanimous Duke, using means of geometric and algebraic arguments, I found that a ball thrown along the mentioned 45 degrees above the horizon moves along a straight line which is about four times the straight line along which a ball moves when thrown parallel to the plane of the horizon, called by the bombardiers (as I said) shooting at the blank point. From this, it also becomes clear that a ball thrown by the same artillery follows a longer straight line in a certain way than in others, and, consequently, produce more [destructive] effect. [51]

The practical implications of this result are explained more fully in the *Quesiti* where, in the very first question from the Duke of Urbino, Tartaglia directly refers back to his final result of the *Nova Scientia*. The Duke's question is whether there is greater effect at point blank at a shorter distance or shooting from below from a longer distance. Tartaglia astonishes the Duke by asserting, based on his theoretical results, that the latter would have the greater effect, since the straight part of the trajectory

[50] Tartaglia, *Nova Scientia* (1558) fol.3v (unnumbered) trans. Valleriani *Metallurgy, Ballistics and Epistemic Instruments,* 73. Valleriani notes that in the 1537 edition 'natural arguments' (ragion naturale), was 'Archimediane'

[51] Tartaglia, *Nova Scientia* (1558), fol.3v (unnumbered) trans. Valleriano, *Epistemic Instruments,* 73. Alternative translation in Drake & Drabkin *Mechanics in Sixteenth Century Italy,* 66.

was four times longer at 45 degrees. Depending on the distances, this angle had the potential to hit a target with greater force from a longer distance. It is important to note that when he talks about his main theoretical result he does not relate it to finding the range, but only to the impact on a target. [52]

In practical terms, the impact on a target was obviously as important as whether or not one could actually hit it. The question of the effect on a target took on a particular importance with the development of new rules for fortifications in the sixteenth century, and the development of the bastion. Valleriani notes that:

> …the bastion could and had to be destroyed following a very precise strategy. For example, first of all, the lower and upper defenses of the bastion itself had to be destroyed.
>
> This meant that entire batteries of cannons had to concentrate their fire on one or a few chosen points whose distance and height had to be calculated with precision.[53]

This helps to explain why the Duke was so interested in the question of effect at different angles of elevation and why Tartaglia saw proposition IX as the culmination of his theoretical achievement.

Tartaglia's elusive range tables

This specificity of goals and results in terms of means available also applies to Tartaglia's promised range tables. The fourth book of the *Nova Scientia* was intended to give rules for the calculation of lengths of all shots made by a given cannon from the result of a given shot. Stillman Drake considered this 'a project way beyond Tartaglia's powers'.[54] This assumes that Tartaglia's whole enterprise in deriving his trajectory was for the purposes of deriving range tables. Later, Drake notes the contradiction between Tartaglia's different drawings in book I and his final

[52] Arend emphasised the importance of effects in Tartaglia's ballistics. See Arend, *Die Mechanik,* 186.
[53] Valleriani, *Metallurgy, Ballistics and Epistemmic Instruments*, 37.
[54] Drake & Drabkin, *Mechanics in Sixteenth Century Italy,* 63, note 1.

proposition in book II – and concludes that "[t]he difficulties implied here (empirical knowledge versus attempted mathematical representation) precluded his development of general ballistic formulae.'[55]

A possible reason for this assumption is a reading backwards of history from Galileo, whose object was to discover the true trajectory and who did derive tables from it. Galileo was not the first to derive the range mathematically from a geometric trajectory. Daniel Santbech's triangular trajectory also enabled ranges to be derived from mathematical formulae, and he was probably the first to attempt it.[56] However, there is reason to doubt that Tartaglia ever had any intention of using his theoretical trajectory alone to derive range tables, but rather his secret tables were probably derived from empirical data and proportional reasoning.

In the dedication of the *Nova Scientia,* after having explained that the result of proposition IX was about *effects,* Tartaglia says that by *calculation* he found the proportion of the increase and diminution of shots made by every piece of artillery when it is raised or lowered over the horizontal plane. He continues, 'Similarly, I also found the method of how to ascertain the characteristics of the mentioned shots in each piece, both large and small, solely on the basis of the information concerning one single shot (provided the piece is always charged in the same manner).'[57] Indeed he goes on to say that he went on to investigate the proportion and order of shots in a mortar too, 'and, similarly, I found the method of how to ascertain the characteristics of the mentioned shots in a short time on the basis of the information concerning one

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[55] Drake & Drabkin *Mechanics in Sixteenth Century Italy,* 66, note 9. Voss, in 'How the "New Science of Cannons Shook up the Aristotelian Cosmos', 384, states: 'From one shot, then, Tartaglia claims he can calculate distances that a single cannon will shoot at any elevation or charge by calculating out the circular arcs that lie between the straight portions of the trajectories.' Whilst one cannot rule out the possibility that this was his method, I have not yet found anywhere in the *Nova Scientia* where Tartaglia makes this specific claim.

[56] Andreas Kleinert, 'Zur Ballistik des Daniel Santbech' *Janus* 63 (1976), 47-59, on 49.

[57] Tartaglia, *Nova Scientia* (1558) fol.3v (unnumbered) translated by Valleriani in *Metallurgy, Ballistics and Epistemic Instruments*, 75. Alternative translation, Drake and Drabkin, *Mechanics,* 66.

single shot.'[58] That Tartaglia mentions tables for different types of gun seems to confirm the empirical basis for his derivation of tables.

The fact that Tartaglia inserts information about the 1:10 ratio of point blank to maximum range into his final proposition is further evidence that he has been doing empirical work and numerical calculations on the proportions between ranges at different elevations. The culmination of his theoretical results (see figure 2, proposition IX) would appear to be totally reliant on a piece of empirically derived knowledge. Though historians of science have debated about the source of this ratio, which he originally said was based on 'Archimedian arguments' (later changed to 'natural arguments') there seems no other way he could have obtained this input other than empirically.[59]

To summarise, it seems likely that Tartaglia used empirical data from practical trials for two practical results. Firstly the 1:10 ratio of point blank to maximum range was the essential starting-point for his culminating proposition, the purpose of which was to derive a conclusion relating to effect on a target. Secondly, he used results from practical trials to derive range tables using proportional reasoning and calculation, which he did not publish.

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[58] Tartaglia, *Nova Scientia* (1558) fol.3v (unnumbered) translated by Valleriani in *Metallurgy, Ballistics and Epistemic Instruments*, 75. Alternative translation, Drake and Drabkin, 66.

[59] For a discussion of possible reasons for this change see Eckholm 'Tartaglia's *ragioni'*, 193-7. Ekholm uses 'Archimedian' to 'physical reasoning'.

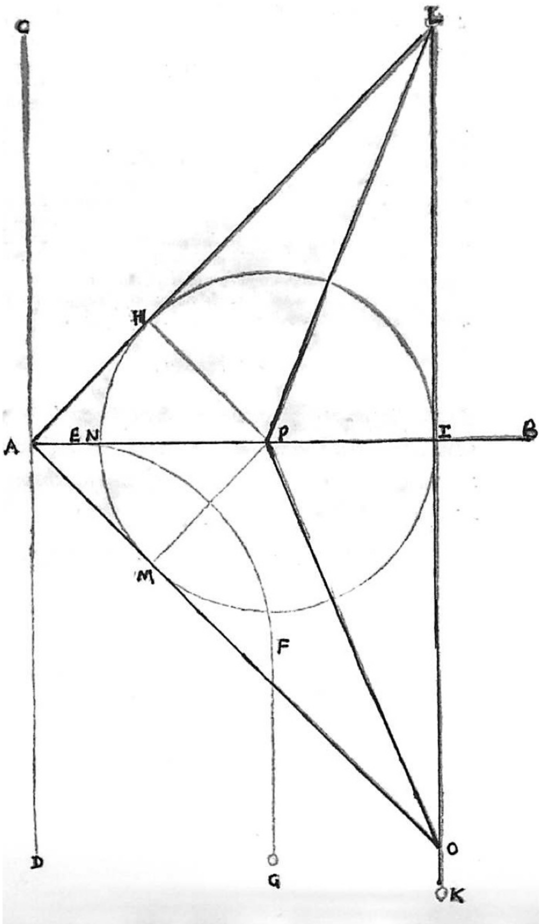

Figure 2. Tartaglia's diagram for his final proposition of the *Nova Scientia*. The violent section of his trajectory consisted of a straight part followed by the arc of a circle, and the natural part was perpendicular to the earth and thus coincided with the point of maximum range. Violent motion gradually decreases and thus the weakest effect is at the end of the circular portion, where natural motion takes over. The starting point of the construction is the given ratio AE to AI (1:10), the ratio of point blank to maximum range. From this ratio the rest of the construction is defined from the two tangents AL and LI that form the top triangle and the symmetry of the construction. From this construction, using Euclidean geometry (mainly similarity of triangles and Pythagoras), and algebra, Tartaglia is able to show that the straight part of the trajectory AH at 45 degrees is about four times the point blank range AE.[60]

Tartaglia's later comments in the *Quesiti* suggest that by this time he had changed his mind about publishing his tables. At the end of the first conversation in the *Quesiti* Tartaglia attempts to reassure the Duke that he would be able to keep them secret from his own gunners, just as 'the servants of Apothecaries which continually compound medicines according as they are appointed by Phisitions to doe and learne

[60] Ekholm, in 'Tartaglia's *ragioni'* provides a full explanation of the geometric and algebraic proof of this ingenious proposition.

not thereby to be Phisitions.'[61] Tartaglia would not have reassured the Duke on this issue if he had intended to publish them for everyone to see. And the Duke's question is a pertinent one. There would be no point in having tables that a gunner could simply take with him if he decided to go to work for another ruler.

Whatever the reasons for not printing, Tartaglia in both books is very confident about his knowledge, which is inconsistent with its being an impossible task. Drake's supposition about the range tables just makes Tartaglia look either delusional, for not seeing the apparently obvious contradictions in his own work or dishonest for making false claims, or both. It is even possible that Tartaglia did indeed pass on his tables to the Duke. This is supported by Valleriani's comments on the practice of data collection by gunners for their own guns, and the fact that rulers would commission their military engineers to compile useful data which would not be for public consumption.[62]

In discussing his tables, Tartaglia explains to the Duke in the *Quesiti* that the difference between the range at the fifth and the range at the sixth point 'doe so little differ, as that upon any small advantage happening either by force of powder, or by any other meanes, the peece being mounted at 5 points will shoote so far as it can doe when it is mounted at 6'[63] This is further evidence of his empirical knowledge and that he had worked out the proportional increase for each elevation from the 'particular experiments' that he mentioned in the *Nova Scientia*, since examination of range tables shows that the range to elevation relationship increases at first in an almost linear fashion but begins to flatten off as it reaches 45 degrees; in the wager that he describes in the dedication to the Duke, where he proves by practical trial that

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[61] Tartaglia, *Quesiti* (1554) 7r. Translated in Lucar, *Three bookes of colloquies,* 4.

[62] For example, the Grand Duke of Tuscany, Ferdinando II, asked his chief engineer, Vincenzo Viviani to provide him with a *tariffa,* or charge table relating the range of shot to charge of powder, based on experiments he had made with a four-pounder (*saltamartino*). See W.E. Knowles Middleton, *The Experimenters: a study of the Accademia del Cimento* (Baltimore: Johns Hopkins Press, 1971), 315.

[63] Tartaglia, *Quesiti* (1554), 6v, trans. Lucar, *Three bookes of colloquies*, 4.

the longest range is at forty five degrees, Tartaglia tested the range at point four and point six, not point five and six, suggesting he was aware that at point five he may not have got the result he wanted. Also he was unhappy that the results did not give quite as clear a difference as he had predicted, even though he won his bet.

Though problematic in practice, with this empirical knowledge it is possible in theory to work out the range for any piece and shot from one trial, assuming the proportionality of shots that Tartaglia proves. Nathaniel Nye, an experienced young mathematician/gunner famous for his masterminding of the Siege of Worcester in the English Civil War explains how it can be done from firing trials in his *Art of Gunnery*. He certainly read Tartaglia, but by this time range tables were provided in other works. [64] Diego Uffano (1613) actually gives proportions that enable a gunner to work out the ranges from one point blank trial as 244/200R, 287/200R, 329/200R etc**.** [65] Though William Bourne was pessimistic about the possibility of devising accurate range tables, as I will show in chapter three, he nevertheless did give some simple proportions that Thomas Harriot used to test how well his theoretical results matched experience.

Tartaglia**,** contrary to later assumptions, was not committed to proving everything through his theoretical trajectory. His approach was eclectic and pragmatic. His final proposition of the *Nova Scientia,* which creatively combines empirical data, Euclidean geometry and algebraic reasoning, is a fitting tribute to his integrative endeavor. His theoretical work was an exploration of where his mathematics, the physics of Aristotle and any other reasonings and experimental data he could bring to bear could take him in deriving useful results.

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[64] See Nathaniel Nye *The Art of Gunnery (*London: William Leak, 1647), 35. Nye recounts Tartaglia's 'little dog' story in discussing how trajectory varies with repeated firing as the gun gets hotter. The poor little dog got sucked into a cannon due to the force that acted like a cupping glass. Tartaglia, *Quesiti,* question 21, 24r-24v. Lucar translation of the *Quesiti,* Book I, 39.
[65] Hall, *Ballistics*, 46. R is equal to the point-blank range.

The *Nova Scientia* appears to be a very different work from the *Quesiti* because of the abstract nature of the first two books, but this is partly because the third book, which presents instruments for measuring distances by sight, has been largely ignored, and had not been translated into English before Valleriani's recent translation. Originally there were to have been five books, the fourth presenting firing tables and the fifth was to have been a discussion of the characteristics of flammable materials, though the *Quesiti* did take up some of these questions. Thus, although it might seem Tartaglia made a drastic change in literary style between the two works, the integration of theory and practice was crucial to both works, and I will argue that an overemphasis on the differences between the two feeds into the conception of Tartaglia as arbitrarily wavering in his epistemological stance to suit his own ends.

Did Tartaglia change his mind about the trajectory?

Mary J. Voss suggests that Tartaglia changed his mind between the *Nova Scientia* and the *Quesiti* about violent and natural motion and the nature of the trajectory:

> In *Nova Scientia* (although Tartaglia later altered his scheme), the cannonball emerges in a straight line from the muzzle through violent motion; it curves in a circular arc as the violent motion becomes increasingly weak, then descends at a tangent to that arc when natural motion begins. If the path diverts from this figure, it does so only 'insensibly'.[66]

This is a rather puzzling statement since Voss actually does say here that Tartaglia, in the *Nova Scientia*, recognized that the trajectory was insensibly curved, so it is unclear what she sees as the difference between the position Tartaglia took on this question in the *Nova Scientia* and the *Quesiti*. She contends, however, that in the *Quesiti* Tartaglia 'dropped reference to the sharp discontinuity between violent and natural motion which Cardano had found so objectionable … In the *Quesiti* the

[66] Voss 'How the "New Science" of Cannons Shook up the Aristotelian Cosmos', 382.

violent motion is always at least a little bit curved, even if not sensibly so, and natural motion downwards is always at play.'[67] Voss is correct in noting a development in Tartaglia's work on motion in the *Quesiti;* yet with regard to the trajectory, Tartaglia stated clearly in the *Nova Scientia*:

> Indeed the transit, that is, the violent motion of an equally heavy body that does not follow the perpendicular to the horizon, never shows any perfectly straight part because of the gravity of that body which continuously pulls it toward the center of the Earth. Nevertheless, that part [of the transit] that is not perceived as being curved is assumed to be straight, and that part that is evidently curved is assumed to be part of the circumference of a circle, as this [assumption] does not influence the argument.[68]

Drake had already dealt with this misconception and suggests that Tartaglia's treatments of the question would better be described as progressive.[69]

That Tartaglia should change his mind or develop his ideas over his lifetime would not be surprising. But this misconception is a symptom of an underlying misunderstanding of what Tartaglia's theoretical depiction of the trajectory was designed to achieve and obscures the difficulties he encountered and how he attempted to deal with them. In particular it obscures the close relationship between Tartaglia's theoretical results and his practical claims.

That Tartaglia was not ashamed of his original model is suggested by the fact that the first question of the *Quesiti* is taken up with an explanation of its practical implications for the Duke. Though Tartaglia explained to him that the reason he depicted the beginning of the trajectory as a straight line was because the 'common people' would not understand him otherwise, because it *looks* straight, this was not his only reason for making this assumption.[70] He was aware that this was the popular

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[67] *ibid*., 386.
[68] Tartaglia, *Nova Scientia* (1558) second book, 11r, translated by Valleriani, *Metallurgy, Ballistics and Epistemic Instruments,* 131. Alternative translations in: Drake & Drabkin *Mechanics in Sixteenth Century Italy,* 84. Note Tartaglia uses *gravita* for weight.
[69] Drake and Drabkin, *Mechanics in Sixteenth Century Italy,* 84, note 22.
[70] Translation from Drake and Drabkin, *Mechanics,*101. Tartaglia, *Quesiti* (1554), 10r. Lucar, *Three colloquies,* 10.

view of bombardiers, and there were good empirical reasons for their thinking so. But it was also necessary to assume that it was straight for his own model to work mathematically. Though Tartaglia's mathematical description of the trajectory may have been useful for the achievement of certain results, as he explains to the Duke, this did not mean that the initial part of the trajectory really was perfectly straight. Moreover, what Tartaglia wanted to impress on the Duke was that his theoretical trajectory did not provide a *causal* explanation of why the range varied with the angle of elevation.

There were two trajectories in Tartaglia's works; the 'real' one, and the mathematical model that enabled him to derive his mathematical results. Both were based on an Aristotelian conception of violent and natural motion. Tartaglia was just as clear in the *Nova Scientia* as he was in the *Quesiti* that the true trajectory was continuously curved, and for the same reasons: that he recognized the continuous action of the weight on the ball.[71] However, as he explained in the *Nova Scientia*:

> Nevertheless, *we shall suppose* that part that is insensibly curved to be straight, and that which is evidently curved *we shall suppose* to be part of the circumference of a circle, as they do not sensibly differ.[72]

Tartaglia *explicitly* states that the straight and circular part of his trajectory is a *supposition*. It is not surprising that Tartaglia's position on this matter caused confusion, which led him to explain his argument about the continuously curved nature of the trajectory with impressive mathematico-physical deductive reasoning in the *Quesiti,* in response to the skepticism of the Duke. He is not saying it is actually true that the insensibly curved part is straight, he needs to make this assumption

[71] Domenico Bertolini Meli, whilst correctly discussing Tartaglia's reasons for his mathematical depiction of the trajectory, also implies a change in the *Quesiti*: 'In the *Quesiti et inventione diverse,* however, Tartaglia did state that the trajectory was always curved.' Bertoloni Meli, Domenico, *Thinking with Objects (*Baltimore: Johns Hopkins University Press, 2006), 57.
[72] Tartaglia, *Nova Scientia,* (1558), second book, 11r. My emphasis.Translation from Drake and Drabkin, *Mechanics in Sixteenth Century Italy*, 84. For alternative translation, see Valleriani, *Metallurgy, Ballistics and Epistemic Instruments,* 131.

otherwise he would not be able to make use of his mathematical knowledge to investigate the problem and gain a practical result. His trajectory is a creative hybrid, an idealization, a simplification, and an approximation. It was a mark of Tartaglia's ingenuity that, as Hall has noted, his abstraction, or approximation, actually looked more like the real trajectory of a cannon than the correct parabolic one, and satisfied rather well the empirical experience of the bombardier.

Voss suggests that after publication of the *Nova Scientia,* Tartaglia was induced to 'locate other sources for helping him formulate other answers most notably in Archimedes and the medieval scholastic mathematician Jordanus, both of whom he associated with the science of weights'. Tartaglia probably encountered the superior version of Jordanus's science of weights, the *De ratione ponderis,* when he met Ambassador Mendoza in 1539. But he discussed the application of the science of weights to ballistics in his conversation with the Duke of Urbino, who died in 1538, and it may be that he had already encountered Jordanus's *Liber de Ponderibus,* which had been published by Petrus Apianus in 1533.[73]

Voss implies Tartaglia was disconcerted by the criticism that he received regarding the *Nova Scientia,* leading him to modify his original views. She says that 'Cardano scorned Tartaglia's muddled understanding of violent and natural motion and the military men plied him with questions such as why a cannon charged with the same weight ball and powder does *not* make equal successive shots.' [74] Tartaglia's answers to such questions in the *Quesiti* show that despite the fact that he claimed he had never fired a gun, he had sufficient knowledge of the many factors that affect accuracy in shooting to be able to deal with them without discomfiture.

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[73] Renn and Damerow *The Equilibrium Controversy,* 29.
[74] Voss 'How the "New Science" of Cannons Shook up the Aristotelian Cosmos', 385.

Though there is every evidence to suggest that Tartaglia welcomed questions, Mario Biagioli goes to the other extreme, arguing that if

> patronage relationships can be seen as articulations of the fundamental social process of gift-exchange, then we can say that Tartaglia was *asking for* questions. In fact, it is only after being asked questions that he could *offer answers* (as he had done with Cardano) hoping to give a gift in return, that is, to establish a patronage relation. In this sense, the literary form of the *Quesiti et invention diverse* offers a remarkable picture of Tartaglia's unsuccessful strategies at social legitimation.[75]

In contrast to both Voss and Biagioli, I would argue that the dialogue form was the ideal vehicle for Tartaglia's pedagogic aims and reflected the dialectic between theory and practice that was at the heart of his endeavor.

It is only by recognizing that Tartaglia's culminating proposition was about effects on a target, that we can understand his insistence on the separation of the two types of motion, the violent and the natural. If the two motions were mixed then this proposition would be rendered meaningless, and even more importantly, would have had no practical use. So it is hardly surprising, then, that he vehemently resisted Cardano's criticisms regarding his rejection of mixed motion. As Alexander Koyré noted, Leonardo da Vinci and Nicholas of Cusa had also strongly insisted that the motion of the trajectory was mixed.[76] Tartaglia justified his separation of the two motions by insisting you could not mix something decreasing (violent motion) with something increasing (natural motion):

> Assuming (as the opponent says) the [body] could travel some part with violent and natural motions mixed together, which may be part CD, it follows therefore that the mentioned body, while going from point C to point D, increases its velocity according to the ratio by means of which it shares a natural motion (because of the first proposition). Likewise, it decreases its velocity according to the ratio by means of which it shares a violent motion

[75] Biagioli, 'Social Status', 66-7.
[76] Alexander Koyré 'La Dynamique de Niccolo Tartaglia', *Études D'Histoire de Pensée Scientifique* (Paris: Presses Universitaires de France, 1966), 106.

> (because of the third proposition). It is absurd that he mentioned body increases and decreases its velocity at the same time.[77]

It was only Galileo's discovery of the independent action of the two components of motion in the trajectory that would solve this dilemma of mixed motion, but as I show in my chapter on the Academia del Cimento, even then it was not immediately universally understood or accepted; Riccioli's criticisms of Galileo, for example, centred on conflicting understandings of how the two components of motion interacted.

Ekholm's contribution on the abacus school tradition gives a further insight into Tartaglia's choice of mathematical analysis of the trajectory in the *Nova Scientia*. She shows how examination of Tartaglia's notation in the culminating proposition of the *Nova Scientia* is useful in providing a sense of his abacus school teaching roots:

> Surveying geometric-algebraic problems in the *trattati d'abaco* reveals that as in the case here, they are most often solved by appealing to the rules of right triangles to solve for an unknown. Tartaglia thus draws on Euclidean propositions to establish proportions between sides of figures, labels an unknown, and solves for it by appealing to known geometrical relationships. Unlike Euclid, he multiplies sides to calculate areas. His combination of the methods of classical geometry with those of medieval abacus algebra to analyse projectile motion is a further example of his innovative mixing of traditions.[78]

Tartaglia was showcasing the abacus methods and his own abilities to apply these methods to a new practical context. It was a particularly exciting and highly topical context in which to embed abacus mathematical techniques. It enabled him to combine a novel way of learning and applying mathematics with a ballistics manual.

Ekholm's contribution shows how Tartaglia was dependent on his knowledge heritage.[79] His abacus roots may be the source of his creativity but they did not

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[77] Tartaglia, *Nova Scientia, first book,* 7r. Translation from Valleriani, *Metallurgy, Ballistics and Epistemic Instruments,*115-16. CD is the circular section of Tartaglia's trajectory.

[78] Ekholm 'Tartaglia's *ragioni',* 199-200.

[79] Jochen Büttner, Peter Damerow, Jürgen Renn, and Matthias Schemmel, 'The Challenging Images of Artillery' in Wolfgang Lefèvre, Jűrgen Renn, Urs. Shoepfelin (eds.) *The Power of Images in Early Modern Science* (Basel; Boston; Berlin: Birkhäuser Verlag, 2003), 13, make a similar point, that Tartaglia could only draw on the 'shared

provide the incipient means to understanding the true nature of the continuously curved trajectory. Nevertheless it did give him a practical result. The goal was determined by the means, and I have shown that Tartaglia's goals were limited by and specific to the means of their solution.[80] Nevertheless, as I will show, using the science of weights he provided a profound and insightful, if flawed, causal explanation for his belief that the ball flies further in a 'straight line' as the angle is increased which, far from being incompatible with his conclusion in proposition IX of the *Nova Scientia,* actually confirmed it.

From the *Nova Scientia* to the *Quesiti*: physical causes and the science of weights

As in the *Nova Scientia*, in the *Quesiti* Tartaglia discusses the practical applications of his inventions to convince the Duke (and the reader) that the true route to invention is the discovery of causes. The context of the discussion is the Duke's astonishment at Tartaglia's contention that a cannon will have greater effect at a higher elevation:

> *Duke*: This is a strange and incredible tale, that one and the same quantitie and power of powder will expel more violently one and the same weight of pellet, by one way more than by an other: therefore I desire to know the reason which causeth you to bee of that opinion.
>
> *Nicholas:* The reason thereof is declared by the accidents happening in shooting, in the last proposition of the second booke of our newe science: but I have omitted there to shewe the very causes of such effects, for that I would not bee tedious unto your Excellencie, and because that is plainlie shewed in the science of weights, the which science is of no small speculation, and dependeth upon geometrie and natural Philosophie: but if it will please you nowe to heare mee, I will presentlie declare the same.
>
> *Duke*: Do so with as much brevitie as you may.[81]

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knowledge' available to him. Another example of the theoretical depiction of the trajectory being determined by the mathematical tools available is Daniel Santbech's triangular trajectories. See Kleinert, 'Zur Ballistik des Daniel Santbech', passim.

[80] For a discussion of the importance of means rather than goals in conceptual development see Peter Damerow et al, *Exploring the Limits of Preclassical Mechanics, A Study of Conceptual Development in Early Modern Science: Free Fall and Compounded Motion in the Work of Descartes, Galileo, and Beeckman* (Dordrecht; Heidelberg; London; New **York**: Springer, 2004), 4.

[81] Tartaglia, *Quesiti* (1554), 8r, trans. Lucar, *Three bookes of colloquies,* 6.

Contrary to Voss's contention that Tartaglia studied the science of weights as a response to criticism, this passage indicates that it was part of his own quest to delve deeper into physical causes. Indeed, taken at face value, this passage suggests that Tartaglia was aware of the explanatory causal value of the science of weights when he wrote the *Nova Scientia*. Tartaglia was interested in the science of weights, not because of criticism or backtracking from his earlier work, but because he saw that his mathematical argument for the angle of maximum range did not explain the cause.

Thus, Tartaglia incorporated the study of the science of weights as fundamental to any understanding of the study of motion and the relationship between mathematical theory and the complexities of the physical world, and, moreover, despite his recognition that its abstract nature might not appeal to many of those, such as the Duke of Urbino, who approached him for answers to more pressing practical problems.

The science of weights forms a significant theoretical portion of the *Quesiti*. It is based on the balance within a circle and the concept of positional weight (*gravitas secundum situm*), which changes as the weight moves from the horizontal position. [82] Thus the active capacity of weight depends on its position as well as its weight (*propria gravita*). Gerhard Arend has noted that the concept of weight by position is a powerful analytical tool. Its importance comes from its usefulness in deriving the mechanical propositions on the lever and the inclined plane.[83]

Though he provides a more thorough exposition on the science of weights to Ambassador Mendoza in book seven and eight of the *Quesiti*, at the beginning of book one Tartaglia attempts to use the science of weights to explain to the Duke the physical cause of why the gun shoots further at a higher elevation. According to

[82] Weight (as in the number of pounds) is referred to as *propria gravita*.
[83] Arend, *Die Mechanik,* 364.

Tartaglia, by treating the gun as a balance, the ball is positionally lighter when the gun makes an angle than when it is level, and thus flies 'more easilie'.[84] This swiftness causes it to go further in a 'straight line', and the faster it goes the less 'heavy' it is because the air sustains it.[85]

We can see how this directly relates to proposition IX in the *Nova Scientia* since it helps to explain why the 'straight' part of the trajectory is four times longer at forty-five degrees than at point blank. Tartaglia's explanation is insightful, ingenious and quite remarkable. As the diagram (Figure 3) shows, he relates the difference in the trajectories to their mechanical and mathematical configuration, and how this configuration affects the 'weight' of the ball, which acts as a deflecting force on the motion of the bullet. At point blank the weight is perpendicular to the passage of the bullet so it has maximum 'effect' on the trajectory, while at other angles the weight of the ball is deflected nearer to that passage and so, has less strength to draw the bullet away from its passage. The changing angle provides him with a causal explanation of why the bullet flies farther as the angle of the shot increases, making the shot 'straighter' and longer at higher elevations.[86] Despite the fact that he did not have the conceptual tools to unite them successfully, Tartaglia's attempt to link the machine mechanics of the balance with the dynamics of the trajectory displays an insight that reflects his unifying epistemology. Tartaglia's analysis links together the two apparently contradictory descriptions of the trajectory in the *Nova Scientia,* because, as he says, as the gun is elevated it does fly further in a 'straight' line, though in reality it is never completely straight because at all points the weight of the pellet has

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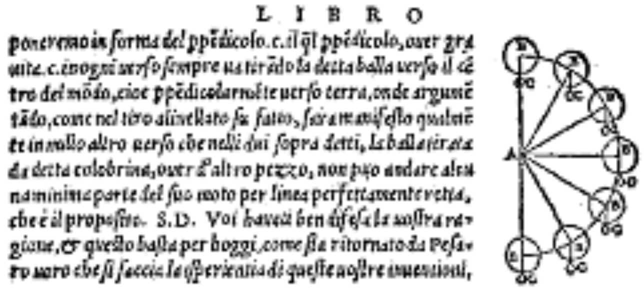
[84] Tartaglia, *Quesiti* (1554), sig.12v. Translated in Lucar *Three bookes of colloquies,* 8-9.
[85] Voss 'How the "New Science" of Cannons Shook up the Aristotelian Cosmos', 388-389, provides a detailed commentary of this section of the *Quesiti.*
[86] For Benedetti's alternative explanation see Drake and Drabkin, *Mechanics in Sixteenth Century Italy,* 226, and for Galileo's early thinking on this problem, which bear some similarities to that of Tartaglia, see I.E. Drabkin and Stillman Drake *Galileo Galilei: On Motion and On Mechanics* (Wisconsin: University of Wisconsin Press, 1960), 112-114.

some effect, as can be easily seen in Figure 3.



L I B R O
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uita.c. e in ogni uerso sempre ua tirãdo la detta balla verso il cẽ
tro del mõdo, cioe ppẽdicolarmẽte uerso terra, onde argumẽ
tãdo, come nel tiro a liuellato fu fatto, sara manifesto qualmẽ
te in nullo altro uerso che nelli dui sopra detti, la balla tirata
da detta colobrina, over d'altro pezzo, non puo andare alcu
na minima parte del suo moto per linea perfettamente retta,
che e il proposito. S.D. Voi haueti ben difesa la uostra ra
gione, & questo basta per hoggi, come sia ritornato da Pesa
ro uoro che si faccia le esperientia di queste nostre inuentioni,

Figure 3. Tartaglia explains how positional weight affects the trajectory: It is truth (as your Excellency doth say) that the waight of a pellet is not so apt to hinder the range of the same when the pellet flyeth out of a piece more elevated…The other cause is for that the said waight draweth the pellet out of his waie or passage perpendicularly towards the grounde, the which kinde of drawing is more vehement or stronger in that place, than in anie other elevation. For the peece being elevated by degrees, the said waight also by degrees doth goe nearer toward his said waie or passage, that is to say, it doth not fall there so perpendicularlie from the sayd passage, but comes alwaies more neare unto the same, and so is of lesse strength and force to drawe the pellet out of his waie or passage, besides this (as before hath been declared) by how much a piece is more elevated, by so the range of his pellet is less crooked, and yet the range of that pellet cannot in any part thereof be perfectly right, except in the two waies before specified, that is to say, when it is shot directly up towards heaven, or directly downe towards the centre of the world: Because in every other waie some part of the waight drawes always the said pellet out of his waie or passage.[87]

While Tartaglia's short explanation of the science of weights is in response to a specific question posed by the Duke, he provides a more thorough exposition of the science of weights in books seven and eight of the *Quesiti*. Stillman Drake considers that one of Tartaglia's lasting contributions is found in the eighth chapter of the *Quesiti* on the science of weights where he attempted to give mathematical rigour to the demonstrations of his medieval predecessors. Drake notes that the most interesting case is that of the celebrated theorem of Jordanus concerning equilibrium on inclined planes. Tartaglia's solution was entirely correct and therefore wrongly rejected by Guidobaldo del Monte and other writers on mechanics in the second half of the

[87] Tartaglia, *Quesiti* (1554), 12r-12v. Translated in Lucar, *Three bookes of colloquies,* 14.

sixteenth century.[88] Tartaglia was the only writer to maintain the correct view until Galileo's and Simon Stevin's proofs. It became a cornerstone, along with the time-squared law of fall, for Galileo's proof of the parabolic trajectory.

The context of the exposition of the science of weights in books seven and eight is a theoretical discussion with Ambassador Mendoza of Aristotle's treatment of the balance in his *Mechanical Questions*. The text of the pseudo-Aristotelian *Mechanical Problems* (written in the fourth century BC) had been unknown throughout the Middle Ages, though Jordanus's science of weights show evidence of at least its indirect influence; in both the science of weights and the *Mechanical Questions,* mechanical effects are explained through an appeal to the speeds of moving powers and weights. When the *Mechanical Problems* was recovered it generated a resurgence of interest from humanists, philosophers, mathematicians and engineers.

Galileo explicitly credited the *Mechanical Problems* with the general theory of the balance, and thus of all mechanics, based on the principle of circular movement which asserts that the farther from the centre of rotation a power or weight is, the faster it will move and the more effective it will be. This explains why a lighter weight farther from the centre, and thus moving more swiftly on the longer radius of the circle, can balance a heavier weight closer to the centre. The balance forms the basis for the explanation of other mechanical effects such as the lever, wheel and axle, wedge, hammers, oars and rudders. The most popular translation and commentary of the sixteenth century was that of the humanist Niccolò Leonico Tomeo, which was published in Venice in 1525.[89]

Tartaglia agrees that the *Mechanical Problems* are 'very good, and certainly

[88] Drake & Drabkin *Mechanics in Sixteenth Century Italy,* 25.
[89] Laird, 'Renaissance Mechanics and the New Science of Motion", 255-7.

most subtle and profound in learning' but adds that they have contradictions that can only be explained by the science of weights, and that only the science of weights provides a causal solution to the problem of the balance. [90] In addition, Aristotle had argued from the theory of the balance that larger balances were more accurate than smaller ones. Tartaglia made the point that in practice small, finer balances, are much more sensitive than large ones. Tartaglia used this to make a point about the material and the theoretical, that Aristotle was proved wrong because the smaller balances such as those used by goldsmiths were more accurate because they were closer to the ideal. To the extent that the material more resembles the ideal, so will the difference between the ideal and the material be reduced. But he believes in the case of the balance it is essential to only base your theory on ideal balances abstracted from the material. Aristotle's conclusion that the bigger scales are more accurate than small ones is false because, according to the science of weights, the smallest possible weight placed an a balance in equilibrium will cause it to tilt towards the lowest point possible.[91]

Tartaglia explains to Mendoza that theory should be verifiable by the senses, 'otherwise mathematics would be wholly vain and useless and devoid of profit to man.'[92] Nevertheless the discrepancy between the material and the ideal is grounded in the nature of matter:

> …for things constructed or fabricated thereof can never be made as perfectly as they can be imagined apart from matter, which sometimes may cause in them effects quite contrary to reason. And for this and other

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[90] Tartaglia, *Quesiti* (1554), 78r, trans. Drake & Drabkin, *Mechanics in Sixteenth Century Italy,*104-5.

[91] Tartaglia, *Quesiti* (1554), 79r, trans. Drake and Drabkin, *Mechanics in Sixteenth Century Italy*, 108. Compare with I E Drabkin and Stillman Drake *On Motion and On Mechanics* (Wisconsin: University of Wisconsin Press, 1960), 143, on the problem of dynamic considerations in static analysis that had long prevented the creation of a unified science of dynamics, 'Galileo swept them aside when he wrote in his *Mechanics:* "And since to make the weight B descend, any minimal heaviness added to it is sufficient, we shall leave out of account this insensible quantity and shall not distinguish between the power of one weight to sustain another, and its power to move it." For by the time he made this remark Galileo had already realised (as his predecessors had not) that in theory at least, motion once commenced did not cease without the interposition of some force.'

[92] Tartaglia *Quesiti* (1554), 79v, trans. Drake and Drabkin, *Mechanics in Sixteenth Century Italy*, 108.

> reasons, the mathematician does not accept or consent to proofs and demonstrations made on the strength and authority of the senses in matter. Consequently, the mathematical disciplines are considered by the wise not only to be more certain than the physical, but even to have the highest degree of certainty.[93]

Tartaglia's apparently contradictory statements reflect the fact that he struggled to articulate the problematic of the relationship between mathematics and the testimony of the senses, yet it was a crucial epistemological question for him. His use of the dialogue form enables him to convey the tension between them. The discussion with Mendoza is a reprise of his conversation with the Duke, who pulls Tartaglia up on the contradiction between his claim that he has never actually shot a gun, and his assertion in the *Nova Scientia* that 'he that have no proofe or experience, is oftentimes deceived. For the eie is that which gives us a true testimonie of thinges imagined.' Tartaglia replies:

> It is true that the outer sense doth tell us the truth in particular things, but not in universal things: for universal things are subject only to understanding, & not to any sense.[94]

Tartaglia should be given credit for articulating in such an engaging way through his dialogues the tensions that were central to the new approach to knowledge that he augured. He offered a window into the difficulties encountered in the early stages of the process of uniting theory to practice. Despite, or perhaps because of, his self-consciously Brescian vernacular style, which made him target for *ad hominem* attacks by his adversaries, he had the capacity to articulate engagingly the most important epistemological preoccupations of the time, and this may have contributed to the resonance of his works.[95] Anna De Pace observes that it was Rafaello Caverni, in his *Storia del metodo sperimentale in Italia* (1891-1900), who first drew attention both to

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[93] Tartaglia *Quesiti (*1554), 78v, trans. Drake and Drabkin, *Mechanics in Sixteenth Century Italy*, 106.
[94] Tartaglia, *Quesiti* (1554), 7r, trans. Lucar, *Three bookes of colloquies,* 5.
[95] For a linguistic appreciation of Tartaglia's writing see Mario Piotti *La Lingua di Niccolò Tartaglia: Un poco grossetto di loquella* (Milano: LED Biblioteca insubrica 3,1998).

the subtlety of Tartaglia's observations and to the fact that this represented the first time that the pseudo-Aristotlelian *Mechanical Problems* was openly accused of being in error.[96]

Conclusion

When Tartaglia talked about the problems of matter he was articulating his own dilemma. This is why it has been important to carefully consider the claims he made, rather than make assumptions based on later discoveries. He was not a charlatan who made overblown promises; he was much more interested in using specific claims to illustrate a method. This is why I have, in this chapter, attempted to ascertain what his practical claims were and how they related to his theoretical results.

As Anna de Pace notes, the counterposition of the material world to mathematical intelligibility, of the certainty of discursive reasoning to the uncertainty of the senses, were not original to Tartaglia; these are rather a constant feature of any platonically-oriented reflection on mathematics. But she affirms that what *was* original was the way he interpreted this opposition. For Tartaglia this was not a reason to abandon any hope of understanding and intervening in the material world. Rather it presupposed a scientific methodology that was directed towards a search for the mediations necessary to bring sensible phenomena into conformity with the ideal. And she notes that there are echoes of Tartaglia in Galileo's *De Motu* where he attempts to create, or imagine, experimental conditions that approximate as much as possible the mathematical ideal, whilst recognizing that if the mathematical conclusions are not immediately verified by experience, this is due to the

[96] Anna De Pace *Il Matematiche e il Mondo: Ricerche su un dibatto in Italia nella seconda metà cinquecento* (Milano: FrancoAngeli, 1993), 246 and 259. De Pace provides a thorough exposition and commentary on the discussion between Tartaglia and Mendoza on the Aristotelian mechanical questions and the science of weights in the context of the debate in the second half of the sixteenth century on the relationship between mathematics and the material world.

impediments deriving from matter.[97]

But there was another impediment for Tartaglia's heterogeneous readership that he consciously struggled to overcome, which was that of language; this question brings together his original work with his translation programme. As he explains in his translation of Euclid, modern science had been held back in its progress by language, and for this reason it has not succeeded in outstripping that of the ancients. Tartaglia's attitude to language was instrumental; his whole endeavor had the aim of breaking down the barriers between language and the subject matter so that it would become accessible to anyone of average intelligence.[98] As Piotti notes, this striving to unify the learned with the vernacular was not just a topos that had become banal even by the fifteenth century, but had its basis in real economic and social concerns.[99]

This orphaned son of a post rider who had to teach himself to read and study the classics, and who was dogged by poverty and misfortune throughout his life, left a huge legacy to his successors, even though some were more inclined to criticize and ridicule his mistakes than to recognize their debt to him. Tartaglia's aim was to discover the universal principles of mechanics that governed bodies in motion. These universal principles were confined in his writings to terrestrial mechanics. In the next chapter, I will show how, under the influence of Copernicus's *De Revolutionibus* the brilliant astronomer and mathematician Thomas Digges widened the scope of these principles to incorporate the behavior of motion in the heavens. He too had a commitment to spreading knowledge to a non-learned, heterogeneous audience. And for Digges this was a political as well as a scientific endeavor. Furthermore, Digges had the means and social position to choose his allegiances according to both his political and his scientific inclinations.

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[97] *Ibid*., 250-251.
[98] Piotti, *La Lingua,* 27.
[99] *ibid*., 23.

Chapter two

Thomas Digges: a new science in defence of religion and commonwealth

> First I say, that like as in all other Artes and Sciences it is a matter of verie great importance to have sure grounds and True Principles without abuse or error. So in this Art Military (whereupon dependeth not onely the lives of so great multitudes, but also the defence or ruin of the greatest Realmes & Monarchies) it were to be wished that men were not to be misled with such false and fraudulent opinions as may bring forth fruits most poysnous and perilous.[1]

> Let us observe that the arrangements, the marching, and the evolution of battalions, nearly as they are now practiced, were revived in Europe by one who was not a military man – by Machiavel, a secretary at Florence…He taught Europe the art of war; it had long been practiced, without being known.[2]

Introduction

In the second half of the sixteenth century England experienced an economic, technological and cultural transformation. Nowhere is this transformation more striking than in the production of cast iron ordinance, where a technical breakthrough in the last years of Henry VIII's reign led to the achievement of an English monopoly of world production. It was a spectacular payoff for Henry VIII's efforts to establish cannon manufacture in England, and of the potential rewards of state-sponsored industrial development. In 1545, William Levett, the rector at Buxted in Sussex, had mastered the process for large-scale production and accepted a government order for 120 guns to be delivered in two years. The capacity of the iron industry rapidly increased from about twenty blast furnaces and twenty-eight forges in Sussex in 1549

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[1] Thomas Digges, *Foure Paradoxes or politique Discourses* (London: H. Lownes for Clement Knight, 1604), 41.
[2] Voltaire, François-Marie Arouet, "Battalion', *A Philosophical Dictionary,* translated from the French (London: W. Dugdale, 1843) I, 198, quoted in Neal Wood's introduction to Niccolo Machiavelli *The Art of War* (New York: Da Capo Press, 1965), xxxii.

to a national capacity of about fifty furnaces and sixty forges by 1574.[3] English iron guns were not as good as bronze but they had the advantage that they could be produced at a much lower cost and English ordinance developed a reputation for reliability that ensured high demand from both allied and enemy nations, and they were particularly suited to deployment on English ships. This helped to boost the effectiveness of English piracy and the potential for encroachment on Spanish colonial power.[4] The increasing tension between Protestant England and Catholic Spain was reflected in the internal political and religious tensions that accompanied the consolidation and expansion of English state power. This provides the backdrop to the shift in my narrative from Renaissance Italy to Elizabethan England, where the mathematician Thomas Digges, stimulated by the national importance of cannon technology for warfare, took up the challenge of the gunners' question.

Thomas Digges (*c.*1546–1595) is noted as being the first person to publish Copernicus's *De Revolutionibus* in the vernacular, and in particular for his astonishing depiction, for the first time, of a heliocentric system within an infinite universe of stars (see Figure 4). What is less well known is that he published the first explicit proposal for a research programme in ballistics. His gifts as an astronomer provided him with insights into his study of the trajectory of a cannon, and he was the first to draw an analogy between the two. Despite the substantial research that has been done on the rich sources of material related to Thomas Digges in Elizabethan State Papers and other manuscripts of the time, as well as his own publications, other researchers have tended to examine specific aspects of Digges's life and scientific

[3] Paul E. J. Hammer, *Elizabeth's Wars,* Basingstoke: Palgrave Macmillan, 2003), 79.
[4] Ruth R Brown, '"A Jewel of Great Value" English Iron Gunfounding and its rivals, 1550-1650' in Carlo Beltrame and Renat Gianni Ridella, eds. *Ships and Guns: The sea ordinance in Venice and Europe in the 15th and the 17th centuries* (Oxford: Oxbow Books, 2011), 98. This monopoly was only broken by the development of the Swedish industry (sponsored by the Dutch), in the early part of the seventeenth century, especially under Gustavus Adolphus, at the same time as the English industry was run down under James I, who tried to limit exports from fear that the guns were getting into the wrong hands. However, with the revival of production from the Civil War onwards, both nations came to share domination of the world market in cast iron guns.

activities in isolation, depending on their historical interest.[5] In each case something has been lacking in terms of the whole picture. Digges's importance as a political figure and his ubiquitous presence in so many different areas of Elizabethan life have not been fully appreciated or explained. I will argue that it is only through the examination of Digges' programme for military reform that we can gain an understanding of his historical and scientific importance. Whilst ostensibly Digges' political and civic duties could be seen as a distraction from his mathematical pursuits and his research on ballistics (as he himself was wont to complain), at a deeper level they are inextricably connected.

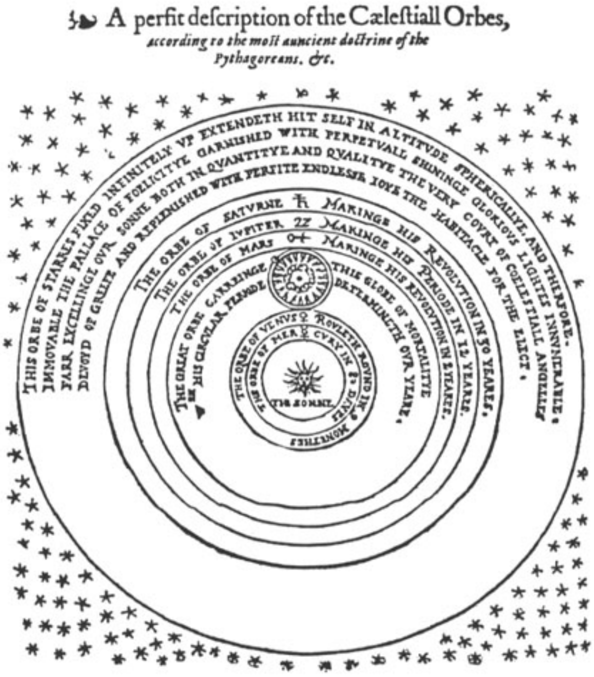

Figure 4. Digges's depiction of an infinite universe from *A Prognostication Everlastinge*.

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[5] Henry J Webb, *Elizabethan Military Science: The Book and the Practice* (Madison: University of Wisconsin Press, 1965) discusses Digges as a military writer and compares him with other military writers of the sixteenth century; Stephen Johnston, in chapter two of his PhD thesis, *Making Mathematical Practice: gentlemen, practitioners and artisans in Elizabethan England* (Cambridge University, 1994) has focussed on how Digges fashioned his identity as a mathematical practitioner, and his turn from contemplative mathematics to active civic participation and practical mathematics; Francis Rarick Johnson, in his *Astronomical Thought in Renaissance England* (Baltimore: John Hopkins Press, 1937), first drew attention to Digges unique importance in the history of astronomy and for the spread of Copernicanism; Eric Ash, in *Power, knowledge and Expertise*, has provided a detailed account of Digges's crucial role as a highly regarded expert mediator in the project to rebuild the Dover Harbour in the 1580s, where his combination of expertise, intelligence, political and social connections, and his close relationship with the Privy Council, combined with a rare integrity, ensured the project's success after a number of false starts.

Through examination of Digges's ballistics I will show how Digges aimed to bring a new method to natural enquiry, the development of *theorike* based on experiment and observation, motivated by a Neo-Platonist belief in the underlying mathematical structure of the universe. He applied this method to the two major investigative projects of this period - the study of motion in the heavens and the study of the motion of the trajectory. His search for unifying laws and the linking of theory to practice is reflected in the contemporary ideal of the scholar-soldier that captures the spirit of the revolution in military practice that was to be successfully put into practice by Prince Maurice of Nassau at the end of the sixteenth century, and which Digges struggled to achieve for the English army. Digges's activism in Parliament and his role as a state advisor, rather than being seen as a distraction from this unifying purpose, were integral to this programme for defence of religion and state.

Digges was a well-to-do member of the gentry whose father, Leonard Digges, was, along with Robert Recorde, one of the first major English vernacular writers on practical mathematics in England. Like Thomas, Leonard was an MP as well as a mathematician, and was also active in putting his mathematical knowledge into practice in civil defence. Calvinist in inclination, Leonard was imprisoned and narrowly avoided execution by Mary Tudor after his participation in the Wyatt rebellion. As a result he lost all his lands, which were later restored under Elizabeth I. Leonard's religious convictions and experience of Catholic persecution had a deep and lasting effect on Thomas, and it was this conviction that led him to turn towards active participation in military and political affairs from the 1570s, as tensions between Spain and England and fear of the Catholic fifth column increased.

Following Leonard's early death when Thomas was only about 13, the mathematician John Dee became his teacher and second (mathematical) father.[6] The influence of Dee on Digges is apparent in Digges's mathematical discourse on the Platonic solids, appended to his father's work on practical mathematics, *Pantometria* (1571). They also worked closely together on astronomy, especially at the time of the supernova of 1572. Both seem to have taken up mathematics for civic purposes in the 1570s and early 1580s, though to some extent their paths diverged as Thomas became increasingly involved in military affairs, civic projects and political lobbying, while Dee focused on his role of court philosopher and astrologer as well as playing an important role as technical adviser in projects of early exploration and trade.

Apart from his one Latin work on parallax and the super-nova of 1572, Thomas used his father's vernacular works on practical and military mathematics, *Pantometria* and *Stratioticos* (1579), and his book on astrological prognostication, as a sort of Trojan horse to make public his own contributions to astronomy, ballistics, military tactics and discipline, pure mathematics, and the popularisation of Copernicanism.[7]

Originally Digges found in Lord Burghley a sympathetic admirer of his pursuit of knowledge. For example, as the foremost expert on astronomy, Digges

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[6] Stephen Johnston, 'Like Father, Like Son' in Stephen Clucas (ed.), *John Dee: Interdisciplinary Studies in English Renaissance Thought* (Dordrecht: Springer, 2006), 65-84, on 65.

[7] Digges's publications comprise: *Pantometria* (London, Henrie Bynneman,1571), republished in 1591, comprising three books on longimetria, planimetria and stereometria dedicated to Sir Nicholas Bacon (father of Francis Bacon). It also included Thomas's mathematical discourse on the platonic solids. This was followed by a treatise on the supernova of 1572, *Alae seu scalae mathmaticae* (London:[Apud Thomam Marsh],1573). A *Prognostication Everlastinge* (London: Thomas Marsh, 1576) was a reediting of Leonard Digges's perpetual almanac, with the addition of the *Perfit Description of the Celestial Orbs*, his vernacular popular introduction to Copernicanism, and also including discourses on magnetism and navigation. This was followed by *An Arithmetical Warlike Treatise Named Stratioticos* (London: Henrie Bynneman, 1579), dedicated to the Earl of Leicester. It was republished in 1590 with answers to the artillery questions in the margins and an additional discourse concerning invasion. He also published *A briefe report of the militarie seruices done in the Low Countries, by the Erle of Leicester* (London: by Arnold Hatfield, for Gregorie Seton, 1587); *A breife and true report of the proceedings of the Earle of Leycester for the reliefe of the towne of Sluce* (London: [By T Orwin], 1590); *A petition to the Queene, for assotiation in religion,* anonymously published posthumously in *Humble Motives for Association to Maintaine Religion Established* (England: English Secret Press, 1601); *Foure Paradoxes, or politique Discourses. 2 Concerning Militarie Discipline, written long since by* Thomas Digges *Esquire 2 Of the worthinesse of warre and warriors, by Dudly Digges, his Sonne (*London: H. Lownes for Clement Knight, 1604).

provided Burghley with a report on the astrological implications of the 1572 supernova.[8] Subsequently, though, he was drawn to the Earl of Leicester's more aggressive foreign policy towards Spain, including his desire for military intervention in defence of Protestants in France and the Netherlands. Politically, this was the period described by Gideon Freudenthal as Tudor progressive absolutism, during which there was an alliance between a section of the feudal aristocracy, of which Leicester was a leading figure, and the nascent bourgeoisie.[9] Though Burghley was more puritan in his personal inclinations than the flamboyant Leicester, he was more moderate and concerned to try and achieve conciliation with Spain, so it is not surprising that Digges allied himself with the more progressive leader who he believed would provide the best opportunity for him to direct his talents towards the service of his country.

Leicester appointed Digges as his muster-master in the Netherlands campaign, a position of 'great responsibility and increasing hardship'.[10] Leicester was financially ruined by the Netherlands campaign (1585-7); he was also probably physically debilitated and emotionally crushed by the experience, and he died just after the defeat of the Spanish Armada in 1588. In 1591 and 1592 Digges reissued *Pantometria* and *Stratioticos,* with additional sections on ballistics, a defence of Leicester's campaign, and the advice he had given to Leicester on preparation for the Spanish invasion of 1588. He struggled on until 1595, continuing to defend Leicester's name against his many enemies and critics, and lobbying the Queen and Parliament for vigilance against the threat of the return of a Catholic monarch.

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[8] Johnston, *Making Mathematical Practice,* 60.

[9] Gideon Freudenthal, *Atom and Individual in the Age of Newton: On the Genesis of the Mechanistic World View* (Dordrecht: D Reidel Publishing Company, 1986),109.

[10] Eleanor Rosenberg, *Leicester, Patron of Letters* (New York: Columbia University Press, 1985), 284. Digges outlines the duty of the muster-master in *Stratioticos* (1579),135. The mustermaster should provide a crucial link between the treasurer and the captains' bands, involving constant reviewing, and provision of meticulous records for the treasury.

Digges's publication of his translation of book I of Copernicus's *De Revolutionibus* as an addition to the republication of his father's almanac called *A Prognostication Everlastinge…*(1576) was an astonishingly astute and daring move that ensured its wide dissemination way beyond the circles of the learned elite. His stated aim was to make Copernicus's arguments for the new astronomy accessible to those who were not experts in mathematics. The *Prognostication* went through eleven known editions and was by far the best of the 'perpetual almanacs' issued in this period, with astrological and astronomical tables and rules for such useful purposes as predicting the weather, calculating the dates of the moveable feasts, and for bloodletting as prescribed by astrological medicine. The utility of almanacs ensured that interest in and knowledge of astronomy was high compared to today and it was this wide readership that was introduced to Digges's robust extension of Copernicanism with its infinite universe.[11] Not only was Digges the first to describe the universe as infinite, but the astronomer Edward Harrison has noted that Digges should be recognised as the first astronomer to consider the paradox of why, as a consequence, the sky is not full of light. Digges explained that 'every quantity hath a certaine proportionable distance whereunto yt may be discerned, and beyond the same it may not be seene.'[12]

Digges's influence was not confined to England; David Wooton notes that Galileo's arrival in Padua in 1592 gave him access to the astonishing library of Giovan Vincenzo Pinelli who had two copies of Copernicus, works by Bruno and 'a Copernican work by Thomas Digges'.[13]

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[11] Johnson, *Astronomical Thought,*123.
[12] Thomas Digges *A Perfit Description off the Caelestial Orbes,* addition to Leonard Digges's almanac *A Prognostication Everlastinge,* sig. N3v, fol. 47v (1576 edition); sig. 2B1v, fol. 47v (1584 edition); Edward Harrison, *Darkness at Night: A Riddle of the Universe* (Cambridge: Harvard University Press, 1987), 37.
[13] David Wootton, *Galileo: Watcher of the Skies* (New Haven: Yale University Press, 2010), 56.

In his introduction to his translation of Copernicus Digges quotes freely from the 'Stellified Poet' Palengenius's didactic poem *Zodiacus Vitae,* with its emphasis on the unity of all knowledge, physical and spiritual. Printed in Venice in 1531, it was translated into English verse by Barnabe Googe.[14] Johnson notes Digges's familiarity with the poem, citing Gabriel Harvey's comment that 'Mr Digges hath the whole Aquarius of Palingenius bie hart: & takes mutch delight to repeate it often.'[15] Giordano Bruno, the notorious proselytiser of heliocentrism and infinite worlds, also cited Palingenius as inspiration. It had wide influence in Elizabethan England since it was used as a textbook in grammar schools throughout the century and beyond, as well as being an inspiration for poets and playwrights.[16]

In quoting Palingenius in his translation of Copernicus, Digges was making a statement. The ideological resonance of the poem was increased by the reaction of the Catholic Church, which had placed it on the highest level of the Index and had dug up the saintly Palingenius's bones and burnt them. Digges was challenging traditional conceptions of the world and robustly opposing the instrumentalism that Catholicism sought to impose on the Copernican hypothesis; for Digges mathematics described the true underlying structure of reality. The translation was all the more significant because uniquely this knowledge was not written solely for a learned elite, and it remained the only substantial vernacular translation of this controversial knowledge until the nineteenth century.[17]

The association of learning with goodness, a fundamental theme in Palingenius's poem, permeates Digges's writings on military science. According to

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[14] Foster Watson, *The Zodiacus Vitae of Marcellus Palingenius Stellatus: An Old Schoolbook* (London: Philip Wellby 1908), 16. The First three books of the *Zodiacus Vitae* was translated into English in 1560, the first six books in 1561 and full work in 1565.
[15] Johnson, *Astronomical Thought,* 163.
[16] Raphael Lyne, entry for Barnabe Googe in the *Oxford Dictionary of National Biography*.
[17] Francis R. Johnson, 'The Influence of Thomas Digges on the Progress of Modern Astronomy in Sixteenth Century England' *Osiris* 1 (1936), 390-410, on 400.

Henry J Webb, Digges, in his military treatise *Stratioticos*, began a new vogue in military analysis; though Digges emphasised that a good general must be "religious, temperate, sober, wise, valiant, liberal, courteous, eloquent, of good fame, and reputation," he also stressed the necessity of securing a man who is "learned in histories, and in those sciences and arts that may enable him of himself without direction from others, readily to conceive and judge of military actions." [18] Chief among the sciences the general should know are those dealing with mathematics.

Whitehorne's translation of Machiavelli's *Arte Della Guerra* in 1560 and his *Certain Waies for the Orderyng of Souldiers in Battelray,* published in the same year were popular works and the latter provided a limited assessment of the utility of cannon, but for the first two decades of Elizabeth's reign 'English military writings emphasized Roman tactics based on the legion, an infantry unit supported in a very minor way by cavalry and artillery.[19]

Digges's military treatise *Stratioticos* represented a revolution in military writing. Whilst Digges took Rome as his model, he adapted this model to incorporate modern developments in artillery, seeing the application of mathematics as integral to the efficient prosecution of military activities. Digges was much more than just another sixteenth century military writer. Neal Wood, in his introduction to Machiavelli's *The Art of War,* highlights his importance to military science:

> The use of the quantitative method for the planning and direction of troop formations and manoeuvres presupposes an emphasis in theory and practice upon discipline and drill. About the same time that the men who were revolutionising warfare were applying mathematics to questions of military organisation, the pioneers of modern science were beginning to describe the relations of natural phenomena in quantitative terms. The marriage of the two interests is found in the person of the illustrious astronomer and mathematician, Thomas Digges… Notions of a mechanistic system of nature and a mechanistic military system seem to have arisen about the same time.[20]

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[18] Digges, *Stratioticos* (1590), 305-6, quoted in Webb, *Elizabethan Military Science,* 56.
[19] Henry J Webb, 'The Science of Gunnery in Elizabethan England' *Isis* 45 (1954), 10-21, on 15.
[20] Neal Wood's introduction to Machiavelli's *The Art of War*, xxxv.

Digges's ideal of the scholar-soldier and his programme for reform of the military has much in common with military theory and political and intellectual movements in the Netherlands where the political and military crisis would make reform of the army a necessity for survival.[21] What I want to show in this chapter is that we cannot look at Digges's political and civic activities, his religious views, or his writings on astronomy and military science in isolation. Stephen Johnston recognises the importance of Digges's integrative programme of uniting mathematics with civic duty, but following E.G.R. Taylor, he mainly focuses on the development of the identity of Digges as mathematician during this period. In my view this is too rigid an analytical tool for understanding him because it ignores his programmatic aims, which bring out the consistencies between his various roles such as mathematician, military and political adviser, and parliamentary activist, rather than focusing on his role as a mathematician. This also enables us to treat Digges historically, seeing his choices in the context of the development of historical events. Nevertheless, Johnston raises important and interrelated issues that are crucial to an understanding of the transformation in knowledge that was taking place in England in the second half of the sixteenth century.

One of these issues is the question of Digges's apparent elitism, which illustrates the complexities of the process of integration between theory and practice in this period. Johnston notes that the umbrella term mathematical practitioner that is

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[21] David R Lawrence's excellent *The Complete Soldier: Military Books and Military Culture in Early Stuart England, 1603-1645* (Leiden: Brill, 2009), 3 and *passim*, notes that there is a growing consensus that military books and manuals were more influential in shaping practice than was long believed. The archetypes of the scholar-soldier during this period were the poet-soldier Sir Philip Sidney (Leicester's nephew) and Robert Devereux, Second Earl of Essex (55-71). See also Mark Charles Fissel *English Warfare 1511-1642* (London: Routledge, 2001), for the importance of the Netherlands wars as a training ground for English soldiers: 'The old adage that the continental wars served as England's military academy bears truth…The Low Countries' theatre of war features siegecraft and combined operations between 'sea beggars' and ground forces…Sizeable and talented contingents of volunteers left England to be apprenticed in the art of war and to fight the international Roman Catholic threat.', 152.

used by historians of science encompasses both the gentleman scholar/mathematician such as Thomas Digges and the craftsman artisan, and a wide range of professional activities and suggests that this led to tensions regarding the role of the mathematical practitioner. A particular instance that he gives is Digges' claim that he was able to convince experienced seamen of the superiority of his methods. Johnston provides convincing new evidence that Digges is the most likely unnamed target of mariner and compass-maker Robert Norman's much quoted defence of unlearned mechanicians who have art 'at their fingers endes' against the learned 'in their studies amongest their bookes'.[22]

Norman, in his work *the Newe Attractive* was the first to describe the phenomenon of magnetic dip and it is a landmark in the history of the development of the experimental method. Digges's addition to the prognostication of 1576 on errors in navigation, consciously aimed at the mathematically expert, disdained the possibility that mariners could tackle the fundamental questions of navigation, citing the proverb *Ne Sutor Vltra Crepidam.*[23] It is not surprising that Norman felt the need to respond to such a dismissal and he turns Digges's words against him. It is the repetition of Digges's use of the proverb *Ne Sutor Vltra Crepidam* that makes it obvious that Digges is his target.[24] Johnston remarks that here was

> … a mere mechanician taking it on himself to publicly reprove a man whose social station was far above his own. Digges was a substantial esquire with increasingly important political connections. Within the elaborately hierarchical society of Elizabethan England, Norman's action required extraordinary temerity. Little wonder that Digges was not mentioned by name.[25]

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[22] Robert Norman, *The Newe Attractive* (London: Richard Ballard, 1581), sig. B1v: 'To the Reader'.

[23] The shoemaker should stick to his lasts. Digges, *a prognostication…* (1584), *A short Discourse touching the Variation of the compasse,* sig.2C2.

[24] Norman, Robert *The Newe Attractive* (1581) sig. B1r-v. in which he sets out his discovery of magnetic dip. Quoted in Johnston *Making Mathematical Practice,*170. Downloaded from mhs.ox.uk 31.05.11. See also Jim Bennett's *Oxford Dictionary of National Biography* entry for Robert Norman.

[25] Johnston, *Making Mathematical Practice,* 176-177.

This is possible, but perhaps it reflects a breaking down of social barriers since technical experts working on civil and other types of projects in this period came from a range of social backgrounds and disagreements between them about the best method of approach abounded.

Digges had to contend with such technical disputes when negotiating rival proposals for the rebuilding of Dover Harbour. William Borough, who encouraged Norman to print his discoveries, was one of the technical experts who bid for the contract for the project. Digges was advisor to the Privy Council that originally approved Borough's bid. Perhaps Norman's comments should be seen more as a healthy symptom of the inevitable tensions arising from the attempt to bring theory and the experience of craftsmen (amongst whom disagreements are not unknown either!) together in the solution of practical problems.

Despite their social differences, Digges, Norman and Borough would otherwise have had much in common. They would have been very much aware of the shortcomings of the average mariner. According to Eric Ash, English mariners were:

> conservative in their training, neither needed nor wanted to acquire the new mathematically based techniques. Indeed, they would have found it very difficult to do so in any case; the mid-century English maritime community lacked the sort of formal institutions, such as the Casa de Contrataciòn, that had facilitated the training of Iberian pilots in the new methods, and the Spanish and Portugese were hardly eager to share the secrets of their success with potential competitors. The quest to introduce mathematical navigation to England began not with English pilots but with the merchants and investors who hired them, as they sought to recover and expand upon their former profits through maritime exploration. [26]

Nevertheless, what Digges perhaps had not appreciated was that there were some problems that lent themselves to solution through the ingenious construction of experiments by those who, as Norman appositely said, had knowledge 'at their fingers

[26] Ash, *Power, Knowledge and Expertise*, 103.

ends'. It was not that Digges did not have practical knowledge and ability, and he emphasised the importance of experiment and the collection of accurate data through improved instruments. The accuracy of his astronomical observations and comments on the crude instruments of mariners are just one illustration of this. But it was possible for exceptional self-educated mechanics such as Norman to make new discoveries and devise ingenious experiments that crossed the barrier between the practical and the philosophical.[27]

And it seems that Digges may have been induced to modify his views after Norman's criticism, because, as Johnston recounts, in an expanded 'note for sea cards' of his second edition of *Pantometria* of 1691, Digges suggested that it would enable:

> The skilful and more learned sort of mariners [to] understand how to make such kind of observations of the variation of their compass in their Indian navigations, especially in circulating and environing the earth, as may reduce that most strange and irregular alteration of the nautical compass to a theoretical certain.[28]

Digges had certainly mellowed. Yet he nevertheless continued to maintain, (rightly as it turned out) that the determination of the theory of the trajectory was beyond the abilities of ordinary gunners. Whilst Norman chided Digges for wanting to use mariners as mere data-collectors to substantiate his theories, it is the case that accurate collection of information and the division of labour that this entailed would form an important part of the Baconian programme of renewal and was, indeed, a crucial component in the development of scientific knowledge.[29] And ironically, as Johnston notes, Norman's friend the navigator and naval administrator William Borough, to whom Norman's treatise was dedicated, actually made the same call for reliable

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[27] See Bennett, *The Mechanics' Philosophy*, passim.

[28] Digges, *Pantometria* (1591), 50. Quoted in Johnston, *Making Mathematical Practice,*188.

[29] See also, for example, Thomas Sprat's *History of the Royal Society,* 20: For some, 'security from noise, leaves the soul at more liberty' but 'it is in *Philosophy*, as in *Husbandry:* Wherein we see, that a few hands will serve to measure out, and fill into sacks, that Corn, which requires very many more laborers, to sow, and reap, and bind, and bring it into the Barn.'

observations from mariners, and was criticized for attempting to ‘fill his honeycombs with someone else’s honey’ in the same way that Norman had criticized Digges.[30]

As we will see, Digges’s approach to natural enquiry, as with the reform of the army and society, like Bacon’s programme, included the recognition of the need for an orderly division of labour. Though hierarchic, this division of labour was intended to optimize effectiveness for the benefit of all. I believe it is in this context that we should see his appeal to his readers to join him in his ballistics research programme, which could easily otherwise be seen as a rhetorical ploy. Thus I would argue that the whole question was far more complex than just being about Digges’s elitism; it was about the difficulties encountered in the programme to forge a new scientific approach to natural enquiry.

For patriotic reasons from his earliest publications, Digges was committed to the task of facilitating the spread of mathematical knowledge and Copernicanism to those who had not received a scholarly education. For this reason alone the accusation of elitism is somewhat problematic, even in the case of a younger, less experienced Digges. Also, as I will discuss later, Digges showed great compassion for the plight of the ordinary soldier when he was muster-master in the Netherlands. But as we will see in the next section, which provides a detailed analysis of his ballistics, his brilliance as a mathematician enabled him to envisage solutions to navigational and ballistic problems of a complexity that he feared few living in England at the time would be able to appreciate or understand.

[30] Johnston, *Making Mathematical Practice,* 187.

Digges's ballistics

'An Arithmeticall Militare Treatise, named *Stratioticos'* was first printed in 1579 and reprinted in 1590. It was completed and augmented by Thomas from the work of Leonard Digges and its main purpose was to teach the application of arithmetic, fractions and algebra applied to a military context. As Webb notes, *Stratioticos,* like *Pantometria*, was divided into three sections, Arithmetic, Algebra, and military Laws, Offices, and Duties, but

> whereas the first book quite often left to the imagination or ingenuity of officers the application of geometry and trigonometry to warfare, the second book strove to present numerous problems actually dealing with field maneuvers. For instance, the section devoted to algebra presented a method of computing the size of camping grounds necessary to hold various numbers of soldiers, a method of determining the range of artillery, and a method of computing the amount of pay and victuals due organizations of different strengths. It also illustrated methods by which a Captain of Pioneers might solve engineering problems, such as the number of laborers needed to cast up a trench of a given size within a given time. It is obvious, therefore, that the *Stratioticos* would prove extremely valuable to an Elizabethan army officer.[31]

Digges provides numerical examples for calculating the charge of powder for any piece, if the charge for one piece is known, using 'the numbers resulting by Cubicall Multiplication', since 'the Rule of Proportion playnlie fayleth'. He suggests an additional 'abate' of ¼ , according to the rule of gunners, if the cannon 'hathe not his proportionall masse of metal'.With regard to the calculation of ranges, he poses the following problem:

> If a falcon that carryeth point blanke 150 pase, at utmost randon randge 1300 pases, I demande howe farre a Culveringe at his utmost randon will reach, that at point blancke, or level, rangeth 250 pase.

In his answer he castigates the gross error of Girolamo Ruscelli Novarese, who claimed that the utmost ranges were in proportion to the weight of their bullets and charges of powder, and that the ranges are in proportion to the charges. In contrast

[31] Henry J Webb, 'The Mathematical and Military Works of Thomas Digges, wih an Account of his Life', *Modern Language Quarterly* 6 (1945), 389-400, on 393.

Digges argues that 'the diversitie of ranges is compounded of sundrie proportions' and may not be reduced to the rule of proportion. However, he maintains that there is one case where the rule of proportion applies, between point blank and utmost range. Thus, he is able to use the rule of three to provide an answer to the problem posed:

> I multiplie therefore 250, by 1300, there ariseth 325000, whyche divided by 150, yieldeth for the fourth proportionall 2166 ⅔, so many Paces shall the Culverine reach at the utmost Randon.
>
> And this by observations in one Piece, by this Arte of Proportion, a man maye discover the force of all other.[32]

It is perhaps worth noting here that the ratio between point blank and utmost randon that Digges gives is approximately 1:9, while the ratio that Tartaglia used was 1:10.

Thomas attached to the end of the work a section entitled 'certaine Questions of great Ordinaunce resolved from his other Treatise of Pyrotechnie, hereafter to bee published'. The questions take on an added significance if they are appreciated as being the first explicitly proposed research programme in ballistics. These questions were not new; as Matthias Schemmel has noted, they formed a 'common core set of questions' arising from the development of cannon warfare that became the shared knowledge of gunners and mathematical practitioners.[33] But this was the first time that they had been so succinctly and systematically brought together as the basis for further research and this is one of the features that marks out Digges's contribution compared to the approach of gunnery manuals.

Stratioticos was republished in 1590 with Digges's rather short answers to these research questions (often just a yes or no) added in the margin. A more structured response appears as an addition to the second edition of *Pantometria* (1591). It comprises an explanatory preface to the reader, a list of definitions and theorems taken from the third book of his promised treatise of pyrotechnia and great

[32] Digges, *Stratioticos* (1579), 64-5.
[33] Schemmel, *The English Galileo,* 25

artillery (which never appeared), followed by an exposition of a number of geometrical problems which Digges considered essential for someone to be able to understand the mathematics of his promised great treatise on artillery, reflecting his (correct) view that an understanding of the nature of the trajectory required mathematical skills at the highest and most abstract level.

Digges first explains that since this is a new science (of 'artilleray'), as with any new science it requires a new lexicon to provide the precision and clarity demanded of a science. Cannoniers do have their own terms that are good enough for them, but

> 'in the remote and hidden mysteries of Randonnes and of the severall Proportions of the mettalline Bodies and Soules or Cylinders of all several Peeces and of the strange varietie of the Circuites of all natural motions: In these mysteries…farre beyond the compasse of ordinary Cannoniers without exquisite knowledge of the *Mathematical Sciences* to intermeddle withal, I find not anye words in use that can serve my purpose, and have therefore chosen such as in my judgement are most proper and effectual, and take away all obscuritye, have set downe every of their *Difinitions* that are not in my *Pantometria,* already sufficiently amplified with new Additions, I have new published as Pathes to leade my ingenious Countrimen to the understanding also of this newe Science, which I shall publish the same in her best perfection.[34]

Digges continues that he has refined and condensed his previous questions and answers on artillery, which were designed 'to stirre up bothe Theoretical and Practical spirites of all sortes more profoundly to searche the hidden secrets of this Arte.' – into about 50 definitions and theorems. He admits that some of his theorems are not geometrically exact, and he has done this purposely to stimulate other practitioners to further research because he himself would not have experienced other 'more rare Secrets then I have hitherto thought upon' if the 'Scales and Theorickes' handed down to him from his father had been perfect. And yet without those first principles,

[34] Digges, *Pantometria* (1591), 175.

he would have got nowhere, despite having the benefit of free access to artillery to experiment with.

Digges trajectory, like Tartaglia's, has three sections, but he asserts that Tartaglia is wrong about the central part being a circle. Like Tartaglia, he says that in reality the trajectory is curved all along, thus admitting that his depiction is still an approximation:

> But if any Peece be discharged upon any angle of Randon, albeit the violent motion contend to carry the *Bullet* directly by the line Diagonall. Yet the Perpendicular motion being not directly opposite, dooth though unsensiblye even from the beginning by little and little drawe it from that direct and Diagonall course. And as the violent dooth decaye, so dooth the natural increase and of these two right lined motions is made that mixte curve Helical Circuite of the Bullet.[35]

This is very similar to Tartaglia's explanation for his assertion that the trajectory is continuously curved, except he replaces 'Perpendicular motion' for weight. Although Digges seems to consider the whole curve as helical in shape, like Tartaglia, he also treats it as three sections. He calls the first part straight 'For Distinction sake'.[36]

There is some ambiguity in Digges's description of the trajectory since he starts by saying (p.178) that after the initial straight part it 'falleth into a Curve Arke' but what he seems to mean is it falls from the 'line diagonall'[37] of the axis but it continues to rise to its highest point then continues its helical route until it reaches the same level as the beginning of the curved section, after which it falls perpendicularly:

> The second parte beeing a curve Circute, beginning at the foresaide declination from the Axis, ascending to the highest altitude above the Horizon, *and ending at a like Altitude to his beginning* I terme for Distinction sake his middell Helicall or Conicall Arke.[38]

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[35] *ibid.*, 183/167 (irregular pagination).
[36] *ibid.*, 178.
[37] *ibid.*, 179. The seventh definition: '*The peeces direct line of that circuit which is always about the* Hypothenusall *for distinction sake, I call the line Diagonall, for that there are several of these Diagonall lines to all Angles of Randon, and together with the Horizontall, doe comprehende the Angle of Mounte.*
[38] *ibid.*, 179, my emphasis. 'Conicall', presumably, means a conic section.

Digges gives no explanation for his assertion that the helical section begins and ends at the same level and it seems likely that it was a convenient approximation to assist in deriving his rules. Digges's use of the term helical is somewhat confusing and from the context it appears that what he meant by helical is that the curve is generated dynamically from the combination of two motions and that it will vary in shape depending on the velocity of the 'natural' motion, and the angular velocity of the 'violent' motion. His seems to use the term helical and spiral interchangeably and his inspiration probably comes from Archimedes' spiral because, like the trajectory, it is derived from two different motions, and the shape of the spiral is partially dependent on angular velocity and partly on the velocity of a point moving in a straight line from the centre. Thus there are analogies between the two though they do not correspond exactly.

He gives a possible indication of the direction of his mathematical investigations by defining a right-angled triangle made by drawing a perpendicular from the highest point of the trajectory to the horizontal. The hypotenuse of this triangle is the line from the point of highest altitude to the 'center of the peeces circular Base'. The angle of elevation is called the 'angle of mounte'. The trajectory that the cannon would follow without the force of gravity is the 'diagonall'. Since he does not provide diagrams that would help the reader to visualise this, I have attempted to reconstruct Digges's trajectory with some of his new terminology.

As my reconstruction (Figure 5) indicates, Digges's trajectory contains a number of new elements designed to aid analysis, which are not to be found in the depictions of the trajectory by other authors. The sixteenth, seventeenth and eighteenth definitions state that:

> And that which forms to discover the different violence of all Peeces at Pointe Blanke howsoever Mounted (by me termed the lines direct of the Bullets

> Circuites, and lines Diagonall) I call for Diffinition sake the Theorike of the Diagonall. The other that discourseth how high Bullets at all Randons can Mounte possiblye above the Horizon, I terme the Scale or Theorike of Altitudes. The other that discourseth the varietie of Ranges, of all Peeces at all degrees of Randome, I call the Theorike or Scale of Randons.[39]

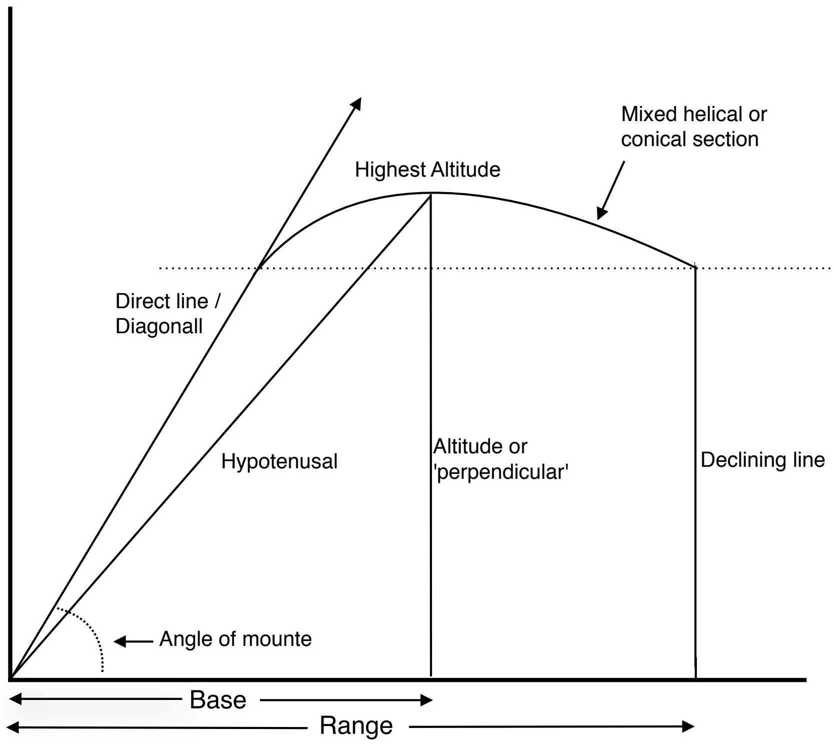

Figure 5. A reconstruction of elements of Digges's trajectory

Digges appears to be investigating a *theorike* about the violence of the force of a trajectory (from the amount of powder and perhaps other factors), which is given by the diagonall, as well as a *theorike* about the variation at different ranges. Of Daniel Santbech, and others, he says:

> So is it far more impossible and absurd to imagine, that any ignorant of those Sciences, should ever be able to approach the Gates of that Art, hitherto by no Nation to any purpose handled, for to passe over the apparent Errors of Daniel Santbech ye German in his booke *de Artificio Eiaculandi Sphaeras Tormentarias*: the false rules of Girolamo Ruchelly, & grosse errors of many others, yt being ignorant of ye Mathematicals, have taken upon them to write of this Art: Even Tartalea the Italian, albeit he were an excellent Geometer, taking upon him to deliver sundrie Demonstrations in this new Science, yet for

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[39] *ibid.,*180.

> want of Practise, and Experience, hath erred even in the first Principles, and so consequently in the whole substance of his discourse. [40]

Despite his insistence on the importance of an exquisite knowledge of the mathematicals, it is Tartaglia's lack of practical experience that he believes has let him down.

Although I suggested that Tartaglia's proposed range tables may have been based on emprirical data, this was not the case with Digges. He does appear to have been working on demonstrative geometrical methods. In Theorem 31 Digges tantalisingly claims that the relation between the range and the angle of elevation can be reduced to a *theorike certain,* and the theorem suggests he has been examining trigonometrical relationships as sources for the *theorike*:

> The Gradual increase and decrease of these Ranges Horizontal, albeit they are equal in the Quadrant, yet are they neither equal nor proportional in the Horizon, neither the Ranges nor their intervals. Neither compared between themselves nor yet conferred with the Chordes or sinus of their Arkes. And yet is there such a kind of Proportional increase and decrease of the Proportion of their intervalles, as may be reduced to a *Theorike* certain.[41]

The fifteenth definition gives the best insight into Digges's complex conceptualisation of the helical part of the trajectory and its reflection of his knowledge of astronomy:

> And as for the discoverye of the strange varietie of the planets courses in the Heavens, Astronomers are enforced to use sundrie kindes of supposed Circles Eccentrical and Concentricall, with their epicycles moving regularly sometimes on their owne Centers and sometimes on Centers of Aequation, which they terms that Planets Theoricke. So likewise in the discovering of the reciprocall most strange varietie of these Bullets Helicall circuites in the Ayre, making continuall alteration according to the Quantitie and Qualitie of the Angles of Randon or angles of Inclination or Declination of the Plaines whereon the bullets playe [ie if the piece is on level ground or pointing up or down a slope] or angles comprehended betweene the Diagonal violent and Perpendicular naturall motions: Being I say likewise enforced to use portions of circles, some Concentricall and some Eccentrical, some uniforme devided on their owne Centers, and some from Centers Equant in more strange manner

[40] Digges, *Stratioticos* (1590), 358.
[41] Digges, *Pantometria* (1691), 183/167 (irregular pagination).

> to discover and finde out these Helicall motions, I doe likewise terme the same Theorike or Scales.[42]

Digges also appears to relate the shape of the conic section to the angle of elevation. He suggest that every particular piece of ordinance has its own conic section according to its optimum conditions of operation (charge etc). He considers that at the 'utmost randon' the shape is a parabola. As the angle decreases you get an ellipse. Above the angle of maximum range, he suggests that the shape is a hyperbola, and that the angle made with the axis of the cone is the complement of the angle of elevation. He then hypothesises whether the parabolic section arises at 45 degrees rather than at the angle of maximum range (which he considers to be 40-44 degrees). Despite his recognition of the effect of air resistance as a factor to be considered, he does not seem to want to consider it as a factor that might affect the empirically derived maximum range, if it was derived empirically.[43] Perhaps this is the reason why he concludes that the arcs are helical, but they 'have a verie neare resemblance' to conic sections. [44]

Digges's rejection of the conic sections, despite a neat but problematic analogy between angle and type of conic, seems to arise from the fact that the spiral, as I have noted, was derived from a dynamic interaction between two motions. Thus Theorem 21 gives Digges's reason for the choice of the Archimedian spiral as the central part of the trajectory. He explains that just as the Archimedian spiral is made with the direct motion of a point in a straight line while it is at the same time being turned in a semidiameter around the circle's centre, so the artillery helical line of the

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[42] *ibid.*,180.
[43] Despite early recognition of the effect of air resistance, there seems to be an underestimation of its effect except when it is windy, which may explain why Digges does not seem to doubt that 42 degrees is the maximum.
[44] It seems to me that Digges might have seen a contradiction here, as his analogy with the conic sections implies a symmetry that fits better with Tartaglia's 45 degree maximum trajectory. Perhaps this is why he decided that the trajectories were 'very neare' conic sections, but actually helical.

bullet's circuit is created by two 'right lined motions' forming a curve according to the different angles of elevation:

> As *Archimedes* line Helicall or Spirall, is made by the direct motion of a pointe carried in a right line, while that right line is Circularly turned as Semidiameter upon his Circles Centre. So is this Artillery Helicall line of the Bullets Circuite created onely by two right lined motions becoming more or less *curve* according to the difference of their Angles occasioned by the severall Angles of Randon. Whereupon by demonstration Geometrical a *Theorike* may be framed that shall deliver a true and perfect description of those *Helicall* lines at all angles made between the Horizon and the Peeces lines Diagonall.[45]

Furthermore, the spiral can be constructed through pointwise construction and Digges is the first to imply a method of constructing a trajectory by mathematical means in this way. As Schemmel has noted, this was precisely what Thomas Harriot succeeded in doing.[46]

As we have seen, an additional attraction of the helix for Digges was its formation from an angular motion in the same way as the gun moves through an angle. It is difficult to see how Digges would have developed this but one can see that he was struggling to make some link between change in angle and change in shape and length of the trajectory, and that he was committed to finding a geometrical explanation, perhaps by drawing on the ingenious combination of algebra and geometry practised in his earlier work on the platonic solids and astronomy.[47]

Finally, Digges includes certain basic geometrical problems (p. 173) of importance to his complete work on artillery. It included finding mean proportionals, squaring shapes, including the circle, finding the proportional relationship between different shapes; how to make a lunar shape from any 'rightlined plaine' shape (p191); how to find the centre of gravity of various right lined shapes, and how to

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[45] Digges, *Pantometria* (1591),168/184 (irregular pagination).
[46] Schemmel, *The English Galileo,* 34 and 154.
[47] Harriot too, unlike Galileo, used algebra in a creative way in his work on the trajectory – reflecting England's vanguard role in the development and creative use of algebra during this period.

double or treble (the volume) of any of the five platonic bodies and their transformations.

His commitment to the development of theory through empirical observation is also something that he carries from his work on parallax methods. In the *Alae* he declares:

> I perceived that the ancients had proceeded in inverted order: from their invented planetary theories they sought the true distances and parallaxes. But they should have instead proceeded the other way around, starting with observed and known parallaxes, and then considering the planetary theories.[48]

Digges explained with admirable clarity the many variables in shooting, how they acted and the extent to which they were subject to control and *theorick*. For example, his correct answer to the question of how the range varies with the length of the bore, that it increases with length of bore up to a certain point only and then declines, was a question of great technological significance relating to the construction and choice of the most efficient guns. [49] He considered that he had successfully resolved the greater part of the research questions,

> and briefly opened divers great *Secrets* of that *Science,* by my father first found out, and never since his death to this houre by any els (stranger or other) discovered, or at least published in any *Language* to my knowledge, leaving the rest and many more (hitherto to the world unknown) to be fully accomplished in that *Treatise:* Wherein also by exact and most rare Instruments shal be taught the perfect Arte to shoot at all Randons from one grade of altitude to 90 with all kinds of peeces, with Rules infallible to find out their severall Ranges, & that not onely on level grounds equidistant from the *Horizon* but also upon plaines, enclining or declining whatsoever the angle of inclination be. [50]

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[48] Translation from *Alae,,* sig. A2v, by Robert Goulding in 'Wings (or stairs) to the heavens: The parallactic treatises of John Dee and Thomas Digges' in Stephen Clucas (ed.) *John Dee: Interdisiplinary Studies in English Renaissance Thought* (Dordrecht: Springer, 2006), 50.

[49] Charles V of Spain had ordered research on this question too and found similar results to Digges. The reason (not understood at the time) is due to the increasing effect of friction that eventually counteracts the increase in range that is achieved by lengthening the barrel. See Mark Denny, *Their Arrows will Darken the Sun: The Evolution and Science of Ballistics* (Baltimore: Johns Hopkins University Press, 2011), 50.

[50] Digges *Stratioticos* (1590)*,* 361.

His great treatise on artillery was ambitious, proposing to provide instruments and infallible rules for accurate shooting for any gun, not only on level ground but also on a slope. The latter case, as we will see in later chapters, was an important practical question that engaged Torricelli and others after Galileo's proof of the parabolic trajectory. As we will see in the next chapter, the analysis of the behavior of the separate components of motion in the trajectory was crucial to the proof of its parabolic shape. As I will show in the next section, there is some evidence that Digges engaged with the physics of falling objects.

Falling objects, ships and Copernicanism

Hitherto unnoticed in Digges's ballistics is theorem five that seems to indicate at least some investigation of natural motion and its relationship to the theory of the trajectory. He says that if you drop two bullets of unequal weight but equal 'quantitie' (the context suggests volume) from a high place the heavier will fall more swiftly but not proportionally to their weights, which renders 'erronious' the axiom of 'a great Philosopher', presumably Aristotle. Digges's recognition that heavier weights do not fall proportionally faster than lighter ones of the same size suggests he had been experimenting with falling objects as part of his research into ballistics. It is (I think) the first time that anyone has suggested that the rate of fall is not proportional to the density. It is different from Benedetti's discovery that objects of the same density will fall at equal speed regardless of size. In fact, Benedetti accepted as a given that the time of fall of two objects of the same shape and size will be in proportion to their density.

Benedetti's result arose from the application of the principle of Archimedes, and he published it in the dedication to his *Resolutio* (1553) to ensure that it was not stolen by others, and in his *Demonstratio* (1554). It was plagiarised by Johannes Taisnier and this version was translated into English by Richard Eden in 1578 as *A very necessarie and profitable Booke concerning Navigation… by Johannes Taisnerius*.[51] Stillman Drake suggests that it is not coincidence that Benedetti's discovery appeared not long after Tartaglia's vernacular publication of Archimedes' *On Bodies*.[52] Digges gives no indication as to how he reached his conclusion on falling bodies, but it seems more likely to have been obtained by experiment, rather than theoretical reasoning – perhaps by dropping objects from the masts of ships.

Given that Digges, like Benedetti, recognised the challenge the result makes to Aristotle's physics, it seems surprising that he does not include a mention of Benedetti's important discovery, since it is introduced by John Dee in his *Mathematical Praeface* (1570), in the context of Dee's discussion of the importance of the science of statics:

> Statike, is an Arte Mathmaticall, which demonstrateth the causes of heavynes, and lightnes of all thynges: and of motions and properties, to heavynes and lightnes, belonging. And for as much as, by the Bilanx, or Balance (as the chief sensible instrument,) Experience of these demonstrations may be had…

Dee proceeds to provide a summary of Archimedes' main conclusions from *On Bodies,* in which he refers to Benedetti's result. He continues:

> By these verities, great Errors may be reformed, in Opinion of the Naturall Motion of thinges, light and Heavy. Which errors, are in Naturall Philosophie (almost) of all men allowed: to much trusting to Authority: and false Suppositions. As. Of any two bodyes, the heavyer, to move downward faster then the lighter. This error, is not by me, Noted: but by one *Iohn Baptist de Benedictis.* The chief of his propositions, is this: which seemeth a Paradox. If there be two bodyes of one forme, and of one kynde, aequall in quantitie or unaequall, they will move by aequall space, in aequall tyme: So that both theyr movynges be in ayre, or both in water: or in any one middle. Hereupon, in the

[51] Drake and Drabkin, *Mechanics in Sixteenth Century England,* 34.
[52] *ibid.,* 32.

> feate of Gunnying, certaine good discourses (otherwise) may receive great amendment, and furderance.[53]

Digges omission of a mention of Benedetti is even more surprising given the fact that as this quote shows, Dee specifically points to its importance for gunnery. We have no way of knowing if his promised treatise would have had a more thorough treatment of weight and fall, but this easily-overlooked comment suggests a possible direction of his enquiry.

R. S. Westman discusses the question of experimentation on falling objects in relation to Digges's hopes to use the new star of 1572 to find a conclusive demonstration of Copernicanism based on parallactic measurements. These hopes were dashed when the 1572 nova disappeared after 16 months, which was inexplicable in terms of his model of the cosmos.[54] He suggests that perhaps as a consequence of his failure to find experimental proof of Copernicanism, in *A Prognostication* Digges attempts to make more robust Copernicus's argumentation based on comparison of probabilities. One of the ways he does this is by the insertion of an additional comment about what happens when an object is dropped from the mast of a ship. Digges has Copernicus say:

> No other wise then if in a shippe under sayle a man should softly let a plummet downe from the toppe alonge by the maste even to the decke: This plummet passing always by the straight maste, seemeth also too fall in a righte line, but beinge by discourse of reason wayed his motion is found mixt of right and circulare.[55]

Westman expresses disagreement with Francis Johnson's view that Digges had probably made this experiment himself. However, Digges repeatedly emphasized the importance of experiment, or 'trials', both for astronomy and ballistics; and his

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[53] John Dee, *The Mathematical Praeface* to *Elements of Geometrie of Euclid of Mengara* (London: John Daie, 1570) sig. C1r.
[54] R S Westman, *The Copernican Question: Prognostication, Skepticism, and Celestial Order* (Berkeley, California: University of California Press, 2011), 272.
[55] Quoted in Westman, *The Copernican Question,* 279.

mention about the speed of falling objects in his work on ballistics indicates that he might have done some trials with falling bodies. Of further interest is that Digges here perceives the fall of the object in a straight line as a dynamic combination of a circular and straight movement, just as he would conceive the trajectory as the combination of two motions.[56]

Westman suggests that Digges would not have written in the conditional mood if he had actually carried out the experiment; but despite his stress on the importance of experiment, Digges never gave specific details of the ballistics trials he made, despite the fact that he refers to the advantage he had in having access to guns to carry out trials. One of the reasons he did not do so may have been his inclination to secrecy that his (justified) political paranoia encouraged. Another reason is that it was only in the course of the seventeenth century that attitudes to the detailed reporting of experiments changed so the historian has great difficulty in judging from printed texts whether experiments were performed or not.[57] However, the addition of the adjective 'softly' suggests experience rather than thought experiment – one might be more likely to describe the manner of dropping the object if one had actually done it.

Johnston notes that in *Stratioticos*, Digges described a fifteen week sea voyage to test out his navigational theories and instruments. Whilst this trip might have occurred after the publication of *A Prognostication Everlastinge* it does not preclude the possibility that he did some experimentation on a ship before then, since his social rank and contacts would have given him access to such opportunities. The fact that he took this voyage shows both his ability to obtain such opportunities and his commitment to testing his theories in practice. Digges was a Platonist, but though he

[56] It seems here Digges considered the motion circular because it followed the motion of the earth, as did Galileo.
[57] Bertolini Meli, *Thinking with Objects,* 11-12.

might have got someone else to climb the mast for him, one is inclined to doubt that he was 'a Platonist who never left his comfortable chair.'[58]

Though Digges's theoretical work on the trajectory was self-confessedly esoteric and difficult, this was only a small part of his output as a popular and successful publisher of works on practical mathematics, and it is important to view him within the context of a community of mathematical practitioners who took an interest in improving gunnery practice. Digges was a practical mathematician who worked with other mathematicians who took on roles as technical advisers on civil and military projects, and in the next section I will discuss some of the contemporaries that he would have encountered and with whom he may have collaborated.

Digges's relationship to other gunnery writers

William Bourne was aware of Digges's *Pantometria* and recommended it in the preface to his *Art of Shooting in Great Ordnance* for those wishing to learn how to calculate the distance to a mark, especially at sea. Bourne's book was completed but still in manuscript form in 1578, the year before *Stratioticos* appeared. It was only published posthumously in 1587, no doubt in anticipation of a Spanish invasion. He had practised gunnery as a citizen volunteer with the garrison manning the defensive bulwark at Gravesend.[59] He was self-taught in mathematics and though he mentions Tartaglia, his work on gunnery bears the mark of personal experience and is not, as some have suggested, simply a plagiarized version of the *Nova Scientia*. His *Inventions and Devises,* which includes possibly the first description of a device for

[58] Westman, *The Copernican Question,* 279.
[59] See entry on William Bourne by G. L'E. Turner in the *Oxford Dictionary of National Biography*.

testing the strength of gunpowder, may have been influenced in its structure and style by Tartaglia's *Quesiti*.

A further connection between Digges and Bourne can be surmised since Bourne's work is dedicated to Ambrose Dudley, at that time master of the ordinance, and brother of Digges's patron the Earl of Leicester. It is not known to what extent people like Bourne, Digges, the mathematical practitioner Thomas Bedwell (who also worked for Leicester), and others might have shared trial information or collaborated in any way in gunnery trials, which would have been sensible given the cost of trials and the possibility of at least some overlap in time periods when they were working on the same problems, especially given the urgency of the military situation.

A possible site where trials might have taken place was the Bishopsgate Artillery Garden. Steven Walton has highlighted that the property belonged to the Ordnance Office and seems to have been used as a site of instruction for gunners for some time. He notes that both Bourne and Digges lamented the poor quality of training for gunners and that it was after the publication of their works that the Artillery Garden began to flourish, and he suggests that the trainees of the artillery garden might have been the target of their writing, rather than the general audience of mathematical practitioners.[60]

Digges was one of a number of mathematicians who worked as advisers in the Earl of Leicester's circle at the time of the Spanish Armada and the wars in the Netherlands. It was a period of political tension and increasing military threat from invasion. One cannot read Digges's works without being struck by what could be interpreted as his extreme paranoia. Digges had even felt the need to robustly defend his pure mathematical discoveries in *Pantometria* from the 'two footed Moules and

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[60] Steven Walton, 'The Bishopsgate Artillery Garden and the First English Ordnance School' *Journal of the Ordnance Society* 15 (2003), 41-51, on 49.

Todes, whom destinie and nature hath ordained to crawle within the earth, and such upon the muck'.[61] While Johnston focuses on Digges's statements as evidence of his negotiating a mathematical identity for himself, I want to look at his defensive statements regarding critics of even his seemingly innocuous pure mathematics, within the more general context of attacks on those who advocated mathematics as part of a programme of modernisation and renewal of learning. Both Digges's and John Dee's writings are loaded with cryptic comments about unnamed critics. To understand his paranoia, I will investigate further the ideological context for Digges's constant need to defend his writings and actions against the enemies who were continually plotting against him.

Plots: real or imagined?

Though he does not get a mention in Carol Z. Wiener's important article on Elizabethan and Jacobean Anti-Catholicism,[62] Digges's writings show him to be an archetypical example of the behaviour she describes. Wiener notes the importance of *Foxe's Book of Martyrs* in galvanising English Protestantism against the Papist threat and in forging an English national identity. She has analysed in detail the nature of the paranoia that it engendered, which as she says, was if anything exacerbated rather than assuaged by the victory over the Armada, since 'to outwit the Roman Church at any point was to grasp a tiger by the tail'.[63] And her research provides ample evidence to show that the threat, both internal and external, continued to remain all too real.

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[61] Digges, *Pantometria,* Preface to the Reader (1571) sig. A4, (1591, sig. A4v).
[62] Carol Z Wiener 'The Beleaguered Isle' *Past and Present* 51 (1971), 27-62.
[63] *ibid.*, 33.

This paranoia, an 'almost insuperable anxiety'[64] permeates Digges's writings and he had considerable influence, through his publications and through his close advisory relationship to Queen Elizabeth's long-term favourite, the Earl of Leicester, as well as through his parliamentary, civil engineering and military activities. Though emotionally charged, his writings are distinguished by the astute and carefully argued quality of his political and military assessments and his persuasive rhetorical style. His combination of exceptional talents and integrity invoked resentment in those that did not measure up to his high standards.

Printed posthumously in 1601 is a tract entitled *Humble Motives for Association to Maintaine Religion Established published as an antidote against the pestilent treatises of Secular Priests.* The first part, written by Digges, is an entreaty to the Queen to take action against the threat of Catholic ambitions by enacting an oath of association for the defence of religion, as the lack of a successor to the aging Elizabeth increased fear for the future. Rigorously argued, and remarkably politically astute and historically prophetic, it describes in detail the characteristics of four religious categories, Protestants of religion, Protestants of state, Catholics of religion and Catholics of state, and how the balance of power can best be shifted from the latter to the former.[65]

Digges's aim was to neutralise the papist threat by drawing a wedge between those (the majority, he astutely believed) who supported the papacy for material reasons and those who were genuinely ideologically committed to superstition. The former would be forced into loyalty on threat of losing their lands, and the latter would be rendered powerless by the loss of their lands.

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[64] *ibid*., 29.
[65] Digges, *Humble Motives,* 11.

Digges worked closely with Elizabeth's Privy Council both as expert mediator on the Dover Project and as a member of Parliament. He played a important advisory role in the succession crisis, in particular for his proposal that Parliament should never be dissolved during Elizabeth's lifetime and that the members of the Privy Council should act as a provisional government after her death, which has been considered a watershed in republican political philosophy.[66]

As I will show, though Digges had no doubt that God was on the side of Protestantism and the Commonwealth, his classically Calvinist providentialism was one that required active intervention. Indeed, this combination of providentialism and interventionism is arguably what gave puritanism its social and psychological drive. Digges pursued a belt-and-braces approach that involved political and military preventative measures. He was deeply concerned about the ability of Britain to withstand either the power of Catholicism for internal subversion or the superior military resources, organisation and discipline of the Spanish Empire. The early years of Elizabeth's reign had seen relative leniency towards Catholics since it was believed that Catholicism would gradually lose its attraction; but the bull of Pope Pius V excommunicating Elizabeth as a heretic made every Catholic a potential traitor and this, combined with high-profile conversions such as that of Edmund Campion and regular plots, led to renewed fear and repressive action against Catholics. Digges was acutely aware of the success and enviable discipline of the Jesuit order, as well as the military threat from Spain. The increasing fear is evident in Digges's writings and mathematics was for him a crucial tool to be brought to bear as part of the forging of the religious and military order and discipline required to overcome the enemy. This makes his relationship with Leicester an important one to explore, and it will be

[66] Ash, *Power, Knowledge and Expertise,* 231.

helpful to look in more detail at the way that Leicester used and was used by his protégées.

Robert Dudley, Earl of Leicester was the leader of the moderate Puritan or 'progressive' party.[67] Both his natural inclinations and political motivations led him to support the advancement of learning of all kinds as a means of strengthening the state. He masterminded a vast, unprecedented propaganda machine; as Chancellor of Oxford University[68] one of his aims was to develop the intellectual level of the clergy as the best means of countering the Catholic threat. This dovetailed with the not unreasonable belief that Catholicism thrived on ignorance and superstition and that a highly educated and articulate clergy was the best way of maintaining the loyalty of the population.

Leicester defended Puritans because he recognised their value as the most militant, committed and effective fighters against Papism and the threat from Spain. They in turn relied on Leicester to get them out of scrapes and to find them positions where they would not antagonise the Anglican bishops, a service that did not prevent them from reprimanding him about the moral danger posed by his patronage of the theatre.[69] They attested their debt to him in the hard times that came after his death, which was the 'signal for the great assault upon the Presbyterian and moderate Puritans which Hatton, Whitgift and Bancroft had been itching to launch for years'.[70] In *Humble Motives*, the anonymous author of the open letter to the archbishops which follows Digges's address to the Queen warns that the attacks on the Puritans plays into the hands of the Papists' strategy of causing division between their enemies, and he reminds them that 'when the Earle of Leicester lived, it went for currant. that all

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[67] In comparison with the 'conservative' Catholic party. See Rosenberg, *Leicester, Patron of Letters,* passim.
[68] William Cecil, Lord Burghley, was appointed Chancellor of Cambridge.
[69] For example, see Rosenberg, *Leicester, Patron of Letters,* 254 and 305 in relation to Leicester's protection of the non-conformist minister John Field in his many scrapes with the Anglican authorities.
[70] Lawrence Stone, *Crisis of the Aristocracy* (London: Oxford University Press, 1965), 737.

Papists were Traitors in action, or affection. He was no sooner dead, But …Puritans were trounced. and traduced as troublers of the state…'[71]

Leicester packed parliament with his like-minded supporters by his patronage of people like Digges. He was often referred to as a Mycenas in the eulogising dedications of his protégées, but Eleanor Rosenburg makes the point that our modern scepticism towards servile and over-flattering dedications should not lead us to dismiss these dedicatory materials as being less than genuine.[72] Digges was one of a number of writers who continued to defend Leicester after his death, and his writings attest to the strong bond of mutual trust and esteem between them. Leicester's policy was to encourage men whose special knowledge was of value to the nation, and he had total faith in Digges's ability and probity.[73]

Stephen Johnston has rightly criticised a crude and narrow depiction of patrons as uniquely powerful in their influence over clients, and asserts that such accounts are 'flawed in their oversimplification and inadequate for the specific case of Digges. For Digges had, in principle, considerable freedom to determine his own actions and allegiances.'[74] The evidence suggests Digges allied himself with Leicester so that he could not only support but also influence the direction of his political policies. As Digges states in his dedication, *Stratioticos* was originally begun to assist in the planning of a campaign by Leicester – probably a reference to the campaign to the Netherlands in 1577 that Leicester originally intended to lead at the request of the States.[75] In 1584-5 after Leicester had successfully nominated him for the parliamentary seat of Southampton, Digges sent him a 'platt of militarie ordinance'

[71] Digges, *Humble Motives*, 25. Also quoted in Stone, *Crisis*, 737. Stone wrongly attributes this to Digges, who nevertheless would likely have been in accord with the sentiment.
[72] Eleanor Rosenberg, *Leicester, Patron of Letters*, 13-14.
[73] See for example Rosenberg (1955), 285, where Leicester is quoted describing Digges as a 'very wyse and stout fellow' and 'very carefull to serve thoroughly hir majesty.'
[74] Johnston, *Making Mathematical Practice,* 55.
[75] Rosenberg, *Leicester, Patron of Letters*, 283.

with a cover letter urging him to lead a force to the Netherlands and offering his services to the campaign.[76]

Through the writings of his other protégés, Leicester might also provide a clue to the identity of the particular enemies to which Digges alludes in his defense of his geometrical treatise. He had many enemies, but it is more difficult to see why Digges should have been so conscious of the sneers and attacks of enemies when presenting his mathematical treatise on the Platonic solids, which would seem to be harmless enough.

The answer might lie with another grouping that expresses very similar sentiments towards anonymous critics. Leicester was the major patron of translators of both classical texts and works written in European languages. Rosenberg notes that the dedications of this particular grouping of translators are characterised by their 'fear of criticism, and their insistent and repetitious self-defense'. She suggests that the anonymous but singularly influential carpers referred to in these texts were Roman Catholics who 'eyed with misgiving the whole educational effort of the Protestant rulers, designed as it was to foster an enlightened public opinion, a codified morality based on the Tudor myth and nourished by the humanities, which would be a bulwark against the agents of the Pope.'[77]

Whilst Digges was not included in this literary grouping, the anxiety, the self-justification, even the reference to the temptations of 'delectable' studies are all there. And while Digges turned to the application of mathematics to strictly military utilitarian ends, the translator group, in parallel, abandoned the classics for religious and utilitarian works, particularly translations of Calvin. The choice between the

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[76] *ibid*., 284.
[77] *Ibid*., 181-2.

contemplative and the utilitarian seems to have become a rhetorical weapon in the grumbling civil conflict.

Puritanism's success in this period depended on the protection of a relatively small number of great magnates – notably the Earls of Bedford, Huntingdon, Leicester and Warwick and the 'irrepressible Duchess of Warwick.'[78] Lawrence Stone calculated that in 1580 there were 66 English peers, '20 of whom were Catholic recusants, about 10 were of strongly Puritan sympathies, about a dozen were supporters of the Anglican settlement, and the remaining 24 were relatively indifferent to religious issues and anxious only to back the winner.'[79] Seventeenth century English Catholicism was largely rural and located in geographical pockets centred round a great house, while Puritanism was a movement where the initiative came from the laity and was much more socially diverse, headed by a handful of peers.[80] Numerically vulnerable, this grouping was much more dependent on propaganda to rally its diverse forces, the outstanding success being the harnessing of large sections of the educational system to its cause.[81]

But if Leicester made use of the pen, he must also have felt the need to have the backup of the sword (or rather, the cannon), and the advantages that might be gained from superior technical advice, provided by Digges and other experts. It is evident that he was preparing himself for all eventualities. According to Lawrence Stone, 'the 1550s and 1560s saw not only an increase in the scale of aristocratic armouries but also their modernisation.'[82] The rapidly expanding cannon industry of the Weald made it possible for peers to amass military hardware without much

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[78] Stone, *Crisis,*734.
[79] *ibid*., 741.
[80] *ibid*., 733.
[81] *ibid*., 735. Note Sprat's *History of the Royal Society*, 22-3, suggested three possible reasons for the recovery of knowledge: Printing, the 'hatred, which was then generally conceived against the blindness, and stupidity, of the *Roman Fryers'*, and the Reformation, 'which put men on a stricter inquiry into the Truth of things'.
[82] Stone, *Crisis,* 219.

difficulty, which they proceeded to do. But the military hardware and stores of gunpowder of the rest of the peers 'pales into insignificance…beside the armaments assembled in the heavily fortified castle of Kenilworth in the 1570s and early 1580s by the Earl of Leicester... The purpose of these extraordinary preparations is not certain. It may have been insurance in the case of civil war: it may have been a blackmail weapon with which to browbeat Elizabeth if he lost favour; it may have been for the protection against attack by his enemies, who were legion. At all events he turned Kenilworth into a fortress …Leicester was in a position to defy all comers, even perhaps his sovereign. He was the last of his kind in English history.'[83]

With his brother Ambrose Dudley (Earl of Warwick) as Master of the Ordinance at the Tower and Leicester's military arsenal at Kenilworth, and with Digges as the foremost amongst a number of experts on military matters at his disposal, it is perhaps not surprising that despite the relative numerical weakness of his party within the aristocracy, his enemies generally chose to rely on the weapons of lies, gossip and rumour rather than open combat.

Leicester's patronage of learning represents the cultural and educational facet of the social, political and economic programme of the moderate puritan faction. Their battle was fought with both ideological and military weapons. Just as the classics texts of Euclid and Archimedes were studied to gain advantage in the precision of gunnery, in the next section I will show how classic texts of Roman history and military methods provided Digges with inspiration for his programme of military reform.

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[83] *ibid*., 220-221.

Digges's programme for military reform: The discourses on military discipline

Though Dudley Digges did not publish any of his father's unpublished mathematical work after his death, he did publish a Latin translation of Thomas's work on geometrical solids, and in 1604 he also published Thomas's treatise on military discipline. It is an impassioned plea for military reform based on his bitter and disillusioning experience in the Netherlands. He was horrified by the corruption and waste and how it not only drives away the best soldiers but also leads to the abuse of local populations, driving them into the arms of the enemy. Digges's logical mathematical mind could not understand why others failed to recognize the false economy of skimping on soldier's pay and provisioning, so that only 'Free-booters and Theeves' are attracted to army life.[84]

He was right that this type of corruption ended up being more wasteful and inefficient than paying an appropriate wage, but given Elizabeth I's ambivalent attitude to the war and the desire to keep costs to a minimum, there was always going to be the temptation, even the necessity, of trying to do things on the cheap.

Problems of state funding did not just apply to the army and would become a particular problem of state administration during the Stuart period where the scramble for appointments and monopoly rights and the expense of James I's permanent Christmas became a particular cause of discontent. Corruption was endemic to the system, drawing in even those who recognized the waste and erosion of moral values that it engendered. Thus despite his own conviction for taking bribes, Francis Bacon has the residents of his utopian *New Atlantis* refuse all offers of gifts of appreciation of their kindnesses since they are generously compensated by the state and to be 'twice paid' would be a dishonour. [85]

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[84] Digges, *Foure Paradoxes,* 3.
[85] Bacon, *New Atlantis* in Brian Vickers, ed., *The Major Works,* 459.

An indication of the antipathy that Digges had encountered in his attempts as muster-master to root out corruption in the Netherlands is given in a letter from Edward Burnham to Walsingham. Burnham reports that:

> Here groweth great difficulties between the muster-master and the captains…I fear me that Sir Thomas Shirley shall have something to do with the muster-master and the auditor, for they run a violent course; I think no more than their instructions do guide them, but it might be done with more moderation. It is a great authority to the muster-master that the treasure cannot be disbursed without his warrant. He was pontifical enough before, this maketh him more.[86]

Digges had a vision of a prosperous commonwealth with a disciplined army that operated on modern efficient lines. In keeping with the keen interest in classical history that had developed in the sixteenth century, he looked to the Roman Empire as his model and believed its defeat was caused by its corruption. Using a characteristically puritan metaphor as rhetorical device, he contrasts the 'Autient' with the 'Moderne Martiall Discipline' devised wholly for the profit of corrupt persons:

> But after they have learned abroad in these mercenarie warres this foule, base, couning and corrupt cowardly Discipline, to bring it home into their native Countries: who seeth not it must of necessity worke in time the very ruin of their State? For as a woman that hath once made shipwracke of her honestie, easily maketh a relaps: So fareth it in these mercenary Fugitives, that having once cast away shame (which onlie or chieflie maketh men resolutelie to sacrifice their lives for their country) afterwards become so far past shame, as they hold it no disgrace by shameful flight at any time to save themselves.[87]

Digges's constant reference to the lessons of the Roman Empire illustrates his fear that history could repeat itself. His accomplishment as rhetorician should not undermine the recognition that his mission to 'awake our Nation out of that secure Dreame, having partlie by myself seene, what extreame disorders growe, and dishonours are received for want of Militarie Discipline', arose from genuine conviction, and as I

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[86] Quoted in Webb, *Elizabethan Military Science,* 191.
[87] Digges, *Foure Paradoxes,* 68.

will show in the next section, his proposals for reform were part of a wider European movement linked to the development of the early modern state.[88]

The European context: Justus Lipsius and military reform

In the 1590s a successful military reform based on study of the classics was instituted in the Dutch army by Prince Maurice of Nassau, as part of his drive to revitalize the campaign for independence from Spain. His new model army based itself on the Roman legion system, but also on the recognition that 'the best tactical mix of musket and pike required a new degree of control, combined with a new style of combat leadership and more training.'[89] It was under Prince Maurice's leadership that English soldiers would finally receive the organization, training and discipline that Digges fervently advocated, though England would have to wait until 1645 for major reform, with the institution of the New Model Army by the Parliamentarians. Whilst the importance of the mathematician and military engineer Simon Stevin as military advisor to Maurice during his successful campaigns should not be forgotten, the thinker who was more than anyone else responsible for inspiring his military reforms was the Belgian savant Justus Lipsius. Maurice was his student during the period when Lipsius taught at the University of Leiden, between 1579 and 1591.[90] Lipsius was a transmitter of Roman Stoicism, who argued for the need for such reforms in the fifth book of his *Politicorum libri sex* (1589). The latter is a collection of aphorisms from ancient historians and philosophers, but the fifth book is more of a technical handbook on how to build up a standing army. Gerhard Oestreich considers that

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[88] Digges, *Stratioticos,* Preface to the Reader, sig. a[1] (1579), sig. B3 (1590).

[89] Gunther E. Rosenburg, 'Maurice of Nassau, Gustavus Adolphus, Raimondo Montecuccoli, and the "Military Revolution" of the Seventeenth Century' in *Makers of Modern Strategy from Machiavelli to the Nuclear Age,* edited by Peter Paret (Princeton, New Jersey: Princeton University Press, 1986), 34. See also Paul E J Hammer *Elizabeth's Wars* (Basingstoke: Palgrave Macmillan, 2003), 173-4.

[90] Neal Wood, introduction to Machiavelli, *The Art of* War, xxxvii.

Lipsius was the main European conduit for the development of interest in the Roman model of state organization. [91]

During the period of Leicester's controversial governorship of the United Provinces, between 1585 and 1587, there were close contacts between the government and important members of the University of Leiden, including Lipsius. Lipsius was enthusiastic about the arrival of Leicester, presenting him with a gift of a copy of his neo-Stoic work, *De Constantia.* He hoped Leicester would provide the strong centralized leadership that he deemed necessary to overcome the disunity and indiscipline of the anti-Spanish forces. We know that the poet-soldier Sir Philip Sidney, Leicester's nephew and would-be political heir, who was killed during the Netherlands campaign, engaged in discussions with Lipsius. In keeping with Leicester's policy of attempting to recruit the best minds for the benefit of the English state, Sidney tried to persuade Lipsius to move to England, but without success.[92]

It is unclear to what extent Digges would have been influenced directly by Lipsius. The similarities between their ideas about reform of the military were likely to have emerged rather from common literary influences and experience in a situation where such discussions were far from academic. Lipsius was an admirer of Machiavelli, and Digges's emphasis in his writing on the importance of discipline, drilling, a hierarchical chain of command, the belief that native troops were more reliable than mercenaries, and his plea for a return to the military customs of the ancient world, are all evidence of the influence of Machiavelli.[93]

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[91] Gerhard Oestreich, *Neostoicism and the Early Modern State* (Cambridge: Cambridge University Press, 2008), 4-5.
[92] Laureys, Marc (ed.), *The World of Justus Lipsius* (Belgie: Brepols Publishers,1998),122-3. Sidney was also responsible for inviting Giordano Bruno to England.
[93] Taylor (1921), 175.

Neal Wood comments that the most interesting intellectual link between Lipsius and Macchiavelli was in Lipsius's

> concept of a neo-stoic military morality or ideology. Central to the concept is the idea of constancy which involves duty, self-control, temperance, life-giving energy, and strength of soul. Here is a distinct echo of the Machiavellian *virtù*, one that was destined to become the military counterpart of the Calvinistic outlook in the economic world.[94]

Digges's writings eloquently and persuasively express the same military principles; the half-soldier, half-scholar depicted in the pen-drawing by J Wijts*, Arte et Marte* ('By Scholarship and War', see Figure 6) encapsulates their conception of the ideal military leader.

Figure 6. *Arte et Marte* ('By Scholarship and War')
Coloured pen-drawing by J Wijts, 1607, in the *album amicorum* of Ernst Brinck, 133M 86, Fol.174.

[94] Neal Wood, introduction to Machiavelli's *The Art of War* (1965), xxxviii.

Both Digges and Lipsius were equally appalled at the disunity, corruption, and lack of discipline of the Protestant side of the war in the Netherlands, and it is not surprising that this might have caused Lipsius to lose faith in the ability of the Protestant rebels to overcome the immense power of Spain and the Catholic Church. The horrors of religious war convinced him that the only chance for peace was under a strong centralised state that could impose peace and toleration under Catholic hegemony. Digges knew from his own bitter and traumatic experience that the combination of Papism and toleration was an oxymoron. The possibility of a Catholic England was not an option; his reponse to the problems he encountered in the Netherlands was to continue his campaign for military reform with added vigour.

The treatise on invasion: Digges as Archimedes' heir

Digges included at the end of the 1590 second edition of *Stratioticos* an addition concerning invasion. It was originally written for Leicester in anticipation of the imminent Spanish invasion. Leicester died unexpectedly soon after the defeat of the Spanish Armada, which must have been a considerable blow to Digges, who continued to defend him posthumously, for example with the publication of a treatise defending his role in the siege of Sluys, for which Leicester had received much criticism.

Acting as military strategic adviser to Leicester, who was responsible for preparations for invasion, Digges sets out the pros and cons of different strategies for combatting an invasion, before deciding on the best option. The benefit of tackling an invader immediately on their landing is that first,

> the furie of the Countrey upon the first fyring of the Beacons is great, *pro Aris & focis* violently running downe to the sea side to repell the disordered Enemie at the first confused landing: which furie if we suffer to grow cold, we shall not so easily enflame againe.

Further there was,

> in this Realme (as in all States divided in religion) no small numbers of traitorous minds, who having time to conferre, and seeing an Enimie of force already landed, may and will then discover their malice, which on a sodaine they dare not nor cannot.[95]

The other alternative was to let the enemy land quietly but to carry away all the food, cattle etc, thus, by '*Time* and *Famine'* to weary the enemy.[96] This tactic is also advantageous when the enemy is 'a select companie of disciplined and well-trained souldiers, whom we seek to encounter with a confused multitude of men untrained, In which match there is no comparison, but losse certaine'.[97]

Both options having their disadvantages and inherent dangers, Digges suggested a middle ground. He sought to capitalise on the three overriding advantages that England had in deterring invasion, being an island, the superiority of the navy, and the possibility of letting the sea do the fighting, all of which would be lost if an invader were allowed to land. For these and numerous other reasons, he utterly disallowed the option of allowing the enemy to land, and emphasised that all possible means should be mustered to ensure that this did not happen.

He proceeds to give detailed advice on mustering (no doubt making use of his experience as muster-master in the Netherlands) to optimise the use of men of different experience and of limited materials, even suggesting that games be set up to motivate trainees to learn quickly how to shoot muskets, thus avoiding powder wastage. He concludes with a description of engines and inventions 'not usuall to be thought on and had in readiness' promising that these and other military matters

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95 Digges, *Stratioticos* (1990)*,* 370.
96 *ibid*., 372.
97 *ibid*., 371.

would be expanded on in his treatise of great artillery and pyrotechnie, 'whose Publication I have for divers due respectes hitherto differed'. [98]

The advice was sound and appears to match the strategy that was in fact implemented, and one can assume that part of Digges's motivation in publicising it was to draw attention to his role in this successful strategy.

In the introduction to *The Heirs of Archimedes* (2005), which addresses the relative neglect by historians of science of the relationship between scientific knowledge and military power, Brett Steele and Tamera Dorland note that from late antiquity to the early Renaissance Archimedes' renown was derived from his status as a worker of military engineering wonders rather than as the Platonic idealist who brought mathematical rigour to mechanics.[99] They analyse Archimedes' masterminding of the defence of Syracuse against Roman invasion in terms of its acquisitional, operational, tactical and political demands. They define 'acquisitional' as the 'technological transformation of civilian resources into military assets, including weapons, ammunition, and armor, in addition to suitable vehicles, food, and fodder'. The operational demands refer to the distribution of military assets in conducting a campaign. In the case of the siege of Syracuse tactical success depended on prediction of the possible modes of attack and 'integrating all the machinery and troops into a synergistic "defense in Depth" which subjected their Roman opponents to three distinct layers of attack.' The political domain in the case of Archimedes came from the benefits of his inventions and improved techniques.

Digges's treatise on invasion addresses these four demands in relation to the threat of invasion by Spain, while *Stratioticos* and *Pantometria* were designed to improve the efficiency of the army by applying mathematics to every aspect of

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[98] *ibid.,* 380. This comment gives an indication of the scope of Digges's proposed work. It was not envisaged as simply a theoretical work on the trajectory.
[99] Brett D Steele and Tamera Dorland (eds.) *The Heirs of Archimedes* (London: MIT Press, 2005), 2.

military operations. In his job as muster-master in the Netherlands he was central to the operational demands of the campaign, but as Henry J. Webb notes, Digges's role in the Netherlands war was not confined to his duties as muster-master,

but also included that of examining fortifications at Sluys, Vlissinge, Ostend, Flushing, and other fortified towns and making recommendations for their repair; his meticulous reports to Burghley and Walsingham exhibited both his engineering ability and his strategic perspicacity.[100]

We can also look to Digges's pivotal role in the success of the Dover Harbour project, which he saw as serving strategic military objectives, as well as having important economic benefits. Digges was not simply an engineering advisor, but an initiator of the project; inspired by the excellence of the Dutch harbours that he had recently examined on a visit to the Low Countries, in 1582 he wrote a lengthy, detailed and persuasive treatise addressed to Elizabeth I, encouraging her to give her consent to the plan.[101]

Conclusion

In *The Heirs of Archimedes,* Brett Steele and Tamera Dorland note that Napoleon had once admitted that had he not pursued a military career, he would have gladly settled for an academic post in theoretical mathematics.[102] Digges was conscious of the choices that the circumstances of the times had forced him to make. No doubt his experience in the Netherlands had taken its toll on his health, and he died before his proposed great works on artillery and fortifications could be published.

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[100] Webb, 'The Mathematical and Military Works of Thomas Digges', 395.
[101] Digges, *A briefe discourse declaring how honorable and profitable to youre moste excellent maiestie, and howe necessary and commodiouse for your realme, the making of Dover Haven shalbe,* published by the Society of Antiquariands of London in *Archaeologica: or Miscellaneous tracts relating to antiquity,* vol.11 (1794), 212-255. See also Webb, 'The Mathematical and Military Works of Thomas Digges', 393-4.
[102] Steele and Dorland, *Heirs,* 28.

The shift away from progressive Tudor absolutism that coincided with the death of Leicester and the rise of the influence of John Whitgift on Elizabeth I meant that Digges's last days were absorbed with political activity. In addition, at the end of his life Digges was also involved in tortuous litigation with members of his family, which, whatever the rights and wrongs of the case, must have caused him great distress. Political enemies seized on it to seriously attack his reputation for probity, which had previously been unassailable. At last they had vengeance for his attempts to clean up the corrupt practices of the captains in the Netherlands campaign.[103]

This chapter has aimed to show how Digges's biography influenced his programmatic aims and it has attempted to show how his science of war draws together the political, religious, scientific and civil aspects of his career. Digges was committed to the advancement of knowledge and learning for both its intrinsic value and its utilitarian value, and I have shown how the ideal of the advancement of learning was for him identified with the struggle in defence of the state, which he identified with the defence of Protestantism.

Digges's attempt to provide his new science with a new lexicon reflected his recognition that these technical challenges required a development of the English language itself. His commitment to making knowledge accessible to unlearned technicians and artisans, for the benefit of the Commonwealth, reflected a political consciousness expressed most audaciously in the publication of his translation of Copernicus's *De Revolutionibus.*

Through detailed scrutiny of Digges's ballistics I have shown that he contributed ingenious and original insights into the problems that arose from gunnery practice. Most remarkably he used the mechanical formation of the Archimedian

[103] For this information I am indebted to Glyn Parry's paper 'Thomas Digges, Persecutor', delivered to the Thomas Harriot Seminar, Birkbceck College, London, 11 July 2014.

spiral to make analogies between the formation of the trajectory of a cannon, motion in the heavens, the motion of a ship under the influence of trade winds, and the motion of an object dropped from the mast of a ship.

Digges's important contributions to astronomy and the spread of Copernicanism in England influenced his insights into the trajectory and his epistemological stance towards both sciences combined a Platonic belief in the truth and power of mathematics to depict reality with an equally strong commitment to empirical and experimental evidence. This combination reflected a shift in the epistemic foundations of mathematics during the Elizabethan period in England that saw a more direct and immediate relationship between the mathematical and the physical.[104]

Digges's aim was to bring a universal method, and law, order and reason to bear whether he was dealing with matters of state, the military, the trajectory of a bullet, or the motion of the heavens. In all these disparate activities he aimed to bring unity between theory and practice, and whether through astronomical observation, gunnery trials, setting off to sea or going to war, it appears he was prepared to put his money where his mouth was.

Using the Archimedian spiral as his inspiration, Digges directed attention towards the possibility of a pointwise construction of the complete curve of the trajectory. The next chapter shows how Harriot and Galileo were able to draw on more advanced tools of geometrical construction to model the continuously curved trajectory. Using the research of Matthias Schemmel and the methodology of historical epistemology, it will show how they were able to independently make the

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[104] Stephen Clucas, '"No small force": Natural Philosophy and Mathematics in Thomas Gresham's London' in Francis Ames-Lewis (ed.) *Sir Thomas Gresham and Gresham College* (Aldershot: Ashgate, 1999), 150. Clucas compares Dee with Harriot but Digges also reflects this changed approach to mathematics.

key breakthrough that enabled them to discover the law of fall and the parabolic trajectory.

Chapter three

Harriot and Galileo: common roots and different routes to the new science

> For water's mass and air's thin nature cannot
> Slow down the pace of all things equally
> But must give way more quickly to the heavier.
> But, by contrast, nowhere at any time
> Can empty void make resistance to anything,
> But as its nature demands it must give way.
> Therefore through the calm and quiet void
> All things must travel at an equal speed
> Though with unequal weight…[1]

Introduction

This chapter will show how the different political, economic, and cultural development of Britain and Italy, combined with the increasing interpenetration of ideas and cognitive tools, led to similarities and differences in the solution to common problems involving the motion of projectiles. I will illustrate this dynamic interaction through a comparison of the development of the ballistic theory of Thomas Harriot and Galileo.

In my chapter on Tartaglia I noted the importance of his attempt to abstract air resistance in his analysis of the trajectory. For Galileo, the consideration of motion in a void and the challenge to traditional Aristotelian conceptions of motion make an appearance in his earliest thoughts on the subject.

In the case of Thomas Harriot we see the iterative process in action as he attempts to obtain as close as possible match between his theoretical predictions and his data from practical gunnery. In addition, he sets up his own practical experiments to test competing theories of fall. Matthias Schemmel, in *The English Galileo*, has

[1] Lucretius, *On the Nature of the Universe,* translated by Sir Ronald Melville (Oxford: Clarendon Press, 1997), 43.

ordered and transcribed all of Harriot's manuscripts on ballistics and presented them in such a way that the process of discovery can be followed in every detail.

Thomas Harriot's work on ballistics does not appear in A. R. Hall's *Ballistics* and he would not have deemed it important since it remained in manuscript form and so could not have influenced future developments in the theory of motion, or have had any influence on gunnery practice. Yet, as Matthias Schemmel has contended, a comparison of the means by which Harriot and Galileo made their discoveries is extremely important from the point of view of historical epistemology. It is for this reason that I have included him in my case studies despite the fact that he does not fit the pattern of most of my other subjects, who in one way or another were conscious public proselytisers of the new science.

A study of Harriot's attempt to solve the problem of the trajectory provides a striking illustration of the fertile ground that England continued to provide for the development of scientific thought, providing continuity with later developments in the seventeenth century despite his lack of published work.

Little is known of Harriot's early life until he appears at Oxford in 1577, but he was probably born around 1560. At Oxford he became close friends with Richard Hakluyt and Lawrence Keymis who were both part of Sir Walter Raleigh's circle.[2] It is probably also at this time that Harriot also became acquainted with Robert Hues and Walter Warner, who, together with Harriot became known as the 'Three Magi' of Henry Percy, the 'Wizard' Ninth Earl of Northumberland. While Harriot was finishing his studies at Oxford, Walter Raleigh was charming the Elizabethan court, making enemies, and waging war in Ireland. Having become a favourite of Elizabeth, Raleigh was granted the use of Durham House and here Raleigh gathered together up

[2] For biographical details see John W. Shirley, *Thomas Harriot (1560-1621): A Biography* (Oxford: Clarendon Press, 1974).

to forty-odd experts in cartography, navigation and mineralogy to train for his voyages of exploration, employing Harriot to teach mathematics and navigation to his sea captains.

In June 1585 Harriot set sail on Raleigh's expedition to establish a settlement in America, returning in June 1586, having been rescued from a hurricane by Sir Francis Drake, who was returning from the sack of Hispaniola. This resulted in Harriot's only publication during his lifetime, his *Briefe and True Report of the New Found Land of Virginia* (1588) and later incorporated in Hakluyt's *The Principall Navigations* (1598-1600), written to defend and gain support for Raleigh's enterprises. It was in 1587, whilst advising officers in preparation for the Spanish Armada, that Harriot became acquainted with Raleigh's friend the Earl of Northumberland, who later became his major patron and from whom he was awarded a pension and his own house and research laboratory at Northumberland's estate at Syon in 1598.

The dating and motivation of Harriot's work on ballistics has been the subject of some debate. Harriot's biographer, John W. Shirley, made the plausible suggestion that Harriot's concern with ballistics was initiated by Raleigh in the years 1590-95. Steven Walton has argued, however, that Raleigh was not greatly interested in artillery; the forts in Virginia, according to drawings by John White which accompanied Harriot's text, were not equipped with cannon, and Raleigh was anxious to maximise the mobility of his ships by reducing the amount of ordinance that they had to carry. On the other hand, a dating of 1595-1600 'would place the timing of Harriot's ballistic work with the peak of Percy's military pretensions'. [3]

[3] Steven Ashton Walton, *The Art of Gunnery in Renaissance England* (unpublished doctoral thesis, University of Toronto, 1999), 60-61.

Whilst this does not seem to preclude the possibility that Raleigh might have been interested in improving the effectiveness of cannon, Jean-Jacques and Pascal Brioist provide further evidence that supports the likelihood that Harriot did not begin work on the trajectory until after he had entered the employment of the Earl of Northumberland in 1587. They argue that one of the texts that Harriot used to compare with his own calculations was the *Corona e palma de Artigliera* of Alessandro Capobianco, published in Venice in 1598. In addition they note that it was in the 1600s that Northumberland arranged for the engineer Paul Ive to translate for him Stevin's work on fortification, not long after Northumberland's return from his unhappy experience in the Netherlands.[4]

In 1549 Sir Thomas Smith in A *Discourse of the Commonweal* lamented the lack of learning in England and argued for the recognition of the value of the knowledge of craftsmen and artisans. He contrasted over-regulation and dismissive attitudes in England with Venice's openness, particularly in welcoming strangers and encouraging innovation.[5] By the last quarter of the century, however, as Deborah Harkness describes in detail in *The Jewel House*, this situation had been transformed. A new generation of practical mathematicians had emerged who learnt from the experts from the Continent, many of whom either were fleeing persecution and religious strife or were encouraged to come to England, notably by William Cecil, to assist with his 'Big Science' projects aimed at achieving English economic and military advantage, and income for the crown. By the turn of the century, then, Britain had achieved significant expertise in practical mathematics and practical mechanics.

[4] Jean-Jaques & Pascal Brioist, 'Harriot, lecteur,' 153.
[5] Harkness, *The Jewel House*, 114.

With regard to theoretical mechanics, since Britain seems to have lagged behind Italy in the production of vernacular works during this period, we need to examine other evidence for the spread of theoretical mechanical knowledge. This we can gain from examination of the contents of the private libraries of the period, of which the Earl of Northumberland's can be ranked alongside other major private collections of the time, notably that of John Dee.[6] The Earl took great interest in military affairs and the strategy of war; according to G. R. Batho the '…only attempt at literary composition which he is known to have made is a book on the art of war. At least five manuscripts survive among the Percy archives which contain drafts and headings in various stages for the book, and one of them is a substantial volume of some 638 folio pages.'[7] A significant number of the books the Earl annotated were on the subject of war, and he read widely on the subject in French and Italian as well as in English. For the Italian works annotations were often concerned with vocabulary as he did not read Italian as fluently as French, and when he was in the Tower he employed an Italian reader to assist him.[8]

Further evidence of the rich sources drawn on by Harriot is provided through the amazing work of digitization of Harriot's manuscripts by the Max Planck Institute for the History of Science. The researchers working on this project have traced references in his manuscripts to twenty three ancient sources (including Apollonius of Perga, Archimedes, Aristarchus and Pappus), three Islamic sources (al-Battani, Alhazen, Thabit ibn Qurra), six medieval sources (including Roger Bacon, Raymond Lull and Jordanus) and sixty six Renaissance sources (including François Viète, Federico Commandino, Guidobaldo del Monte, and Giambattista

[6] Gordon R. Batho, 'The Library of the 'Wizard' Earl: Henry Percy Ninth Earl of Northumberland (1564-1632)' *The Library* 15 (1960), 246-261.
[7] *ibid*., 250.
[8] *ibid*., 255.

Benedetti).[9] The influence of the algebra of Viète was particularly important in providing Harriot with alternative tools that he brought to bear on his investigation of the trajectory that were not used by Galileo.

Whilst Thomas Digges did not allow himself the luxury of a contemplative life, Raleigh and the Earl of Northumberland had it thrust upon them when they were confined for long periods in the Tower of London. The mathematician John Wallis commented that:

> Their prison was an academy where their thoughts were elevated above the common cares of life; where they explored science in all its pleasing forms, penetrated her most intricate recesses, and surveyed the whole globe till Sir Walter Raleigh's noble fabric arose, his *History of the World,* probably by the encouragement and persuasion of his noble friend.'[10]

Stephen Clucas has noted that there is also considerable manuscript evidence that the Northumberland circle participated in the revival of Archimedian mechanics through the work of Federico Commandino and Guidobaldo del Monte. Not only was Northumberland's copy of Del Monte's text heavily annotated, but the 'absurdities' and 'imperfect conclusions' that he notes in del Monte's text indicates that he critically engaged with the text. Clucas continues that Nathaniel Torporley's short treatise *De Ponderis Aquae* was written in response to a question proposed by the Earl and is a consideration of Archimedes' ideas on water pressure. Harriot too was working through the same Archimedian texts and applying mathematical principles to hydrostatic subjects, notably to problems of water flow, that resulted in practical improvements to the plumbing system at Syon House.[11] Harriot drew on the same theoretical basis of uniformly difform change to solve this problem in Elizabethan

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[9] Jacqueline Stedall, Matthias Schemmel, Robert Goulding: Digital edition of Thomas Harriot's manuscripts, http://echo.mpiwg-berlin.mpg.de/content/scientific_revolution/harriot/maps/2_SOURCES.pt. Accessed 8.8.14.
[10] Quoted in Batho 'The Library', 248.
[11] Stephen Clucas, 'Harriot and the Field of Knowledge in the Renaissance' in *Thomas Harriot: An Elizabethan Man of Science* ed. Robert Fox (Aldershot: Ashgate, 2000), 146-73, on 111-113.

plumbing, representing the water flow of the conduit in a time-velocity diagram, as he would bring to bear in his consideration of ballistic trajectories.[12]

Whilst I cannot dwell on all of Harriot's diverse interests and scientific achievements, the above example of his application of a general principle or method to different practical problems are illustrative of an approach which saw common principles behind diverse phenomena. Clucas criticizes the attempt to separate out Harriot the alchemist and student of the occult portrayed by Yates from Harriot the 'real scientist' who investigated optics and mechanics portrayed by his biographer J. W. Shirley. Both provide a distorted picture of the field of knowledge in the Renaissance, that did not separate natural enquiry into discrete packages.[13]

As part of a programme for uncovering the historical epistemology of mechanics, Schemmel investigates the shared knowledge of pre-classical mechanics by showing how Harriot and Galileo drew on the same sources to approach the 'challenging object' of the nature of the trajectory. Though their paths to the discovery of the law of fall and the parabolic trajectory were not identical, there were striking similarities in the analytical tools they used, in the problems they encountered and the contradictions and misconceptions that they had to overcome on the way to making their breakthroughs, as well as in the problems they were unable to solve.

Schemmel has shown in detail, as Drake, Renn, Laird, Valleriani and others have have done with Galileo, how both drew on tools and concepts from pre-classical mechanics in order to derive results which were also true in classical mechanics. But what transformed those tools and enabled these scientists to make key breakthroughs

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[12] *ibid*., 113 and 117. Schemmel, in *The English Galileo, passim,* refers to uniformly accelerated motion as uniformly difform motion, derived from medieval diagrams of motion.
[13] Note the two were united in practice in Lord Burghley's 'Big Science' projects. See Harkness, *The Jewel House,* chapter four, '"Big Science" in Elizabethan London', 142-180.

was their application to the practitioners' knowledge of mechanics and ballistics and the problems they encountered. As Schemmel says:

> Pre-Classical mechanics was built on very heterogeneous components of knowledge that stemmed from various sources. Among these one finds Aristotelian physics; ancient traditions other than the Aristotelian, some of which were just being rediscovered in early modern times: calculatory techniques from medieval science; and most importantly, the knowledge of practitioners, such as engineers and gunners…these traditions became entangled in the work of a new social group of intellectuals who have, in the literature, been termed the "engineer–scientists."[14]

He continues that what distinguishes these practitioners was their reflection on practical knowledge:

> They strived for a mathematisation of that knowledge, they edited ancient works on mathematics and translated them into the vernacular to make them accessible to a broader readership, they systematically scanned the existing practical knowledge for its usefulness, they performed 'experiments', the results of which they hoped to be of direct practical use, they wrote manuals and books to communicate their knowledge and emphasise the importance of their science to society in programmatic writings.[15]

Indeed, it is the programmatic self-consciousness of Tartaglia, Digges and Galileo and their commitment to the dissemination of the new science that distinguishes them from their contemporaries. Though Harriot did not publish his scientific discoveries, he was the major technical adviser to the expansionist modernizing ruling elite of late Elizabethan England, and the publication of his *Briefe and True Report of the New Found Land of Virginia* is evidence of his conscious recognition of the necessity of gaining support for a programme of exploration and settling of the New World. The trials, both literal and metaphorical, of Harriot's patrons and the accusations of occult activities made against the 'School of Atheism' over which, enemies claimed, Harriot presided, indicates the climate in which this group worked and may have had some bearing on Harriot's reluctance to publish his discoveries.

[14] Schemmel *The English Galileo,* vol. 1, 5.
[15] *ibid*., 16.

The high level of scientific creativity that Harriot achieved is illustrated by the much-quoted letter from Sir William Lower, who was not alone in urging Harriot to publish. Whilst I will focus on Harriot's achievements in ballistics, Lower's letter gives an illustration of the scope of Harriot's original discoveries:

> Doe you not here startle, to see every day some of your inventions takn from you; for I remember longe since you told me as much, that the motions of the planets were not perfect circles. So you taught me the curious way to observe weight in Water, and within a while after Ghetaldi comes out with it, in print. A little before Vieta prevented you of the Gharland for the greate Invention of Algebra. Al these were your deues and manie other that I could mention; and yet to great reservediness hath robd you of these glories. But although the inventions be greate, the first and last I meane, yet when I survey your storehouse, I see they are the smallest things, and such as in Comparison of manie others are of small or no value. Let your Countrie and frinds injoye the comforts they would have in the true and greate honor you would purchase your selfe by publishing some of your choise works.[16]

Genesis of the ballistic theories of Thomas Harriot and Galileo

I will now look in more detail at the common influences as well as the differences in approach of Galileo and Harriot. One of the important early influences on Galileo was his introduction to the study of Euclid by Ostilio Ricci, who was attached to the Tuscan court. Stillman Drake suggests that the text that Galileo studied was in all likelihood the Italian translation published by Tartaglia. In the universities only Latin texts were used but Ricci, who is thought to be a former pupil of Tartaglia, taught the court pages in Italian. Drake adds that although there were two Latin editions of Euclid in the sixteenth century, only Tartaglia's Italian version fully elucidated an important difference between them. Tartaglia clarified the difference between the Eudoxian theory of book five and the medieval arithmetical theory of proportion based mainly on book seven. This clarification was not available in the mathematics taught in the universities. According to Drake:

[16] Quoted in Gordon R. Batho, 'Thomas Harriot and the Northumberland Household' in Fox, Robert (ed.) *Thomas Harriot: An Elizabethan Man of Science* (Aldershot: Ashgate, 2000), 28-47, on 39.

> The importance of Eudoxian proportion theory to Galileo's science cannot be exaggerated. Until the application of algebra to the general solution of geometrical (as well as arithmetical) problems, not achieved until after Galileo's work was completed [with Descartes' analytic geometry] rigorous connection of mathematics with physical events was possible only through some theory of proportionality. Physical concepts were therefore much affected by the proportion theory employed. Arithmetical theories, basic to medieval developments in physics, were not easily reconcilable with mathematically continuous change, especially change of speed; for speed seemed to exist only in connection with motion and not instantaneously. Now, continuous change of speed was a necessary assumption in the study of actual falling bodies, and as a result of this the Eudoxian theory establishing proportionality was essential to any great advance over medieval physics.[17]

Drake also notes that Ostilio Ricci introduced Galileo to the study of Archimedes, probably Tartaglia's 1543 selection. Ricci inherited from Tartaglia the tradition of the study of mathematics 'not as abstract concepts, but as a collection of researches linked to military science, architecture, and in general to practical affairs.'[18] The early fruit of this combination was Galileo's invention in 1586 of the hydrostatic balance, described in *La Bilancetta*, which marked his 'scientific debut'.[19]

We know from the introductory dialogue of Galileo's *Two New Sciences* that Galileo drew inspiration from the Venice arsenal. In the case of Harriot we have no evidence that he was ever directly involved with gunnery practice, though his practical involvement in voyages of exploration and the Armada defences makes it doubtful that he would not have had some familiarity with guns. Yet from his manuscripts it seems that Harriot gained his knowledge of gunnery from the study of gunnery manuals rather than from his own experience.

As Schemmel notes, the difference between artillery 'trials' described in gunnery manuals, and scientific experiments was that the conditions of the trials in

[17] Stillman Drake, *Galileo at Work: his scientific biography* (Chicago: University of Chicago Press, 1978), 4. Drake notes a parallel with Galileo's father Vincenzio's disputes with traditional music theory which has an analogous basis in the difference between discrete and continuous variables, and notes that Galileo assisted his father with experimental work in music theory.
[18] Ludovico Geymonat, *Galileo Galilei: A biography and inquiry into his philosophy of science* trans. Stillman Drake, (New York: McGraw-Hill Book Company, 1965), 7.
[19] *ibid*., 10.

the former are only described incompletely and they did not serve any theoretical purpose, such as deciding between competing theoretical interpretations of projectile motion, but were performed to provide gunners with immediately applicable range tables. But Harriot was able to turn the trials of Alessandro Capobianco, Luis Collado and William Bourne into 'scientific experiments after the event'.[20] Furthermore, J. J. and P. Brioist have discovered in Harriot's manuscripts evidence that Harriot also read Tartaglia's *Quesiti.* They did not consider if he read this in the original Italian or Lucar's translation. We know that Harriot was an excellent linguist from his work in learning the language of two Algonquian Indians in prepararation to his voyage to Virginia, but, as I show below, it seems that he made use of Lucar's widely available version, published in 1588, with its comprehensive supplementary material.

It is possible, then, that Harriot was only familiar with the first three books of the *Quesiti* that specifically related to gunnery and he might not have read the *Nova Scientia* at all. Harriot was involved in preparations for the Spanish Armada when both Bourne's and Lucar's works were published.

With regard to Harriot's use of Tartaglia's work, I will give Schemmel's transcription from Harriot's manuscript and the corresponding section from Lucar's translation of Tartaglia. Schemmel did not recognize the source of this manuscript as being Tartaglia, but correctly suggested that it was taken from a contemporary manual on artillery:[21]

> a
> The higher a peece is elevated the longer right line doth a bullet
> make beinge shot.
> [s]ome times a peece beinge elevated doth lesse **exployte** then a
> peece lyinge level, according to the distance from the
> marke & sometime greater

[20] Schemmel *The English Galileo,* vol.1, 29.
[21] *ibid*., 297.

<by[?] how much the objecte Doth> cut of from the right line of ,his f> the <[bullets [?] free motion;> by so much is it of greater or lesse faste <[th]en the right line of a bullets [fre]e motion of any peece at any distance of any <elevation> Randon[?]

b
[t]he pellet of a culveringe weying xxty pond weight the [p]eece lyinge level flyeth 200 paces in a righte line **sensible**.
[a]t [?] 45 degrees elevation 800.p[r]oved at Verona.[22]

Lucar
…for a Peece which lieth level will never shoote so farre in a right line as it will doe when it is somewhat elevated at the mouth: and by howe much the more a Peece is elevated at the mouth, by so muchit shootes the more farther in a right line.[23]

Lucar
…yet it is doubtful whether a shoote out of a peece elevated will doe a greater effect than a shoote out of a peece lying level in unequal distances, because in our question this is to be considered whether the peece which is on the plaine at the foote of the hill be more distant from the Fort than the other peece which is on the toppe of the hill. For such a difference may be much greater than the difference of his shoote in a right line, or the difference of his effects in equal distances, and then the peece from the toppe of the said hill will doe a more greater **exploite** than the Peece which is on the plaine at the foote of the said hill.[24]

Lucar
Nicho. That I may shewe unto you my meaning herein by a figure, I will suppose that the pellet of a Culvering doth waie 20 pound weight and that the Culvering according to that experience which was made at *Verona* (as I have declared in the beginning of my booke of nwe science dedicated unto your Excellencie) in the place of equalitie, that is to say lying level, will shoote in a right line about 200 paces, and that such a culvering at the elevation of 45. Degrees, that is to say at the 6. Point, or at 72 . minutes of our quadrant(by the reason alleaged in the last proposition of my seconde booke of our new science) will shoote in a right line about 800 paces.[25]

I have highlighted the word exploite/exployte because in his interpretation Schemmel is unsure of the meaning of *exployte* while from Lucar's version it is clearer that it means damage, effect or impact. 'More greater exploite' in the Italian original was *far maggior effetto*, which makes the meaning much clearer. The fact that both Harriot

[22] Schemmel, *The English Galileo,* vol. 2, 575. Transcription in vol. 1, 296. This is folio 47 Add. MS 6789 of the Harriot manuscripts.
[23] Lucar *Three colloquies, 6.* Tartaglia, *Quesiti* (1554), 8r.
[24] *ibid.*, 9. Translated from Tartaglia, *Quesiti (*1554) 9v. *The Oxford English Dictionary* gives one definition of 'exploit' as 'to act with effect'.
[25] *ibid.*, 9.

and Lucar use the same word further supports the likelihood that Harriot used the Lucar translation. Harriot is looking at the section at the beginning of the *Quesiti* that discusses the greater effect on a target when the piece is elevated rather than at point blank, because it moves further in a 'straight' line. He is also considering the question of the first part of the trajectory being only a *sensible* straight line. Schemmel has difficulty in interpreting this paragraph but it is clearer when we read the Lucar version.

Harriot includes the word *sensible* (which I have highlighted) in section b, which is not actually in Tartaglia's text at this point. However, in the following pages Tartaglia enters into a long discourse explaining why the straight part of the trajectory is insensibly curved.[26] It is also the section where Tartaglia attempts to use the science of weights to explain to the Duke of Urbino why the cannon fires differently at different elevations, explaining that , 'by how much the swiftness thereof doth decrease, by so much in that moving the waight thereof doth more increase, the which waight provoketh and draweth the said heavie bodie towards the ground.' [27] So although this is an isolated manuscript, it seems Harriot studied the Tartaglia translation thoroughly and it could have played a role in Harriot's transition from considering the trajectory as an approximation consisting of straight and curved parts to his attempt to find tools which would allow him to analyse the separate components of the continuously curved trajectory and to think about the factors involved.[28]

A crucial first step in considering the trajectory as continuously curved was to be able to describe the two separate components of motion mathematically. As the following extract shows, Harriot, like Galileo, made a crucial step towards the

[26] Lucar, *Three colloquies,* 10, from Tartaglia, *Quesiti* (1554), 10r.
[27] *ibid*.,12. Tartaglia, *Quesiti*, 11v.
[28] Schemmel, *The English Galileo*, vol. 1, 35.

development of the concept of inertia by recognisng that the violent motion could be considered as uniform provided that there was no air resistance:

> Now I say because of the bullet's gravity the crooked line is made. If the gravity be abstracted the motion wold be only in the right line…& if the resistence of the ayre …or medium be also abstracted his motion would be infinitely onward[.][29]

However, as Schemmel noted, though Harriot's statement could serve the same purpose as the law of inertia, it was conceived within the framework of the preclassical concepts of violent and natural motion and in his final trajectory Harriot conceived of the oblique motion as being decelerated, which from the point of view of classical mechanics meant counting gravity twice.[30]

As we have seen, the mechanical combination of two motions was known from the work of Archimedes on the spiral, and Digges had already considered the central part of the trajectory as derived in this way. Harriot applied this approach of combining two motions to the whole trajectory, but instead of combining a straight with a circular motion, he needed to combine two rectilinear motions. Having defined violent motion as uniform and rectilinear, Harriot then had the task of mathematizing the continuously accelerated motion of fall.

Uniform motion can be represented as a rectangle with time as the length, speed as the height and area as the space travelled, whilst uniformly difform (continuously accelerated) motion can be represented similarly by a right-angled triangle. This approach to the study of motion was derived from the medieval doctrine of the configuration of qualities and motion that originated in the first half of the fourteenth century at Merton College, Oxford. The geometrical scheme for representation of such motions was probably invented by the Parisian scholastic

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[29] Quoted in Schemmel, *The English Galileo*, 1, 35.
[30] *ibid.,* 36 and 236.

Nicole Oresme in the middle of the century. [31] Harriot, as well as Galileo, Descartes, Beeckman and others used the Oresmian diagrams to represent continuously accelerated motion. Harriot made extensive use of the *Liber De Triplici Motu* of Alvarus Thomas (1509) who presented a thorough analysis of the theory of proportions and the science of motion derived from this medieval calculatory tradition.

The method of the medieval calculatory tradition enabled the case of uniformly difform motion (uniformly accelerated motion) to be transformed into the much simpler uniform motion using the mean degree theorem, often referred to as the Merton Rule. Applied to motion, this theorem provides the useful result that for a given period of time, for uniformly difform motion that starts from (or ends at) zero, the distance travelled is equal to that travelled in uniform motion if it were travelling at the speed given by the middle instant of time. Using a temporal interpretation of the medieval concept of extension and the area as space traversed and with the application of the mean degree theorem, Harriot was able to arrive at the time squared law of fall. He was then able to obtain the geometrical representation of projectile trajectories by plotting the points the motions would reach in equal intervals of time.

In their treatment of the three variables time, degree of velocity[32] and distance travelled, both Harriot and Galileo also had to deal with the question of whether the degree of motion is uniformly increasing with respect to space traversed or time elapsed, which Galileo discusses in the *Two New Sciences.*[33] It was only when the implications of both were explored that it became apparent that they represented different motions. In this they were way ahead of most of their contemporaries.

[31] *ibid.,* 54.
[32] *ibid.,* 73. Harriot uses the Oresmian 'degree of velocity', which he identifies with Alvarus' 'velocitas'.
[33] *ibid.,* 125, Schemmel references his letter to Paulo Sarpi as evidence that Galileo also believed for some time that he had found a proof of the times squared law based on the assumption of a proportional increase of velocity with space traversed.

Harriot carried out calculations that verified the times squared law[34] prior to his realisation of its incompatiblilty with the alternative assumption that velocity increased in proportion to space traversed, and Schemmel provides a detailed account of the process by which Harriot came to the realization of his error.[35] The anomalous results that he got from his assumption of the latter led him to doubt his previous deduction of the time squared law and he carried out further calculations based on successive approximations of series of uniform motions that confirmed it. Similar calculations for uniformly increasing motion with respect to space gave a logarithmic relationship that Harriot was not able to recognize as meaningful though the numerical results of his approximations were remarkably accurate.

Schemmel also describes how Harriot at some time between 1600 and 1606[36] carried out experiments to settle the question of which of the two possibilities (that motion in fall was uniformly difform with respect to space or with respect to time) were indicated by practice.[37] His method of measuring velocity circumvented the difficulty of measuring time of free fall in the early modern period. Harriot set up an experiment by which he attempted to measure the velocity of a bullet dropped on to a balance. Despite the problematic nature of these experiments from the viewpoint of classical mechanics,[38] Schemmel was able to carry out reconstructions of the experiments which did suggest that motion of fall is uniformly accelerated with respect to time rather than space.[39]

In my discussion of Harriot I am mainly focusing on the tools that he brought to bear and which he developed in the course of his research, and refer the reader to

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[34] *ibid.,*128.
[35] *ibid.*, 54.
[36] Schemmel classifies this as the intermediate period in his researches.
[37] *ibid.,* 98.
[38] For example, the Aristotelian assumption of a proportionality of force and velocity.
[39] *ibid*.,101. Schemmel notes, on 98, that similar experiments using a balance were carried out by Galileo (to measure the force of percussion of falling water), and Beeckman to measure *impetus*.

Schemmel's heroic research for further detail. One aspect of Harriot's approach was his algebraic formalism that enabled him to move from geometrical representations, for example diagrams of motion, to numerical values for sought quantities. The methods he used enabled him to transform proportions to equations that could be manipulated according to algebra, which were then often transformed back into a proportion which could form the basis of numerical calculations. This creative combining of geometrical, arithmetic and algebraic methods seems to have been a notable characteristic of the practical problem-solving approach of English mathematicians that Harriot used to great effect, and it provided him with an approach to the solution of problems of the trajectory that was not open to Galileo.[40]

As I have previously noted, despite Harriot's early comments on the uniform nature of 'violent' motion, his later theoretical approach to the trajectory conceived of it as two motions, decelerating along the line of shot and obeying the law of the inclined plane, the angle of inclination being the angle of elevation. [41] In the case of horizontal shots, the inclination is reduced to zero and the motion along the line of shot is uniform, as it would be in classical mechanics. Harriot employs the correct theory of the inclined plane, that acceleration depends on the sine of the angle of inclination.

This last approach to the trajectory appears to be the result of Harriot's attempt to achieve the closest possible match between his theoretical results and those of the practitioners' experience that was contained in the gunners' manuals. This meant he had to capture two characteristics, the variation of the range with elevation and the asymmetry of the trajectory. For Harriot, the asymmetry was an essential

[40] *ibid.,* 49-50. Application to inclined plane version of the trajectory, 175-190.
[41] This corresponded to the Aristotelian distinction of violent and natural motion where the motion along the line of shot is decelerated. *ibid*., 225.

characteristic of the trajectory, while for Galileo this was only an accidental feature and the symmetry of the trajectory was fundamental to his theoretical insight.[42]

Harriot compared his theoretical results with the range tables he found in Luys Collado's Spanish version of his gunnery manual *Platica Manual de Artilleria,* Alessandro Capobianco's *Corona e Palma Militare di Artiglieria,* and William Bourne's *Art of Shooting in Great Ordinance*. William Bourne did not actually provide range tables, and as I remarked in the last chapter, he was pessimistic about the possibility of providing accurate tables. However, he does provide some ratios 'according to the proofe that I have made, but yet not to my contentation'. What he does is provide rough proportions for the increase in range as the elevation is raised, based on his experience. Schemmel provides a table based on Harriot's manuscripts that show how Harriot was able to transform these scales to derive ranges that corresponded to his own theoretically derived ranges, so that he could compare them.[43]

The inclined plane version of the trajectory enabled Harriot to satisfy both aspects of the trajectory, its asymmetry and the variation of range with angle, which he had not succeeded in capturing in all his previous hypotheses. In this final version Harriot used his facility with algebraic formalism to derive a formula for the time of flight in terms of given quantities and from this derived a procedure to calculate the ranges at different angles. He found the maximum range to be 27 degrees 55 minutes. So while Harriot was able to achieve a pleasing match between the theoretical and empirical shape of the trajectory, the result as far as the maximum range was concerned was less than satisfactory. To explain the deviation from empirical ranges, Harriot tried to incorporate a change in velocity with the increasing angle of

[42] *ibid*., 233.
[43] *ibid*., 194. Original data in William Bourne, *The arte of shooting in great ordnaunce* (London: Thomas Woodcocke, 1587), 27.

elevation. Both of his adaptations contain similar conceptions to those of Tartaglia when he suggests that the higher the elevation the faster the bullet can fly.

However, even with an amendment to the velocity by considering the inside of the gun as an inclined plane and subtracting from the fixed velocity an amount varying with the sine of the projection angle, Harriot still was not able to overcome the problem of the deviation from empirical ranges.

Nevertheless, Harriot was able to prove the parabolic shape of trajectories following from the inclined plane conception of projectile motion using a proposition on parabolas that was part of the ancient theory of conic sections from the works of Appollonius.[44] For the case of horizontal projection (zero slope), this result was also correct in classical mechanics.

Harriot provides a paradigmatic example of how the practitioners' knowledge that formed part of the shared knowledge of early modern scientists informed and drove theoretical investigation into the trajectory. As Schemmel shows, both Harriot and Galileo drew on the shared knowledge of pre-classical mechanics that led not only to similarities in approach but also to similarities in the problems encountered and the misconceptions they had to overcome. Nevertheless within this framework, as Schemmel has also shown, there was also scope for individual pathways to similar results.[45]

The significance of the void in Galileo's conceptual framework

While Harriot was able to discover the parabolic trajectory by a pointwise combination of natural and violent motion, in the case of Galileo his insight into the parabolic nature of the trajectory preceded his discovery of the law of fall. If we

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[44] *ibid.,* 227.
[45] *ibid*., 231.

compare Galileo to Harriot, it seems possible that Galileo's grounding in Aristotelian physics and the development of his critique of Aristotle made him more overtly aware of the importance of deriving results based on motion in a void. This made him uncompromising in his commitment to the abstraction of motion from factors such as air resistance.[46] This emphasis can be traced back to his disputations with Aristotelianism during the period that he was teaching at the University of Pisa (and before, as a student).

We know that Galileo considered the trajectory to be parabolic from 1592 when he carried out an experiment with Guidobaldo del Monte on his way to take up his position at the University of Padua.[47] What was crucial to Galileo's thought was the symmetry of the parabolic trajectory that this experiment confirmed, and his belief that the catenary (the shape of a hanging chain) was parabolic and therefore that the forces that acted on them could be analysed similarly. In this, Galileo showed his epistemological commitment to similarity in diverse physical phenomena, and he would continue with this belief all his life and intended to deal with it on a proposed fifth day of the *Two New Sciences,* since he saw in the catenary a way of providing gunners with an easy means of deriving ranges. This was superseded by Torricelli's developments of Galileo's work, which enabled it to be adapted more easily for the use of gunners.[48]

In this section I will draw heavily on Renn *et al*.'s excavation and reconstruction of the development of Galileo's thought on motion and the trajectory. They contend that to say that Galileo discovered the parabolic trajectory in 1592 does not imply that he realized its implications. In addition, they show how Galileo

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[46] Also, as I noted in chapter one Galileo read the *Nova Scientia*, which approached the analysis of motion axiomatically and attempted to abstract air resistance.
[47] Jürgen Renn, Peter Damerow, Simone Rieger, 'Hunting the White Elephant: When and how did Galileo discover the law of fall?', *Science in Context* 14 (2001) 29-149, on 30.
[48] *ibid*., 123.

continued in his belief in the parabolic nature of the catenary even when his own experiments indicated a discrepancy between the two, continuing to attempt to find an indubitable proof. They suggest that Galileo

> trusted the proof that he believed to be true within his theoretical framework more than the outcome of an experiment. He did so for good reasons. As an experienced practitioner he knew many reasons why an experiment could fail. It would have been silly to give up such beliefs as the truth of the law of fall, the parabolic shape of the projectile trajectory, and the parabolic shape of the catenary only because he could only approximately demonstrate their validity by some experimental arrangements.[49]

Galileo's recognition of the difficulty of reproducing experimentally what had been proved theoretically is a leitmotif in the historical development of the theory of motion. Galileo was acutely aware of the problem and was always careful with his words. Analogy was a crucial source of his creative genius but at the end of day four of the *Two New Sciences* he is careful to say that the catenary only approximates the parabola:

> SALV. Besides I must tell you something which will both surprise and please you, namely, that a cord stretched more or less tightly assumes a curve which closely approximates the parabola…and the coincidence is more exact in proportion as the parabola is drawn with less curvature or, so to speak, more stretched; so that using parabolas described with elevations less than 45° the chain fits its parabola almost perfectly.
> SAGR. Then with a fine chain one would be able to quickly draw many parabolic lines upon a plane surface.
> Salv. Certainly, and with no small advantage as I shall show you later.[50]
> [referring to use for deriving trajectories]

Galileo's unique qualities derive from an early life that was far more diverse in influences and experiences than other scientists such as Guidobaldo del Monte.[51] Galileo 'drew his inspiration from all parts of the culture of his time to work out a

[49] *ibid.,* 131.
[50] Galiileo Galilei, *Dialogues Concerning Two New Sciences,* trans. Henry Crew (Evanston and Chicago: Northwestern University, 1946), 278. Galileo Galilei, *Discorsi e Demonstrazioni matematiche intorno a due nuove scienze* in *Le Opere di Galileo Galilei* VIII e. Antonio Favaro (Firenze: G. Barbera, 1890-1909), 310. Hereafter, *Two New Sciences*.
[51] Renn, Damerow, Rieger, 'Hunting the White Elephant' 71.

new science beyond its ken.'[52] The influence of Galileo's early experience in musical experiments with his father, Vincenzo, an innovative musical theorist, have been described by a number of writers.[53] Whilst his early life served as an apprenticeship into his career as engineer-scientist, in 1581 his father arranged for him to study medicine at the University of Pisa. He wanted Galileo to avoid the precarious existence that the life of the artist-engineer offered. Though Galileo never showed much interest in becoming a doctor, and left his studies in 1585 to try to make a living teaching mathematics, it was at the university that he was introduced to scholastic philosophy. Whatever his reasons, he applied himself to its mastery, especially of Aristotelian physics.

Galileo rapidly made a reputation for himself as a gifted mathematician. He acquired important patrons in Guidobaldo del Monte and Christopher Clavius, whom he visited at the Jesuit Collegio Romano in Rome, and in 1588 he gave a lecture to the prestigious Academy in Florence on the location of hell in Dante's Inferno.[54] Thanks to the reputation he developed during this time, he was appointed to the post of chair of mathematics at Pisa in 1589. It was probably in his period at Pisa (1589-1592) that he began working on his treatise *De motu,* a polemical work unremittingly critical of Aristotle's theory of motion. Its main theme was the weights of bodies and motion in media, inspired by Archimedian principles.

Michele Camerota and Mario Helbing have argued that the influence on Galileo of the vibrant cultural atmosphere of the University of Pisa in the second half of the sixteenth century, where the debate on motion was the main topic in scholarly disputes among the local professors, has been underestimated. They describe in

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[52] Giorgio Santillana, preface to Geymonat *Galileo Galilei*, vi.
[53] Renn, Damerow, Rieger, 'Hunting the White Elephant', 71.
[54] Indicating his early interest in the relationship between dimension and strength, his first new science. Galileo's early theorems on centres of gravity (reprinted in the *Two New Sciences*) particularly impressed his two patrons (Geymonat, *Galileo Galilei,* 10).

particular the debate over the motion of the elements between Girolamo Borro and Francesco Buonamici, the two most celebrated Aristotelians of the sixteenth century. The disputatious atmosphere at the University of Pisa was accompanied by experiments involving the dropping of objects from the Leaning Tower to test empirically Aristotle's assumptions about natural motion.[55]

In Galileo's early work, *De motu,* he explains how Aristotle's argument that the speed of motion in a medium depends on the ratio of the weight of the mobile to the density of the medium leads to anomalous results. Aristotle concluded that were the density of the medium zero, the speed would be instantaneous, which is impossible and therefore a void is impossible. Galileo, however, by explaining, at length, with numerical and diagrammatic examples, that the speed of motion depends on the difference between its weight and the weight through which it moves, shows that far from motion in a void being impossible, it is only in the void that a body has its true weight. He concludes:

> Therefore, the body will move in a void in the same way as in a plenum. For in a plenum the speed of motion of a body depends on the difference between its weight and the weight of the medium through which it moves. And likewise in a void [the speed of] its motion will depend on the difference between its own weight and that of the medium. But since the latter is zero, the difference between the weight of the body and the weight of the void will be the whole weight of the body. And therefore the speed of its motion [in the void] will depend on its own total weight. But in no plenum will it be able to move so quickly, since the excess of the weight of the body over the weight of the medium is less than the whole weight of the body. Therefore its speed will be less than if it moved according to its own total weight.[56]

Later, he continues:

> Themistius, following Aristotle's view, writes in his discussion of the void (on *Physics* 4.74): "Since, then, the void gives way evenly, and yet does not give way at all (for since the void is nothing, to hold that the void gives way is the

[55] Michele Camerota, and Mario Helbing, 'Galileo and Pisan Aristotelianism: Galileo's "De Motu Antiquiora" and the Quaestiones de Motu Elementarum of the Pisan Professors', *Early Science and Medicine,* 5 (2000) 319-365, on 334.

[56] I. E. Drabkin and Stillman Drake, *On Motion and on Mechanics* (Madison: The University of Wisconsin Press, 1960)*,* 45. Galileo Galilei, *De motu*, in *Le Opere di Galileo Galilei* I, 281.

> mark of an over-acute mind), the result is that differences between heavy bodies and light bodies, i.e., between their motions, are removed; and, as a consequence, the speed of all bodies moving [naturally] becomes equal and without difference." How false this is will become clear presently when we have shown that only in a void can the true differences of weights and motions be determined, and that these cannot be discovered in any plenum.[57]

Galileo shows here that he still believes that bodies fall in a vacuum with a speed proportional to their specific gravities, rather than that all bodies fall at the same speed in a vacuum. But what I want to emphasise here is Galileo's early commitment to the existence of the void and the recognition of its significance for any attempt to analyse motion.[58]

What also marked Galileo out from his Pisan academic contemporaries, however, was that at the end of what could be taken as an abstract polemic about the nature of motion, Galileo, probably influenced by but not satisfied with Tartaglia's treatment of the problem, attempted to answer the gunners' question of why objects projected move further in a straight line the less acute are the angles they make with the plane of the horizon. For Galileo questions of motion were linked, even at this early stage in his life, to the challenge of the gunners' questions and his desire to solve them.

In 1592 the heavy financial burdens on Galileo due to family responsibilities were made more acute by the death of his father. As I have noted, with the help of Guidobaldo del Monte and other supporters he had been able to obtain a post at the University of Padua on a considerably higher salary. Renn *et al*. have provided persuasive evidence that it was on his way to Padua whilst staying with Del Monte that he carried out an experiment that involved rolling a ball along an almost upright mirror from which he was able to obtain an impression of a parabolic trajectory.

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[57] *ibid*., 61. Galileo Galilei, *De motu*, in *Le Opere di Galileo Galilei* I, 294.

[58] David Wootton, *Galileo: Watcher of the Skies,* 62, notes the difference between Galileo's early conception of motion and his later discovery that all bodies in a void fall with equal speeds.

Details of the experiment were recorded in Del Monte's notebook, which bears a remarkable similarity to Galileo's account in the *Two New Sciences*. [59]

Despite the close collaboration and mutual esteem between Del Monte and Galileo, Renn *et al* suggest there were differences between them in the focus of their interests and their epistemological approaches. The study of motion was never one of Del Monte's primary concerns and there is no evidence he pursued the insight that the parabolic trajectory provided, whilst for Galileo the outcome of the experiment was a crucial part of his scientific development and led him to the law of fall.[60]

The experiment with the rolling ball tracing a parabolic trajectory is much more in keeping with Galileo's approach. He attempted to test theories derived from reason, by devising experiments that minimized the effect of factors other than the key variables being investigated. The conditions for his experiments are derived from the assumptions of his theoretical reasoning. For example, in *De motu* in the discussion of his solution of the inclined plane problem he says that:

> …this proof must be understood on the assumption that there is no accidental resistance…we must assume that the plane is, so to speak, incorporeal … the moving body must be [assumed to be] perfectly smooth, of a shape that does not resist motion, e.g., a perfectly spherical shape, and of the hardest material or else a fluid like water… And so the conclusion remains that on a horizontal plane itself the motion of the body is neither natural nor forced. [61]

By this time Galileo's background had two strands, a technical and a philosophical one. After moving to Padua, however, Galileo's activities took on a more practical turn that much more paralleled that of his patron Del Monte: running his own workshop, inventing his own instruments (such as the military compass, which was sold to prestigious clients throughout Europe), and producing works on mechanics,

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[59] Galileo, *Two New Sciences,* trans. Henry Crew, 142. *Le Opere di Galileo Galilei* VIII, 185.
[60] Renn, Damerow and Rieger, 'Hunting the White Elephant', 70.
[61] Galileo, *De motu,* trans. Drabkin and Drake, *On Motion,* 65-66. Galileo, *Le Opere di Galileo Galilei* I, 288-299. Drabkin notes this is a step toward the elimination of the dichotomy of natural and forced motion, towards the inertial concept.

military technology and military architecture.[62] It was, of course, at this time that he developed close contacts with the Venetian arsenal which had recently been expanded, both providing technical advice and in return being provided with a rich source of inspiration and technical knowledge, to which he pays tribute in the opening lines of the *Two New Sciences:*

> Salviati: Frequent experience of your famous arsenal, my Venetian friends, seems to me to open a large field to speculative minds for philosophizing, and particularly in that area which is called mechanics, inasmuch as every sort of instrument and machine is continually put into operation there. And among its great number of artisans there must be some who, through observations handed down by their predecessors as well as those which they attentively and continually make for themselves, are truly expert and whose reasoning is of the finest.
> Sagredo: You are quite right. And since I am by nature curious, I frequent the place for my own diversion and to watch the activity of those whom we call "key men" (*Proti*) by reason of a certain preeminence that they have over the rest of the workmen. Talking with them has helped me many times in the investigation of the reason for effects that are not only remarkable but also abstruse, and almost unthinkable.[63]

During this time Galileo planned a military treatise that from a fragment of its contents was intended to have been a substitute for Tartaglia's work and indicates his intention to put to use the outcome of his parabola experiment with Del Monte. Matteo Valleriani provides details of Galileo's outline of this proposed treatise, in which he intended to integrate the practical knowledge required by the bombardier with related theoretical speculations particularly relevant to his theoretical mechanics. The fourteen-point outline follows the pattern of questions raised by gunners that I have noted in Tartaglia's and Digges's work. The third heading raises the question whether the artillery works more powerfully from a certain distance rather close by, for which Tartaglia attempted to provide a physical explanation. Galileo, however, discounts the possibility that this could happen as counter to any reasoning of the

[62] Here he also taught Aristotle's *Mechanical Problems*.
[63] Galileo, *Two New Sciences,* trans. Stillman Drake (Madison, WI: University of Wisconsin Press, 1974), 11. Quoted as Appendix I in Freudenthal and Mclaughlin (2009), 89. *Le Opere di Galileo Galilei*, VIII, 49.

nature of violent motion. Other headings include whether the ball moves in a straight line when it is not shot perpendicularly; what line the ball describes when it is in motion; at which elevation it shoots furthest and why; whether when the ball falls down perpendicularly, it does so with the same force and velocity as it went up; and whether the longer the cannon the further it can shoot, and why.[64]

From the earliest (*De motu*) period and his Paduan period, Galileo's interest in motion was integral to his interest in the ballistic trajectory to solve gunners' problems. As Renn *et al*. point out the table of contents for Galileo's treatise on artillery indicate Galileo:

> was well aware of the practical importance of the outcome of the experiment recorded in Guidobaldo del Monte's notebook. In particular, he refers to the symmetry of the trajectory, its continuously curved shape, and its dynamical composition exclusively in terms of improving the precision of artillery. Possibly, however, Galileo's planned treatise was intended only for purposes of private teaching. Such a usage of the knowledge acquired by the experiment of projectile motion certainly fits well with Galileo's efforts – extended over a period of 40 years- not to make this knowledge publicly available.[65]

Confirmation of Galileo's discovery of the law of fall comes with a letter to Paolo Sarpi of 16 October 1604 in answer to Sarpi's difficulties with their belief in symmetry of projectile motion:

> Thinking again about the matters of motion, in which, to demonstrate the phenomena [accidenti] observed by me, I lacked a completely undubitable principle which I could pose as an axiom, I am reduced to a proposition which has much of the natural and the evident: and with this assumed, I then demonstrate the rest; i.e., that the spaces passed by natural motion are in double proportion to the times, and consequently the spaces passed in equal times are as the odd numbers from one, and the other things. And the principle is this: that the natural moveable goes increasing in velocity with that proportion with which it departs from the beginning of its motion.[66]

Renn *et al.* infer that Galileo was familiar with the law of fall as an inevitable result of the symmetrical shape of the parabolic trajectory from 1602. As Wohlwill argued,

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[64] Valleriani *Galileo Engineer* (Dordrecht; Heidelberg; London; New York: Springer, 2010), 87-89.
[65] Renn, Damerow and Rieger 'Hunting the White Elephant …', 79.
[66] Renn, Damerow and Rieger, 'Hunting the White Elephant …', 84-85.

'…the Latin sections of the "Fourth Day" of the *Discorsi* are in truth what they claim to be: they are parts of a manuscript concerning a new theory of motion that had been temporarily finished in Padua. Galileo's theory of projectile motion, as it is presented in the *Discorsi,* is the product of the glorious period of his best years to which the majority of his greatest discoveries belong, and not a discovery made in old age.'[67]

Conclusion

In this chapter, drawing on the research of Matthias Schemmel, Jürgen Renn, Matteo Valleraini and others, I have shown how both Harriot and Galileo achieved similar results with regard to the law of fall and the parabolic trajectory. Both Harriot and Galileo made the crucial breakthrough in recognising that in the absence of resistance, motion would continue indefinitely. Despite the fact that simultaneous discoveries and priority contests are not uncommon in the history of science, the epistemological implications of Schemmel's work seem all the more significant for having remained uncovered for so long. In Harriot's case we see the iterative process in action as he used the empirical data from gunnery manuals as his reference point as he tested and adapted his theoretical trajectory. We saw how, using a creative combination of geometric, algebraic and arithmetic methods, he was able to achieve a theoretical trajectory that closely matched in shape that of visual perception, but was unable at the same time to match his theoretical trajectory with empirical data that suggested that the maximum range was close to forty five degrees.

Galileo's early experiment with the polished ball and his belief that the catenary was parabolic led him to focus on the symmetry of the trajectory as his goal rather than achieving a perfect match with visual perception. In the case of Galileo,

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[67] Emil Wohlwill, 'The Discovery of the Shape of the Parabolic Trajectory' first published 1899, reprinted in in Renn (ed.) *Galileo in Context* (Cambridge: Cambridge University Press, 2001), 375-411, on 409.

from his earliest engagement with the study of motion, he considered that only in a void could the true nature of motion be discovered; in his implicit commitment to the existence of the void, Galileo was playing with fire, since this challenged the Aristotelian postulate, adopted by the Roman Catholic church, that a vacuum was impossible. Worse, it associated him with the anti-Aristotelian, Epicurean, anti-religious sentiments of Lucretius's *De Rerum Natura*. In the next chapter I will show how the question of motion in a void was fundamental to the contention that followed the publication of the *Discorsi* in 1638. With the discovery of the law of fall and the parabolic trajectory, this chapter marks a turning point and a new stage in the struggle to defend Galileo's theory of motion, as well as its application to the practice of gunnery.

Chapter four

'Nature does not make these blunders': Evangelista Torricelli's defence of the new science[1]

Introduction

Galileo's account of the parabolic trajectory and the law of fall led to criticism both for its theoretical premises and its inability to correspond with gunnery practice. A. R. Hall highlighted the difficulties that Galileo's disciple Evangelista Torricelli had in dealing with the criticism that Galileo's range tables did not conform to the results of practical trials. In this chapter I will argue that the *Discorsi e demonstrazioni matematiche intorno a due nuove scienze* (1638), which contains Galileo's theory of the parabolic trajectory and his range tables, was as much of a Copernican work as the *Dialogo dei due massimi sistemi del mondo* (1632). Thus the 'failed' trials should be seen in the context of a more general struggle for the acceptance of *the* new science in this wider sense. I will examine Galileo's claims regarding the practical application of his discoveries in the fourth day of the *Two New Sciences* and Torricelli's development of, and attempts to defend, Galileo's theory of motion. In addition, I will show how the challenge to Galileo's law of fall and theory of the trajectory is inextricably linked to criticism of his attempt to apply the theory to gunnery practice.

The critical response to Galileo's theory of motion

After the publication of the *Two New Sciences* in 1638, Galileo's law of fall came under particular scrutiny. But in addition there were claims that the predictions for the range of the trajectory (improved by Torricelli) did not coincide with gunnery

[1] Evangelista Torricelli (Florence) to Giovanni Battista Renieri (Genoa), 8 August, 1647. *Opere dei Discepoli di Galileo,* I, 391: la natura non fa questi spropositi. My translation.

practice. Scepticism on the former point came mainly from France, centered around the Minim friar and 'intelligencer' Marin Mersenne, whose vacillation made him perhaps the unwitting conduit for attempts by the Jesuits to undermine the new science.

Whilst, as we will see, the disputes were the result of different theoretical conceptions and different attitudes towards experimental evidence, to be expected with any radical new theory, there is some evidence to suggest that the water was further muddied by the Jesuit propensity to cavil. A. R. Hall, in his desire to furnish support for his agenda, focused on an exchange between Evangelista Torricelli and Giovanni Battista Renieri regarding a query about gunnery trials. Hall provides an amusing account of Torricelli's consternation in trying to defend the Galilean theory against the evidence of practical trials presented by Giovanni Battista Renieri. In Hall's picture of events, Torricelli, somewhat discomfited by Renieri's persistent presentation of experimental evidence that contradicted the Galilean predictions, finally resorts to the defence that anyway he was writing for philosophers not gunners. However, a detailed examination of the interchange and its context presents a more complex picture.

Galileo discussed the application of his theory for the use of gunners at the end of the fourth day of his *Two New Sciences.* But to really appreciate Galileo's understanding of the impediments to the application of the theory of the trajectory to the practice of gunnery, it is necessary to look at what Galileo wrote about air resistance on the first day, because of its relevance to what he says later.

It would be easy to assume from reading Hall that Galileo did not take the question of air resistance seriously, yet in day one he discusses in detail the effect of air as a medium, including a detailed description of an experiment to determine the specific gravity of air.[2] Galileo, in a classic example of his genius in exposing with the utmost clarity the reality behind the misconceptions of intuitive sense experience, explains on day one that the difference in the speed of bodies of different specific gravity in dense media such as water is much greater than the difference of their speeds in air. Thus if, using thought experiment, you imagine this taken to its limit, in a vacuum that difference in speed would be reduced to zero.[3] He continues:

> Salviati: … I begin by saying that a heavy body has an inherent tendency to move with a constantly and uniformly accelerated motion toward the common center of gravity, that is, toward the center of our earth, so that during equal intervals of time it receives equal increments of momentum and velocity. This, you must understand, holds whenever all external and accidental hindrances have been removed; but of these there is one which we can never remove, namely, the medium which must be penetrated and thrust aside by the falling body. This quiet, yielding, fluid medium opposes motion through it with a resistance which is proportional to the rapidity with which the medium must give way to the passage of the body; which body, as I have said, is by nature continuously accelerated so that it meets with more and more resistance in the medium and hence a diminution in its rate of gain of speed until finally the speed reaches such a point and the resistance of the medium becomes so great that, balancing each other, they prevent any further acceleration and reduce the motion of the body to one which is uniform and which will therefore maintain a constant value. There is, therefore, an increase in the resistance of the medium, not on account of the change in its essential properties, but on account of the change in rapidity with which it must yield and give way laterally to the passage of the falling body which is being constantly accelerated.[4]

This increase in the resistance of the medium with the speed of the body is important for Galileo's discussion of his application of his theory of the trajectory to practice, and he returns to it in day four. After the presentation of the parabolic trajectory both Sagredo and Simplicio raise questions regarding the divergence of the theory due to the fact that the earth is spherical, which Salviati is able to counter by reference to the precedent of Archimedes, pointing out the relatively short distances involved in

[2] Galileo, *Two New Sciences* trans. Crew and de Salvo, 76-8. *Le Opere di Galileo Galilei* VIII, 123-4.
[3] *ibid.,* 70. *Le Opere di Galileo Galilei* VIII, 117.
[4] *ibid*., 71-2. *Le Opere di Galileo Galilei* VIII, 118-119.

artillery compared to the size of the earth.[5] But Simplicio's objections also include the problem of the resistance of the air, and Salviati admits that the problem is, indeed, virtually insurmountable:

> Simplicio: …Besides, I do not see how it is possible to avoid the resistance of the medium which must destroy the uniformity of the horizontal motion and change the law of acceleration of falling bodies. These various difficulties render it highly improbable that a result derived from such unreliable hypotheses should hold true in practice.
>
> Salviati: All these difficulties and objections which you urge are so well founded that it is impossible to remove them; and as for me, I am ready to admit them all, which indeed I think our Author would also do.[6]

Salviati explained that the perturbation of the air defies description because of the

> infinite variety of ways corresponding to the infinite variety of forms, weight, and velocity of the projectiles…hence, in order to handle this matter in a scientific way, it is necessary to cut loose from these difficulties; *and having discovered and demonstrated the theorems, in the case of no resistance, to use them and apply them with such limitations as experience will teach***.** And the advantage of this method will not be small; for the material and shape of the projectile may be chosen, as dense and round as possible, so that it will encounter the least resistance in the medium. Nor will the spaces and velocities in general be so great but that we shall be easily able to correct them with precision.[7]

Here Galileo expresses his acute insight into the dialectical relationship between theory and practice that expresses the central theme of my thesis: that to treat matter scientifically you have to cut loose from its infinite complexities. In terms of Newton's perfect mechanic, Galileo's perfect mechanic would have to be divine, but like Tartaglia, Galileo does not give up on the possibility of intervening into the real world of matter, rather he recognizes that theoretical abstraction provided the means to mediate between the theorems and material complexity. Bacon also expresses the

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[5] *ibid*., 241. *Opere di Galileo Galilei* VIII, 274.
[6] *ibid*., 240. *Opere di Galileo Galilei* VIII, 274.
[7] *ibid*., 242. *Opere di Galileo Galilei* VIII, 275-6. My emphasis.

same scientific optimism in his criticism of the acataleptics who, when faced with the inscrutability of the world, insist that therefore nothing can be known.[8]

On the one hand Galileo shows himself fully aware of the problem that air resistance poses to practice, but suggests that they might still be used and applied 'with such limitations as experience will teach'. He considers the two effects of air resistance, one of which is that a lighter object is subjected to greater resistance than a denser object, the other that the resistance increases according to the speed of the body. For the first, by relating to experience, he concludes that for projectiles other than fire-arms, the retardation of speed is small. In the case of the second, he describes an experiment with pendulums of equal length which are swung, one to make a small arc and the other a large arc. The speed of the latter would be considerably more than the former, since it passes through a longer arc in the same time. Nevertheless, whilst it would be expected that the frequency of the vibrations would be less in the faster pendulum, this was not the case, indicating, Galileo argued, that the resistance of the air does not significantly affect motions of (normal) high speed more than those of low speed.[9]

Sagredo and Salviati conclude from this experiment that for most machines where the velocities are not very great, the errors are of small consideration.[10] However, Simplicio asks Salviati why he makes an exception of the projection of firearms, to which Salviati replies that the violence with which they are launched is supernatural (*sopranaturale*), i.e., far greater than any velocity that a body could ever reach in the natural motion of fall. The result of this is a deformation of the trajectory making the parabola less inclined and curved (*meno inclinato, e curvo*) at the beginning than at the end. Thus Galileo shows himself perfectly aware of the

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[8] See, for example, *Novum Organum* Aphorism 37, *The Oxford Francis Bacon* XI, 79.
[9] Galileo, *Two New Sciences* trans. Crew and de Salvo, 243-4. *Le Opere di Galileo Galilei* VIII, 276-8.
[10] *ibid*., 245-6, *Le Opere di Galileo Galilei* VIII, 278.

difference between his perfect parabola and the shape of the trajectory well known from gunnery practice and encapsulated in Tartaglia's three-section model. He continues, however, that this difference is of little consequence (*poco ò niente)* for practical purposes which is the preparation of a table of ranges for high elevations shot from mortars that use small charges and in which the momentum (*impeto*) is not supernatural.[11]

Galileo is highly motivated to put his theory to practical use despite his full awareness of the problem that air resistance poses. Paul Lawrence Rose has suggested that day four was subject to last-minute modifications to ensure that Galileo did not make unwarranted claims about the range tables included at the end of the *Discorsi*. He notes that A. R. Hall simply assumed that Galileo failed to take account of air resistance.[12] As I have shown above, on the contrary, Galileo had put considerable thought into the nature and extent of the problems of air resistance; the concept of motion in media is integrally related to motion in a void in Galileo's science, as he attempts to effect the marriage of theory with concrete reality.

Nevertheless, Rose provides evidence in a letter to Diodati written only weeks before publication of the two new sciences, that Galileo considered his tables *would* be of use to artillery and that this suggests he must have modified his claims during this short period:

> I wish to put an end to the treatise on projectiles and to send it as soon as possible to Signor Elzevir [his publisher]. I say put an end to because in revising and reordering it I have been discovering continually the most beautiful propositions in which this topic abounds; but I wish for now to close the treatise with a table which I have proved and calculated for artillery and mortar trajectories, showing their flights and with what proportion they increase and diminish according to the various degrees of elevation. The

[11] *ibid*., 245-6. *Le Opere di Galileo Galilei* VIII, 278-9.
[12] Paul Lawrence Rose, 'Galileo's Theory of Ballistics' *The British Journal for the History of Science,* 4 (1968), 156-159, on 156.

> practice of this table will be useful to gunners, its theory of great delight to philosophers (*speculativi*).'[13]

Rose may be correct in suggesting that Galileo's equivocation probably arose from his desire to provide the long-sought general solution whilst still retaining his integrity. It is possible, however, that when writing to Diodati he was in the hope that perhaps the tables would be of some use for cannon if adapted to allow for air resistance; Galileo was always more cautious in the wording of his written works than in his private letters and discussions. Time was running out for him and he had to make the best of what he had. As Rose notes, the equivocation did not get past Descartes, who, in a letter to Mersenne, wrote a long, detailed and wide-ranging critique of both the theoretical foundations and the practical claims of the *Two New Sciences,* culminating in a deft pun that captures the essence of the problem that he believes Galileo has by-passed:

> …It ought to be noticed that in proposing his suppositions he excepts from them artillery so that he may demonstrate them more easily. But that, all the same, towards the end it is principally to artillery that he applied his conclusions. In a word, *il a tout basti en l'air.*[14]

Rose blames Torricelli for failing to make the qualifications in his *De motu* that Galileo put in the two new sciences, and thus for creating the confusion over the applicability of Galileo's theory. But this is not necessarily fair to Torricelli. The problems that Galileo and Torricelli (like Tartaglia and Digges) encountered with regard to practical application reflects the difficulties of their integrative programme and the diverse audiences for which their works were intended. This is one of the factors that singles them out from their contemporaries. They had a much more difficult task compared to those who directed their attention solely towards a scholarly audience.

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[13] *ibid*., Translated and cited by Rose on 156.

[14] *ibid.,* Letter to Mersenne 11 October, 1638. Quoted and translated by Rose on 158. 'He has built (*bâti*) his whole theory on air'.

Torricelli's *De motu gravium*

With regard to Torricelli, what claims did he actually make about the applicability of his improved quadrant and range tables for gunners? Is it true, as Hall and Rose imply, that Torricelli did not make the necessary qualifications on the application of the theory to practical gunnery? Here is what Torricelli says on the operation of his gunners' quadrant:

> Let us now come to practice; and by help of an instrument, let us resolve some of the propositions above demonstrated. We will make a military quadrant, which with invariable certainty sheweth *(at least to Geometrical Philosophers, if not to practical Gunners)* what Mounture or Elevation ought to be given to any Piece, to the end that the length of the Range may prove to be of such a certain measure.[15]

He explains the nature of the problem:

> By the help of this [Tartaglia's] Quadrant Gunners have with long observations composed such a *Praxis,* as that they know how many points they are to mount *v.gr.* a Culvering of 40 pound Ball to hit a mark distant; for example, 700 Geometrical paces, or at any other distance.
>
> But the truth is, the observations are so fallible, and the Gunners so few that have made them, and made them exactly, that the use of artillery, taking from it the Range of Point Blank, must needs have very little of certainty in it.[16]

But he continues further on:

> Therefore to derive some rule from the Experiments, it were necessary to make them exactly, at all the Grades of Randons, in all sorts of Pieces, with all varieties of Powders, and different matters of Balls, and happily one might say, it were necessary also that every Gunner made them by himself. Things almost impossible to reduce unto Rules, from which any certainty might be gathered, if the Theorick and Geometry had not given us a manifest Science thereof, by means of that one sole Proposition of *Galileo,* in which first of all men he hath advertised and taught us, *That projects do all move in a Parabolical Line.* Upon this supposition we will ground the Instrument promised: *and though by the impediment of the medium the Parabola's [sic] become too deformed, or by many other accidents the Ranges prove very inconstant, yet it sufficeth us to have given indubitable satisfaction to the School of Mathematicians, if not to that of Gunners.*[17]

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[15] Evangelista Torricelli, *De motu gravitum* in *Opere di Evangelista Torricelli* II ed. Gino Loria & Guiseppe Vassura, (Faenza: G.Montanari, 1919-1944), 217. Translation from *The Doctrine of Projects Applyed to Gunnery*, in *The Compleat Gunner* (London: Rob. Pawlet, Tho. Passinger, and Benj. Hurlock. 1672), 16. Hall, *Ballistics,* 96, notes that it was written by Thomas Venn and John Lacey. My emphasis.
[16] *ibid*., 16. Torricelli, *Opere di Evangelista Torricelli* II, 219.
[17] *ibid*., 17. My emphasis. Torricelli, *Opere di Evangelista Torricelli* II, 220.

The section I have underlined in this extract is crucial because Hall does not mention the clear statement that Torricelli had made in the *De motu* about the problem of air resistance, thus implying that it is only later, in his exchanges with Roberval and Renieri, that he finally resorted to the argument that his book was 'written for philosophers, not gunners'.[18] Torricelli never claimed that the instrument that he devised for bombardiers based on the supposition of the parabola gave perfect accuracy in practice – it was perfect only for the satisfaction it brought to the mathematical mind.

Torricelli's wry comments indicate his awareness of the problems of practice and appear to be an attempt to preempt potential critics. The *De motu* was written in Latin, but the practical section on the use of the *squadra* was written in Italian. That Torricelli wrote this section in the vernacular is testimony that, like Galileo, he had a commitment to the utilitarian programme for the technical training of practitioners, as well as appealing to philosophers. This dual commitment reflected the dual roots of their science and the practical origin of the problem.

Galileo and Torricelli had not only made a theoretical discovery and demonstration of the law of fall and the trajectory, but had also produced numerical values that the trajectory would take at different elevations and an instrument for easy calculation. This was a stunning achievement into which they had put considerable effort. Do we then expect them to have kept the results of their efforts to themselves until they were sure that they matched practice perfectly? It was only by comparing the theoretical predictions with practice that the possibility of progress could be made

[18] Hall, *Ballistics*, 98, referring to letter to Renieri, Aug 1647.

on controlling the other variables that affected practice. As we will see, this understanding is exhibited by Torricelli in his discussions with Renieri.

In Torricelli's replies to questions from Mersenne, Roberval and Giovanni Battista Renieri he reiterates what he and Galileo had made absolutely clear already. As the quotes above from the two new sciences show, Galileo's understanding of the role of air resistance in projectile motion was at the cutting edge of the knowledge of the time, and Torricelli would go on to work on the effects of percussion as well as executing, with the help of Vincenzo Viviani, his groundbreaking barometric experiment which proved the existence of a vacuum and the effect of the pressure of air, which caused a sensation throughout Europe. Indeed, the latter became his main claim to fame in popular historiography.

It was precisely the Galileans who were the pioneers in the development of theoretical and experimental work on the void and pneumatics. Furthermore, in their short lives the two most talented disciples of Galileo, Torricelli and Cavalieri, were also pioneers, through their work on indivisibles, in the development of the calculus in mathematics.[19] As we will see, many critics either did not read or did not understand the clear distinction made by Galileo between fall with and without air resistance. Galileo and Torricelli were very aware of the limits of experiments but their main focus of attention was on improving the axiomatic foundations of the new science.

Criticism from France and Torricelli's mathematical realism

As Hall recounts, the French mathematician Roberval complained in a letter to Torricelli that air is a resisting medium and so there can never be either a uniform or a

[19] See the biography of Evangelista Torricelli in The Mactutor History of Mathematics archive, http://www-history.mcs.st-andrews.ac.uk/Biographies/Torricelli.html., accessed on 15/1/15.

uniformly accelerated motion.[20] Torricelli could say no more than Galileo had written and effectively suggested that Roberval should go back and read the book properly. Finally, when Roberval categorically denied that the Galilean theory of motion corresponded to natural experience, Torricelli, quite understandably, changed tack in order to cut off all further discussion. Anticipating Roberval's final letter, of which he had got wind via Mersenne, he wrote to Michaelangelo Ricci:

> That the principles of the doctrine *de motu* are true or false is of no consequence to me. Since, if they are not true, we imagine that they truly conform as we have supposed and then we take all the other speculations that are derived from these principles, not as something mixed, but purely geometrically. I imagine or suppose that each body or point falls or rises in the given proportion and horizontally with uniform motion. When this is the case, I say that all that Galileo said, and what I said, follows. If then the ball of lead, of iron, or of stone does not follow that supposed proportion, too bad: we will say that we are not speaking of them.[21]

Here we see a divergence between Galileo and Tartaglia as mathematicians compared to Bacon who emphasized vigilance in the face of the danger of losing contact with concrete reality and thereby severing the link between *res/mens*. As with Thomas Digges, as mathematicians Galileo and Tartaglia could delight in the default position of the beauty and truth of theoretical abstraction (*if* the principles are not true), just as they might delight in listening to a beautiful piece of music.

In his reply to Roberval, as Hall notes, Torricelli asks Roberval to discuss the theory solely on its mathematical merits. Like Galileo he used the example of Archimedes, who believed that projectiles moved in a spiral, and wrote a whole book about the curve, but this did not mean that his geometry was worse just because his

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[20] Hall, *Ballistics,* 97.

[21] Evangelista Torricelli to Michelangelo Ricci, 10 February, 1646. *Opere dei Discepoli di Galileo Galilei* II (Firenze: Barbèra, 1935), 276 also quoted in Fabio Toscano, *L'Erede di Galileo* (Milan: Sironi Editore, 2008), 167: 'Che I principi della dottrina *de motu* siano veri o falsi a me importa pochissimo. Poiché, se non son veri, fingasi che sian veri conforme habbiamo supposto e poi prendansi tutte le alter specolazione derivate da essi principii, non come cose miste, ma pure geometriche. Io fingo o suppongo che qualche corpo o punto si muova all'ingiù et all'insu con la nota proporzione et horizontalemente con moto equabile. Quando questo sia, io dico che seguirà tutto quello che ha detto il Galileo et io ancora. Se poi le palle di plombo, di ferro, di pietra non osservano quella supposta proporzione, suo danno: noi diremo che non parliamo di esse.' My translation.

theory was wrong. This was not as unhelpful as it might seem, since it was only by dealing with the mathematical foundations and the need to abstract from air resistance that Roberval could have been won over to the Galilean science.

Hall asserts that 'Torricelli demanded to be treated as a philosopher and a mathematician, not as a physicist.' [22] It is absolutely the case that on many occasions Torricelli defended the freedom of the geometer not to be confined by apparent physical constraints. He was, first and foremost, and always, a mathematician:

> The Geometer has the special privilege to carry out, by abstraction, all constructions [*operationes*] by means of the intellect. Who, then, would wish to prevent me from freely considering figures hanging on a balance imagined to be at an infinite distance beyond the confines of the world? [23]

There is no question that he was sincere in his statement about the validity of mathematical truths regardless of whether they corresponded to any known physical reality. But the important thing, as we will see in his exchange with Renieri, is that in the case of the trajectory he did actually believe in the physical truth of the theory, as likewise with the Copernican theory. In this Torricelli's epistemological stance conforms absolutely with that of Galileo.

Whilst, as we will see, Torricelli was extremely helpful in answering Renieri's questions about why his ballistic experiment might not have conformed to the theory, in contrast, he was reluctant to be embroiled in controversy with his French interlocutors. Torricelli provides a poignant example of the effect that the Galileo affair had on his disciples. In 1632, when the *Dialogue* had just been published, but

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[22] Hall, *Ballistics,* 97. Galileo used this argument on a number of occasions, for example in his letter to Balliani of 7 January 1639, cited in Moscovici, *L'Expérience*, 45-6. Moscovici considers Galileo's argument to be specious given that Galileo wrote that the movements he was interested in were real. Moscovici suggests that Galileo's argument doesn't so much betray his disdain for experience, but rather a defensive attitude designed to prevent and circumscribe criticism.

[23] Torricelli *De dimensione parabola*, in *Opere di Evangelista Torricelli* I, 97, translation quoted in Paolo Mancosu and Ezio Vailati, 'Torricelli's Infinitely long Solid and its Reception in the Seventeenth Century', *Isis* 82 (1991), 59.

before it had been banned and Galileo condemned, he is an enthusiastic supporter of Copernicanism, as is evidenced by his first letter to Galileo:

> In the abscence of the Rev.mo Padre Matematico di N. Sig.re [Castelli],[24] it rests to me; his most humble servant and disciple, with the honour of acting as his secretary…
> I am fully informed of everything. I am by profession mathematician, though young, scholar of the Fathers in Rome for 6 years and two years more with the Jesuits. I was the first in the house of Father Abbot, and also in Rome, to study your book minutely and continually to the present, with such enjoyment (*gusto*) …having already practised all the geometry, Apollonius, Archimedes, Theodosius, and having studied Ptolemy and having seen almost everything by Tycho, Kepler and Longomontanus, I am finally forced by the many congruences to adhere to Copernicus, and to be by profession and denomination (*setta*) a Galileist.
> Father Grienberger…confesses that your book gave him great pleasure (*gusto grandissimo*) and that there are many good things in it, but that opinion does not praise it, and that though it may well appear to be, should not be held as true. Father Scheiner, when I spoke to him of it, praised it, shaking his head; he also said that reading it was wearying on account of the many digressions…In the end he said that you had brought evil on him and that he did not want to speak of it…For the rest I consider myself fortunate to have been born in a century in which I have been able to be acquainted with and to revere … a Galileo, that is, an oracle of nature…[25]

Torricelli, in describing Galileo as an 'oracle of nature' shows in this letter that, like Galileo, he believed that mathematics revealed the true laws of nature. But he never spoke openly of Copernicanism or wrote on astronomy again after Galileo's

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[24] Father Benedetto Castelli (of 'letter to Castelli' fame) was Galileo's pupil who remained faithful to him to the end, at some personal cost, as well as training a number of disciples in the Galilean science (notably Torricelli, Cavalieri and Borelli).

[25] Evangelista Torricelli (Rome) to Galileo (Florence) 11 Sept 1632. Galileo, *Le opera di galileo Galilei,* XIV, 387 and *Opere di Evangelista Torricelli,* III, 35-36, also quoted in Toscano, *L'erede,* 46-50. My translation: Nella absenza del Rev.[mo] Padre Matematico di N. Sig.[re], sono restrato io humilissimo suo discepolo e servitor, con l'honor di suo secretario…Io sono pienissimamente informato d'ogni cosa…Sono di professione matematico, ben che giovane, scolaro del Padre R.[mo] di 6 anni, e duoi altri havevo prima studiato da me solo sotto la disciplina delli Padri Gesuiti. Sono stato il primo che in casa del Padre Abbate, et anco in Roma, ho studiato minutissimamente e continuamente al presente giorno il libro di V.S. con quell gusto…havendo assai bene praticata tutta la geometria, Apollonio, Archimede, Teodosio, et che havendo studiato Tolomeo et visto quasi ogni cose del Ticone, del Keplero e del Longomontano, finalmente adheriva, sforzato dale molte congruenze, al Copernico, et era di professione e di seta galileeista. Il Padre Grienbergiero…confessa che il libro di V.S. gli ha dato gusto grandissimo e che ci sono molte belle cose, ma che l'opinione non la loda, e se ben pare che sia, non la tien per vera. Il Padre Scheiner, quando gliene ho parlato, l'ha lodato, crollando la testa; dice anco che si stracca nel leggerlo per le molte disgressioni…Finalmente dice che V.S. si è portato male con lui, e non ne vol parlare… Del resto io mio stimo fortunatissimo in questo, d'esser nato in un secolo nel quale ho potuto conoscere et riverir con lettere un Galileo, cioè un oracolo della natura…
Grienberger and Scheiner are nicely represented here; Torricelli and Scheiner at the two poles and Father Grienberger wavering in the middle. The able astronomer Scheiner was Galileo's 'stubborn and vindictive' opponent in the Sunspots controversy of 1615 and was probably an instigator of Galileo's trial in 1633. Note Scheiner later abandoned his sunspot theory, which was designed to accommodate the phenomenon with the perfection of the heavens. See Geymonat *Galileo Galilei,* 65.

condemnation, and he refused to be drawn on any questions that might antagonize the enemies of the new science. Evidence suggests that this was a self-imposed censorship representing the tactical social and intellectual retreat that resulted from the Galileo affair .[26]

It may seem like a diversion to discuss Torricelli's self-censorship regarding Copernicanism, but I aim to show that the repercussions were wider and impacted on the response to Galileo's theory of the law of fall and the trajectory.

R. S. Westman, in 'The Astronomers' Role…' discusses the changing role of the mathematician in the early modern period. He notes that Copernicus had asserted implicitly 'the *right* of the astronomer to make new kinds of claims about the physical world.' [27] He suggests whilst it was the Jesuits that played an important role in increasing the status and importance of the mathematician, they were also ultimately responsible for the attempted restriction of this role. As we have already seen, Christopher Clavius was an important patron and correspondent of Galileo. According to Westman,

> the most significant development in the role of the university astronomer was the emergence of a powerful Jesuit educational strategy. The Jesuit Order was a militant one. Its founder, Ignatius Loyola, had been a soldier. His followers stressed discipline and obedience. Christopher Clavius was the leading professor of mathematics at the Collegio Romano from 1565 to 1612, and one of his principal aims was to make the teaching of mathematics a vehicle for inculcating discipline in students and for attracting bright ones into the fold. One cannot help but notice that *both* Protestant reformers and the Catholic Counter-Reformers saw mathematics as a weapon in the overall conflict for the salvation of souls…it was Clavius who engineered a detailed policy for promoting the mathematical disciplines in the *Ratio studiorum* of 1586. [28]

Nevertheless there were inevitable disagreements about how far along this path it was safe to go without coming into conflict with church doctrines. There were inevitable

[26] Toscano *L'Erede,* 160.
[27] Robert S. Westman, 'The Astronomer's Role in the Sixteenth Century: A Preliminary Survey' *History of Science* 18 (1980), 105-147, on 111.
[28] *ibid.,* 131.

dangers in the educational aspirations and pretensions of the Jesuits who styled themselves as, and indeed became, the intellectual avant-garde of the church. Westman maintains that though as an educational policy Clavius's proposals for reform were largely successful, resulting in an increased number of mathematics professorships, in the end the reform was a moderate one that aimed at reconciling Aristotle with the mathematicians rather than overthrowing him; what the Jesuit mathematician could not question was the doctrine of the motionless earth.[29]

The reason for the limitations of Clavius's attempted reform is outlined in Rivka Feldhay's work on Galileo and the church, in a detailed account of the history of the progress of the *Ratio studiorum* after 1586. According to Feldhay, in the first version, mathematics

> is seen as the key to an understanding of physical reality on the one hand and as the model of correct rational procedure on the other. Both functions ensure it a major role in the overall scheme of knowledge leading from physical reality to ultimate (metaphysical) reality and theology. Accordingly, the professional and social status of mathematicians is recognized as equal to that of philosophers.
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> The *ratio* of 1586 was printed in only a few hundred copies. It was probably not meant for public dissemination. According to the letter of the general order to the provincial superiors, it was intended to be criticized by censors of the various provinces and possibly corrected. The objections, however, were harsh and immediate. A number of memorials were written against it, and a certain Father Enrique Enriquez submitted it to the Spanish inquisition, contending that it was a declaration of war on the teaching of Thomas Aquinas. [30]

The final version of the *Ratio* that was published thirteen years later, in 1599, represented the culmination of a battle between the mathematicians and philosophers of the Roman College. As educators of the Italian elite and its ruling class, it was

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[29] *ibid.*, 132.

[30] Rivka Feldhay, *Galileo and the Church: Political Inquisition or Critical Dialogue* (Cambridge: Cambridge University Press, 1995), 222.

essential for the Jesuits to maintain their intellectual credibility but at the same time firm boundaries had to be maintained between the mathematical and the physical.[31]

The extent of the vigilance required by the Inquisition reflected the difficulty of negotiating a route between heretical and dangerous ideas, useful and necessary scientific knowledge, and the maintenance of Jesuit intellectual standing. Every case had to be assessed on its merits, which required the creation of a huge bureaucratic machine and the accompanying vested interests and potential for manipulation and for the settlement of individual scores. The difficulty of assessment and divisions between different factions on what constituted heresy, combined with even a relatively small number of high-profile cases *pour encourager les autres* meant that the self-censorship of scientists such as Torricelli was probably more rigorous than any external censorship could have imposed.

It was not just with respect to Copernicanism that Torricelli was vulnerable. As I have noted, Torricelli was the first scientist to create a sustained vacuum and to discover the principle of the barometer. However, the question of the existence of the void was the subject of heated debate between those who believed in the possibility of a vacuum and the beliefs of the plenists (amongst whom the Jesuits were the major contingent). Whatever his reasons, Torricelli never published details of the experiment or publicly drew the obvious philosophical conclusions from it, whilst when news of the experiment reached France it caused a sensation.[32] Paolo Galluzzi considers it implausible to accept that Torricelli was unable to perceive the philosophical implications of what he had achieved or, alternatively, was uninterested in them. He suggests that another possibility was that he did not want to incur the

[31] *ibid.,* 230. The Jesuits were modernisers compared to the Dominicans who saw any intellectual flexibility as potentially the thin end of the wedge. See also Dennis C Smolarski 'The Jesuit Ration Studiorum, Christopher Clavius, and the Study of Mathematical Sciences in Universities' *Science in Context* 15 (2002), 447-457.
[32] Galluzzi, Paolo 'Vecchie e nuove prospettive torricelliane' in *La Scuola Galileiana: prospettive di ricerca. Atti del Convegno di studiodi Santa Margherita Ligure, 26-28 ottobre 1978*, *La Nuova Italia*, (Florence: La Nuova Italia, 1979), 42.

hostility of the majority of the Jesuits.[33] Indeed, it seems that he feared that the reach of the Jesuits was capable of extending even beyond the grave, since on his deathbed he instructed his friend Ludovico Serenai to erase all the marginal comments (presumably insulting) that he had made in the books of Niccolò Cabeo and Athanasius Kircher.[34]

As a result of his comments on the nature of mathematics Torricelli has been considered by Koyré and others as more of a 'pure' geometer than Galileo. As the quote from Hall above indicates, he too, took Torricelli's statements at face value, since, as with Koyré, it suited his overall agenda. However, there are many examples where Galileo makes similar assertions to Torricelli. Galluzzi provides an example from the introduction to *De motu* in which Galileo affirms that his theory, although a faithful reconstruction of real motion, can be justified *per se* as an edifice rigorously deduced from first principles. As we have seen, on numerous occasions Torricelli quotes Galileo practically verbatim and was the disciple who had the clearest understanding of his approach to the determination of scientific truths.

Galluzzi suggests that we have to completely reject Koyre's view and rather to think carefully about the conditions that were operative and above all on the theoretical and experimental objections directed at the Galilean scientific conceptions to which, on the death of *il Maestro*, he found himself forced to reply.[35] Galluzzi considers that Torricelli's response to the practical application of the theory of motion to gunnery practice provides overwhelming evidence of his belief that the theory

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[33] *ibid.*, 44. See letter from Torricelli to Michelangelo Ricci, 28 June 1644, in Paolo Galluzzi and Maurizio Torrini (eds.) *Le Opere dei Discepoli di Galileo,* I, 130-132. Torricelli appears to be referring to Ricci's comments in his letter of 18 June where he refers to theologians who are afraid that to accept the existence of the void because of its association with Epicureanism. Also published in *Opere di Evangelista Torricelli* III*,* ed. Gino Loria and Vassura Guiseppe (Faenza: G. Montanari, 1919), 193.
[34] *ibid.*, 44.
[35] *ibid.*, 36.

really did describe physical reality.[36] However, before discussing in detail the exchange between Torricelli and Giovanni Battista Renieri I will examine in more detail the response in France and Italy to the publication of the *Two New Sciences.*

Copernicanism, the law of fall, and the Jesuit challenge

As Galluzzi notes, if, after the condemnation of Galileo, his young and enthusiastic admirer was not able to continue the exploration of the heavens initiated by the Master, there was nothing stopping him from engaging in the study of mechanical questions, and refining their mathematical and geometrical procedures.[37] However, Torricelli was thwarted somewhat in his desire for a quiet life by the enthusiastic espousal of Galilean science by Pierre Gassendi, who explicitly linked Copernicanism and the Galilean theory of motion. According to Galluzzi, Gassendi, in his *De motu impresso* (1642) 'proposed a bold integration of the *Dialogue* and the *Two New Sciences,* accomplishing that organic objective which had been in Galileo's mind since 1609, but which the anti-Copernican sentence of 1633 had prevented him from achieving.'[38] Perhaps the relationship between Galileo's two major works is best summarized by Ludovico Geymonat when he said that the *Two New Sciences* 'was in reality a Copernican work. But unlike the *Dialogue,* it was not a Copernican manifesto; rather, it was a work developed entirely in accordance with the new Copernican direction of modern science, deepening its principles and broadening its development.'[39]

Gassendi's *De motu impresso* arose from two letters to his friend Pierre Dupuy in 1640. They described the result of an experiment carried out on an

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[36] *ibid.*, 39.
[37] *ibid.*, 24-5.
[38] Paolo Galluzzi, 'Gassendi and l'Affaire Galilée of the Laws of Motion' in *Galileo in Context*, ed. Jürgen Renn (Cambridge: Cambridge University Press, 2001), 241. Originally printed in *Science in Context* 13 (2000) pp. 509-545. Originally published in Italian in 1993.
[39] Geymonat, *Galileo Galilei,* 176.

excursion from the port of Marseille sponsored by Louis de Valois, Governor of Provence, to verify Galileo's suggestion in the *Dialogo* that a heavy ball dropped from the masthead of a fast-moving ship would arrive exactly at the bottom of the mast rather than some way behind it. Gassendi made a clear link between this strong experimental refutation of a major argument against the Copernican theory and Galileo's theory of motion, since he argued that the trajectory of the body falling from the mast would be a parabola made up of the two motions, one 'uniform, rectilinear, and horizontal motion 'impressed' on the object by the moving ship, and of a uniformly accelerated and vertically downward motion which was caused by an external principle'.[40]

Gassendi's publication provoked an immediate, violent and threatening response from the Jesuit Pierre Le Cazre, Rector of the Dijon College.[41] The latter took the opportunity to further threaten Gassendi for his adherence to the philosophy of Epicurus, which denied the existence of Aristotle's accidental forms, with serious consequences for the Eucharist mystery, since accidental forms could not therefore be separated from substance.[42] Le Cazre was thwarted, however, by the fact that Gassendi carefully and successfully protected himself from the accusation of heresy by treating the heliocentric theory in purely hypothetical terms. In addition he was a respected and influential man of the Church who was absolutely ready to submit to its decisions.[43] Gassendi's attempted synthesis, however, provided the Jesuits with an alternative route to the undermining of the foundations of Galilean science by

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[40] Carla Rita Palmerino, 'Infinite Degrees of Speed: Marin Mersenne and the Debate over Galileo's Law of Free Fall', *Early Science and Medicine* 4 (1999), 269-328, on 300. Baliani carried out a similar experiment at Genoa three years earlier, the first to report results as numerical data, though Galileo had seen tests made at Venice many years before, see Drake, *Galileo at Work: His Scientific Biography* (Chicago: University of Chicago Press, 1978), 404.
[41] Galluzzi, 'Gassendi and l'Affaire Galilée', 242 and 245.
[42] *ibid*., 244. This was also the accusation of the Jesuit Father Orazio Grassi in response to the atomistic interpretation of phenomena expounded by Galileo in *The Assayer*. See Pietro Redondi, *Galileo Heretic* (London: Penguin, 1989), 163.
[43] *ibid*., 247.

questioning his law of fall; thus, as Galluzzi has argued, there is strong evidence that, at least to some extent, the reality of the law of fall became a proxy for the argument for the reality of Copernicanism.

This ulterior motive does not imply that the reaction to Galileo's science arose solely from an attempt at a posthumous undermining of Galileo's credibility. On the contrary, it is a reflection of how far ahead of even the best of his contemporaries Galileo and his small number of immediate disciples were, and the difficulties they had in coming to terms with the completely new conception of motion that the new science represented. As Renn and Damerow explain:

> If science essentially concerns the truth, based on solid facts and guided by logical principles, then scientific controversies should arise only in unfortunate circumstances or when errors and misunderstandings occur… In view of the fact that controversies in science are so common and so closely related to its conceptual development, it seems, however, more plausible to assume that they are not simply a social or psychological phenomenon, but rather constitute an essential epistemic element of science and a medium of its historically developing rationality.[44]

In the case of the reaction to the *Two New Sciences* the difficulty of breaking conceptually from adherence to the Aristotelian model intersected with ecclesiastical prohibition backed by repressive state power, so that genuine and understandable conceptual differences were inextricably intermingled with an external agenda.

The Jesuit Le Cazre denied the validity of Galileo's odd number rule of fall, arguing rather that the distance increases in double geometric proportion (1, 2, 4, 8, 16…). He also denied Galileo's contention that a heavy body moving naturally would pass through infinite degrees of velocity before reaching its final velocity. This was because he believed that velocity increases according to distance, not time.[45] He also denied the validity of the postulate proposed by Galileo at the opening of the *De motu naturaliter accelerato* (included in the *Two New Sciences)* that the speeds acquired by

[44] Renn and Damerow, *The Equilibrium Controversy,* 12.
[45] As noted above, at first Galileo and Harriot did not make this distinction.

one and the same body moving down planes of different inclinations are equal when the heights of these planes are equal. In replying to Le Cazre, Gassendi pointed to the absurd consequences of his theory of fall and noted that Torricelli had already demonstrated the said postulate in his *De motu.*[46]

Mersenne took the opportunity of a trip to Italy to inform the disciples of Galileo of the animated debates that were taking place in France.[47] Michaelangelo Ricci was obliged to be his host for some months from the end of 1644 and the early part of 1645. Having received a summary of Le Cazre's *Physica demonstratio* from Mersenne, Ricci passed it on to Torricelli noting that 'with these foundations the Jesuit presumes to scale the impregnable fortress to the detriment of Galileo and his school and, with a thousand boasts of himself and scorn of Galileo, exposes himself as no less lightweight in his morals than in his scholarship'.[48]

A more subtle challenge to the Galilean theory of motion came in August of 1644 from the Jesuit mathematician Honoré Fabri who conceived a novel, complex and sophisticated apologetic strategy that nevertheless was eventually to leave him open to criticism and suspicion of heresy from more conservative members of the Order.[49] Fabri's theory of fall had the advantage of incorporating a causal explanation that particularly appealed to Mersenne since, along with Descartes and Roberval, Mersenne could not conceive of a satisfactory description of fall that did not incorporate an explanation of its cause. This conviction became even stronger, both

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[46] Note this latter point had also been one of Descartes' (many) criticisms in his letter to Mersenne. Also see Domenico Bertolini Meli *Thinking with Objects,* 119, for an account of the issues relating to the postulate and Torricelli's solution, reflecting his 'concerns about interpreting Galileo and formulating a science of motion in a rigorous axiomatic fashion'.

[47] He wanted to buy lenses from Torricelli and to compare differences in units of measurement in Italy. Torricelli was renowned throughout Europe for the perfection of his lenses, which were in great demand.

[48] 'Con questi fondamenti presume il Gesuita d'alzar rocca inespungabile a' danni del Galileo e della sua scuola e, con mille vanti di se medesimo e scherno del Galileo, si dimonstra non men leggiero ne'costumi che sia nella dottrina' *Opere dei Discepoli di Galileo* I, 229. M Ricci (Rome) to Evangelista Torricelli, in Florence 2 April 1645. Partially quoted in Galluzzi*,* 'Gassendi and L'affaire Galilee', 257.

[49] Galluzzi, 'Gassendi and l'Affaire Galilée', 250, and W.E. Knowles Middleton, *The Experimenters: A Study of the Accademia del Cimento* (Baltimore: Johns Hopkins Press, 1971), 324.

for Mersenne and Baliani, as they became convinced of the impossibility of deriving conclusive results from experiments.

For Fabri, motion 'was produced by *impetuosité*: degrees of equal impetus were acquired in single instances of time and the velocity of motion increased by the summing of this impetus'.[50] This produced a natural motion that increased according to the natural numbers. When Mersenne pointed out that experience confirmed the Galilean law, Fabri was not put out, explaining that the instants were so small that it would be impossible to measure them and therefore in practice the two theories would be indistinguishable.[51] Thus when it suited them, the opponents of Galileo could dispense with the confirmation of practice, whilst at the same time demanding total conformity to practice for Galileo's laws. Fabri's theory conceived of motion as a discontinuous process, with a falling body starting from rest initiating motion with a determined degree of velocity, thus also contradicting Galileo's affirmation that a falling body starting from rest goes through infinite degrees of speed.

Mersenne enlisted the opinion of Theodore Duchamps, Le Tenneur and Christaan Huygens to respond to Fabri. Le Tenneur was also urged by Gassendi to take over from him the defence of Galileo, since he wished to retire from the debate. An overwhelming argument that was decisive for all these mathematicians who supported Galileo, as well as for Torricelli, against alternative theories of fall was that only the odd number theory of fall exhibits what we would now call scale invariance.[52]

Despite the strong defense of the Galilean theory by Deschamps, Le Tenneur, the young mathematical prodigy Christiaan Huygens, Gassendi and Torricelli,

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[50] Galluzzi, 'Gassendi and l'Affaire Galilée', 252.

[51] Mersenne confirmed the Galilean law of fall by experiments in his short pamphlet *Traite des mouvemens, et de la cheute des corps pesans*, 1634. See Palmerino 'Infinite Degrees', 269-70.

[52] See Palmerino 'Infinite degrees', 296. Palmerino's example: for time 1t and 2t respectively the spaces are 1s:2s:3s ≠ 3s:7s:11s (Fabri); 1s:2s:4s ≠3s:12s:48s (le Cazre). But 1s:3s:5s = 4s:12s:20s (Galileo).

Mersenne was increasingly swayed by the criticisms of Fabri and Le Cazre. Le Tenneur, who provided a detailed, spirited and powerful response to Fabri, became increasingly aware of Mersenne's vacillation and appealed to him not to play into the hands of 'nos beaux docteurs sourcilleuz'.[53]

Another significant character in the second Galilean affair is Giovanni Battista Baliani, a long-term correspondent of Galileo who provides the most likely link between the general challenge to Galileo's new science and the ballistic trials described in the letter from Giovanni Battista Renieri to Torricelli. The first edition of Baliani's work *De motu* of 1638 'presented an analysis of motion substantially convergent with that of Galileo'.[54] Their results were, however, based on quite different foundations that left Baliani susceptible to the influence of Galileo's critics. By the second edition of 1646 it was now Fabri's natural number theory that took central position. Baliani also insisted on the necessity of a causal explanation of fall, having convinced himself that experimental verifications were inevitably unreliable. In addition, he now denied that the trajectory was parabolic.

Mersenne stayed for some time in 1645 in Genoa with Baliani on his Italian trip during which time he would have had ample opportunity to discuss Fabri's hypothesis. Based in Genoa, Baliani was able to provide Mersenne with the ideal stop-off point both on his outgoing journey to Rome, where he stayed with Michaelangelo Ricci, and on his return. Baliani was an important catch for the anti-Galilean cause and the temptation to succumb to the flattering attentions of the likes of Father Grassi and Nicolò Cabeo must have been too strong to resist, especially

[53] Referring to, most likely Le Cazre and Fabri. Le Tenneur to Mersenne, 21 May 1647, quoted in Galluzzi, 'Gassendi and l'Affaire Galilée', 261.
[54] Galluzzi, 'Gassendi and l'Affaire Galilée', 265

since his original work, though not unfavourably received, had been eclipsed by the attention given to the *Two New Sciences.*[55]

Galluzzi also notes the evidence provided by Claudio Constantini in his book *Baliani e I Gesuiti* (Constantini 1969) that Frs. Grassi, Confalonieri and Cabeo, presumably spurred on by their success in gaining Baliani's support in challenging Galileo's work on motion, later even urged him to join the Company in the battle against the supporters of the vacuum.[56] By October 1647, despite his long and productive correspondence with Galileo, Baliani objected to Mersenne's having described him as a disciple of Galileo, maintaining the independence and priority of his work on motion.[57]

Whilst the evidence is supportive of the influence of the Jesuits in sowing the seeds of doubt into Mersenne's mind, Baliani's vacillation, like that of Mersenne and Descartes, also reflects his attempt to mediate between two different conceptions of science, originally identified by R. S. Westfall as reflecting 'two distinct currents of thought about nature, one mathematical-descriptive one mechanical-philosophical, engaged in dynamic interaction between one another'.[58]

Descartes adopted a mechanical explanation of gravity that eventually led him to give up on any attempt to find a law of fall. The void in which the Galilean laws applied did not exist for Descartes.[59] He criticized Galileo for not allowing for air resistance because it was impossible to abstract air resistance in a plenum. Action

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[55] *ibid*., 266-267. Galluzzi notes that Cabeo in his 1646 introduction to his commentary on Aristotle's Meteorologia praised Baliani's modesty 'that had induced him not to follow Galileo in the pretension of putting forward as *scientia* a series of propositions dependent upon clearly false principles'.

[56] *ibid*., 266. See also Redondi, *Galileo Heretic,* 297. Redondi notes that Baliani had not been able to replicate Torricelli's experiment, and only finally succeeded in November 1647, after receiving detailed instructions from Mersenne.

[57] Galluzzi, 'Gassendi and l'Affaire Galilée', 268.

[58] H. F. Cohen, 'A Historical-Analytical Framework for the Controversies over Galileo's Conception of Motion' in Carla Rita Palmerino and J.M.NM.H Thijssen (eds.), *The Reception of the Galilean Science of Motion in Seventeenth Century Europe* (Dordrecht: Kluwer Academic Publishers, 2004), 85.

[59] William R. Shea 'The "Rational" Descartes and the "Empirical" Galileo' in Palmerino, ed., *The Reception,* 67-82, on 74.

could not take place at a distance and Descartes conceived of the cause of fall as a pressure exerted by a vortex of very quickly rotating subtle matter upon gross bodies near the earth. This meant that a body beginning to fall from rest cannot be zero, since it is imparted with the speed of the matter with which it is in contact. Thus Descartes' mechanical philosophy was incompatible with Galileo's contention that a falling body moves through infinite degrees of speed.[60]

Gassendi, however, attempted to bridge the gap between the mathematical mechanics of Galileo and his atomist mechanical philosophy, since he believed in the truth of both despite the fact that he was unable to achieve their seamless integration. He thus became a key figure in the dissemination and popularization of Galileo's science.[61] Nevertheless, Gassendi's attempt to integrate a mechanical explanation into Galileo's law of fall meant that his original interpretation of the law of fall probably increased Mersenne's confusion since it implied a discontinuous rather than a continuously accelerated motion and blurred the distinction between motion in a vacuum and motion in a plenum. Whilst Galileo saw the medium as an obstacle, Gassendi initially believed that in order to explain the odd number rule, it was necessary to introduce two causes of fall, the *vis attrahens* (from the earth) and the *vis impellens* representing the impulse provided by the action of the air. It was only in the course of his attempts to reply to the criticisms of Le Cazre that he realised that it was possible to arrive at the law of fall without the addition of the effect of the medium. According to Palmerino:

> The resulting theory was of course much more economical and coherent…the hypothesis that the air exercised a propelling force on the falling bodies had not only rendered the correspondence between downward acceleration and upward deceleration highly problematic, but it had also introduced, in his *De motu,* a radical break between a mechanics of the *plenum* and a mechanics of the *vacuum:* for according to this treatise, heavy bodies should have obeyed

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[60] H. F. Cohen, 'A Historical-Analytical Framework' in Palmerino, ed., *The Reception,* 84.
[61] Palmerino 'Gassendi's interpretation of Galileo's theory of Motion' in Palmerino, ed., *The Reception,*164.

> the Galilean laws of acceleration only in a *plenum;* but at the same time, uniform rectilinear motion could only have been preserved in a *vacuum*![62]

While Palmerino does not discount that Mersenne's skepticism was related to the fact that Gassendi had forged such a strong connection between Galilean dynamics and Copernican cosmology, she suggests that there were 'theory-inherent problems that he was not able to reconcile'.[63] Ironically, considering the criticisms that were directed at the practical applicability of Galileo's science, Mersenne continued to believe that Galileo's law was 'for all practical purposes, verifiable and useful'. This was similar to his approach to Copernicanism, which he considered to be the most accurate and useful cosmological theory whilst maintaining that it could not be proved true. He came to consider that because of the inevitable inexactitude of measurement, motion could never be really understood without an understanding of ultimate causes.[64]

It will now be possible to examine Torricelli's exchange with Giovanni Battista Renieri in the light of the disputatious context of the reception to the Galilean science of motion, in which Torricelli was a reluctant participant.

The Renieri-Torricelli correspondence

The correspondence that took place between G. B. Renieri and Torricelli between 2 August and early September of 1647 has been a fruitful source for historians of science since A. R. Hall drew attention to the important evidence it provided regarding the tensions between Galileo's theoretical predictions and the results of

[62] Palmerino 'Infinite degrees', 310.
[63] *ibid*., 310.
[64] *ibid*., 328.

practical experience with guns. Since then it has been discussed more thoroughly, amongst others, by Michael Segre, Serge Moscovici, and D. B. Meli.[65]

Before considering the letters in detail, it will be useful to discuss further a third party who is important to an understanding of the significance of the letters, Jean Baptiste Baliani. The role that he played in the equivocation regarding Galileo's law of fall has already been noted. So who was Baliani? Baliani came from a wealthy Genoan patrician family. Moscovici notes that as a man of science in Italy at that time he was unusual because of his amateur status; it was more usual in Italy to hold a university/patronage position or to be a member of a religious order.

Baliani's life was devoted to civic duties. He was appointed governor of Sarzana in 1623 and in 1624 he entered the Genoa Senate. He had been appointed prefect of the fortress ofSavona in 1611 and he became its governor in 1647, at the time when the ballistic experiments described by Renieri took place. He was an ingenious, original, and independent thinker whose insights derived from the mechanical problems that arose from the civic projects with which he was involved, which particularly related to motion and hydraulics. His civic positions provided him with ideal opportunities to carry out practical experiments to test out theoretical propositions; as Moscovici notes his wide scientific interests and insights were sharpened and maintained by the social and political activity that remained his primary concern.[66]

Galileo's friend Salviati first met Baliani in 1613 and wrote to Galileo in praise of his qualities. There followed a long period of stimulating and fruitful correspondence between them that lasted right up to Galileo's death. In one of his

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[65] Michael Segre, *In the Wake of Galileo* (New Brunswick, N J: Rutgers University Press, 1991), 94-99, and 'Torricelli's Correspondence on Ballistics', *Annals of Science,* 40 (1983), 489-499; Serge Moscovici, *L'Expérience du Mouvement;* Bertoloni Meli, *Thinking with Objects.*
[66] Moscovici, *L'Expérience,*13.

earliest letters to Galileo he requested his help in devising a method to weigh air. In contrast to Galileo's belief that water is pumped solely by the action of a vacuum, Baliani, having carefully considered the matter in relation to the hydraulic problems he encountered in Genoa, explained in a letter of 1630 his reasons for believing that it was atmospheric pressure that raised water. Galileo, ignored the suggestion, but Torricelli later proved Baliani to have been correct with his barometric experiment. In his letter to Ricci describing his experiment Torricelli motivates his hypothesis regarding the action of atmospheric pressure in terms almost identical to that presented in Baliani's letter.[67]

Given his position as governor of the Fortress of Savona, it seems highly likely that Baliani was one of the gentlemen to whom Renieri refers to in his first letter to Torricelli as initiating the ballistic trials that were carried out in Genoa. Moscovici suggests Torricelli would have known that Baliani was his real opponent in the debate. He further suggests that the reason A. R. Hall did not note this was because he underestimated Baliani's importance as the first person both to challenge that the trajectory was parabolic in his 1646 *De motu* and also to put it to the test of practice.[68] But I think rather that Hall would not have been interested that this correspondence represented a theoretical as much as a practical challenge to the parabolic theory. However, Torricelli's painstaking explanation of the theoretical reasons for the truth of the theory, in both of his letters to Renieri, suggest that this is what was really at stake.

It would also be helpful to have some background knowledge of G. B. Renieri, but we have virtually no information on him other than that he was the brother of Vincenzo Renieri. An exploration of possible connections of interests between the

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[67] Segre *In the Wake,* 84; Toscano *L'Erede,*140.
[68] Moscovici, *L'Expérience,*190.

Renieri brothers provides another clue to the wider context of the experiments at Genoa.

Vincenzo Renieri met Galileo in 1633 and was entrusted by him to update his tables on the motions of Jupiter's satellites. This is an indication of the trust which Galileo, and later Torricelli placed in Renieri, since the work on Jupiter's satellites was crucial to Galileo's attempts to put Copernican cosmology to practical use in the solution to the longitude problem.[69] This is yet another example of Galileo's epistemology of practice. In 1641 Renieri was appointed to Galileo's old post of professor of mathematics at Pisa. He corresponded frequently with Galileo, and then with Torricelli, mainly in relation to this astronomical work, but their correspondence suggests a mutual friendship and trust that goes beyond professional interests.[70]

Given Baliani's break from the Galilean theory, it seems likely that he would have been keen to test out Galileo's predictions, and as governor he was in the perfect position to do so. To be fair to the other patricians present, it is unlikely that they would have read either Galileo's or Torricelli's work properly, if at all, and may well have missed their caveats. They would have had a material interest in putting the range tables to the test in order to improve the defences of the region. G. B. Renieri, influenced by his brother, was probably, as he said, generally in sympathy with the Galilean theory. From his letter it seems possible that he held a position at the fortress where he was involved in the procedure of the experiments. The results of the test gave a point blank range about four times longer than predicted. Renieri, wishing to defend Galileo, was left in perplexity when faced with this discrepancy:

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[69] See for example, Geymonat, *Galileo Galilei,* 97-100. Galileo had entered into negotiations with Spain and then the Dutch Government, presumably through the mediation of the Grand Duke of Tuscany who would have gained prestige from the fact that he was Galileo's patron.

[70] See, for example, *Opere di Torricelli*, III, 379, Torricelli to Vincenzo Renieri 16 June 1646. See also Drake, *Galileo at Work*, 414.

> The misery of my wits, accompanied by the desire to learn with the stimulus of these gentlemen, especially by the novelty brought by the work of Signore Baliani, in which he absolutely denies that the motion of projectiles is parabolic, although the reasons he adduces do not seem to me sufficient, as you may have seen from his work, I have not been able to contain myself from resorting to you to explain this to me…[71]

He goes on to assure Torricelli that every precaution had been taken in carrying out the experiment. With some ingenuity, he asked if it was possible that the trajectory might be parabolic, but that the axis was not perpendicular to the horizon, as an explanation, even providing a diagram showing a skewed parabola in relation to the earth. He indicates that he has been urged by the gentlemen to obtain an explanation, and also wishes to be able to obtain the truth in order to counter the bombardiers, with their 'foolishness and nitpicking'.[72]

Torricelli's engagement with the problem is shown by the promptness of his reply, dated 8 August. His introduction is abrupt: 'I'll leave off the ceremony and respond only to the business of your letter.' Mario Biagioli, the expert on courtly etiquette, notes that a friendly rebuff of ceremony was a recognised sign that the recipient had been accepted as an 'intimate': 'The refusal of ceremonies was itself a ritual…an epistolatory rite of passage'.[73]

Significantly, Torricelli defends the theory before discussing the particulars of the trial. This may be because he knows that this is what is really at stake because of Baliani's involvement, whether behind-the-scenes or more directly. He argues that either you accept the suppositions on which the parabolic theory is based, in which case the theory must be true, or, if they can be refuted, then he will accept that

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[71] Giovanni Battista Renieri to Evangelista Torricelli, 2 Aug 1647. *Opere dei Discepoli di Galileo*, I, 388; Torricelli, *Opere* III, 459. 'L'infelicità del mio ingegno, accompagnata dal desiderio d'imparare con lo stimolo anche di questi signori, massime per la novità apportata dall'opera del Sig.r Baliani, nella quale assolutamente nega il moto de'proietti essere parabolico, sebbene le ragioni che egli adduce non mi paiono sufficienti (conforme V.S. dall'opera sua avrà forse visto), non m'ha potuto contenere di non ricorrere da V. S., acciocché ella mi pieghi questo passo…'. My translation.

[72] *Opere dei Discepoli di Galileo* I, 390. 'con le loro balordaggini cercano di trovare il pelo nell'uovo'. My translation.

[73] Mario Biagioli, *Galileo, Courtier: The Practice of Science in the Culture of Absolutism* (Chicago: University of Chicago Press, 1993), 27.

everything he has said is false. The first supposition is that the descent of heavy bodies in equal times is as the odd numbers from one. This 'is as true that *nature, even if it wanted to, could not do otherwise.*' To show this Torricelli then provides an example that shows that only the odd number rule has scale invariance, emphasizing that *'nature does not make these blunders and inconsistencies, but always observes the same laws'*.[74] Torricelli makes it clear that he believes the supposition to be *physically* true.

The second supposition is that the spaces passed horizontally in equal times are equal to each other, abstracting impediments, and he explains why this is also true. He then gives an account of the effect of the impediment of the air that reiterates essentially what was made clear in Galileo's work and his own, adding that in practice the horizontal impetus at the mouth of the machine is four to six times that at the end of the shot, an assertion that suggests that he has at least attempted, whether by experiment, or otherwise, to estimate the velocity of a shot from a cannon and the effect of air resistance on its velocity from the beginning to the end of the shot.

Torricelli could have left it at that, but he considered that the discrepancy was far greater than would be expected even allowing for the impediment of the air and other accidental factors. He suggested three possible reasons for error, the soul of the piece might not have been level (*l'anima dell'artiglieria non fusse livellata)*,[75] the plane where it was fired may have been inclined, or the cannon may have raised itself up as it fired. He provided detailed suggestions for ensuring the piece was level, using a number of quadrants rather than just one, and devised an experiment using a frame to be set at a distance from the mouth of the piece that would allow the height of the

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[74] Evangelista Torricelli (Florence) to Giovanni Battista Renieri (Genoa), 8 August, 1647. *Opere dei Discepoli di Galileo,* I, 391. My emphasis: 'La prima di queste due supposizioni è tanto vera che la natura, quando ben anche volesse, non può fare altrimente… e la natura non fa questi spropositi e questi incostanze, ma osserva sempre la medesima legge.' My translation.

[75] ibid., 392. Thomas Digges, in the 1591 edition of *Pantometria,* on page 177, explains that the 'Soule' of the piece is the vacant cylinder.

shot to be measured at different distances. This would tell whether the gun was firing horizontally or at a slight angle. He also suggested dropping a cannonball at the same time as the shot was fired, and from the same height, to measure whether they reached the ground at the same time, since if there was a significant discrepancy this would also indicate that the piece was not horizontal. Torricelli's reply takes the form of detailed and extremely thoughtful instructions for conducting an experiment, enabling Renieri to eliminate factors that might impinge on the results.

In contrast to Torricelli's reluctance to be drawn into controversy with Mersenne and Roberval, this problem had captured Torricelli's interest because it stimulated his experimental ingenuity. He ends the letter with great affection, indicating that what he has written is for Renieri only, because of the esteem in which he holds him. He considers Renieri to be a compatriot, but not of a country or region, rather a compatriot in spirit. Far from complaining at being bothered, he wants more information so that he can make a better assessment. This is someone who is intensely interested in the practical outcome of his theoretical work.

Renieri replied on 24 August, having carried out the tests according to Torricelli's instructions. Unfortunately this time the results seemed to defy all logic, since the bullet not only shot through the first frame at less than half the height that would have been predicted, but then appeared to reverse curvature. Michael Segre provides a detailed analysis of this apparently anomalous result, noting that what Torricelli would not have known was that such a result would have been quite possible with an oddly rotating cannonball.[76]

What has not been noted, however, is Renieri's comments after this account – that the bombardiers themselves were dumbstruck by the result and had made the

[76] Segre, *In the Wake,* 97.

excuse that the falconet was *sboccato* or foulmouthed – in other words, it was completely distorted – and therefore it went *de frico (*haywire?). This fits with Segre's analysis, but indicates that the bombardiers were well aware that the bullet's trajectory would have been completely compromised by the condition of the mouth of the piece, even though they had never heard of spin.[77]

Renieri continues that another more suitable cannon had been ordered but that the tests had had to be postponed because of the rain. Nevertheless, he closes by saying that 'by these principles, however, I am confirming the opinion of Sig.s Galileo and yourself', but adds that he will keep Torricelli advised on further developments. It is not easy to be definite about what he means here. Does he mean that he has been satisfied by Torricelli's theoretical explanation, or that he is satisfied that with uncompromised equipment he will be able to obtain a result which is reasonably in keeping with the theory? However one interprets this, Renieri is expressing his satisfaction that Torricelli has answered his doubts and questions.

Torricelli replied at the beginning of September, explaining he had been hanging on since Renieri had implied he would be writing again regarding the new trial. He again explained the theoretical basis of the parabolic trajectory. Perhaps as a result of the difficulties encountered in the recent abortive trial, he emphasised that in the case of projectiles fired with gunpowder the results of experiences will be very different from theoretical predictions. It was because of this, to avoid controversy, that he had always made it clear in his books that he was writing for philosophers rather than gunners.

[77] In the eighteenth century, Benjamin Robins used a remarkably similar experimental set-up of firing musket balls into evenly spaced tissue-paper curtains to investigate the effect of spin imparted on the musket ball as it struck the musket barrel's side during firing, as the cause of its deflection. See Brett D. Steele, 'Muskets and Pendulums: Benjamin Robins, Leonhard Euler, and the Ballistics Revolution', *Technology and Culture* 35 (1994), 348-382, on 363.

Despite the fact that it was intuitively obvious that the results of the trial were anomalous, as the Bombardiers' reaction showed, Torricelli provided a detailed mathematical proof of why the result was mathematically impossible. The effort he made in doing this indicates his engagement with the problem that Renieri had posed. While in general the impression has been given, especially by Hall, that Torricelli used the excuse that he was writing for philosophers not bombardiers in order to get off the hook and cut off the correspondence, Torricelli's concluding remarks further indicate his continued engagement with the practical tests and wish to be informed on their progress:

> If these gentlemen had made new observations I await from you with suspense the usual favour of letting me know. Perhaps also the experiment might succeed if it is somewhat more adjusted. I don't know how they have made the measurement of the fall of the ball. If by chance they had supposed that the ground were horizontal, and then measured from the earth up to the hole in the canvas, that would be most fallacious.[78]

There is no record of any further correspondence. At this time Torricelli's health was deteriorating. He died in October 1647, at the age of only forty-one. His friend Vincenzo Renieri died on 5 November. Bonaventura Cavalieri also died in November 1647. Since Castelli had died in 1643, this meant that most of the hard core of Galileo's disciples were all gone by the end of 1647.

Conclusion

In this chapter I have shown that the question of the practical application of Galileo's theory of the trajectory was not separable from the wider questions regarding the acceptance of his new science. The challenge to the law of fall came firstly from from

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[78] Evangelista Torricelli to G. B. Renieri, September 1647. *Opere di Evangelista Torricelli,* III, 478-480, *Le Opere dei Discepoli di Galileo*, I, 406-7: 'Se cotesti signori averanno fatto nuova osservazione aspetterò da V.S. con ansietà il solito favore dell'avviso. Forse anco potrebbe l'esperienza riuscire alquanto più aggiustata. Non so come abbiamo fatto a misurare la scesa della palla. Se per sorte avessero supposto che il terreno sia orizontale, e poi misurato dalla terra fino al buco fatto dalla palla nel telaro, ciò sarebbe fallacissimo'. My translation.

plenists like Descartes; secondly from those, such as Mersenne and Baliani, who were not satisfied with a law that did not include a causal explanation; and thirdly from Jesuits like le Cazre and Honoré Fabri who had both conceptual differences and ulterior motives for not accepting the theory. What was implicit in Galileo and what was made more explicit by Gassendi was the connection that was made between motion in the heavens and motion on earth, which provided an additional incentive from some quarters to undermine the law of fall and the parabolic trajectory.

Baliani forms the connection between these theoretical disputes and the unsatisfactory gun experiments that were designed to test the practical applicability of the theory, and he was explicitly mentioned by G. B. Renieri in his letter to Torricelli with regard to these experiments. But examination of the correspondence between Renieri and Torricelli suggests that while Torricelli was reluctant to enter into correspondence with his French interlocutors, whether it be on the Copernican system or the question of the validity of the law of fall, he had a very different attitude to the question of the applicability of his theory to practical gunnery. He showed engagement with the problem of the divergence between predicted and experimental behavior rather than discomfiture. He urged Renieri to write as soon as possible with the results of further experiments, and continued to make suggestions as to measures that might enable Renieri to check that the gun was accurately positioned and that the ground was completely level. He showed familiarity with the many factors that might lead to the anomalous results, and what measures might be taken to eliminate the impediments in order to achieve a closer correspondence with his predictions.

As a contingency Torricelli was able to retreat to the safe haven of mathematical certainty, but his letters show that he wanted to investigate how the difficulties of gunnery practice could be overcome; he had no doubt that the theory was true in the material world. Whatever protestations Torricelli might make, for whatever reasons, his tell-tale comments, that nature does not make blunders, that Galileo was an oracle of nature, reveal his conviction that nature always works in conformity with itself. The belief in the underlying unity of nature was a threat because it allowed the spectre of Copernicanism to insinuate itself into every aspect of enquiry into matter and motion. This chapter has shown how the two theories of motion in the heavens and motion on earth became intertwined. Torricelli was careful to avoid any confrontation by keeping silent about his Copernicanism, but in the next chapter I will show how the Accademia del Cimento was able to use the theory of the motion of a projectile to launch a rearguard action against the ban on Copernican ideas.

Chapter five

'Causes not experiments': the internal life and external face of the Accademia del Cimento

> Salviati: … The knowledge of a single fact through a discovery of its causes prepares the mind to understand and ascertain other facts without need of recourse to experiment …[1]

Introduction

For a short period (1657-1667), the Cimento Academy based at the court of Galileo's patrons the Grand Duke Ferdinand II and Prince Leopoldo in Florence, was a pioneer of the experimental method in science.[2] Its only publication, the *Saggi di naturale esperienze* (1667), was also noted and admired as a model of Tuscan literary style and for the clarity of its prose. It provided a means of restoring Italian pride in its scientific and cultural achievements after the condemnation of Galileo, as well as of increasing the personal prestige of Prince Leopold as a patron and philosopher in his own right.[3]

My interest in the Accademia del Cimento relates to a series of experiments on guns that the Academy carried out in 1662, the historical significance of which has

[1] Galileo Galilei, *Two New Sciences,* trans. Henry Crew and Alfonso de Salvo, 265. Galileo, *Opere* VIII, 296.

[2] Its motto, *provando et reprovando* (try and retry) is taken from Dante's *Divine Comedy (Paradiso, Canto III): Quel sol che pria d'amor mi scaldò I petto, di bella verità m'avea scoverto, provando e riprovando, il dolce aspetto,*(http://www.divinecomedy.org/divine_comedy.html, accessed 22.07.14). The academicians made a point of repeating experiments, repeating other's experiments and making their results available for others to test. Cimento means ordeal, but it can also suggest risk or danger, echoing Bacon's metaphor of experiment as torture of nature to reveal her secrets.

[3] The first substantive modern account of the Cimento was *The Experimenters (*Baltimore: Johns Hopkins Press, 1971), by W.E. Knowles Middleton. Middleton's research highlighted the anti-Aristotelian thrust of the work of the Academy itself and the experiments described in the *Saggi.* After a lull of about thirty years, there has been a marked increase in interest in the Cimento and research into its rich source of archival material. In particular see: Luciano Boschiero, *Experiment and Natural Philosophy in Seventeenth Century Tuscany: The History of the Accademia del Cimento* (Dordrecht: Springer, 2007); Marco Beretta, 'At the Source of Western Science: The Organization of Experimentalism at the Accademia del Cimento (1657-1667) *Notes and Records of the Royal Society of London,* 54 (2000), 131-151, and Marco Beretta, Antonio Clericuzio, and Lawrence M. Principe, (editors) *The Accademia del Cimento and its European Context* (Sagamore Beach, Mass: Science History Publications, 2009); Dominico Bertoloni Meli, 'Shadows and Deception: From Borelli's "Theoricae" to the "Saggi" of the Cimento' *British Journal of the History of Science,* 31 (1998), 383-402.

hitherto not been appreciated.[4] Their importance lies in their ability to widen our understanding of the scope and programmatic coherence of the *Saggi* as a whole. There has been some historiographical dispute as to the nature and purpose of the Cimento Academy and further examination of the gun experiments will help to gain an insight into some of these debates.

The *Saggi* gun experiments represent an example of the use of the cannon as an instrument of philosophical enquiry that goes beyond the usual 'gunners questions' of the relationship between the angle of elevation and the shape and range of the trajectory. I will show how they provide support for an understanding of the *Saggi* as a means for the promotion of Galilean science and as a challenge to Aristotelian conceptions of motion. In particular, I will show how the fourth gun experiment in particular enabled the academicians to subtly counter the arguments against Copernicanism put forward by Giovanni Battista Riccioli in his *Almagestum novum* (1651), whilst avoiding the danger of openly advocating Copernican ideas regarding the motion of the earth. Finally, I will show how the publication of the *Saggi* experiments supported the gently progressive but irenic political and social programme of the Medicis.

Before I move on to examine the experiments, it will be helpful to provide some historical background on the academy and discuss some of the issues that have been raised by other historians. Luciano Boschiero has examined the philosophical background and scientific interests of the members of the Cimento, and their independent researches, which he characterizes as physico-mathematical.[5] In contrast he claims that traditional histories have categorized the Academy as being dedicated

[4] Domenico Bertoloni Meli, *Thinking with Objects,* 203, discusses the experiments briefly in relation to the increasing practical and theoretical interest in the investigation of projectiles and air resistance from the mid-seventeenth century, commenting that the results 'allegedly' supported Galileo's predictions.
[5] Boschiero, *Experiment and Natural Philosophy*, 29, uses this in the sense that Descartes and other contemporary scientists used the term *physico-mathematicus* for someone who applies mathematics to physical problems.

to 'atheoretical' experimentation, comparing it to similar categorisations of the Royal Society.

My own preference is to categorise the Cimento as Galilean; it was an homage to and vindication of the Galilean scientific method. Edgar Zilzel considered that Galileo's Paduan workshop was the first 'university' laboratory in history and I suggest therefore we should see the Cimento as a continuation and development of Galileo's legacy.[6] This is not to suggest that Galileo developed his method in a vacuum. To take just one example, he was well aware of the pioneering experimental work on the magnet of William Gilbert's *De Magnete* (1600), which he praises in the *Two World Systems:*

> Salviati: I have the highest praise, admiration, and envy for this author, who framed such a stupendous concept regarding an object which innumerable men of splendid intellect had handled without paying any attention to it. He seems to me worthy of great acclaim also for the many new and sound observations which he made, to the shame of the many foolish and mendacious authors who write not just what they know, but also all the vulgar foolishness they hear, without trying to verify it by experiment; perhaps they do this in order not to diminish the size of their books. What I might have wished for in Gilbert would be a little more of the mathematician, and especially a thorough grounding in geometry, a discipline which would have rendered him less rash about accepting as rigorous proofs those reasons which he puts forward as *verae causae* for the correct conclusions he himself had observed. His reasons, candidly speaking, are not rigorous, and lack that force which must unquestionably be present in those adduced as necessary and eternal scientific conclusions.[7]

As Paolo Rossi has observed:

> It is in Galileo that we find for the first time the full convergence of the two traditions: the one based on the experimentation and practices of artisans and technicians, and the other based on the great corpus of theory and methodology of European science. The entrenchment of the theory of mechanical practice, as had already been noted by Leonard Olschki, and its transformation into science was the work of Galileo: empirical mechanics and the science of motion were fused into a solid whole of theoretical knowledge.[8]

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[6] Edgar Zilzel, 'The Sociological Roots of Science', *Science* 30 (2000), 941.
[7] Galileo Galilei, *Dialogue Concerning the Two Chief World Systems,* trans. Stillman Drake, (Berkeley and Los Angeles: University of California Press,1974), 406. *Dialogo sopra I due massimi sistemi del mondo* in *Opere di Galileo Galilee* VII, 432.
[8] Rossi, *Philosophy, Technology and the Arts,* 112.

There were many reasons why experimental methods took centre stage in this period. Not least is that Galileo had opened up discussion on many areas of enquiry that were eminently suited to experimental enquiry (e.g. heat and cold, the void, air pressure, magnetism). What was new with the publication of the *Saggi,* compared to Galileo's works, was the detailed recording and witnessing of experiments described 'historically, rather than in the form of an argument'.[9] The *Saggi*, the external face of the Cimento, focused solely on the experimental part of the scientific method of the Academicians, but there was much more behind the experimental facade.

The Academy and its experiments

Though it never had any official status or membership, the Academy comprised twelve known members including Prince Leopold, the Grand Duke Ferdinand and two successive secretaries, first Alessandro Segni followed by the young Count Lorenzo Magalotti, the author of the *Saggi*. Many of the members, because of other commitments, could only play a limited active role in the experimental work of the academy. By far the most active members were Vincenzo Viviani, Galileo's assistant in the last years of his life, and Giovanni Alfonso Borelli. Viviani's early education and collaboration with Galileo was centred strongly on respect for ancient mathematicians with the intention of firmly establishing Galileo's terrestrial mechanics.[10] Viviani was also involved in the collection of Galileo's correspondence and works, and writing his biography. In addition to pursuing his own researches,

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[9] Borelli commenting on Magalotti's first draft of the Saggi, quoted in Middleton, *The Experimenters,* 68. Borelli may have been influenced by the presentation of Boyles' *New Experiments*, published in 1662.

[10] See Boschiero *Experiment and Natural Philosophy,* 40-44, for an account of the collaboration of Viviani and Galileo in using Eudoxian proportion theory (Euclid's Elements Book V definition 5) to find a more conclusive demonstration that the ratio of the time of descent along an incline to that of the vertical is the same as the ratio of the incline to the vertical, and therefore proving equal degrees of speed along inclines with the same vertical height. This was added by Viviani as a *Scholium* to later editions of the *Two New Sciences.*

including a reconstruction of book V of Apollonius' *Conics*, he was also often absent for long periods on duty as Leopold's chief engineer. Borelli was the most talented mathematician and scientist left after the death of Torricelli and like Torricelli was a former pupil of Benedetto Castelli.

Borelli crossed all boundaries, doing research into mathematics, astronomy, mechanics, medicine and chemistry, and he inspired the new science of biomechanics with his posthumously published treatise on animal motion.[11] What unites his approach to all these apparently diverse interests was his mechanistic, atomist philosophy and his Copernicanism. Stefania Montacutelli notes that in *Il Saggiatore* (*The Assayer*, 1623) Galileo contrasted mathematically and geometrically representable matter (shape, size and relative motion), necessary for any analysis aimed at defining the physical properties of a body, with the secondary qualities such as colour, taste and smell. Borelli developed his own mechanical theories through the integration of the Galilean theory of matter with the contributions of Torricelli, Gassendi and Descartes.[12] However, he disagreed with Descartes' plenism and insisted on the importance of experiment to support his mechanical views.

The interrogative character of the experiments can be illustrated by those designed to counter the arguments that were made by the Aristotelian members against Torricelli's barometer experiment. Considerable ingenuity was used to come up with elaborate explanations for the phenomena observed without acceptance of the existence of a void. This challenged the Galileans to devise ever more ingenious

[11] Antonio Clericuzio 'The Other Side of the Accademia del Cimento: Borelli's Chemical Investigations' in *The Accademia del Cimento and its European Context,* edited by Marco Beretta, Antonio Clericuzio, Lawrence M. Principe (Sagamore Beach, Mass: Science History Publications, 2009), 17-30, on 17.
[12] Stefania Montacutelli 'Air "Particulae" and Mechanical Motions: From the Experiments of the Cimento Academy to Borelli's Hypotheses on the Nature of Air', in Beretta, Clericuzio, Principe (eds.), *The Accademia del Cimento,* 59-72, on 60.

experiments to eliminate the possibilites suggested by their opponents.[13] The Aristotelians believed in the interconversion between water and air and thus were able to complain that in the barometer experiments the void actually contained water vapours, or in the case of mercury, vapours from the mercury. This led to tensions between the experimenters. For example, on April 24 1659, Magalotti complained to Leopold:

> … it is the most universal subterfuge of all those who deny the vacuum, to have recourse to these exhalations of the mercury, violently extracted from its bulk in some way, to fill the space left open by its fall.

Middleton notes the Academicians had good reason to work to counter the challenges of the Aristotelians since opposition to the vacuum continued in ecclesiastical circles to the end of the century, not to mention opposition by the followers of Descartes.[14]

In *Il Saggiatore* Galileo made clear the crucial difference between the approach of the adherents of the Aristotelian philosophy and that of the new science. Here Galileo argued that you cannot be objective if you have a pre-existing philosophical axe to grind. This is where Galilean and Baconian science overlapped; Bacon similarly emphasized the necessity of eradicating 'idols', likening the sense perceptions and the mind to an 'uneven mirror which mingles its own nature with the nature of things, and distorts and stains it'.[15]

Marco Beretta discounts the detrimental impact of the Inquisition on the work of the Cimento because he disagrees with the suggestion that the Cimento compromised between the need to examine nature following an itinerary free of previously established dogma, and the prohibitions of the Church on teaching or

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[13] Middleton, *The Experimenters,* 265-269. See also Redondi, *Galileo, Heretic*, chapter nine that catalogues the continuing vigilance of the Jesuit 'theological police' in undermining and threatening those who they suspected of having atomist or Copernican views. Redondi, on 295, suggests that it was Galileo's nemesis Father Orazio Grassi (Sarsi), who, after being 'exiled' to his native Savona by Urban VIII after the Galileo trial, immediately returned to Genoa after Urban's death in 1645, and provided behind-the-scenes direction to the Jesuit polemic against the vacuum.

[14] Middleton, *The Experimenters,* 399.

[15] Bacon, *Novum Organum,* Aphorism 41, *The Oxford Francis Bacon, XI,* 81.

probing philosophical and scientific questions. Beretta downplays the effect of censorship because he argues, rightly, that the void was more contentious than Copernicanism, and this occupied much of the experimental work of the Academy. But one can recognize the influence of the Inquisition without implying that it was necessarily detrimental to the nature and quality of the Cimento's work. Because of his position, Leopoldo was able to create a sort of utopian island of free enquiry within the Cimento but this meant that great care had to be taken in the public presentation of its work.

Beretta also disagrees with those who consider that Bacon influenced the empirical choices of the Academicians. He asserts that it is 'clearly documented that most of the Academicians had not even read the works of the Lord Chancellor'.[16] In this he follows Middleton. I do not think we should so readily discount the existence of a Baconian spirit underlying the work of the Academy. An Italian translation of Bacon's moral *Essays* and *Wisdom of the Ancients* dedicated to Cosimo II de Medici by Bacon's friend Tobie Matthews was published in 1617 and 1618. As a Catholic convert Matthews had found it propitious to spend a number of years in Europe, where it appears he moved in Tuscan court circles. In the preface Matthews expresses the hope that *Del Progresso delle Scienze* will also soon be translated, and refers to Bacon's admiration for Cosimo's father the Grand Duke Ferdinand and Cosimo I.

In addition, Mordechai Feingold has suggested a number of links between English and Italian mathematicians and natural philosophers. Isaac Barrow, an admirer of Bacon, Gassendi and Descartes, as a Royalist, like Matthews also decided it might be advisable to undertake a Continental tour in 1655. While in Florence he became particularly friendly with Carlo Rinaldini, Viviani and Borelli. Feingold

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[16] Beretta, 'At the Source of Western Science', 140.

questions whether it is coincidence that Rinaldini, who played an important role in the foundation of the Academy, prepared a broad-based experimental program for Leopoldo based in part on an extensive survey of both scholastic and modern works of natural philosophy which bore a strong resemblance to that of the Oxford Philosophical Club of which Barrow had been a member in the early part of the decade.[17]

There were conflicting views within the Academy as to the extent to which they should be communicating with or collaborating with other groups of experimenters in Europe. An equivocal attitude was expressed in a letter from Borelli to Prince Leopold that was written quite early in the life of the Academy, in November 1658. It concerned Borelli's fears regarding a proposed correspondence with Montmor's Paris academy, which was a prototype of the French Academie des Sciences. Borelli, whilst he was very pleased to hear of the French interest in experiments and speculations, nevertheless entertained doubts and suspicions that

> [T]he foreigners will make themselves the authors and discoverers of the inventions and speculations of our masters, and of those that we ourselves have found. This fear makes me go slowly in beginning this correspondence with those gentlemen of the Parisian academy, since in writing, one cannot do less than communicate something or other, and I fear that this may give those foreign minds an opportunity to rediscover the things; I am speaking of the causes, not the experiments.[18]

Not only does this letter testify to the natural philosophical interests of the academy, but Borelli seems to have been concerned that even simple descriptions of experiments could give something away because of the difficulty in describing an experiment without exposing the theory behind it. For Borelli, at least, there was no

[17] Mordechai Feingold 'The Accademia del Cimento and the Royal Society' in Beretta, Clericuzio, Principe, eds., *The Accademia del Cimento,* 229-242, on 235.
[18] BNCF. Ms. Gal. 275, ff.126v, translated by Middleton, *The Experimenters,* 300. The original Italian is also given in Boschiero, *Experiment and Natural Philosophy,* 238.

such thing as 'atheoretical' experimentation; the design of his experiments emerged directly from his mechanist-corpuscularian philosophy.

In Magalotti's preface he discusses the role of experiments in natural enquiry. Invoking the legend of Icarus, Magalotti describes how man:

> in his immoderate desire to understand the marvelous power of it [God's universe] and to take the measure and proportions of such a beautiful harmony, then, wishing to enter too completely into the truth, he came to create an indefinite number of falsehoods.[19]

He explains that the seeds of false opinions arose from human rashness, and continues:

> …man, improperly fitting causes to effects, takes their true essence neither from one nor the other, but putting them together forms a false science within his own mind.[20]

This sounds very Baconian, but he then continues:

> Here it must be confessed that nothing is better for this than geometry**.** Which at once opens the way to truth and frees us in a moment from every other more uncertain and fatiguing investigation. The fact is that geometry leads us a little way along the road of philosophical speculation, but then abandons us when we least expect it. This is not because it does not cover infinite spaces and traverse all the universal works of nature in the sense that they all obey the mathematical laws by which the eternal Understanding governs and directs them; but because we ourselves have not yet taken more than a few strides on this long and spacious road. Now here, we are no longer permitted to step forward, there is nothing better to turn to than our faith in experiment…There is no doubt that to be able to do better we must at some time to have seen the Truth unveiled, an advantage possessed only by those who have acquired some taste for the study of geometry.
>
> Yet besides trying new experiments, it is not less useful to search among those already made, in case any might be found that might in any way have counterfeited the pure face of Truth.[21]

It is perhaps surprising that a publication dedicated to the experimental method has a preface containing a eulogy to the power of geometry for revealing truths in nature. Yet the divergence from conscious or unconscious Baconianism here is perhaps more

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[19] Middleton, *The Experimenters,* 89.
[20] *ibid.,* 90.
[21] *ibid.,* 90.

apparent than real; Magalotti is saying that experiment serves as a necessary heuristic that not only keeps the enquiry on track but also compensates for the fact that our mathematical tools are as yet relatively primitive and so are not able to penetrate into the deep complexities of matter, even though he is confident that the whole of nature is governed by mathematical laws. Experiment could also provide important confirmatory evidence and could act as a tool of persuasion, especially for those who did not possess the skills required to follow the mathematical demonstrations. Galileo used experiment for this purpose in his publications, and so did the Cimento. Both mathematics and experiment can therefore complement each other, and are both essential to the process of natural enquiry. As his comments on Gilbert show, Galileo also could not conceive of a one-sided approach to natural enquiry that did not make use of the power of mathematics, and neither could the Cimento Academicians.

Another aspect of the way in which the *Saggi* only represented one face of the Academy is that on the whole it dealt with experiments abstracted from any utilitarian purpose. Beretta seems to be the only historian who has given any attention to the possible utilitarian motivations behind the enquiries of the academy. He gives as examples the reclamation of the marshes in the Val di Chiana, the control of river water, and the interest manifested by Leopoldo and Ferdinando in the construction and improvement of instruments to be used in navigation.

The form that the *Saggi* took did not lend itself to the highlighting of the utilitarian purposes of the enterprise. However, Viviani's employment as Leopoldo's chief engineer suggests there might have been more integration between experiment, theory and practice in the everyday life of the experimenters than is suggested by the *Saggi.* Middleton provides an illuminating anecdote that suggests that this might be the case. He describes how in 1665 Viviani had made experiments with a four-pounder gun (*saltamartino*). Viviani had produced a charge-table (*tariffa*) for the Grand Duke relating the range of shot to the charge of powder. The Grand Duke had passed it on to Borelli who made a snap judgment that it was incorrect. On being shown by other experts to be in the wrong, Borelli, rather than admitting his error, proceeded to deny that he had ever seen the table, thus contradicting the word of the Grand Duke. An incandescent Viviani gave vent to his outrage in a letter, probably to the courtier Bruto Molara, that Borelli should

> take liberty with a Grand Duke who is as acute as Campanella or Pico de la Mirandola and not inferior to them in memory' by feigning ignorance of the tariffs. He continues that '… on one occasion His Highness, perhaps out of compassion and to point out my excessive stupidity, said these exact words about this man: he is quick, he knows more about things than any of us; he is a Sicilian and I know that he likes to play the tyrant.[22]

In addition to the evidence this anecdote provides of the utilitarian practices of the academicians in everyday court life, the reference to Mirandola and Campanella provide evidence for both Viviani's humanist philosophical worldview and the countercultural philosophical environment that the court provided. It also provides insight into the Grand Duke's astute assessment of the talents and character of his academicians and the difficult task that both he and Leopoldo must have had in maintaining a peaceful and productive environment for collaborative work and holding the Academy together.

Leopoldo and Ferdinand had a genuine interest in and talent for technical and scientific innovation and given their increasingly unpropitious economic and political fortunes they also knew that one way to retrieve the situation was through turning superior scientific and technical knowledge to their advantage.

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[22] *ibid*., 315-6. Borelli was actually from Naples, but he was secretive about his past. His father had been imprisoned with Campanella and Borelli probably had lessons from Campanella while he was in prison. So he might well have chosen to give the impression he was Sicilian.

The restricted content and delays in publication of the *Saggi* unfortunately attenuated its eventual impact. Most of the experimental work that was done took place in the first five years and the second half of the period of its existence was taken up with the long process of drafting and commentating on drafts, redrafting, and guiding through the process of approval by the Inquisition.

By the time the *Saggi* reached the Royal Society the response was rather subdued, perhaps because by this time the selection of experiments did not stand out as particularly exceptional in comparison with those already carried out by members of the Society. Nevertheless its influence is shown by the fact that in 1731 the Dutch Petrus van Musschenbroek (1692-1761) edited a Latin edition (*Tentamina experimentorum naturalium captorum in Accademia del Cimento*…) published in Leiden in which van Musschenbroek illustrated the experimental developments and theories following the publication of the *Saggi*. Finally, from Musschenbroek's Latin edition, an edition in French was published in 1755. Middleton's conservative assessment of the influence of the *Saggi* is modified by Alfonso Mirto, who concludes that the fact that 'in almost three centuries the *Saggi* had nine or ten editions and reprints (depending on how one counts) in Italian, French, English and Latin, shows that the text… not only had a great literary importance but also a scientific value.[23]

The fact that the *Saggi* was written in Italian might have also limited its impact, but it was translated for the Royal Society as *Essayes of Natural Experiments* by Richard Waller FRS in 1684 and dedicated to Sir John Hoskyns, President of the Royal Society. None of the considerable and significant work on astronomy that the Academy conducted was published. As I have noted, anything printed on astronomy is likely to have attracted unwarranted attention. Leopoldo's discomfiture at having to

[23] Alfonso Mirto, 'Genesis of the *Saggi* and its Publishing Success in the Seventeenth through Nineteenth Centuries', in Beretta, Clericuzio and Principe, (eds.), *The Accademia del Cimento,* 135-150, on 149.

make any public comment relating to astronomy is illustrated by the case of the debate over Christiaan Huygens' new theory of the existence of rings around Saturn, in which Leopoldo was requested to adjudicate against the Aristotelian hypothesis of Fabri who put forward a complex scenario involving positing the existence of four stars that orbited not Saturn but two points behind it to create the observed phenomena.[24] This theory was not strongly geocentric but was designed to put forward a hypothesis that opposed Huygens' Copernican-based model.

Since observing Saturn's phases would involve years of telescopic observations, the academicians devised an ingenious experiment, which might under different circumstances have taken pride of place in the *Saggi*. They built two alternative models of Saturn, representing the alternative hypotheses of Huygens and Fabri, and set them up with four hidden torches for illumination. The models were then observed through telescopes to see which one resulted in the phenomena observed by astronomical observation. To ensure objectivity impartial observers who had no knowledge of the models or their purpose were brought in to describe what they saw. The result overwhelmingly supported Huygens' hypothesis. But rather than make the result public Leopoldo wrote private, carefully-worded separate letters to the adversaries that ensured that Huygens knew of the confirmation whilst pacifying Fabri and ensuring that he did not pursue the matter further.[25]

Actually Giorgio Strano makes a case for doubting that Borelli ever built the Fabri model. He was unable to find any record or illustration of the model in the documents of the Academy. The Fabri model would have been extremely complex to construct and having confirmed what he knew to be correct anyway by reproducing

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[24] In 1610 Galileo had been the first to discover that Saturn appeared to consist of a central sphere plus two smaller ones adjacent on either side. See for example R. S. Westman 'Science and Patronage: Galileo and the Telescope' *Isis* 76 (1985), 11-30, on 23.

[25] A detailed account is provided by Boschiero in *Experiment and Natural Philosophy,* 199-216.

the desired effect from the Huygens' model, it is in keeping with Borelli's character and views on Fabri, that he might have decided that he had wasted enough of his precious time on the fantasies of Aristotelians.[26]

Most of the contents of the *Saggi* is taken up with experiments on air pressure, the creation of a vacuum, the freezing process of liquids and properties and effects of heat and cold. The results challenged Aristotelianism, but the Church, though not happy with the mechanical and corpuscularian views that the experimental results implicitly supported, would not have been able to find fault with them both because of the way that they were presented, and the fact that, unlike with the case of Copernicanism, it had not made any official adjudication on these questions. Nevertheless, they had to be handled with great care and this is shown by the length of time spent over drafting, checking and commenting by other academicians (including Viviani, Borelli, Rinaldini) and advisers on its literary style such as Michaelangelo Ricci. The manuscripts provide evidence of the tortuous process that the secretary of the Academy, Magalotti, despite his considerable literary talent, had to go through in order to achieve the appropriate wording to introduce and explain the experiments.

Having discussed some of the factors that the academicians had to take into consideration in the public presentation of their work, we can now examine how the gun experiments fitted in with their objectives.

The *Saggi* experiments on guns

The inclusion of the experiments on guns appears to be somewhat anomalous since they do not fit in with the overwhelming preoccupation with matters relating to air

[26] Giorgio Strano, 'Saturn's Handles: Observations, Explanations and Censorship from Galileo to the Accademia del Cimento' in Beretta, Clericuzio, Principe, eds., 73-90, on 89-90. The dispute was complicated in that it also involved rivalry between the two leading telescope makers Giuseppe Campani and Eustachio Divini.

pressure, the vacuum, heat and cold and magnetism. And yet we must not forget that motion in a void was crucial to Galileo's theory of motion and the trajectory, providing a unity and coherence to the ostensibly diverse experiments. This coherence has been missed by other researchers who have not recognized the gun experiments as integral to the Saggi's programmatic agenda. Nor have they recognized their significance in the context of the fact that the Academy found it prudent to avoid any thing to do with astronomy, or with any whiff of Copernicanism. As Boschiero notes, while the Cimento could get away with publishing on air pressure, the vacuum and the effects of heat and cold with clear natural philosophical underpinnings, astronomy was a different story.[27] Just as the Jesuits used their challenge to Galileo's law of fall as a proxy for their attack on Copernicanism, it seems possible that the Academy used the experiments on guns as a proxy for Copernicanism, but this time in its defence.

The gun experiments confirm Galileo's ungeneralised concept of inertia, the relativity of motion, and the independent action of the two separate motions that make up the trajectory. In doing so, whilst they did not prove the Copernican hypothesis, they destroyed the main argument of the Aristotelians against the motion of the earth.

The experiments on projectiles are slipped in towards the end of the *Saggi*, the last ones that form a themed series, followed by a description of a small number of miscellaneous experiments. They are in keeping with the aim of the other experiments in the *Saggi* in that they are used as tools of persuasion of Galilean theoretical positions through the means of empirical evidence.

[27] Boschiero, *Experiment and Natural Philosophy,* 215.

The experiments were carried out in the spectacular setting of the Fortezza Vecchia of the port of Livorno (Leghorn), about 25 kilometres south of Pisa.[28] According to Middleton, who provides an annotated translation of the *Saggi,* the 'experiments concerning projectiles' took place on April 2 1662 but the first experiment at least had also been performed by Carlo Rinaldini[29] at Livorno in January 1658.

Figure 7. Fortezza Vecchia, Livorno.

The position of the fortress provided an ideal setting for the first two experiments; Galileo had actually suggested that the experiment be carried out over a lake to ensure a flat surface for the shots.

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[28] The draining of the marshlands and the building of the port of Livorno (Leghorn) was carried out under first Cosimo I and then Ferdinand and Leopoldo. According to J. R. Hale, *Florence and the Medici: the Pattern of Control* (London: Thames and Hudson, 1972), 162, Livorno 'was developed from a handy anchorage and a few hovels into a busy metropolis.' Money and resources were lavished on it, and it seems likely that Viviani, as chief engineer, would have been heavily involved with this exceptional operation.

[29] One of the only two Academicians who were adherents to the Aristotelian philosophy, he appears to have been an able mathematician and engineer and taught the condemned works of Galileo and Gassendi's atomism at the University of Pisa, presumably hypothetically. Boschiero, *Experiment and Natural Philosophy,* 95.

The description of the experiment begins with a paraphrase from the second dialogue of Galileo's *Dialogue on the two chief world systems* in which he asserts that the time taken to reach the ground of a gun shot horizontally for any distance (charge) will be exactly the same (allowing of course for air resistance) as the time taken for the same ball if dropped perpendicularly from the same height.[30] The experiment was designed to show the independence of the vertical and horizontal components of motion. Galileo first described the isochronism of projectile motion in a letter to Antonio de'Medici on the 11 February 1609. Meli notes that in related manuscripts showing parabolic trajectories and a comparison of experimental and theoretical values of where the falling ball hits, and should hit, the ground, Galileo marked this difference with the word *doveria* (it must be).[31]

It is perhaps worth emphasizing the conceptual novelty that led Galileo to the idea for this experiment and its counterintuitive quality. Galileo conveys this wonder in the dialogue between Sagredo and Salviati:

> Sagredo: …Now it seems a marvelous thing that in the same short time of a straight fall from a height of, say, a hundred yards to the ground, the same ball driven by powder could go now four hundred, now a thousand, again four thousand, or even ten thousand yards, so that all shots fired point-blank would stay in the air for an equal time.
>
> Salviati: This reflection is very beautiful by reason of its novelty, and if the effect is true it is most remarkable. And I have no doubt as to its correctness. Barring the accidental impediment from the air, I consider it certain that if, when one ball left the cannon, another one were allowed to fall straight down from the same height , they would both arrive on the ground at the same instant, even though the former would have travelled ten thousand yards and the latter a mere hundred. Of course we are assuming the earth to be perfectly level; to guarantee this, the shots might be made over some lake. The impediment of the air would then be one of retarding the very great speed of the shot.[32]

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[30] Galileo, *Two World Systems,* trans. Drake, 155. *Opere* VII, 181.
[31] Bertoloni Meli, *Thinking with Objects,* 326.
[32] Galileo, *Two World Systems,* trans. Drake, 155.

The height of the fortress was 50 *braccia* and a falconet was loaded with 7⅓ *libbre* of iron and four *libbre* of fine powder.[33] The shots were made at point blank out to sea with a bandaged ball which was seen to hit the water at a distance of two thirds of a (Florentine) mile in 4½ vibrations of the pendulum. The academicians devised their own bifilar pendulum with adjustable lengths for more accurate and convenient measurement of time in their experiments.[34] The ball dropped in the perpendicular direction from the same height landed after four vibrations. In a second experiment a bandaged ball reached the water in five vibrations while an unbandaged (naked) ball took longer, five and a half vibrations.[35] This result was considered to confirm Galileo's theory, allowing for air resistance.

The third experiment was designed to investigate Galileo's hypothesis regarding the greater significance of the effect of air resistance when we are dealing with the 'supernatural' speeds of cannon and musket fire.[36] In the *Discorsi* he had predicted that if a lead shot was fired vertically directly onto a stone pavement from a height of one hundred or more cubits, and another shot made in the same way but at a height of a few cubits, the lead shot from the greater height would be the less dented. This is because the effect of the air resistance on the 'supernatural' speed of the bullet shot from a height was greater than any increase in speed caused by the greater distance of fall.

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[33] I prefer to leave units in the original. Middleton, *The Experimenteers*, xiii, has provided details of Florentine weights and measures and their modern equivalents 1 braccio (ell) =20 soldi = 58.36 cm. 1 *libbra*= 12 *once* = 339.5 grammes. A cubit is about 45 cm.

[34] An excellent online didactic on the Cimento bifilar pendulum may be found on the website of the *Museo Galileo* on http://brunelleschi.imss.fi.it/cimento/etop1.html?center=esperienze/varie/ependolo2.html. Accessed 4.9.2014.

[35] Middleton, *The Experimenters,* 241, notes that Baliani did the same experiment before 1646 and published in his *De motu*. No differences in time (of fall) had been recorded between the canon shot and simply dropping the cannon perpendicularly.

[36] *ibid.,* 242, Middleton notes 'supernatural' means a greater speed than the natural speed of fall can attain.

The academicians tried this experiment using a rifled arquebus against an iron breastplate and found that the denting was much less in the breastplate hit from a greater height, confirming Galileo's hypothesis.[37]

This experiment and experiment number five might have been included because Galileo's discovery of the trajectory had led to an increased scientific interest in the study of the effect of air resistance as the next step to making his theory of the trajectory more applicable to artillery practice. But it may also have been designed to remind critics of Galileo who had not read the two new sciences properly how Galileo, far from ignoring the effect of air resistance, was in the vanguard not only in understanding air resistance but in attempting to measure its effects.[38]

In the fourth experiment the introduction states that it was devised to confirm Galileo's statement in several places (i.e. in the *Discorsi* and the *Dialogo*), that the power impressed on projectiles is not destroyed by a new direction of motion. This is a key experiment for refuting the arguments of the anti-Copernicans and is extremely powerful in its counterintuitive implications. The experiment involved setting a small cannon onto a wagon pulled by six horses. The cannon was pointed perpendicularly upwards. Shots were fired (with equal amounts of powder) both with the wagon stationary and at full gallop over a plane. With the wagon stationary the ball fell back to the mouth of the gun. With the moving wagon, the wagon travelled 74 ells from the beginning of the shot, but the shot came down only four ells behind the gun. In addition the times of the falls in both cases were equal.

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[37] Experiments made December 29, 1661 and 5 January, 1662. See Middleton, *The Experimenters,* 242.
[38] Hall *Ballistics*, 107, referring to Mersenne's *Ballistica* notes that Mersenne, 'wrote more accounts of things done with his own hands and seen with his own eyes than anyone before the great age of the scientific societies'. He carried out many ingenious experiments to measure the effect air resistance, including trials with a crossbow where the time of ascent of a projected body was always found to be less than the time of descent, the difference increasing with the velocity of projection.

This experiment not only provided an additional proof of the equal time of fall regardless of horizontal distance travelled, it also confirmed Galileo's statements relating to inertia and the relativity of motion.

The fifth experiment was the same as the fourth but the gun was replaced by a large crossbow. The experiment was made firstly with lead balls and then with clay balls. In the case of the lead balls they fell behind only six ells in a course of 78 ells. The clay balls fell behind seventeen and a half ells over a distance of 100 ells, illustrating the different effect of air resistance for the lead and the clay balls. The academicians once more noted that the experiment confirmed the opinion of Galileo both of the effect of air resistance, and of its greater effect on (the impetus of) lighter bodies.

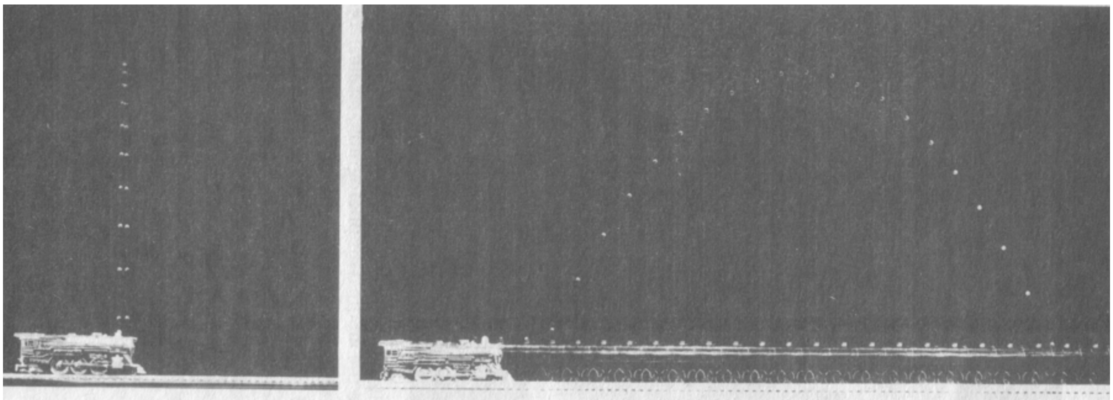

Figure 8. Photograph from I. B. Cohen's *The Birth of the New Physics* (1992), 114, first published in 1961. Using stroboscopic photography and a toy train rather than a wagon and horses, it shows a result similar to the fourth gun experiment, with the ball landing only slightly behind the moving train.

The anonymous experimental discourse of the *Saggi* allowed the Academy to confirm key aspects of Galilean science to its readers and to show that Italian science had not been extinguished by his condemnation, but the experimental orientation was not solely determined by this purpose but also represented an organic development inspired by the work and writings of Bacon, Galileo and others. However, I will suggest in the next section that there was another more particular motivation for publishing the gun experiments.

A context for the gun experiments: Riccioli's *Almagestum Novum* (1651)

Like the experiment of dropping a stone from the mast of a moving ship, the fourth experiment countered the sole, apparently common-sense argument against the motion of the earth, such as that birds would be forced to fly backwards if they tried to fly against the motion of the earth. This was one of a number of arguments put forward by Giovanni Battista Riccioli (1598-1671) in his *Almagestum Novum* (1651).

The iconic frontispiece of the *Almagestum Novum* depicts Astraea, the goddess of Justice balancing the two rival systems, that of Copernicus's heliocentric system and Tycho Brahe's geo-heliocentric system that was adopted by most Jesuit astronomers when the Ptolemaic system became effectively impossible to defend. In an attempt to provide a veneer of scientific rigour and objectivity Riccioli did present forty-nine arguments in favour of Copernicanism but seventy-seven in favour of Tycho's system.

Figure 9. Frontispiece, Riccioli's *Almagestum Novum*

The frontispiece itself was triumphalist and highly provocative towards the disciples of Galileo and caused them dismay that this would be seen as representative of the state of Italian science. Of course, they were not able to respond, being prohibited by the church from attempting to argue in favour of Copernicanism. Torricelli gave vent to his outrage in a letter to Vincenzo Renieri:

> What an impudent set! They want to make complete fools of us in every area of knowledge! You just read the frontispiece here enclosed and then forget about astronomy.[39]

The *Almagestum Novum* was an important work of public relations for the Catholic Church in its endeavor to maintain its scientific credibility and authority, and it was a perfect vehicle for teaching new generations of the elite. As I note below, the young Robert Hooke was recommended to read it by his tutor at Oxford for the quality of the experiments described in it. Riccioli was a highly esteemed astronomer and relentlessly meticulous experimenter who, as Bertoloni Meli notes, along with Mersenne stands out as marking an important change in the way that experimental data were reported in print.[40]

Riccioli rightly pointed out that it was impossible to actually prove the Copernican hypothesis because of the inability of astronomers to detect stellar parallax, which, as Robert Hooke noted in his work on parallax, was the only argument of the seventy-seven provided by Riccioli to disprove the Copernican hypothesis that the 'Inquisitive Jesuit' needed to have bothered with.[41] Huygens replied positively to Hooke's attempt to prove parallax, '[T]o which we also shall contribute our labours; and the Vault, that is our observatory, being 28 Fathom deep, will in time be very useful for that Purpose. This, if it succeed, will prove an almost entire Conviction of the *Anti-Copernicans,* since there will remain for them nothing

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[39] Torricelli's response in a letter to Vincenzo Renieri of 13 April, 1647. Opere di Evangelista Torricelli III, 442. This is a tame translation provided by Alfredo Dinis in 'Giovanni Battista Riccioli and the Science of his Time' in *Jesuit Science and the Republic of Letters,* ed. Mordechai Feingold (Cambridge, Massachusetts: MIT Press, 2003), 195-224, on 212.
[40] Bertoloni Meli, *Thinking with Objects,* 108.
[41] Hooke, Robert *An Attempt to Prove the Motion of the Earth by Observations* (London: John Martyn, 1674), 5. Hooke studied Riccioli's *Almagestum Novum* intensely, having been recommended it by Dr. Seth Ward when he was undertaking astronomical studies at Oxford in 1656-7. See *The Diary of Robert Hooke (1672-1680)* eds. Henry W Robinson and Walter Adams (London: Taylor and Francis, 1935), xviii: 'contriv'd a way to continue the motion of the *Pendulum*, so much commended by *Ricciolus* in his *Almagestum*, which *Dr Ward* had recommended me to peruse'.

but this un-grounded Subterfuge, to say, that the Center of the Sphere of the fix'd Stars continually changes its Place for an Annual Motion.'[42]

Huygens was a correspondent of Leopoldo, and as I have noted in chapter four was one of those mathematicians who strongly defended Galileo's law of fall on mathematical grounds. His comments illustrate his strong desire to prove the existence of parallax in order to counter the only anti-Copernican argument that he considered to be worth taking seriously. Nevertheless, Riccioli's work was extremely influential and what emerged from his arguments in the *Almagestum Novum* and subsequent debates with Borelli, Stefano Degli Angeli and others was that he did not accept the concept of inertial motion, the relativity of motion or the independence of the horizontal and vertical components of motion – the three concepts that the gun experiments were designed to demonstrate.

In particular, Riccioli could not accept that when two different motions were combined they would not interfere with each other and that one would not dominate the other. This same issue was crucial to both the trajectory of a cannon ball and the question of what happens when a stone is dropped from a tower onto a moving earth. Riccioli had attacked Galileo's 'beautiful thought' on this question that he introduced into the *Two World Systems*, but the question of what trajectory a stone would follow if one imagined it could fall freely to the centre of a hypothetically moving earth captured the attention of scientists all over Europe, including Mersenne, Newton and Robert Hooke.[43] In the next chapter we will see that it was also discussed in some detail by Francois Blondel in his *L'Art de Jetter les Bombes.* In the case of Riccioli's

[42] Huygens' comment on parallax in letter to the Royal Society in response to Hooke's treatise on parallax published in *Philosophical Transactions,* 9 (1674) 90.
[43] For Galileo's 'beautiful thought' see Galileo, *Two World Systems,* trans. Drake, 164-167. *Opere di Galileo Galilei* VII, 189-191.

debate with Stefano di Angeli and Borelli, it was used as a proxy for an argument with Riccioli on Copernicanism.

The resonance of the controversy between Stefano Degli Angeli, Professor of Mathematics at Padua and Riccioli is indicated by the fact that in 1668 it was summarised by Jacob Gregory in the *Philosophical Transactions.*[44] He noted that 'Riccioli in his *Almagestum Novum* pretends to have found out several new demonstrative Arguments against the Motion of the Earth, *steph de Angelis*, conceiving his Arguments to be none of the strongest, taketh occasion to let the world see, that they are not more esteem'd in *Italy*, than in other places'. He continues:

> The *second* Argument is much insisted upon by *Angeli,* to make his solution clear to vulgar capacities; but the substance of all is, That the –ball hath not only that violent motion, impressed by the Fire, but also all these motions proper to the Earth, which were communicated to it by the impulse received from the Earth: for, the Ball, going from West to East, hath indeed two impulses, one from the Earth, and another from the Fire; but this impulse from the Earth is also common to the mark, and therefore the Ball hits the mark only with that simple impulse, received from the Fire, as it doth being shot towards North and South; as *Angeli* doeth excellently illustrate by familiar examples of Motion.[45]

Alexander Koyré provides a detailed treatment of this crucial debate in 'A Documentary History of Fall from Kepler to Newton: *De Motu Gravitum Naturaliter Cadentum in Hypothesi Terrae Motae'*. [46] It is a particularly useful work because it includes substantial extracts from 'long-forgotten' authors such as Borelli, Angeli and Riccioli that have never been translated into English, and Koyré notes its significance to Newton's return to the study of terrestrial and celestial mechanics:

> It is well known that it was Hooke's invitation to Newton (of November 1679) to resume his scientific correspondence with the Royal Society, or, more exactly, Newton's reply, attempting to determine the trajectory of a heavy body falling from the summit of a high tower, followed by Hooke's criticism,

[44] *Philosophical Transactions,* 3 (1668) 693-698 and 693.
[45] *ibid.,* 696.
[46] Alexander Koyré, 'A Documentary History of the Problem of Fall from Kepler to Newton: *De Motu Gravium Naturaliter Cadentum in Hypothesi*', *Transactions of the American Philosophical Society* 45 (1955), 329-395; section on Riccioli, 329-395.

> and the ensuing polemics, that turned Newton's mind away from "other business" and back to the study of terrestrial and celestial mechanics
> It is less well known that the problem discussed by the two great scientists – the trajectory of a body *in hypothesi terrae motae* – had behind it a rather long, complicated and extremely interesting story.[47]

But back to Riccioli. He was a hardliner who supported the decision of the Inquisition against Galileo with the argument that toleration of Copernicans in the interpretation of scripture would set a dangerous precedent, arguing that if the Copernicans were

> allowed the freedom with which they interpret the ecclesiastical decrees' one 'might fear that such a freedom would not be limited to astronomy and natural philosophy, but would touch the holiest dogmas; it is therefore important to keep the rule of interpreting all sacred texts in their literal meaning. In the case of the motion of the earth, we have no need to put this rule aside'.[48]

The role that the *Saggi* experiments might have played in countering the specific key arguments against Copernicanism that Riccioli presented has not previously been noted. Yet Borelli's prominence in this later controversy with Riccioli and his leading role in the choice of experimental content of the *Saggi* would seem to be more than just coincidence.

Riccioli's arguments must have caused some concern to the Jesuit Honoré Fabri. Fabri was a correspondent of the Academy and might well have been aware of the rhetorically powerful gun experiments. He may have believed that unless a more sophisticated anti-Copernican approach were taken, there would be a danger of bringing ridicule on the Church. In the second dialogue of his anti-Copernican work, the *Dialogi physici in quibus de motu terrae disputatur* (Lyons, 1665) he argued that all the standard arguments against Copernicansm were inadequate. Fabri's attempts to retain the scientific credibility of the anti-Copernican cause did not go down well with

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[47] Koyré,'A Documentary History' 329. See also Ofer Gal, *Meanest Foundations and the 'Compounding of the Celestial Motions of the Planetts'* (Dordrecht: Kluwer Academic Publishers, 2002), 3. Gal sees this correspondence, initiated by Hooke, as having far more significance than simply reviving Newton's interest in celestial and terrestrial mechanics. Rather he credits Hooke's question, and his insights, as being key to turning Newton on to the consideration that a closed (orbiting) curve could be derived from two rectilinear motions, one inertial and tangential and the other arising from the centripetal rectilinear attraction of the sun. This opened the door to Newton's unification of celestial and terrestrial mechanics.
[48] Dinis, 'Giovanni Battista Riccioli', 213-4.

other Jesuits or the ecclesiastical authorities, and Fabri himself was placed under scrutiny.[49] Later, he was imprisoned by the Inquisition and had to rely on Prince Leopold's mediation to obtain his release.

Dodging and weaving

The *Saggi* had to go through a tortuous process to pass the scrutiny of the Inquisition. It provided a clear presentation of some of its main experimental results, which quietly and uncontroversially challenged intuitive preconceptions of the nature of motion. Mario Biagioli has dismissed the lengthy process of production as evidence of Leopoldo's fear of 'status-pollution'. The Cimento is described as a 'retrospective invention' where the 'frequent strong tensions and explicit disagreements recorded in the academicians' private correspondence were made invisible by the *Saggi*'. [50] As I have indicated, however, there were other very good reasons why the *Saggi* took the form that it did.

To fully understand the difficulty of the situation in which Leopoldo and Ferdinando found themselves it is necessary to understand the contradictions inherent within the Medici dynasty. There was a long tradition of humanism and interest in the development of science and technology. But the dynasty was historically dependent for its existence on its close collaboration with the Papacy. In the same way, successive unhappy marriages were brokered in an attempt to shore up the dynasty's shaky legitimacy. Indeed, the association with the church could be seen as yet one more necessary but difficult Medici marriage.

[49] Domenico Bertoloni Meli ('Shadows and Deception', 393) argues Borelli feared that the 'cranky brain' of Fabri had nevertheless come up with some concepts regarding the motion of the satellites of Jupiter that might be considered by some to be similar to his own planned publication.
[50] Biagioli, *Galileo, Courtier,* 359. Mordechai Feingold, in 'The Accademia del Cimento and the Royal Society' in Beretta, Clericuzio, Principe, eds. *The Accademia del Cimento,* 229-242, on 230-231 argues persuasively against Biagioli, Jay Tribby, Paula Findlen and Paulo Galluzzi for maintaining, amongst other things, that the Cimento was merely a product of princely whim.

The high price paid for the papal support that brought Cosimo 1 to power became increasingly burdensome during the Dukedom of Ferdinand II, when like a suffocating incubus it drained resources and creativity from an economically ever more desperate state. Not only did Ferdinando have to accept the condemnation of Galileo, but, as J. R. Hale notes, the price for using Rome as 'fulcrum for the levers of foreign policy' was 'the flooding of his territories with land-hungry and non-taxpaying religious orders and the filling of monasteries and convents with non-productive fugitives from Tuscany's potential workforce. The freight of nuns carried by the capital alone approached 5000. Whereas Cosimo I had treated church appointments and property…with unbending *étatisme,* Ferdinando genuflected.' As Ferdinand wryly commented: 'In sixty or seventy years the nuns will have swallowed up everything'.[51]

Although the Medici used their patronage of science to enhance their status, this ran counter to the other forces that they had to play off. Nevertheless the humanist and cultural heritage, patronage of science and technological projects such as the development of the port of Livorno, were major achievements in its history.

The importance of education in determining the character of Medici rule is highlighted by the very different reigns of Ferdinand and Leopold compared to Ferdinand's son, the future Cosimo III. Cosimo II was a pupil of Galileo, and Leopoldo and Ferdinand's teacher was Iacopo Soldani, also a pupil of Galileo and a firm adherent of Galileo's 'new philosophy'.[52] Unfortunately Ferdinand allowed control of Cosimo's education to fall into the hands of his pious and bigoted wife Maddalena who, perhaps with an eye to the effect that their education had had on Ferdinand and Leopoldo, astutely ensured that Cosimo was educated by priests. As

[51] J. R. Hale, *Florence and the Medici,* 165.
[52] Middleton, *The Experimenters,* 22.

Tuscany spiralled into decline, its reliance on the power of religion and bigotry to control and channel the discontent of the population became all the more necessary. Under Cosimo III anti-semitism was ratcheted up, and on 10 October 1691 the teaching of atomism in the University of Pisa was banned. It was decreed that only the philosophy of Aristotle could be taught. Cosimo also introduced a law forbidding Tuscan residents from attending any other university, so there was no escape.[53]

Middleton notes that Ferdinand divided up the administration of government between his brothers, creating a sort of 'despotism by committee' because he was not really that interested in the business of government and preferred his scientific experiments and other amusements. Even in the field of research there seems to have been a division of labour, with Leopoldo concentrating on physics while Ferdinand concentrated particularly on medical research. The experimental work of the Medici was just one part of a very well organised state political and administrative machine. Hale eloquently describes how Tuscany was forced to 'dodge and weave, here yield a little and there steal a miniscule advance, with an absorption as dedicated and time-consuming as that required of the chief competitors in the diplomatic market'. This dodging and weaving might be equally applicable to the relationship between science and the church that Leopold and Ferdinand had to negotiate.

It seems hard not to agree with Middleton that the Medici brothers were rather benevolent despots. Yet even the very delicate situation that Leopoldo found himself in when he was asked by Huygens to adjudicate in the dispute with Fabri is reduced by Mario Biagioli to the desire to ensure that the activity of the Cimento 'would not lead to status-tainting disputes':

> They [the academicians] were instructed [by Leopoldo] to perform careful experiments on *models* and, without passing any final judgement on the

[53] Marco Beretta, 'Lucretius as Hidden Auctoritas of the Cimento', in Beretta, Clerlucio, Principe, eds., *The Accademia del Cimento and its European Context*, 1-16, on 16.

> contenders' claims, to report what their experiments *suggested* about the tenability of the contending *hypotheses.*[54]

It is difficult to see what was so sinister and self-serving about carrying out an experiment using models, or about Leopoldo's passing on a problem to his most gifted academicians to work on. Indeed, the ingenuity of Borelli's experimental approach and Leopoldo's tactful and judicious behaviour might by others be considered admirable.

The persistence with which Leopoldo attempted to engage the Royal Society in correspondence in the face the less than enthusiastic response from Boyle who ignored Leopoldo's attempts at engagement, do not strike one as the behaviour of someone who is obsessed by their own status, though he was no doubt aware of the usefulness of using science as a mean of developing good diplomatic relations with a country that he recognised as being an increasingly dominant power in the world.[55] It is understandable that he wanted to do everything in his power to ensure that the *Saggi* was a fitting product to represent both himself as a highly esteemed and learned patron of science and to show that despite the condemnation of Galileo, Italy could nevertheless take a leading role in the promotion of the new experimental science. It seems likely he also had in mind the aim of countering the image of Italian science created by Riccioli.

The difference between Leopoldo and Charles II of England is suggested by a letter that Magalotti sent to him in 1668 from his trip through Europe to distribute the *Saggi* to Leopoldo's scientific contacts. Magalotti confides that

> I have become greatly disillusioned about my reception by the King, for while I was given to understand that the so effective protection that he gives to the

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[54] Biagioli, *Galileo, Courtier,* 360. Italics in original. See also Mario Biagioli, 'Scientific Revolution, Social Bricolage, and Etiquette' in *The Scientific Revolution in National Context*, eds. Poy Porter and Mikulas Teich (Cambridge: Cambridge University Press, 1992), 11-54, on 27.

[55] See Hale, *Florence and the Medici,* 164: Cardinal Ferdinand (later Ferdinand I) had obtained inside knowledge of the extent to which Spanish power was damaged by the Armada. This had allowed him to obtain a level of independence from Spain not open to Cosimo I.

> famous Royal Society was the effect, if not of kindness, at least of his esteem for these studies, I have learned that he is accustomed to call his Academicians by no other title than *mes furets* (my ferrets).[56]

Middleton suggests that it 'would be easy, but quite wrong, to make the facile assumption that the Accademia del Cimento was merely one of the diversions of the Medici princes, on a par with hawking and the pleasures of the bedchamber'.[57] In contrast, Biagioli asserts that 'the experiments of the Cimento were not part of a "laboratory life". Rather, they resembled those courtly activities - like dancing and fencing - that characterised the daily life of a prince.' [58]

To equate scientific activity with the pleasures of the bedchamber obscures what is historically significant about the former activity. This is brought home by the vast difference in the social programme of Leopoldo and the next Medici duke, Cosimo III, of whom J. R. Hale remarked:

> Restlessly he moved about his dominions, but it was to touch relics, to promote the sale of indulgences, and give lustre to religious processions, not to inspect public works or hear complaints.[59]

Biagioli is a master of the intricacies of court etiquette, an appreciation of which is invaluable to historical research on this period in Italy. But his emphasis on Leopoldo's status-enhancing agenda trivializes what the academicians were trying to achieve and tends to blur historical distinctions rather than illuminating them. Whilst it is true that the cultural symbols of rule are particularly important when the perceived legitimacy of the ruler is shaky, as in the case of the Medici, this fails to recognize that there was a genuine modernizing and proto-Enlightenment agenda that was nevertheless subordinated to the compromises that had to be made to retain power.

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[56] Middleton, *The Experimenters,* 291. It may have been 'my fools' rather than 'my ferrets'. See W. E. Knowles-Middleton's 'What did Charles II call the Fellows of the Royal Society?' *Notes and Records of the Royal Society of London*, 32 (1977), 13-16.
[57] *ibid.,* 56.
[58] Biagioli, 'Scientific Revolution, Social Bricolage', 30.
[59] Hale, *Florence,* 186.

Furthermore, Biagioli's comment ignores the dialectic between work and leisure that was crucial for the development of science. Magalotti in his preface refers to the work of the Academy as a form of recreation for Leopoldo from the responsibilities of state. This was true, and not just a rhetorical trope. It is noteworthy that the puritanical and immensely devout Boyle used a similar argument for the practice of science as worthy pastime, with his comment of the virtues of science as a diversion from 'bags, bottles and mistresses'.[60] But this does not mean that the experimental work was not a serious business, and an important part of the Medici state machine.[61] What is significant is not that Leopoldo acted to preserve his status. It was the efforts that went into using his power and resources for the preservation of the Galilean legacy and to defend Copernicanism in the only way that was left open to him.

Despite the difficulties of the ebb and flow of ecclesiastical censorship, Borelli managed to publish prolifically across the whole spectrum of natural enquiry. The experiments he devised at the Academy were for him a means of comfirmation of his mechanist and corpuscularian views by which he was able to bring a synthesis to his diverse enquiries, including the unity of celestial and terrestrial mechanics. In his own publication of the Cimento experiments, *De motionibus naturalibus a gravitate pendentibus* he puts them into the context of his own causal explanations and interpretations.[62]

That Borelli wrote an astronomical work, *Theoreticae Mediceorum Planetorum* (1666) is perhaps surprising. This work, which predicted elliptical orbits for the satellites, aimed not simply to describe but to provide the causes of their

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[60] John Hedley Brooke, *Reconstructing Nature: the Engagement of Science and Religion* (Edinburgh: T. & T. Clark, 1998), 110.
[61] Pamela Long, *Artisan/Practitioners and the Rise of the New Sciences, 1400-1600* (Corvallis, OR: Oregon State University Press, 2001), 123 and 125, makes a similar point about the Duke of Ferrara, Alfonso D'Este who learnt pottery as a 'leisure' activity – but she then explains his interest was 'tied to the economic well-being of the state'.
[62] Stefania Montacutelli, 'Air "Particulae"', 67.

motions.[63] Some idea of its significance is given by the fact that it is referred to by Newton in the *Principia* and in correspondence with Halley.[64] The work emerged as a result of the Grand Duke's request to calculate the positions of the Medician planets on the basis of Galileo's tables, so as to allow their positions to be observed each evening. These observations stimulated discussions that led almost accidentally to Borelli's realisation that he had the basis of a book. This had not been his intention as he was hoping to continue working on his anatomical work on animal motion.[65] As Meli notes, the 'barely veiled Copernicanism would have provided the censors with ample ground for extensive investigations and negotiations, thus delaying publication'.[66] But in fact it was passed in a couple of weeks due to the Grand Duke's timely intervention at the point where it was in danger of being sent to Rome for closer scrutiny.

An indication of the delicate balance that had to be maintained is shown by the case of Alessandro Marchetti, a protégé of Borelli, who wanted to publish *Della Natura delle Cose*, his Italian translation of Lucretius' *De Rerum Natura*. The Latin of the original was notoriously difficult, which limited the influence of its dangerous atomist and anti-religious ideas to a very small elite.[67] An accessible Italian translation was potential dynamite. Prince Leopold, by this time Cardinal, forbade publication. This was all the more surprising since it was Leopold who had originally encouraged Marchetti to work on the translation. According to Middleton, the work had caused a sensation and manuscript copies were circulating widely; two copies were ordered for the grand-ducal library and two copies were ordered by Michelangelo Ricci, one for himself and one for 'one of the most noted prelates of the

[63] Bertoloni Meli, 'Shadows and Deception', 385 and 389.
[64] *ibid.*, 384.
[65] *ibid.*, 386.
[66] *ibid.*, 389
[67] Stephen Greenblatt, *The Swerve: How the Renaissance Began* (London: Bodley Head, 2011), 256.

Papal Court'.[68] On more than one occasion, Borelli had been forced to urge caution on Marchetti, emphasising that the princes wanted the new philosophy to be spread without fanfare (*clamori*).[69] It seems that the outspoken Marchetti was attracting a little too much attention for the prudent prince.

Conclusion

So far the selected case studies have shown how the cannon as a challenging object stimulated research into the motion of a trajectory, for the dual benefit of both philosophy and the practice of gunnery. This chapter illustrates how it continued to provide an opportunity for experimental support for the conceptual foundations of the new science of motion, and as a proxy for the discussion of Copernicanism. Borelli feared that by collaborating with other academies, he would miss out on the credit that he deserved. He was convinced that his experiments owed their ingenuity and creativity to his mechanist, atomist philosophy and his Copernicanism. But he feared that others would be able to deduce the causes from the experiments and that his work would be eclipsed. In the case of the *Saggi* gun experiments, however, it seems likely this was what the Galilean experimenters hoped would happen; that the experiments would speak for themselves, as a powerful confirmation of Galilean science, achieved without 'clamori'. Their significance was all the greater because they were written in Italian, which made them more liable to inquisitional scrutiny than Borelli's Latin works.

Medici absolutism was weak and subservient to the church hierarchy; in the next chapter we will see how gunnery would be used to promote the new science under an absolute monarchy that could largely dictate its own terms to the papacy.

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[68] Middleton, 'Science in Rome, 1675-1700, and the Accademia Fisicomatematica of Giovanni Giustino Ciampini' *The British Journal for the History of Science,* 8 (1975) 138-154, on 153.
[69] Mario Saccenti, *Lucrezio in Toscana: studio su Alessandro Marchetti* (Firenze: Leo S. Olschki, 1966), 39.

Chapter six

François Blondel, absolutism and the art of launching bombs

Introduction

The enigma of Francois Blondel (1618-1686) may be illustrated by two contrasting views of his *L'Art de Jetter les Bombes* (1683):

> It is obvious that since the divided charge of powder alone produces all of those effects, goodbye to all the square-edges, angles, parabolas, conic functions and to this entire hodgepodge of mathematics, which is useful only for displaying the profound knowledge of the author, but which could never be understood by any gunner…Where the devil was this author's head when these gunners taught him that and why did he not take this opening that their experience gave him?[1]

> M. Blondel does not content himself with historical remarks, but examines these matters philosophically, and reasons in depth on the principles of geometry and physics, which are the foundation of the rules of the art of launching bombs. Most engineers are happy with experience, but it is certain that those who go back to the cause, and who examine the arts in their principles, are best able to lead them to perfection, and so it is very wrong to imagine that the speculations of physicians and geometers serve nothing to civil society.[2]

In the first quotation Blondel emerges as an armchair theorist who doggedly refused to accept the obvious, that Galileo's theory of the trajectory was useless to the practising gunner. Pierre Bayle, on the other hand, acknowledges Blondel as a philosopher who understands that it is only through the investigation of causes that practice can be perfected.[3]

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[1] Quoted in Anthony Gerbino *François Blondel: architecture, erudition, and the scientific revolution* (London: Routledge, 2010), 125. It is taken from a marginal note quoted in Azier "Les debuts de l'Artillerie" (1931), 189-190. Azier dated the note from the late seventeenth or early eighteenth century.
[2] Pierre Bayle, *Nouvelles de la Republique des Lettres* (Amsterdam: Henry Desbordes, Mois de Juin 1684), 432. Also quoted in Gerbino, *François Blondel*, 8.
[3] Until the appearance of Antony Gerbino's excellent *François Blondel: Architecture, Erudition, and the Scientific Revolution* (2010), Blondel had not received much historical attention. The only other substantial works are the biography by Anton Mauclaire, Placide and Vigoureux, M, *Nicolas François de Blondel: ingénieur et architecte du roi 1618 – 1686* (Laon: Imprimerie de l'Aisne, 1938). Michel Blay discusses Blondel's work on ballistics in 'Le Developpement de la balistique et la pratique du jet des bombes en France a la mort de Colbert' in *De la Mort de Colbert à la Révocation de l'Edit de Nantes: Un Monde Nouveau?* Actes du XIV colloque du Centre Méridional de Rencontres sur le XVII[e] siècle, Janvier 1984 (Marseille: A. Robert, 1985).

In this chapter I will make three main claims. Firstly, that the most committed, persuasive, popular vernacular elucidation and defence of Galileo's new science in the 17th Century was contained in Blondel's book on the art of launching bombs. Secondly, that historians have failed to see *L'Art* within the context of the wider programme for the reform of knowledge that Blondel saw as the basis for the perfection of practice in the arts. Thirdly, that Blondel only believed the theory to be practicable if it was tied to a programme of education and training, meticulous experimental trials and data-collection, and technological rationalisation. His theory was not a substitute for practice and experience but a means of guiding practice. At the time that he was writing Blondel believed that the French state had the both the resources and the will to implement such reforms. French absolutism, with its generous and committed sponsorship of science by the state, could be seen as providing the ideal conditions for the implementation of such a programme.

Finally, Blondel's hopes for the reform of the art of launching bombs should also be seen within the context of the conjunctural importance of siege warfare and the conscious application of science to war that was central to the programme of territorial expansion of Louis XIV. The use of mortar fire was integral to the scientific planning of sieges. The particular importance of mortar fire in this period gave greater plausibility to Blondel's argument for the applicability of the theory because the trajectory of mortar fire more closely matched the shape of the parabola.[4]

To appreciate the conjunctural importance of Blondel's emphasis on the use of the theory for mortar fire and bombs rather than cannon fire, it is necessary to understand the operational difference between the two. A mortar is only capable of 'indirect' fire. It is good for lobbing in siege situations, having the effect of

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[4] See Denny, *Their Arrows*, 80, 'External Ballistics 101'. The calculation of ballistic trajectories in a vacuum '…is an approximation instead of the true trajectory, but the approximation can be pretty good, at least for heavy, slow projectiles.'

terrorising the enemy and ensuring a speedier surrender. Mortars have the advantage of being portable; cannon require greater logistical resources. A cannon is a high pressure, high velocity machine, whereas mortar fire works with low pressure and low velocity. Since air pressure is related to speed, the effect of air resistance in mortar fire is less than in cannon fire and the trajectory resembles a parabola. Mortars are short and fat and bombs set to explode on landing are used, not cannonballs. The bombs are lobbed into the muzzle at the time of ignition of the powder and success involves careful timing so that the bomb explodes on landing.

Black powder mortars came into use in siege warfare because the high trajectory allows one to drop shells on an enemy, for example inside a fort or town, while cannon tend to be used at lower trajectories for field warfare or to break down the walls of a fortress.[5] In a siege ideally one would have a carefully considered combination of both cannon and mortars to achieve optimum effect.

In addition, the use of mortar fire became notorious with the invention by the French of Bomb Ships, which they first employed in the siege of Algiers in 1682. According to Spencer C. Tucker's *Handbook of 19th Century Naval Warfare* 'The bomb vessel… drew only about 8-10 feet of water, which enabled it to manoeuvre close to shore. It was strongly built to enable it to withstand the shock of the discharge of its heavy mortars.' He also notes that the French discovery led to a 'Mediterranean arms race when the British also built bomb vessels'.[6]

There are, in a sense, two books that comprise *L'Art de Jetter les Bombes*. Because of its title it has been treated by historians of science simply as an unremarkable, unoriginal and practically quite useless work on gunnery. What has been missed is its didactic and polemical purpose, in particular regarding the law of

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[5] B.P. Hughes, *British Smooth-bore Artillery: The Muzzle Loading Artillery of the 18th and 19th Centuries* (Harrisburg Pa.: Stackpole Books, 1969), 86.
[6] Spencer C. Tucker, *Handbook of 19th Century Naval Warfare* (Stroud: Sutton Publishing Limited, 2000), 5.

fall and the parabolic trajectory, but also, as I will show, by implication Galileo's new science as a whole, including his defence of Copernicanism, updated with the contributions of his disciple Torricelli and others.

It might be thought that by 1683 this would not have been necessary. But Galileo remained condemned by the Catholic Church. There was no other popular work in French in this period that compares with *L'Art* for its admirably clear pedagogic style and persuasive, one might even say evangelical, exposition and defense of Galileo's science. Not only this but it had the imprimatur of the king, the royal seal of approval. Given Galileo's condemnation by the church, official sanction of the new science is a significant event.

Blondel had close associations with Mersenne, Gassendi and Roberval in France, and Vincenzo Viviani in Italy. He was deeply immersed in the debates and attacks on the new science following the death of Galileo that I have described in previous chapters. He must have believed that even in 1683 (or rather around 1675 when it was written), in France at least, there was a necessity for such a work of promotion, and that this would be an important contribution to Colbert's educational programme of modernisation of the state. To understand the motivation for the publication of *L'Art de Jetter les Bombes* and the particular form that it took, it is necessary to examine further the historical conditions pertaining in France during this period, and to show how the trajectory of Blondel's life and work was affected by the expansionist drive of French absolutism.

As Antony Gerbino notes, Blondel was an unlikely scholar and could arguably be described as the archetype of the man of action. He was steeped in the military culture of his time and like the legendary military engineer Vauban and a generation of French engineers, his military career coincided with France's entry into the Thirty

Years War, followed by the upheavals of the Fronde, when military service offered the best and possibly the only available career path for a talented and ambitious youth.[7]

Blondel was born in Ribemont in Picardie. His mother belonged to the local provincial nobility and his father, after studying law at Toulouse, bought the office of *avocat du Roi* in Ribemont, and was several times mayor of the town.[8] After a minimal education of which little is definitely known, but which probably consisted of a grounding in mathematics, classical languages and literature, Blondel, at the age of seventeen, enrolled as an infantry cadet and went to fight against the Imperial forces in Lorraine, Alsace and Burgundy. He gained rapid promotion to high offices, having gained a reputation for his bravery and his ability.

Having attracted the attention of Cardinal Richelieu, in 1639-40 he was deployed on diplomatic missions to Italy and Spain, probably to spy on military structures. He may well have visited Galileo on this trip, as he later claimed to have studied under him personally.[9] This would also fit in with his later relationship with Galileo's assistant Vincenzo Viviani, for whom he acted as intermediary with regard to a pension from Louis XIV. On his return he was appointed as sub-lieutenant on one of Richelieu's galleys, and in recognition of his naval prowess he was appointed governor of the naval stronghold of Palamos. He later took part in the Italian campaign, and after being made field sergeant in 1651 he went on a tour of inspection of several strongholds along the coast of Provence. He gave up his military career in 1652.

One of Blondel's most important patrons was Henri-Auguste de Louménie, comte de Brienne. Under his patronage Blondel began to obtain important

[7] Gerbino, *Francois Blondel,* 11.
[8] *ibid*., 11.
[9] *ibid*., 17.

architectural commissions and in 1652 he entrusted Blondel with the care and education of his teenage son, whom Blondel accompanied on a four-year tour of the European courts. This was at the height of the Fronde, and Blondel once more distinguished himself by his ability to negotiate dangerous and delicate situations. On his return he obtained the post of professor of mathematics at the Collège Royale, a post that he held for thirty years, teaching 'mixed' and practical mathematics.[10]

After Brienne's fall from grace, the ever-resourceful Blondel soon managed to ensure that he came to the notice of Colbert, Louis XIV's first minister. As both architect and military engineer, Blondel was on his way to becoming Colbert's trusted expert mediator as part of his programme of achieving French military, mercantile and cultural domination of Europe and the colonies.

Under Colbert's patronage Blondel obtained titles of nobility. In 1669 he became a member of the Académie Royale des Sciences, and he was charged with his most important architectural project, designing a series of improvements in the capital, the enlargement of the Porte Saint-Antoine and the construction of three new city gates, including the Porte Saint-Denis, which celebrated Louis's victories on the Rhine.[11] In 1671 he was appointed Director and Professor of the Académie d'Architecture. His esteem as an educator was confirmed with his appointment in 1673 by Louis XIV as tutor in mathematics to the fourteen-year-old Dauphin and it was during the subsequent ten years that Blondel published his major works.[12]

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[10] *ibid*., 25

[11] *ibid*., 33

[12] These were: the *Comparaison de Pindare et d'Horace* (Paris: chez Claude Barbin, 1673), translated into English by Sir Edward Sherburne as *The Comparison of Pindar and Horace* (London: Tho. Bennet, 1696); *Resolution des quatre principaux problèmes d'architecture* (Paris: Imprimerie Royal, 1673); *Cours d'architecture ensegné dans l'Academie Royale d'Architecture,* 3 vols (Paris: Impreimerie de Lambert Roulland, 1675-1683); *Histoire du calendrier romain qui contient son origine et les divers changemens qui lui sont arrivez* (Paris: l'autheur et Nicolas Langlois, 1682); *Cours de mathématique contenant divers traitez composez et ensignez à Monseigneur le Dauphin* (Paris: l'auteur et Nicolas Langlois,1683); *L'Art de jetter les bombes* (l'auteur et N. Langlois, 1683); and *Nouvelle manière de fortifier des places* (Paris: l'auteur et N. Langois, 1683).

The military context

The Thirty Years' War, followed by the disruptions of the Fronde, had left France debt-ridden, inefficient, corrupt, and with a decrepit infrastructure. The United Provinces and to a lesser extent England ruled the seas and dominated trade. Louis XIV was delighted with the rivalry that had developed between these former Protestant allies as he saw the two maritime powers 'launch year by year fleets of more than a hundred vessels, which mutually destroyed one another in some of the most stubborn fights that have ever taken place, the net result of which was the weakening of both powers'.[13] It provided the perfect opportunity for Louis to make France the dominant power both on land and sea, and for Colbert to put into action his major plan to capture control of East Indian trade from the Dutch.

Louis's personal rule began in 1661 on the death of Mazarin. Within a remarkably short time of the initiation of Colbert's financial reforms, the sheer size of population of the increasingly centralised French state compared with any of its rivals, enabled Louis to become the richest monarch in Europe with wealth to lavish in any direction that he chose. By 1663 the ever-impoverished Charles II did not take much persuading to sell Dunkirk to Louis for five million *livres*. As Voltaire recounts, Louis 'set thirty thousand men to work on the fortifying of Dunkirk, both by land and sea. Between the town and fortress he constructed a harbour large enough to contain thirty ships of war, so that the English had scarcely sold this town than it became a source of dread to them.'[14]

By 1668 Louis had conquered Flanders and Franche-Comté, but with the formation of the Triple Alliance of England, the Dutch Republic and Sweden, he was

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[13] Voltaire, *Louis XIV,* trans. Martyn Pollack (London: Dent, 1926), 75.
[14] *ibid*., 74.

forced to give up the latter at the Treaty of Aix-la-Chapelle (1668).[15] During the following years of peace Louis prepared his ambitious scheme to conquer the whole of the Netherlands. He 'made sailors of his Frenchmen by the efforts of Colbert' and 'made further improvements in the art of war on land with the industry of Vauban'.[16]

Louis XIV was again able to buy the support of Charles II, who was always in need of funds to enable him to maintain his independence from Parliament. Charles II's policy was inadvertently politically astute as well as financially beneficial to himself, since the relationship with Louis XIV proved useful to the English in their policy of domination of trade and colonies, as Christoper Hill has noted:

> Under Charles II England was unable to mobilise sufficient striking power to beat the Netherlands to their knees. That was done for us by Louis XIV of France. The Anglo-French maritime treaty of 1677 allowed English ships to carry Dutch cargoes whilst France and the Netherlands were at war, and England could cut in on the Dutch carrying trade, especially in the Mediterranean.[17]

The scale and the rapidity of the French conquests was overwhelming and at this point none of the other European powers, alarmed as they were, had the courage to take the initiative, nor the willingness to forget old and new enmities, to make an effective unified challenge. It was, indeed, a daunting task; the French army and navy had become the largest, most disciplined and well-trained in Europe. The port of Toulon was 'constructed at immense cost to hold a hundred ships of war, together with a splendid arsenal and powder-magazine. On the Atlantic the port of Brest was

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[15] He had annexed part of Spanish Netherlands as compensation for Marie-Therese's unpaid dowry – it was a long-hatched plan since Louis and Mazarin probably guessed Spain would never be able to pay.
[16] Voltaire, *Louis XIV*, 106.
[17] Christopher Hill, *The Century of Revolution* (London: Routledge, 2010) p. 211. See also Eric Williams *Capitalism and Slavery* (Virginia: University of North Carolina Press, 1943), 40: 'The bitter commercial warfare of the second half of the seventeenth century between England and Holland represented an effort on the part of England to break the commercial net the Dutch had woven about England and her colonies. "What we want," said Monk with military bluntness, "is more of the trade the Dutch now have"'.

formed on the same grand scale. Dunkirk and Havre-de-Grace were filled with ships and at Rochefort 'nature herself was brought under subjection.'[18]

The one miscalculation that was made in the campaign against the Netherlands in 1672 was that too many towns were garrisoned, which weakened the attacking strength of the army and, crucially, allowed the Dutch to rally sufficiently to prevent the taking of Amsterdam. Nevertheless in 1674 Vauban went on to capture Besançon after a nine-day siege and in six weeks the whole of Franche-Comté had once more surrendered to France.

In 1677, Charles II, under pressure from parliament to make an alliance with Holland, arranged a marriage between the Prince of Orange and his niece, at which point Louis decided to stop his generous subsidy of the English monarchy.[19] This angered Charles II but by clever diplomacy Louis nevertheless prevented England from successfully uniting with the Dutch against him, and between 1678 and 1679 he was able to sign favourable peace treaties at Nijmingen with the Dutch and with the Holy Roman Empire that retained his glory and left him master of Europe. Thus in the dedication to Louis in *L'Art de Jetter les Bombes*, Blondel could boast of Louis' magnanimity in having 'given peace to her enemies'.

Vauban and siege warfare

A state of equilibrium had developed in Europe in relation to siege warfare. The new method of fortification (*trace italienne)* that had been developed to counter the effect of cannon attack resulted in long sieges with little to show at the end for the huge cost and loss of life involved. The widespread adoption of the *trace italienne* in the Netherlands in its 80-year revolt against Spain had enabled even small towns to resist

[18] Voltaire, *Louis XIV*, 129.
[19] Freudenthal, *Atom and Individual,* 144.

capture for several months.[20] Jamel Ostwald suggests that the disparity between the power of defence of the new design of fortifications compared to the frailty of the attack could be considered a prime cause of the indecisiveness in Europe's sixteenth- and seventeenth-century wars which held back the centralisation of early modern governance into royal hands.[21]

With Vauban's new scientific approach to siege warfare and fortification, the French for a time had the advantage, in both attack and defence. If it is argued that no gunner would have had time in the heat of battle to use the mathematical methods and instruments based on the new science this was not necessarily the case with the meticulously planned sieges carried out by Vauban, where mortars and guns were carefully positioned as an integral part of the plan of siege, and where mathematical precision applied to every aspect of the plan. Also, guns would often have to be placed at a different level from the target, and this might explain why Blondel put considerable emphasis on the solution to this problem.

It was in the siege of Maastricht (1673) that Vauban's mathematical approach to siege warfare was first put to the test. The traditional method of procedure in a siege was to dig a narrow trench perpendicular to the town wall by which the troops were able to approach. It was a method that was enormously wasteful of the lives of the attackers, who could easily be mown down along the line of the trench by enemy fire. The old method also left the attackers vulnerable to being picked off in sorties from the besieged town or fortress. Vauban was exceptional in that he valued the lives of the attacking soldiers rather than treating them as expendable, and his aim was to ensure the success of a siege in the minimum time and with the minimum loss of his men.

[20] Jamel Ostwald, *Vauban under Siege* (Leiden: Brill, 2007), 5.
[21] *ibid*., 1.

The new method involved the digging of larger, well-protected trenches parallel to the defensive wall, and from there to push forward with a short, wide perpendicular trench, which facilitated the construction of the next parallel trench nearer the fortification. These parallels held troops who could protect those digging the advancing perpendicular trench without interfering with their work. The capture of Maastricht had great importance as demonstration of the power of this new scientific approach to warfare.

Petit Renau and the bomb ships

I have already noted the importance of mortar fire in the siege warfare that was crucial to the military successes of the French state during this period. But the French also made a new invention, the *galiote à bombes,* or bomb ship, that enabled mortars to be fired from ships, something that was previously considered far too dangerous to even consider. Although this took place when *L'Art* was about to be published, it adds further evidence of the interest in the use of mortar fire in this period. The use of mortars to bombard Algiers was suggested by Bernard Renau (who was known as *Petit Renau* because of his youth and small stature).

Admiral Duquesne had been putting the new French fleet to use in sweeping the seas to clear it of the troublesome pirates of Algiers and Tripoli.[22] The new invention of Renau enabled Louis to inflict a humiliating defeat on the pirates by attacking the Ottoman border province of Algiers. The impact of Renau's invention is perhaps indicated by the fact that a new frontispiece was provided for the second edition of *L'Art,* published the year after the bombing of Algiers, which took place between 1682 and 1683, illustrating mortar fire from ships besieging a port. News of

[22] Voltaire, *Louis XIV*, 129.

the bombarding of Algiers and the release of Christian slaves must have caused a sensation and it is not surprising that the publishers might have wished to capitalise on it.

Renau had attracted the attention of Colbert by his ingenuity in suggesting a much improved method of ship construction, which had helped to break down the monopoly of the master carpenters. [23] Apparently Admiral Duquesne was initially sceptical about Renau's proposal, and once more Blondel's value as expert technical adviser is indicated by the fact that in May 1681, the Comte de Seignelay asked Blondel and Edme Mariotte to assess the new ship design.[24]

Renau next proposed the construction of another new type of ship specially designed to have the strength to launch mortars. As the story goes,[25] at one of the navy councils attended by Renau, the discussion turned to Algiers and the war Louis was proposing to make with the *Barbaresques.* They discussed all the known means of attack on land and all had been dismissed as too dangerous. Eugène Sue, in his *Histoire de la Marine Française* (1845), dramatises the initial reaction to Renau's suggestion:

> Renau, waking from his reverie, *asked why they didn't bombard Algiers?* At this incongruous question, he [Admiral Duquesne] replied with indulgence, allowing for his extreme youth, that it was excusable to be absorbed in his mathematical calculations, but they had already decided not to try any attack on land. To this Renau replied that he knew this perfectly well, but he proposed to *bombard Algiers by sea.*
>
> To bombard Algiers by sea! This proposition was somewhat grotesque and bizarre (*insolite*), such that M. le duc de Vernandois could hardly contain within the confines of decency the hilarity it caused. Bombard Algiers by sea! Old Duquesne and Colbert, not jocular by nature, were having a field day. At last, when the council had calmed down a little, and the mocking gaiety had been reduced to a few last snickers, Colbert furrowed his thick eyebrows, and

[23] Eugene Sue, *Histoire de la Marine Francais,* Tome III (Paris: Au Dépot de la Librairie, 1845) 396.
[24] Gerbino, *François Blondel,* 123: *Proces Verbaux* 3 May, 1681, Vol 9, fol 99 and 29 Nov 1681, vol. 9, fol.124, A.A.S. I am making an assumption here, as Gerbino does not mention that it was Renau's design, but the date fits with when Renau would have made his suggestion.
[25] Whilst Sue's account has an anecdotal quality, I have included it since it illustrates the resonnance of this story of French inventiveness and ingenuity in French naval history.

> though he was very fond of the adopted son of his cousin de Terron, asked him severely why the presence of S. A. monseigneur de Vernandois had not restrained him from coming out with such foolery.
>
> Renau, taken by surprise at such laughter, soon calmed down and replied to Colbert:
> -Alas, Sir, I was wrong not to have demonstrated to you what I mean by this proposition.
> -Explain yourself then, says Colbert.
> At these words the council listened with great curiosity.
>
> Then Renau, with great simplicity, rolled out his plan of construction [for the new type of boat]. As he explained, the attention grew; he was listened to with ever increasing attention and regret that such an appealing but utopian suggestion could never be realised…Renau insisted, he was taunted; it was allowed that his theory of *galiotes à bombes* was a noble wish for a young man, an illusion allowed for by the fact that he was only twenty eight; but to wish to bring it to reality, that was to mock the gravity of the members of the council.[26]

Renau admitted that they were right to not believe him, because they had not yet seen the proof, but his belief in his *galiotes* was so strong that he convinced Seignelay to get authorisation from his father, Colbert, to commence construction of one of the proposed boats at Le Havre. Five *galiotes* were finally built and equipped to accompany the eleven ships that left Toulon with Duquesne destined for Algiers on 12 July 1682.

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[26] Eugene Sue, *Histoire,* 398-9: 'Renau, sortant de sa rêverie, *demanda pourquoi on ne bombarderait pas Alger?* //A cette question incongrue, il lui fut répliqué avec l'indulgence due à son extrême jeunesse, qu'il lui était sans doute bien pardonnable de demeurer absorbé dans ses calculs mathématiques, mais qu'il devait se rappeler qu'on était convenu de rejeter toute entreprise par terre. A cela Renau répondit à son tour, qu'il savait parfaitement bien qu'on avait résolu de ne faire aucune attaque par terre, mais qu'il proposait de *bombarder Alger par mer. // Bombarder Alger par mer!* Cette proposition avait quelque chose de si grotesque et de si insolite, que ce fut à peine si la présence de M. le duc de Vernandois put contenir l'hilarité qu'elle provoqua dans des bornes décentes. Bombarder Alger par Mer! Le vieux Du Quesne et Colbert, assez peu rieurs de leur nature, s'en donnaient à coeur joie. Enfin, quand le conseil se fut un peu rassis, et que sa gaieté moqueuses ne se trahit plus que par quelques derniers éclats çà et là comprimés, Colbert fronça ses épais sourcils, et, bien qu'il aimât beaucoup le fils adoptif de son cousin de Terron, il lui demanda séverement comment la presence de S. A. Monseigneur le comte de Vermandois ne l'avait pas retenu de dire de pareilles sottises? //Renau, un instant surpris de ces rires, se calma bientôt, et repondit à Colbert: - Hélas! Monseigneur, j'ai tort en effet de n'avoir pas tenté de vous démontrer avant ce que j'entendais par cette proposition. – Expliquez-vous donc allors, - dit Colbert. A ces mots le conseil prêta l'orieille avec un sentiment de vive curiosité,// Alors Renau, avec une grande simplicité, déroula son plan de construction. A mesure qu'il avançait dans cet exposé, l'attention s'éveillait; on le suivait avec un interêt toujours croissant, en regrettant, par exemple, que de si belles utopies ne pussent se realizer jamais// Renau voulut insister, on le railla;on lui accorda bien que sa théorie de galiotes à bombes etait un noble désir de jeune home, une illusion toute premise à ses vingt-huit ans; mais quant à vouloir presenter sous le jour d'une réalité, c'était, lui dit-on, plus que se moquer de la gravité des membres du conseil'. My translation. Emphasis in original French text.

As Eugene Sue's detailed account goes on to illustrate, the subjugation of Algiers and the successful release of Christian slaves was no easy task even with the help of the terrorising effects of the *galiotes de bombes*. As I noted earlier, one of the difficulties with mortars is ensuring that the missile does not detonate before it lands, and this in fact happened early in the first engagement of 30 August 1682, with terrifying consequences:

> They engaged in fire. Renau, on the *Fulminante,* rejoiced at the effect that the projectiles produced, then a *carcasse* with which the mortar had just been charged caught fire, and instead of describing its parabola, fell inside the *galiote,* and set fire to the sails and some sulphur sticks. The crew of this *galiote* were terrified, and believing that the two hundred bombs on board had caught fire, despite the orders of the captain and Renau, jumped into the sea; and the other *galiotes* hurried to make the open sea before they too were damaged by the explosion of this horrible machine, which soon became the target of intense fire from the Algerian artillery. One of the bravest officers of the army, major Ramondi, who commanded one of the supporting boats of the *galiotes*, seeing *la Fulminante* in flames and its whole crew spilling into the sea, had the generous idea to make his way there in the hope of saving the *galiote*. But this might be dangerous for the crew of his own boat if the *galiote* exploded. Therefore the intrepid major, half threatening, half praying and promising, persuaded his crew to swim straight to the *galiote*. Almost the whole force of the Algerians was at this point directed on the *galiote*; Before reaching *la Fulminante,* the major lost eight men of his twenty nine crew; at last he arrived on board and what did he see? Renau and de Combes, with marvellous *sang-froid,* in the process of covering with raw hide the bombs that could have caught fire… the fire quickly died out, thanks to the help given by the major's crew, and *la Fulminante* was once more the first to engage in the fire, which lasted until daybreak.[27]

[27] Eugene Sue, *Histoire*, 408-9. 'On engagea le feu. Renau, monté sur *la Flminante,* jouissait de l'effet que les projectiles allaient produire, lorsqu'une carcasse don't on allait charger le mortier prit feu, et au lieu de decrier sa parabole, retomba aussitôt dans l'interieur de la galiote, et mit le feu aux voiles et à quelques mèches soufrées. L'équipage de cette galiote, terrifiée, et croyant déjà voir en feu les deux cents bombes qu'elle avait à borde, malgré les orders du capitaine et de Renau, se sauve à la nage; et les autres galiotes, interrompant un moment leur feu, se hâtent de prendrele large pour n'être pas abimés par l'ecplosion de cette horrible machine, qui devînt bientôt par ses flames ardentes le point de mire de l'artillerie algérienne. Undes plus braves officiers de l'armée, le major de Ramondi, qui commandait une des chaloupes préposés au soutien des galiotes, voyant *La Fulminante* en feu et tout son eqipage déborder, eut la genereuse idée de s'y render, en blâmant la faiblesse des fuyards, dans l'espoir de sauver peut-être cette galiote. Mais s'approcher d'un pareil bâtiment, qui pouvait faire une explosion mille fois plus dangereuse que celle d'un brûlot, paraissait au moins imprudent à l'équipagede la chaloupe de l'intrépide major; pourtant celui-ci, moitié menaçant, moité priant, promettant, finit par decider son equipage à nager droit sur la galiote. Presque tout le feu des Algériens était alors dirigé sur ce point enflame; et avant d'accoster *la Fulminante,* le major perdit huit hommes des vingt-neuf qui armaient sa chaloupe; enfin il accoste…Et que voit-il? Renau et de Combes s'occupant avec un sang-froid merveilleux à couvrir de cuir vert les bombes qui auraient pu s'enflammer…le feu fut bien vite éteint, grace à l'aide que donna l'équipage de la chaloupe du major, et *la Fulminante* fut encore la première à engager le feu, qui dura jusqu' au point du jour'.

After the defeat of Algiers, Louis turned to Genoa, which had been selling gunpowder to the Algerians and was building galleys for Spain. In 1684 the bomb vessels discharged 14,000 shells on the town of Genoa, and 4,000 marines landed and captured the city. The Doge was forced to travel to Versailles to renounce his support for Spain and to beg clemency of Louis.[28]

The financial cost of the bombardment was, however, enormous. Eugene Sue wrote that Hassan Mezzomorto, the bey of Algiers, on hearing how much Louis had spent on the bombardment of Algiers remarked: 'Your emperor only needed to give me half of what he spent and I would have destroyed Algiers myself'.[29]

Colbert and the bellicose dynastic versus the bellicose mercantilist programme

In early 1672, on the eve of the declaration of war on Holland, Gottfried Wilhelm Leibniz sent the following memorandum to Louis XIV:

> France needs peace in the west, war at a distance. War with Holland will probably ruin the new Indian companies as well as the colonies and commerce lately revived by France, and will increase the burden of the people while diminishing their resources. The Dutch will retire into their maritime towns, stand there on the defensive in perfect safety, and assume the offensive on the sea with great chance of success. If France does not obtain a complete victory over them, she loses all her influence. In Egypt…on the contrary…victory will give the dominion of the seas, the commerce of the East and India, the preponderance of Christendom, and even…empire.[30]

Leibniz, whose patron was the Elector of Mainz, feared French military might and its threat to the German states. He travelled to Paris in 1672 in the hope of gaining an audience with the king. Like Colbert, Leibniz recognised that the best way to undermine the mercantile wealth and power of the United Provinces was by breaking down the Dutch monopoly of East India trade. The Dutch stranglehold on trade was a

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[28] Vincent Cronin, *Louis XIV* (London: Collins Harvill, 1990), 210.
[29] Eugene Sue, *Histoire*, 428. 'Votre empereur n'avrait qu'à me donner la moitié de ce qu'il a dépensé, et je ruinais Alger moi-même'.
[30] Leibniz, *Mémoire* to Louis XIV quoted in Glen J Ames, *Colbert, Mercantilism, and the French Quest for Asian Trade* (Dekalb, Ill: Northern Illinois University Press, 1996), 186.

major factor in holding back French economic development. Yet Louis and his Secretary of State for War, François-Michel le Tellier, Marquis de Louvois, were obsessed by the quest for territorial expansion of France's borders.[31]

Louis had many reasons to feel resentment towards Holland, and for a time there was a merging of dynastic territorial aspirations and the desire to undermine Dutch colonial trade. Between 1661 and 1669, after benefiting from Colbert's fiscal miracle, Louis, realising how much he owed Colbert, was quite willing to go along with his plan to set up the *Compagnie royale des Indes Orientales* in 1664 and undertake a massive programme for the construction of a formidable royal fleet to send to the East Indies and establish a trading base there which could challenge the Dutch. For Colbert, the navy 'was not merely a military instrument, it was a potent weapon by which commerce might be protected, colonies defended, trade expended, the prestige of France increased, and the glory of Louis XIV made even more resplendent'.[32] The prospects were good. The Dutch had succeeded in alienating many local rulers in the East Indies and there was a good chance that the French could use this situation to establish themselves in the region.

Nevertheless, it was a daunting task to try and compete against the firmly-established bases and trade of the Dutch and the English, and vast sums were wasted simply from inexperience and the requirement to recruit experts of sometimes dubious reliability and integrity such as the ex-employee of the VOC (Vereinigte Oost-Indische Compagnie), François Caron, a French Protestant who headed up the first expedition. Caron's generosity with French money rivalled that of Louis XIV himself.

Unlike the English East India Company and the VOC, which were financed by mercantile capital, Louis was forced to effectively finance the whole dazzlingly costly

[31] Ames, *Colbert*, 54
[32] Charles Woolsey Cole, *Colbert and a Century of French Mercantilism* I, 450, quoted in Ames, *Colbert,* 52.

venture himself. The weak French merchant class was just not interested and had to be cajoled and threatened into making even minimal promises of investment (which many never actually paid). The French bourgeoisie was

> firmly wedded to sinecures and safe investments in land and venality of office that were part and parcel of the Bourbon state. Just as importantly, as the war progressed, it became obvious that the overwhelming majority of the aristocratic ruling elite in Paris discerned no vital link between interests in Europe and those in Asia. Dynastic and geopolitical goals in Europe remained dominant; *outré-mer* mercantile or even military goals in Asia remained decidedly and definitively ancilliary.[33]

In contrast, the close relationship between the company directors of the VOC (the Heeren XVII) and the States General provided a unity of purpose and flexibility that enabled the Dutch to ultimately rally their forces to defeat the French challenge both at home and in the colonies. The demands of the 1672 war against the Dutch prevented the French from supporting their colonists in the East Indies just when they needed it, and Colbert's dreams of colonial power were shattered.

Nevertheless, despite not achieving his aim of conquering the Netherlands, Louis, through the clever diplomacy in which he delighted, had still managed to emerge from a tricky stalemate situation as master of Europe. The dazzling siege victories masked the huge cost and underlying contradictions that were already causing financial problems and would ultimately lead to military humiliation and economic disaster.

Religion and the new science

The new science was embraced by Louis XIV and he made a great display of attracting scholars such as Cassini and Huygens to his court and providing pensions to those adherents of the new science such as Viviani who were recommended as being

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[33] Ames, *Colbert*, 177. Carlo M Cipolla, *Guns and Sails* (London: Collins, 1965), 67, describes French private enterprise as being 'conspicuous by its inertia'.

of particular merit. Tied to the church though he was, he was not prepared to allow ecclesiastical dictate to interfere either with his personal enjoyment or his programme to harness the new science to the interests of the state.

Thus Louis' relationship to the Catholic Church and the Pope was a complex one that required delicate balancing of benefits and costs. Though he took pleasure in humiliating the Pope, religious homogeneity was extremely important, since it guaranteed loyalty to the state and to himself as absolute monarch. A large proportion of the artisans who had been brought into France to develop infant industries under Colbert's mercantile programme had been Protestants.[34] But harrassment of Huguenots increased in intensity culminating in the revocation of the Edict of Nantes in 1865. About half a million skilled and talented Huguenots managed to escape the persecutions initiated by François-Michel Le Tellier, Marquis de Louvois, Louis XIV's secretary of state for war, to further boost the inventive resources and prosperity of the Protestant countries of Europe. The revocation of the Edict of Nantes could be seen as representing the final nail in the coffin of Colbert's mercantilist programme.

Military culture dominated French society, and France led Europe in military innovation during this period. Colbert was at the centre of an apparatus that drew together military leaders, engineers, innovators, mathematicians and scientists, and many, such as Blondel, were all of these rolled into one. *L'Art de Jetter les Bombes* was one of the products of his programme of reform and modernisation, and I will now examine the text in detail.

[34] Cronin, *Louis XIV,* 265.

L'Art de Jetter les Bombes

The frontispieces of the two editions of *L'Art* place it within the context of the importance of siege warfare in the military conquests of this period that I have discussed, and in particular of the contemporary interest in the use of bombs and mortars in this type of warfare. The frontispiece of the second edition graphically depicts mortars being used in a bombardment from the sea. It seems likely this was an attempt to capitalise of the renown of the recent successful French bombardment of Algiers (and the fears of the non-French readers).


L' ART
DE JETTER
LES BOMBES.
PAR MONSIEUR BLONDEL MARECHAL de Camp aux Armées du Roy, & cy-devant Maître de Mathematique de Monseigneur le Dauphin.

A Paris.
Chez { L'AUTHEUR au Faux-bourg S. Germain ruë Jacob, au coin de celle de S. Benoist. Et NICOLAS LANGLOIS ruë S. Jâques à la Victoire.
M. DC. LXXXIII.
AVEC PRIVILEGE DU ROY.

Figure 10. Frontispieces of 1683 edition and the 1686 German edition (which has the same frontispiece as the 1685 French edition). The latter illustrates the use of mortars in a sea bombardment. The German edition included Blondel's *Nouvelle Manière de Fortifier les Places.*

As I explained in the previous chapter, Galileo made the distinction between the 'supernatural' speed of a cannon shot and the much slower speed of mortar shot, which he argued complied much more closely to the predictions of his parabolic theory. Although Blondel refers to both, as I will show, the title of the work, the frontispiece, the fascination he shows for the invention of other instruments for the projection of missiles, suggest that he considered it was in mortar fire that he was most confident of the practical applicability of the theory.

The printer's address to the reader explains that although Blondel presented the manuscript to Louis XIV in 1675, since France was at war with the most powerful nations in Europe, he would not allow it to be placed in the public domain for France's enemies to make use of it. This also applied to Blondel's *La Nouvelle Maniere de Fortifier les Places*, the publication of which was also held back until 1683.

The printer also provides a guide to the text. He explains that the book is divided into four parts. The first is an historical account of what has been written on the subject of bombs and the ranges of cannon up to the present. The second teaches the diverse practices on the subject particularly for throwing bombs in all sorts of positions of the mortar by the use of sines, instruments, tables and the compass of proportion. The third part is pure theory and demonstrates in depth all that has been said in the practical section. The fourth (final) part provides answers to all the objections that might be made, both to the theory and to the practice.[35]

Blondel, in his dedication writes that it is perhaps somewhat inappropriate to offer a treatise on the art of launching bombs when Europe was at last at peace and when the science of artillery is only required for the production of fireworks of celebration, but that he hoped it would nevertheless please the king to see the rules of an art that had served him so usefully in his conquests and victories. He hopes the king will approve his purpose of preventing such a noble art from perishing, in

[35] Blondel, *L'Art* (1685), 'L'Imprimeur au Lecteur', unpaginated.

reducing it to the certain rules of mathematics, and providing the opportunity for pupils to perfect it. Indeed, it is in times of peace that one should study the profession of war rather than wait until the time when it is needed in practice.

In part one Blondel provides a critique of the false opinions on the trajectory that existed before Galileo. He writes engagingly, giving first an account of the origin and use of bombs and including anecdotes about the English military engineer Francis Malthus who was brought to France by the late king to train his gunners, and of whom Blondel may well have had personal knowledge from his own military experience. Of Malthus he concludes that:

> The whole of his science was pure experience. There was no knowledge of mathematics, nor of any other science that might provide knowledge of the motion of bombs and the curve that it describes in its passage through the air, or of the difference of their ranges following the difference in their elevations. The mortar was pointed randomly and gropingly, or rather by the estimate of the length of the place you wanted to throw the bomb, after which you gave it more or less elevation.[36]

He dispenses with the theories of a comprehensive list of contributors to the art, beginning with Tartaglia, and including, amongst others, the 'subtle and ingenious', but erroneous rules and tables of Diego Uffano, which he meticulously exposes as containing errors and inconsistencies even when taken on their own terms.[37]

The second part is given over to the practice of shooting according to Galileo's theory. Blondel explains that because the theory is quite difficult, rather than befuddle the minds of those whose main purpose is the use of the theory, it is better to teach first the practice, and then to explain the reasons and foundations.[38]

He explains that this practice was mostly invented by Galileo, principal mathematician of the Grand Duke of Tuscany, and by Torricelli his disciple, who explained that in order to know the different ranges of a piece of artillery or mortar at

[36] Blondel, *L'Art* (1685), 7.
[37] *ibid.,* 22.
[38] *ibid.,* 76-7.

all its elevations, it was necessary first to make an exact trial, firing the piece or mortar at a well-known angle with the greatest possible precision to provide a certain foundation for all the others, since from a single sure and faithful experience, one gains knowledge of all the other effects.[39]

He explains that if you want to know the range of your piece at any other elevation, the sine of double the angle of elevation of the angle used for your trial (the first elevation) must be in the same ratio to the sine of double the required angle, as the range of the known angle (the first range) is to the range of the angle required.

He then provides, in textbook fashion, the first of many numerical examples in the book: if for an angle of 30 degrees the range is found by experiment to be 1000 *toises (*or any other unit), to find the range of the same piece with the same charge, elevated to 45 degrees, it is necessary to take the sine of the angle of 60 degrees (8660) and make this the first term in the rule of three, of which the second term is the sine of 90 degrees (10000). The third term in the rule of three is the first range, 1000 *toises.* If 8600 gives 10000, what will 1000 give me? The answer is about 1155 *toises* for the required range of the piece at an angle of 45 degrees. For angles more than 45 degrees you just have to take the sine of double the angle of its complement, for example if you want the range at 50 degrees you have to take the sine of double 40 degrees (i.e. sine 80 degrees).[40] To make the task even more straightforward Blondel provides a simple table of sines for double the angle for all possible angles.[41]

Contrary to the impression given by the length and complexity of the demonstrations in the book, the section in fact explains very clearly with numerical examples how to find the range using a widely-known arithmetic technique, the rule of three, required for many applications by artisans and military engineers. Blondel

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39 *ibid.,*,76.
40 *ibid.,* 77-8.
41 *ibid.,* 81.

was careful to put the practical technique first before he dealt with the theory, and there seems no reason to think that a gunner with good arithmetical skills could not be trained to use this method without having any knowledge of the theory of the trajectory whatsoever. It was expected that any gunner worth his salt should be able to use the rule of three in the numerous measuring and scaling operations involved in the practice of gunnery, and even Nathaniel Nye, whose English Civil War manual aimed to reduce the art of gunnery to its simplest techniques, considered the ability to use the rule of three to be a basic requirement for a good gunner, though tables and other rule of thumb methods were provided to make the task easier for those who struggled with arithmetic.[42]

However, despite the efforts of Blondel to provide simple rules and numerical examples, 'simplicity' is a relative term, and here the difference between Blondel's vision for the future training of bombardiers and reality is evident. Michel Blay observed that in the second part his book Blondel explains in a very clear and often new manner the usage of tables of sines as well as instruments for aiming, and that after a trial shot, a simple rule of three allows calculation of the new angle of fire to reach whatever target.[43] But nevertheless he suggests the reason Blondel's work was rejected by practical artillerists was that he did not provide tables associating angles with ranges.

Blay notes that Bélidor, in his *Le bombardier français ou, nouvelle méthode de jeter les bombes avec précision* (Paris: L'imprimerie royale, 1731) used the same method as Blondel, but almost the whole book was taken up with tables for firing.[44] Far from being a critic of Blondel, Bélidor reproached Blondel's detractors for not

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[42] Nathaniel Nye, *The Art of Gunnery* (London: William Leake, 1647), 41: 'He ought to have skill in Arithmetick, to Adde, Subtract, Multiply, Divide, to work any conclusion by the single or double Rule of Three…'.
[43] Michel Blay, 'Le Developpement de la Balistique et la Pratique du Jet des Bombes en France a la Mort de Colbert' in *De la Mort de Colbert à la Révocation de l'Edit de Nantes: Un Monde Nouveau?: Actes du XIV colloque du C.M.R. 17 (janvier 1984),* 38
[44] Blay 'Le Developpement', 48.

appreciating him, saying rather that Blondel and the other members of the Academy who worked on this subject had not been understood by those who were put off by the abstract nature of the subject and believed that practice was all that was necessary, since there is no time for complicated calculations in the middle of a battle.[45]

Unlike Blondel, Bélidor was careful to put the corresponding mathematical theory in his *Nouveau cours de mathématiques* (1725) and devoted *Le bombardier français* solely to practical firing tables. He was delighted to have made an extremely useful discovery that he believed no-one else had noticed before; according to the Galilean theory, the range at forty five degrees is exactly double that at fifteen degrees.[46] Bélidor's attempts to reform artillery challenged the existing social hierarchy as well as the traditions of bombardiers, attracting powerful admirers but also enemies. His difficulties in convincing traditionalists of the benefit of the Galilean tables is shown by the fact that *Le bombardier français* contains a signed witness statement from the officers of cannoniers, bombardiers and artillery at the École de la Fère, where Bélidor worked as professor of artillery, confirming their accuracy in practical trials.[47] Bélidor concluded from his trials that if the mortar is charged carefully, a reasonable level of accuracy can be achieved using the tables for distances of twenty to four and five hundred *toises,* which, he suggested, was all you needed in a siege.[48]

In the second book of the practical section, Blondel describes with worked examples the use of three gunners' quadrants modified for the use of Galileo's theory. The first is Torricelli's modification of Tartaglia's *squadra,* the second is Torricelli's

[45] Bernard Forest de Bélidor, *Le Bombardier français, ou, nouvelle méthode pour jeter des bombes avec précision* (Amsterdam: au depens de la Compagnie, 1734), 4-5.
[46] *ibid*., 6.
[47] *ibid*., 15.
[48] *ibid*., 11.

half–circle model, and the third is another half circle instrument that could be used without a table of sines.

The third book of part two explains the practice of firing when the target is not on the same level as the cannon or mortar, a situation more common than otherwise in a siege and so it is not surprising that much attention was given to this problem. Again many examples are given for the solution of a range of problems and using a number of different instruments, including the construction and use of a universal instrument that could be used in the same way for all sorts of elevations, on a level or otherwise.[49] In all cases Blondel's use of numerical examples reduces the technique to set of procedures to be followed.

Part three is taken up with theory and demonstrations. Blondel introduces this section by explaining that Galileo was the first to reason correctly on motion and this was the foundation on which the practical rules had been based. He begins by explaining that there are two types of motion. The first is equal and uniform, by which a mobile body travels equal spaces in equal times, which Galileo considered to be the natural motion of bodies that move in a circle, such as the celestial bodies. Then remarkably Blondel says that rather than spend time explaining certain misconceptions regarding second kind of motion (the law of fall), that the speed increases according to space not time, he will content himself with describing two thoughts that Galileo had on this subject, of which the first was described in his dialogues on the system of the world, and the other, that seemed to be his true sentiment, is explained in his discourse on mechanics.[50]

But why Blondel chose to complicate matters by focusing first on Galileo's thoughts in the *Dialogue,* is a bit of a puzzle. It relates to the trajectory of an object

[49] Blondel, *L'Art* (1685), 164-170.
[50] Blondel, *L'Art*, 178-9.

dropped from a tower on to a hypothetically rotating earth and its 'admirable consequences' (*suites admirables)*. I have discussed the significance of this question in the last chapter with regard to Riccioli's *Almagestum Novum,* and its importance in discussions between Hooke and Newton. But why would Blondel have a long discussion on this question in a book that purported to be about launching bombs?

The inclusion of Galileo's first thought suggests that his intention in writing *L'Art* was not just about practice but it was designed to treat the question of motion in its wider philosophical context, as Bayle recognised. Thus he wanted to provide his own exposition on a question that had animated all the most important mathematicians and natural philosophers of the day. Secondly, as I noted in the last chapter, the discussion assumed a moving earth, and might have been Blondel's way of insinuating this question into his work on ballistics.[51]

Blondel explained that in Galileo's *Dialogue* Salviati discussed the line described by a body falling from the top of a tower to its base. Galileo described the motion of a body falling from the top of a tower with uniformly accelerated motion whilst also following the circular uniform motion of the earth, and suggests that the curve it describes would be circular, or very close to circular.[52]

Blondel illustrated his demonstration with a diagram showing a cross-section of a turning earth, while Galileo had only drawn one quadrant in his *Dialogue*. He explains that a weight falling from point A with a movement composed of its own gravity perpendicularly to the centre of the earth C and that which carries it around

[51] See Koyré, 'A Documentary History' and Edward Grant 'In Defense of the Earth's Centrality and Immobility…' *Transactions of the American Phoilosophical Society,* 74 (1984), 1-69. See also Bertoloni Meli, *Thinking with Objects,* 202: ' Blondel argued that Galileo's fancy appeared very plausible and led to results indistinguishably close to the odd-number rule.

[52] Galileo, *Two World Systems,* trans. Stillman Drake, 164-167.

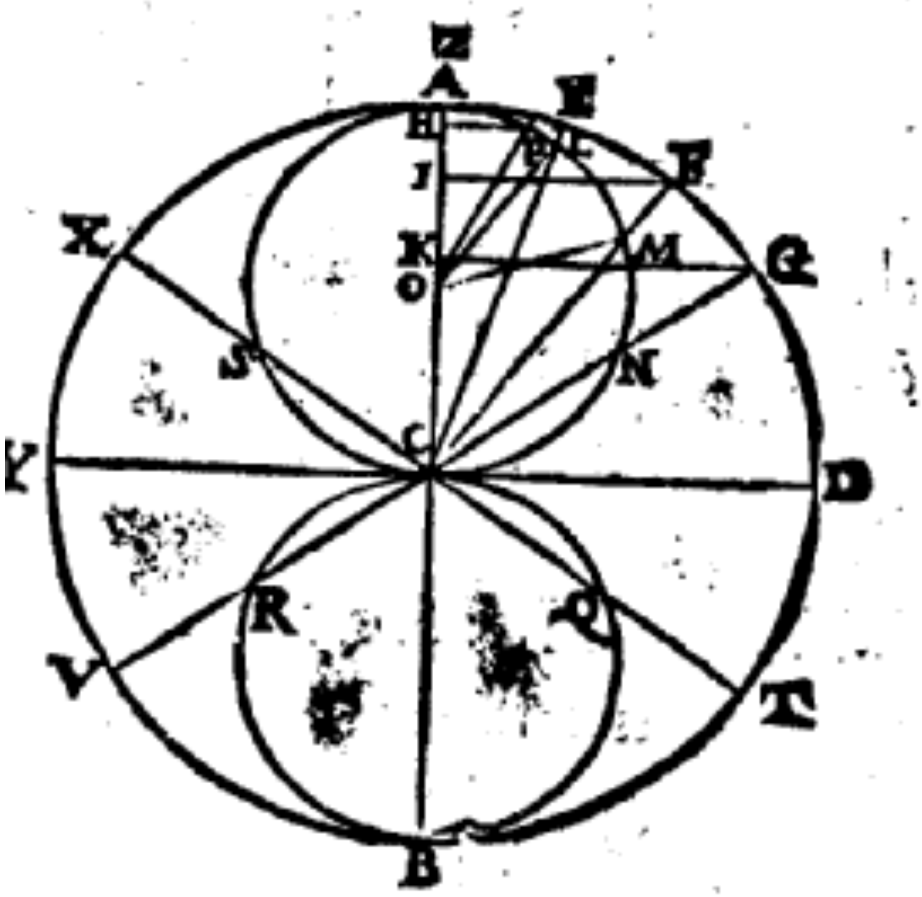

Figure 11. Blondel's illustration, adapted from that of Galileo in the *Two World Systems*, of the effect of an object dropped onto a rotating earth.[53]

uniformly by the daily motion of the earth describes the curve ALMN. He explains that the acceleration in speed of the fall of bodies is conjectured to be as the *sinus versus* (versed sine) of the arcs of the equator of the earth, as it moves on its own centre in 24 hours. The versed sines are represented by the distances AH, AI, AK, representing the distance covered as, due to the earth's circular motion, the mobile Z moves uniformly from A to E to F etc. Blondel then draws out the admirable consequences of this thought. The marvellous result is that in 24 hours, the body ends in exactly the same position as if it had stayed at rest at the first point of its fall. It would take exactly six hours to arrive at the centre, six hours to reach the opposite side of the globe, six hours to return back to the centre and six hours to return to where it started. Thus the mobile travels twice the diameter of the earth, there and

[53] For Galileo's version see Galileo, *Two World Systems* trans. Drake, 165.

back, in 24 hours, an idea which clearly delights Blondel, even if the theory is flawed.[54] Not only is the largest section of the book taken up with theory and demonstrations which are not necessary to the practical aim implied by the title of the book, but here we have a section which is superfluous to the demonstration of the theory of fall and the parabolic trajectory given in the next section.

The rest of the theoretical part provides a faithful, detailed and clear exposition of Galileo's law of fall and the derivation of the parabolic trajectory, followed by demonstrations of the practical section including the theory supporting the instruments and discussion of the contributions of other members of the French Academy of Science.

One of the most important sections of book three is Blondel's extension of Torricelli's work on firing at a target that is below the horizon. We will see in the next chapter that this problem is also taken up and developed by Edmund Halley. Blondel explains that the Jesuit Milliet de Challes was right when he said that the need for the use of Torricelli's proposition was not as common as its converse, that you are much more likely to want to know the elevation required to hit a target above or below the horizon when you already know the range, rather than seeking the range from a given elevation. Here we see clearly how the practical demands of war set the direction of theoretical investigation.[55]

Blondel was not satisfied with de Challes' solution since it was achieved 'gropingly, and not very geometrically' and he decided to work on a solution himself. He was able to find a solution by analysis, but sought the help of the Academy in

[54] Blondel, *L'Art*, 186
[55] *ibid*., 280. See also Blay 'Le Developpement', 39.

finding a more elegant solution, and he presents the solutions of Buot, Roemer and La Hire, not his own.[56]

Finally, Blondel provides a demonstration of the universal instrument described at the end of the second, practical part, derived from the method of Jean Cassini. Cassini, Blondel writes, has given the resolution of the whole doctrine of projection of a mobile by a single proposition, showing that in all cases, there are three lines that are continuously proportional which he calls the line of equality, that of impulsion, and that of the respective fall. He finally explains how, by the use of an additional rule, it is possible to use the universal instrument without having to resort to the rule of three. Here we see the development of theory driving the invention of improved instruments designed to take all the calculation out of the art of firing bombs.[57]

In part four Blondel anticipates the criticisms both of the theory and its applicability to practice. His approach is to play devil's advocate; he presents as strong a case for his opponents as possible and then demolishes each argument one by one. It will be seen that the objections that Blondel sets up are essentially the same objections to Galileo's theory that Torricelli and Gassendi had to answer, which we considered in the previous chapter.

The first objection to the theory is that the horizontal line is not straight, and the perpendiculars are not parallel (because of the shape of the earth). Thus the line of fire cannot be parabolic. The second objection is that the force impressed on a mobile is not perpetual, equal and uniform; and even if this were true in the case of zero resistance, the presence of air resistance will always mean that the shape of the trajectory is very far from being a parabola. The third objection is that the resistance

[56] *ibid*., 283.
[57] *ibid*., 356.

of the air alters the proportions of motion caused by the weight of the shot. Following Galileo's terminology, he explains that the speed that an external force impresses upon a body using gunpowder can be called supernatural, since it is greater than any speed that can possibly be reached in natural fall, where the increase in speed of fall causes an increase in the force of air resistance such that at some point a maximum velocity is reached.[58] The fourth objection is that two motions cannot come together without experiencing some alteration. As we saw in the last chapter, this was one of Riccioli's main conceptual difficulties. The fifth objection is that the spaces travelled are not as the squares of the time; that the shape would be very different if the spaces were as the *sinus versus* or some other proportion; and since there has not up to now been a demonstration of the hypothesis, there is room for doubt, especially since different opinions have been advanced by men of great reputation.[59]

Finally, the sixth objection is that the theory is contrary to experience. Blondel admits that it is difficult, if not impossible, to speak scientifically and with certitude of all the effects of resistance in general, because of their almost infinite irregularity, acting on mobiles in a thousand different ways, not only according to their weight, material, shape and direction, speed and the duration of their motion, and the spaces they travel, but also because of the variations in rarity and density of the media, their hardness or softness, toughness, weight, configuration, spring, configuration, situation, state of rest or agitation, the facility or difficulty they have in taking the impression of external causes and if they retain it for a long time or lose it quickly.

He continues that though it is not possible to make a complete science of this subject, with round heavy shot and where the speeds are not excessive, such as with

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[58] *ibid.*, 376-7.
[59] *ibid.*, 381.

bombs projected by mortars, and in a rare medium such as is the air, this resistance is minimised. In this case the trajectory resembles a parabola. Indeed, he goes so far as to say that the alteration from air resistance is practically insensible.[60] He also argues that since the calculations are taken from a test that was made with the utmost precision and care, whatever influence of the air there is on the test will be incorporated into the calculations, though there will be some variation between shots at complementary angles because the higher ones will have further to travel in the air.

Blondel derives much of his argumentation with regard to the fourth objection from Galileo's *Two New Sciences*. However, he does provide an interesting reflection in the process of dealing with this question. It is about the fact that the path of the ball does not always follow the direction of the piece. He suggests that it appears that when the charge is lit it does not provide one sudden impulsion, but that it is probable that the vehemence of this impression is communicated by an infinity of percussions that the little grains of lighted powder make, not just directly on to the ball, but by hitting the sides of the soul (*l'âme*) of the piece; there would be no thickness of metal that could resist if it all went off at once in the same place.[61] Besides, if this happened, the length of the piece would make no difference to the range, since the bullet, having once received all the impressed force, would always travel the same distance whatever the length of the piece, which is contrary to experience.

Blondel suggests this could serve as a rule for the relationship between the length of the cannon to its diameter, which should be such that the powder of the charge should be completely used at the moment that it leaves the cannon. If it is too

[60] *ibid*., 394.

[61] The soul of the piece is the cavity enclosed by the barrel of the gun.

short, a good part of the powder leaves with the bullet and is wasted, which often happens with pieces that are heated or where the piece is longer than it should be.[62]

He suggests that since the long cannon have greater range than shorter, one can augment the short cannon by digging little spiral canals in the soul (cavity), while pushing with violence onto the powder a lead ball slightly bigger than the diameter of the cannon, so that on exiting it is constrained by the contour of the canals of the spiral. By this means the ball will be taking more time to exit the cannon even though it is short, than it would take from another cannon as long as the extended canals, and so would provide the same force. Blondel appears here to be an early proponent of rifling.[63] Further, he suggests one could maximise the force of short cannon by digging round the base of the breech in the form of a bell or cloche.

Returning to the subject of the fourth objection, Blondel argues that a bullet will never shoot absolutely horizontally because of the action of the powder, which is lifted insensibly towards the top of the mouth and grazes it on emerging from the mouth of the piece. The effect of this is shown in well-used cannon by the formation of a noticeable groove. These reflections do not come across as the musings of an armchair theorist. Blondel is interested in developing a better understanding of what happens to the gunpowder and the bullet inside the gun and how this affects the force and the trajectory, making suggestions for improving gun technology to take these problems into consideration. We will see in the next chapter how Edmund Halley was similarly occupied with finding methods to optimise the use of powder and materials.

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[62] It should be noted that such technical questions had long been debated and the French emphasis on rationalisation and optimising efficiency by approaching such questions scientifically and quantitatively would have made this a particularly important topic for consideration. For example, to manufacture a cannon with longer barrel than necessary was both a waste of metal and added unnecessary weight. Minimising the use of powder was also important.

[63] Benjamin Robins in the middle of the next century was an early advocate of rifling (where helical grooves are cut out of the gun barrel). According to Mark Denny, *Their Arrows,* 52-54, rifling was known in Europe from the sixteenth century, but it was only in the nineteenth century that the technical obstacles to its use were overcome. See also Steele, 'Muskets', 364: Benjamin Robins dismissed arguments such as the one given by Blondel for the improved accuracy of rifled guns.

As a result of these problems Blondel notes that the predictions of the theory of equal times of fall from the same height may not comply with the results of trials. And there are many other effects such as differences in air pressure at different heights, as has been shown by the 'admirable experiences of the barometer'.[64]

With regard to the law of fall Blondel presents Galileo's arguments showing that the theory that the speed increases in proportion to space leads to absurd conclusions and he particularly targets the Jesuit Le Cazre, a major opponent of Galileo's law of fall and opponent of Gassendi, as I have noted in the previous chapter.[65] He defends Galileo's argument that a falling body acquires at every moment equal degrees of speed, maintaining that Galileo's theory is the only one that has all the conditions necessary for a physical theory, i.e. that it does not lead to absurdities and conforms to the ordinary laws of nature, being simple, uniform, easy, and because it explains everything that happens in the accelerated motion of falling bodies.[66]

He then describes numerous experiments that support the theory including Galileo's inclined plane experiment from the *Two New Sciences*, followed by Baliani's and Huygens' reasoning on the matter. He describes many experiments made at the Académie Royale des Sciences to investigate the trajectory using jets of water and mercury. Not only does he provide a graphic representation of the perfect parabolas described by the mercury jets, but he also provides a table comparing results for the heights and ranges for the experiments alongside the results by calculation using the tables of Galileo and Torricelli, which correspond almost exactly. Just in case there were any doubting Thomases, Blondel emphasised that the

[64] Blondel, *L'Art*, 435: 'les experiences admirables du Barometre'.
[65] *ibid*., 442 and 458.
[66] *ibid*., 451.

experiment was repeated many times before the Academy, also at the observatory, and even in the presence of the Dauphin.[67]

Finally, in the fourth book of this fourth part of *L'Art*, Blondel deals with the practical difficulties which are 'ordinarily given either too much or too little consideration'. The first objection is that theory is not necessary for the practice of war; professional soldiers, even officers, and particularly those who have not been cultivated in the art of letters in their youth, are strongly of the opinion that it is a profession that requires neither books nor rules; that those who only rely on theory will be prevented from taking part in the action, and that mathematics often only serves as presumption (*présomption*).[68]

Blondel replies to this first objection that it is true that practice is necessary in all the arts, particularly that of war, where one only becomes expert after long experience and observations taken with great care and judgement. Those who only have theory will be at a disadvantage at first, but one has to distinguish the time and place when theory can usefully be applied. It is very wrong to say that theory is useless, since there is nothing more certain that judiciously applied practice would never contribute better to perfect a man of war than when it is founded on the solid study of precepts, noting that the King himself wanted no time to be wasted in teaching the Dauphin all that could be learnt of the theory of this subject, and even spent time correcting the mistakes of his son in his designs of fortifications, which he would not have done if he had not believed this knowledge would be useful to him in the future.[69]

It is easy to dismiss this appeal to authority in the place of substantial arguments but at the time Blondel was writing, the French absolutist state was at its

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[67] *ibid*., 501.
[68] *ibid*., 503.
[69] *ibid*., 509-10.

zenith and the king resplendent in the flush of the success and glory of his expansionist military conquests as well as from his prestige from support for science and the arts, especially of architecture. Louis XIV was extremely accomplished in the military arts and adored by subjects such as Blondel, Colbert and Vauban whom he astutely trusted and promoted for their complete loyalty and ability.

War provided legitimacy for the study of mathematics; Blondel contends that most people of quality currently work in that area of mathematics that principally relates to war. Knowledge of mathematics enabled the King's ministers to root out the charlatans who have had such a high reputation in the past, and distinguish them from the solid knowledge of men of merit.[70]

Blondel contends that the ability of those who currently work for the King in batteries, particularly in that of bombs, does not contradict his point about theory, because while it is true that they have brought this art to a high degree of perfection by the sole force of their genius and great application, it is also the case that they would have reached this level of capacity more easily with the aid of theory. Their knowledge ends with the knowledge of particulars, only discovered through observation, which they would however have learnt more assuredly from the rules that this doctrine teaches. Further, since it is not always easy to gain sufficient practice in this subject, rules that enable the student to reach perfection quickly can be helpful to the perfection of the art.[71]

Finding evidence in support of Blondel's contention that a knowledge of theory offers a short-cut to perfection in practice is not easy. But Brett Steele has suggested that Napoleon's study of the advanced mathematical ballistics of Benjamin Robins and Leonhard Euler contributed to his success as a military commander. In the

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[70] *ibid*., 510.
[71] *ibid*., 511.

Siege of Toulon, which thrust Napoleon into national prominence, 'his scientific understanding of cannon and mortar fire was an important element in the development of his victorious strategy, which required precise information regarding his artillery's effectiveness at different ranges'. Steele suggests that the confidence exhibited by such a young artillery officer with little combat experience 'certainly came in part from his theoretical understanding of ballistics.'[72]

Blondel's second objection comes from those who admit that the rules are correct in theory, but argue that they cannot succeed in practice, because the irregularities and inequalities in matter to which the theory must be applied, inevitably corrupt even the most well-conceived and carefully executed operations.[73] He concedes that it is difficult to comprehend how the impression that a bullet or bomb receives with each shot can always be the same, which is essential if we are going to use these rules. Or that the force will not change with the use of so many different powders, both in their force and their effect. Who does not know that the same powder, i.e. that which is made with the same composition, acts in proportion to whether it is more or less granulated, whether it is damp or dry, new or old? How is it possible to guarantee that the weight of charge is always exactly the same? What difference in the range is caused simply by the manner of charging the piece or mortar, either loosely (*à nud*) or packed tightly using a *tampon*, or if the powder is strong or somewhat *battuë,* if the piece is hot or cold, if it recoils or retains its position, and who can say whether the piece has been pointed with the necessary precision, no matter how much care has been taken and no matter how accurate your instruments?[74]

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[72] Steele 'Muskets', 371-2.
[73] Blondel, *L'Art*, 504
[74] *ibid*., 506.

What is more, how can one be sure that the piece or mortar is absolutely straight, equal and well rounded inside? That the bomb fits perfectly, that it is perfectly round? That the line of the axis is exactly in the centre, so that the action of the powder is evenly distributed around its circumference, and does not produce more impression on one side than another? That the bombs are all the same size and weight. That the level for the battery is equally stable and level? That the mortar is well mounted on its carriage so that the axis of its trunnions cross the soul and are always parallel to the horizon; and many other particulars of this nature, the least of which is capable of altering the direction of the ball and of rendering useless all the ingenious precautions of theory?[75]

In answer to this second objection, Blondel admits to all the said difficulties and perhaps an infinity of others of which we are not even aware, and which cause the bizarre and surprising effects that we often see in artillery. But he maintains that these obstacles can be sufficiently surmounted by those who dedicate themselves to this *metier* and they do not preclude the use of the theory.[76]

He continues that those who presently serve in batteries of bombs, who only act from the knowledge they have from experience, encounter exactly the same difficulties and obstacles, but this does not stop them from firing accurately. That is to say, they know from practice the means of knowing and overcoming those problems, and of employing the rules that they have learnt from long experience, by means of which they can send the bomb wherever they want. Why then, from their example, can we not say that those who follow the theory can also by experience, understand and correct the faults, before applying the theory?[77]

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[75] *ibid*., 507.
[76] *ibid*., 512.
[77] *ibid*., 513.

He continues that, as with all arts, simple knowledge of precepts is not sufficient. For perfection it is necessary to apply it to the subject, and in this application one finds all sorts of resistance and obstinacy of matter, which makes for a thousand obstacles and impediments that can only be overcome by practice and experience.[78]

He makes a comparison with music and architecture. What benefit would one get from the theory of music if one did not accustom oneself by long practice to singing the notes well, training the ear to the accuracies of consonances, to be able to judge the good or bad that their combination makes? The science of the orders of architecture and the perfect knowledge of its precepts can be of no great value to the architect, if he does not know by practice how he must choose the ornaments that are most fitting for a specific building. What are the materials that he should use, what mixture must he make to provide strength? What is the quality of the terrain on which he must build, and what measures must he take with the foundations to make sure they are solid? And a thousand other particulars that render edifices imperfect and defective when they are ignored or neglected.[79] He waxes lyrical about the great achievements of man in music and architecture and in overcoming the perils of exploration of new lands that have been accomplished through the considerable advances

> provided by the knowledge of theory, which sets out the practical, and perfects experience; which all together make for a happy outcome to most of the most present and pressing dangers.[80]

Blondel explains the advantages that it is hoped will be achieved from the institution of the company of bombardiers for making experiments on the projection of bombs

[78] *ibid*., 514.
[79] *ibid*., 515
[80] *ibid*., 516: '…que l'on tire de la science de la Théorie, qui dresse la pratique & perfectionne l'expérience; qui toutes ensemble font heureusement prendre le bon parti dans les périls les plus présens & les plus pressants'. My translation.

which he hopes will bring the art to perfection. This will be the case as long as they carry out exact and faithful tests at all elevations, carefully noting the lengths of ranges according to the different angles, without correcting them according to false principles as they seem to have done in the past if their tables are anything to go by.

Above all they have to practise the precise and correct use of elevations, so that by undertaking a series of tests they can make rules for the difference in the powder, determining whether the lengths of jets made with different powders at the same elevation are proportional to the different points that they reach in the same *éprouvette*.[81] That they learn to judge the difference of the ranges following a different quantity of charge, and following different ways of charging with or without a tampon, with powder more or less fine, or where the mortar is more or less heated.

He explains that these experiences, if made well and repeated a number of times, will bring great light to the security of the application of the rules of the theory, producing marvellous effects for the art, of which the difficulties, though they appear to be numerous, are not of great consequence. After all, one does not expect the shots of bombardiers to always have the precision of mathematical tables; if they differ by only a few *pies,* this is only to be expected when dealing with matter.

Conclusion

L'Art de Jetter les Bombes should be seen firstly, within the context of the wider cultural and educational programme of the French state; secondly, as a persuasive explanatory textbook and work of propaganda that for the first time gave state legitimacy to Galileo's new science; and thirdly in the context of the spectacular

81 An *éprouvette* is an apparatus for testing the strength of gunpowder.

French successes in siege warfare and especially the conjunctural importance of mortar fire in this period, to which it was hoped the theory might prove applicable.

What is absolutely clear from my examination of *L'art* is that Blondel did not see abstract theory as a substitute for practical knowledge, but only as a means of guiding it to perfection. He envisaged the theory being applicable within the context of a vision of technical rationalisation, education and training that became increasingly unrealistic as military and financial pressures grew, but as I have noted, might not have seemed impossible in the heady days when *L'Art* was written.

L'Art … is a bellicose work that appears to glory in the destructive power of artillery, couched in the characteristic double-speak of 'peacemaking'. In this it is very much a work that must be taken in the context of the hubris of the time. Blondel, trained from an early age as a soldier, put loyalty to Louis and the glory of the French state above all else. He was destined not to experience the cognitive dissonance that military defeat and humiliation can bring, or the pacifist sentiment it is wont to engender.

The two successors who embodied Blondel's programme in the eighteenth century were Bernard Forest de Bélidor and his pupil Jean-Baptiste Vaquette de Gribeauval, who, against his traditionalist rival Florent-Jean de Vallière, rationalized the production of French cannon, producing lighter, more maneuverable cannon that nevertheless did not compromise on range. As I have noted, though Blondel had his critics who believed him to be hopelessly out of touch with the realities of warfare, Bélidor was not one of them. Like Pierre Bayle, whom I quote at the beginning of this chapter, he understood Blondel's wider agenda and his integrative programme; that it was about both causes *and* utility.

Brett Steele has contended that the technological limitations to the application of the Galilean theory, 'far from weakening Galileo's influence on early modern gunnery, inspired efforts to improve the consistency of artillery fire in order to take full advantage of his theory's power.'[82] This was Blondel's vision, which was finally achieved with the eighteenth-century military investment in mechanical uniformity and interchangeable parts, and which culminated in the French artillery reforms of Jean Baptiste Vaquette de Gribeauville, instituted after France's humiliating defeat in the Seven Years' War (1756-63).[83]

Blondel was not an exceptionally gifted theoretical or applied mathematician if one is comparing his talents with contemporary mathematicians such as Huygens. His strength was in his ability to explain engagingly the discoveries of others rather than in the originality of his own work. Indeed, *L'Art de Jetter les bombes* is noteworthy for Blondel's generosity in crediting the work of others rather than claiming any merit for himself. His aim was not to blow his own trumpet but to promote understanding of the new science and to present arguments against its detractors. That he should have felt the need to do so reflects the equivocation of the French absolutist state before the challenge that the new science represented. Presenting Galilean science in the context of a work on ballistics was a means of overcoming that tension. It is for these reasons that it deserves recognition, rather than being dismissed as just another gunnery manual with theoretical pretensions.

As the next chapter will show, in a Europe that was becoming increasingly dominated by the struggle for world supremacy through the control of colonies and trade, members of the Royal Society, and in particular Edmond Halley, would take very seriously the efforts of the French to harness the power of mathematics to serve

[82] Steele, 'Muskets', 352.
[83] *ibid*., 352 and 356.

the art of war. Despite his recognition of the limitations of Galileo’s theory, this encouraged Halley to investigate how it could be used to improve the accuracy and efficiency of firing mortars.

Chapter seven

Gunnery and the Royal Society:

the establishment of the new science

Introduction

In this chapter I will show how members of the Royal Society and their wider network of contacts attempted to carry out a programme of practical experiments and trials related to gunnery, as well as developing ballistic theory. The main focus of my chapter will be Edmond Halley since he was both central to discussions on the development of theory by Newton and others who were investigating mathematically the effect of air resistance on the trajectory, but he also did not give up on the possibility of using the results of Galileo's theory, and practical experiments with guns, to achieve improvements that would be useful to immediate practice.

Halley is most well known for his contributions to astronomy. In his later years he succeeded Flamsteed as Astronomer Royal, and prior to this he had occupied the Chair of Geometry at Oxford. As Clerk to the Royal Society he revived its journal, the *Philosophical Transactions,* and was crucial to its continued survival at a time when it was wracked by internal tensions and lack of direction. It is less well known that he became a highly respected sea captain of exceptional qualities who put his knowledge of astronomy into practice to successfully navigate his own ship, the *Paramore Pink*, on his Atlantic voyages of 1698 and 1699.[1]

[1] The *Paramour Pink* was built especially for Halley's voyage. There was some resentment from the crew at Halley's perceived lack of practical experience as a seaman and captain. He had to cut his first voyage short because of problems with insubordination, particularly from his first lieutenant, who had a grudge against him, and simply countermanded his orders with regard to navigation of the ship. Halley was finally forced to place his first officer under arrest and took over total responsibility for navigation on the return journey from Newfoundland.My source for Halley's biographical details are the 'Memoir of Dr. Edmond Halley' in Eugene Fairfield McPike (ed.) *Correspondence and Papers of Edmond Halley* (Oxford: The Clarendon Press, 1932); Colin A. Ronan, *Edmond*

These voyages had been undertaken at the express wish of the king, who had been informed of Halley's 'ingenious Theory of the Magnetic Needle' and was 'desirous the variation shou'd, for the Benefit of Navigation, be carefully observed, in diverse parts of the Atlantic Ocean'. He was additionally instructed to 'attempt a discovery of what Lands lay to the South of the Western Ocean'.[2] Halley went on in 1701 to chart the English Channel, possibly using it as an opportunity to spy on the French in the run-up to war,[3] and in 1702 during the War of the Spanish Succession he was sent on an urgent secret state mission as Queen Anne's technical expert to advise the Emperor Leopold of Austria on fortification of the ports and havens of the Adriatic, where his proposals for the forts at Buccari called upon his earlier thoughts on gunnery.[4] He also invented and carried out experiments on devices to enable sea-divers to breathe under water, and obtained a patent and formed a company for salvaging wrecks.[5]

The effective use of guns never engaged Halley to the extent of problems of astronomy and navigation, but in the turbulent political times in which he was undertaking his voyages to chart the skies and the seas, and with the constant fear of attack by pirates, Halley had an interest in considering all practicalities; gunnery defence was not a trivial matter, and he put his mind to the most mundane yet crucial practical questions, such as finding a method to improve the stability of guns on board

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Halley: Genius in Eclipse (London: Madonald, 1970); and Alan Cook, *Edmond Halley: Charting the Heavens and the Seas* (Oxford: Oxford University Press, 1998).

[2] Memoir of Dr. Edmond Halley, in Eugene Fairfield McPike (ed.) *Correspondence,* 8.

[3] Halley's chart of the English Channel was published in 1702, but he also provided the Admiralty with details of how further measurements could be made and how positions inaccessible to an enemy shore could be deduced. See Colin A. Ronan, *Edmond Halley: Genius,*182 and 185.

[4] See Alan Cook, *Edmond Halley: Charting* 292-313 and 317. Ronan, on 185, notes a payment to Halley of £36 'out of the secret service'.

[5] Cook, *Edmond Halley: Charting,* 238-9 and Ronan *Edmond Halley, Genius,* 105.

ship during storms.[6] But more importantly, as I will show, for Halley all these questions were linked philosophically and mathematically.

I will analyse in detail the short treatise that Halley published in the *Philosophical Transactions* in 1686: 'A Discourse on GRAVITY, and its Properties, wherein the Descent of Heavy Bodies, and the Motion of Projects is briefly, but fully handled: Together with the Solution of a Problem of great Use in GUNNERY', and a supplement to this work that he wrote in 1695. The 'Discourse' was Halley's vehicle for preparing the public for the imminent publication of Newton's *Principia,* and he used the discussion of the problem of how to hit a target above or below the horizon as an illustration of the application of Newton's theory of gravitation.

The Royal Society

The creation of the Royal Society inaugurated a renewed interest in experiments with guns. Next to navigation, and not unrelated to it in its theoretical foundations and practical import, the promise of improvements to gunnery was probably the major incentive given to Charles II to encourage him to grant a charter to the Society.

A number of the founding members of the society, such as Prince Rupert and Sir Robert Moray, were military men with a record of loyalty to the Stuart monarchy during the Civil War and the Interregnum. Charles II's cousin, Prince Rupert, was a member of the Restoration Privy Council. He had symbolic value for the Royalist cause because he combined royal birth with a reputation for military ability gained through many years experience as a career soldier in the Thirty Years' War, and for his high-profile military leadership of the Cavaliers in the Civil War. The

[6] See: 'A Method of Enabling a Ship to Carry its Guns in Bad Weather', read to Society April 30, 1695 in MacPike *Correspondence (1932),*164.

impoverished Prince, who, according to Lorenzo Magalotti, was forced to survive on a pension of

'four thousand miserable pounds sterling a year', sought to compensate for his misfortune through the fusion of aristocratic favour with the spirit of enterprise, invention and drive for imperial expansion characteristic of the time.[7] Amongst his many inventions, he gained a patent for his secret method of treating iron guns to improve their performance, and lucrative contracts for the supply of them to the government, on the basis of the claimed benefit that his iron cannon had all the advantages of brass cannon but at a fraction of the cost.[8]

It was recognised that such technological improvements were essential to the success of any ballistic theory, and Rupert was singled out for praise in the dedication to Prince James written by Thomas Streete in's *The Genuine Use and Effects of the Gunne* (1674).[9] Streete notes that '…his Highness *Prince Rupert* having prepared a way, by which all Gunnes and their Shot may be made fit for extraordinary service; there remaineth not any thing more than is herein contained, for the compleating of the practice of this Warlike and Mathematical Art'.[10] Sigismondo Alberghtetti, in his *Nova Artilleria Veneta* (1703), also praised Prince Rupert's efforts to make technical improvements in the production of guns that would allow a better fit of the bullet to the bore of the gun.[11]

Another technical question of vital interest to anyone interested in accuracy in ballistics was the problem of recoil. One of the most well known experiments on guns

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[7] Lorenzo Magalotti, *Relazioni d'Inghilterra* (1668) translated as *Lorenzo Magalotti at the Court of Charles II* by W. E. Knowles Middleton (Waterloo, Ontario: Wilfrid Laurier University Press, 1980), 39.

[8] See Sarah Barter Bailey, *Prince Rupert's Patent Guns* (Leeds: Trustees of the Royal Armories, 2000), passim.

[9] Robert Anderson, *The Genuine Use and Effects of the Gunne* (London: Printed by J.Darby for William Berry… and Robert Morden…, 1674). Also note Thomas Venn, *Military and Maritine Discipline,* Book III, under the title 'The Compleat Gunner' (1672) included translated extracts from 'The doctrine of projects by those late famous Italian authors Galilaeus and Torricello…that the benefit of his pains might redound to the English reader that is especially delighted and exercised in the affairs of Mars'.

[10] Anderson, *The Genuine Use*, Dedication, A2v.

[11] Hall *Ballistics*, 70.

for the Royal Society, because of the account contained in Sprat's *History of the Royal Society*, was an investigation of the effects of recoil carried out by Lord Brouncker, a gifted mathematician who became first president of the Royal Society. Hall notes that the public interest of these ballistic researches 'may be judged from the places where they were made, firstly in the court of Gresham College in the presence of the Society, and later in the Tiltyard at Whitehall before Charles II and his brother.'[12]

The purpose of the experiment, according to Hall's account, was 'whether or not the recoil began before the projectile was shot from the piece, and further, if it did, whether the recoil could affect the direction of the shot's flight. Brouncker was able to answer both questions affirmatively, though he found the problem less simple than had been expected.'[13] Brouncker's programme was ambitious - his meticulous, systematic tables of results relating quantity of powder to size and direction of recoil, and his attempt to model the results algebraically, indicate that his aim was to use his experimental results as a basis for achieving mathematical precision in modelling the behaviour of the bullet inside the gun, according to the velocity of the bullet and the radius of the bore of the gun.[14]

In 1667 Sir Robert Moray published an appeal in the *Philosophical Transactions* for volunteers to implement a proposed research programme into the art of gunnery.[15] Moray was an experienced army officer and courtier who returned to

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[12] *ibid*., 66.
[13] *ibid*., 66.
[14] Thomas Sprat, *The History of the Royal Society of London For the Improving of Natural Knowledge* (London: J. Martyn, 1666), 233-239. Sir Robert Moray communicated Brouckner's paper to Christiaan Huygens. See Christiaan Huygens *Oeuvres Complètes,* III (La Haye: Nijhoff, 1888-1950), 323-328. The version in the Huygens letter is identical but easier to follow because the English typesetters probably had problems with typesetting of the algebraic notation.
[15] 'Experiments for Improving the Art of Gunnery', *Philosophical Transactions*, 2 (1667) 473-477, on 473. Accessed from http://royalsocietypublishing.org/journals. See also A.R.Hall, *Ballistics, 119,* footnote 2.

England with Charles II and like Prince Rupert went on to play a significant role in the Restoration government as a Privy Councillor.

The entry, entitled 'Experiments for Improving the Art of Gunnery', states that it was intended to achieve the three 'Grand Desiderata' of gunnery. Firstly, finding the point-blank distance; secondly, finding the quantity of powder for the just charge of any piece; and thirdly, what gun (for size, bore, weight, metal etc) shoots the farthest.[16] Moray's very detailed and systematic instructions include a similar experiment to the one proposed by Torricelli to Renieri to test whether the gun was in the correct position to fire point blank, described in an earlier chapter. This involved fixing pieces of canvas or sheets of paper fixed to the ground by stakes between the gun and the mark at several places. To fine-tune the quantity of powder to obtain the optimum charge for maximising the range, he proposed first reducing and then increasing an initial ordinary charge by successive increments of 1/16. Different types of gun, starting with a 'Culverin-bore', charged with the optimum charge of powder, were to be successively cut shorter by two inches and then fired to find how this affected the range.[17] This was the sort of research that would normally be carried out and financed under the direction of a ruler, and illustrates the role that the Royal Society took upon itself and how it differed from the French and Spanish monarchies.

There seems to be no record of anyone explicitly implementing Moray's programme of experimentation but whether by coincidence or because of Moray's stimulus, London weaver Robert Anderson, an able amateur mathematician, did undertake a series of experiments in ballistics with a gun cast at his own expense, the results of which are included in his *Genuine Use and Effects of the Gunne.* Anderson's book was significant as the first work (as far as I can tell) to attempt to

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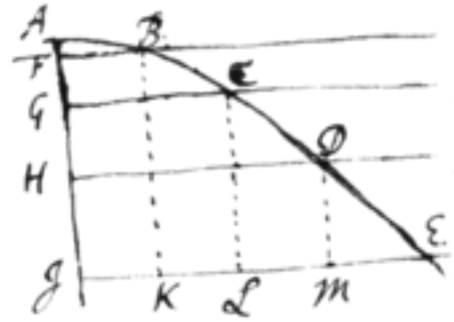
[16] *ibid.,* 473.
[17] *ibid.*, 473-7.

combine the results of experiment and observation with a theoretical approach based on Galileo's parabolic theory.[18] Anderson never achieved the honour of membership of the Royal Society, but he, and Thomas Streete who compiled the tables of projection that comprise a substantial part of the work, were part of its wider circle. Halley carried out astronomical observations with Streete and noted how some of his own results differed from those given by Streete's *Astronomia Carolina,* a copy of which which he nevertheless took with him on his sea voyages.[19] Anderson's book resulted in a dispute on ballistic theory between the mathematicians John Gregory and John Collins. Collins consulted both Wallis and Newton asking their opinion. Newton's letter to Collins on 20 June 1674 included the following discerning comments on Anderson's work:



> I thank you for your kind present. M^r Andersons book is very ingenious, & may prove as usefull if his principles be true. But I suspect one of them, namely that the bullet moves in a Parabola. This would be so indeed were the horizontal celerity of the bullet uniform, but I should think its motion decays considerably in the flight. Suppose for instance a bullet shot horizontally from A moves in the line AE, & AI being perpendicular to the horizon in it take AF AG, AH, AI, &c in proportion as the square numbers 1, 4, 9, 16 &c: & its certain that if in one moment of time the bullet descend as low as F, in the next moment it shall descend as low as G, in the 3^d as low as H &c. And therefore drawing the horizontall lines FB, GC, HD, IE; the bullet at the end of the first moment will be somewhere in the line FB suppose at B, & at the end of the 2^d moment it will be somewhere in the line GC suppose at C &c. But that FB, GC, HD & IE are in Arithmeticall progression (which is the condition of the Parabola) seems not probable; for if it were so, the celerity of the bullet would increas becaus the spaces AB, BC, CD, DE described in equall times are the latter bigger than the former: whereas I should rather think that the celerity decreases very considerably. And perhaps this rule for its decreasing may pretty nearly approach the truth, viz: Letting fall the perpendiculars BK, CL, DM &c to make IK, KL, LM &c, a decreasing Geometricall progression. If you should

[18] Anderson followed up this work with *To Hit a Mark* (London: Robert Morden, 1690), which included further more detailed results of his own and Eldred's experiments, which he attempted to reconcile with the parabolic theory.

[19] Cook, *Edmund Halley, Charting,* 53. Halley published an appendix to the *Astronomia Carolina* in the 1716 publication (Ronan, *Edmond Halley, Genius,*198).

> have occasion to speak of this to the Author, I desire you would not mention me becaus I have no mind to concern my self further about it.[20]

It seems that the controversy caused by Anderson's work may have prompted the coming together in 1674 of a working group of members of the Royal Society who were keen to test the parabolic theory in practice. It included Brouncker, Oldenburg, Hooke, and Jonas Moore, who was Surveyor of the Ordinance with an office in the Tower.

The only record of these experiments seems to be the following three brief entries in Hooke's diaries where Hooke notes that the trajectory of the bullet was very near to a parabola. The next entry, which mentions trials with 'Granadoes', suggests this result might have been from the shooting of mortars rather than cannon:

> 11 Sept 1674 With Lord Brouncker, Sir J Moore, Oldenburg, &c., to Blackheath.
> 17th Sept To Lord Brounckers. To Blackheath. Tryd Experiment of bullet with good [results] found it very neer a parabola
> Sept 23 Lord Brounckers with Sir J More. He told of his observation of Granadoes tried the day before at Blackheath[21]

Sir Jonas Moore also alludes to experiments on 'granados' in a contribution to a discussion on the motion of ascending light bodies at a meeting of the Royal Society on April 18 1678, which could be the same ones mentioned by Hooke. Moore describes using frames to track the trajectory, as suggested by Moray. His experience suggests that the anomalous results that Renieri observed in his account to Torricelli (see Torricelli chapter) were not necessarily exceptional:

> Sir Jonas Moore alledged, that in shooting Granados he had found, that the greatest random was below forty-five degrees of inclination. And that shooting

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[20] Ms Add. 7597/2/18/37, Cambridge University Library, Cambridge, UK, Newton Catalogue ID: NATP00256. accessed via The Newton Project Correspondence: http://www.newtonproject.sussex.ac.uk/catalogue/record/NATP00256, on 5.9.2014. Also published in H. W. Turnbull, ed., *The Correspondence of Isaac Newton,* I, (Cambridge: 1959), 309-11. Hall *Ballistics,* 120-125 gives a detailed account of the debate on Anderson's work.

[21] Robert Hooke, *The Diary of Robert Hooke MA MD FRS 1672-1680,* edited by Henry W Robinson & Walter Adams BA (London: Taylor & Francis, 1935), 121-2. 'Granado' appears to be an obsolete form of 'grenade', a small explosive device.

> at twenty degrees would fly much farther than shooting at seventy: the reason of which was the density and resistance of the air to the body passing through it, whereby that, which was shot at seventy degrees, passing through a greater quantity of air, received a greater impediment and hindrance from moving exactly in a parabolical line, than that which was shot at twenty.
> Sir Jonas Moore farther observed, that the different density of the air at one time more than another would cause a greater impediment and deviation of the bullet at that time more than another: that the motion of the air or wind would often bend the bullet considerably out of its directed way: that the hollowness of the shell would many times make it pass in a curve and not in a straight line; for that he had several expanded canvasses set up exactly in a straight line; and that upon shooting directly in the line he had observed, that a bullet had passed through the first and last, and yet missed all the intermediate ones by deflecting either to the right or left side of them.
> It was then moved, that some experiments should be made at the column on Fish-street hill, of the velocity of the descent of heavy bodies, and what the resistance of the air is to that motion.
> Mr Hooke affirmed, that he had a design to make several experiments concerning that and other matters at that place; of which he would give the Society an account; as he had formerly done of those made at St Paul's before the fire of London. He took notice, that there were in Ricciolus's *Almagestum Novum* a great number of experiments made at Bologna at the tower of the Asinels.[22]

In the following meeting on 25 April 1678, there was a discussion on the resistance of the air to bodies moved through them,

> and particularly the figure, in which a granado is moved; how near it approaches a parabola; and in what it varies from it; that in the motion of lesser bodies in lesser spaces the figure is so near a true parabola, that it is not possible, by any instrument yet known, certainly to describe one nearer the truth.[23]

These examples, characteristic of the minutes of the Royal Society, illustrate how members slipped effortlessly and seemingly unconsciously between discussions of problems relating to gunnery practice and experiments designed to elicit natural philosophical knowledge on the nature of motion and the effect of the resistance of the air. There were many discussions and experiments of the society that related in some way to gunnery, including the devising of experiments to measure the velocity

[22] Thomas Birch, *The History of the Royal Society of London,* III (London: A Millar, 1756-57), 400.
[23] *ibid*., 401

of a bullet and in testing Prince Rupert's super-strength gunpowder.[24] Both Prince Rupert and Hooke designed *eprouvettes* for testing the strength of gunpowder.

Sir Jonas Moore was an important member of the Royal Society not only because he was one of its most active members, but for his close patronage relationship with the Duke of York (later James II), to whom he had briefly been mathematics tutor during the Civil War. With the help of this royal patronage he became Assistant Surveyor of the Ordinance in 1665 and Surveyor General of the Ordinance in 1669. He in turn acted as an important patron of both John Flamsteed and Edmond Halley. He was instrumental in gaining royal consent for the construction of the Royal Observatory and in obtaining the post of first Astronomer Royal for Flamsteed, at a salary of £100 payable out of the office of the Ordinance.[25]

Both Moore and the Secretary of State Sir Joseph Williamson helped the twenty year old Halley to obtain royal support to leave his studies at Oxford in 1676 and travel to St Helena to chart the fixed stars of the southern hemisphere under the protection of the East India Company. Halley's friendship with Robert Boyle, a director of the East India Company from 1662 to 1677, no doubt also helped to facilitate this expedition.[26] As Simon Schaffer, in describing nineteenth-century links between colonialism and astronomical observation, has observed: 'Precise celestial knowledge had long been a tool of colonial power, a sign of that power's legitimacy, and a rationale for its exercise.'[27]

Moore was a practical mathematician and writer of mathematical textbooks, the last of which, published posthumously, was written for the use of pupils of Christ's Hospital School; his comprehensive *Mathematical Compendium; or Useful*

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[24] eg. Birch, *History,* I, on 461 (August 24, 1664) and on 474 (Oct 5, 1664).
[25] Birch, *History,* IV, 108.
[26] Ronan, *Edmond Halley, Genius,* 33.
[27] Simon Schaffer, 'Keeping the Books at Paramatta Observatory' in *The Heavens on Earth*, edited by David Aubin, Charlotte Bigg and H. Otto Sibum (Durham: Duke University Press, 2010), 125.

Practices in Arithmetic, Geometry, and Astronomy (1674) contains a section on the qualities required of a gunner. Listing the mathematical knowledge required, he makes a distinction between the average gunner for whom such knowledge cannot realistically be expected, and someone who aspires to be a Master Gunner, for whom it is essential.

Moore provides two sets of tables, the first providing, for each of ten types of piece, the weight, height of the bore, height and weight of shot, quantity of powder for proof and for service, paces point blank and for utmost random, the number of horses required to draw it, and the number of men required to draw it. The second gives the randoms at the six points of the gunners' quadrant. The following extract includes a wry comment on the propensity of gunners to disagree, and also suggests an estimate of a 10:1 ratio between the point blank range and the maximum range of the piece – the same ratio that Tartaglia used as the basis for the main theoretical result of his *Nova Scientia* (see chapter 1). Moore's figures for point blank and utmost random are exactly 10:1, suggesting that his figures were rounded and meant to be guidelines only:

> For the shooting of great guns, and the knowledge of the true distance that any Piece will carry to, is a matter that depends upon many uncertainties, an exact answer will never be given to such questions, there is such varieties in the truness of the Bore, in the heights of the Shot, in the levelling and direction; in the Air, Wind etc. But for all these difficulties an able gunner will go near the mark and he considers *Point Blank*, or *Right Ranges,* the *Middle Ranges* and *utmost ranges;* the former table gives you the utmost Random accounted near ten times the former level Range; and for all other mountures while [until?] Gunners have agreed, which I shall not live to see, take this Table to every six points of the Gunners Quadrant for these Guns, viz to 45°.[28]

Halley, who even had a small mortar made to his specifications to carry out tests (see below), also mentions experiments at the end of his *discourse* where he discusses the effect of the impediment of the air on the parabolic theory. In these experiments

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[28] Jonas Moore *A Mathematical Compendium* (London : printed for Richard Mount at the Postern on Tower-Hill, third edition, 1693), 84-89.

Halley was keen to show that the impediment of the air was less significant with very heavy shot than with smaller, lighter shot:

> However it be, tis certain, that in large Shott of *Mettal,* whose weight many thousand times Surpasses that of the *Air,* and whose force is very great, in proportion to the *Surface* wherewith they rest thereon; this *Opposition* is scarce discernable: For by several *Experiments* made with a *Morterpeice* Extraordinary well fixt to the *Earth* on purpose, which carried a Solid Brass Shott of 4½ Inches *Diameter,* and of about 14 Pound weight, the *Ranges* above and below 45 *Degrees* were found nearly equal; if there were any difference, the under *Ranges* went rather the farthest, but those differences were usually less than the Errours committed in ordinary *Practice,* by the unequal Goodness and Dryness of the same sort of *Pouder,* by the Unfitness of the *Shott* to the *Bore,* and by the Loosness of the Carriage.
>
> In a smaller *Brass-Shott* of about an Inch and a half *Diameter,* cast by a *Crossbow* which *ranged* it, at most about 400 *foot,* the force being much more Equal than in the *Morterpiece,* this difference was found more Curiously, and Constantly and most Evidently, the under *Ranges* out went the upper. From which Trials I conclude, that altho' in small and light *Shott,* the *Opposition* of the *Air,* ought and must be accounted for; yet in *Shooting of Great and Weighty* B*ombs,* there need be very little or no allowance made; and so these *Rules* may be put in *Practice* to all Intents and Purposes, as if this *Impediment* were absolutely Removed.[29]

Moore's and Halley's experiments show how they both used the theory of the trajectory, that shots at equal angles above and below 45 degrees should shoot the same distance, to test the effect of air resistance. Halley's purpose seems to have been more complex in that he was interested in showing how the importance of the effect of air resistance varied with different instruments of projection and types of shot. Halley shows here how he was concerned to find conditions (eg. speed and relative size and density of the shot), that might minimise the effect of air resistance so that the parabolic theory might be of use.

I will now examine the two contributions relating to gunnery that Halley published in the *Philosophical Transactions*, the first in 1686, and the second in 1695.

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[29] Edmond Halley, 'A Discourse Concerning Gravity and Its Properties: Wherein the Descent of Heavy Bodies and the Motion of Projects is Briefly, but Fully Handled: Together with the Solution of a Problem of Great Use in Gunnery', *Philosophical Transactions*, 16 (1686-7) 3-21, on 20-21.

The distance in time between the two works, and subsequent reports, suggest that Halley's engagement with gunnery issues was not a casual or fleeting one.

Edmond Halley and the solution of a problem of great use in gunnery

Robert Merton, in his seminal *Science, Technology and Society in Seventeenth-Century England,* singled out Halley among his contemporaries for his particular eagerness to turn 'the most abstruse theories of science to immediately practical aims.'[30] Halley was by no means the only prominent member of the Royal Society to take an interest in gunnery, or in the practical applications of science in general, but he does provide us with a particularly acute example of the tension between the desire to use theory and experiment to improve practice combined with an appreciation of its limitations. Not only was Halley aware of Newton's ground-breaking work on motion in a resisting body, introduced in Book II of Newton's *Principia*, but he was at the same time in correspondence with John Wallis, and encouraged him to publish his mathematical solution based on the hypothesis that the resistance is proportional to the velocity. It cannot be argued that Halley was unaware of the need to provide a theory that included the effect of air resistance.

Although A. R. Hall dismisses Halley's work, as he does Blondel's, as having nothing new in it, elsewhere in his book Hall portrays Halley as having a realistic scepticism towards the applicability of scientific theory to ballistic problems. In connection with Benjamin Robins' experiments in the seventeen thirties which showed wide variations in the ranges and directions of shots, which he put down to the effects of air resistance upon the diverse motions of spherical projectiles, Hall comments that: 'In short there is every reason to believe with Halley that ballistical

[30] Robert K Merton, *Science, Technology and Society in Seventeenth Century England* (London: Harvester,1978), 193.

theory was of small purpose in the existing conditions of technique, since gunners "loose all the geometrical accuracy of their art from ye unfitness of ye bore to ye ball, and ye uncertain reverse of ye gun, which is indeed very hard to overcome".'[31]

Hall gives the impression that Halley dismissed the application of theory to gunnery practice as being of no value, but if so it is difficult to explain the considerable attention Halley and others in the Royal Society paid to this question. Here is Hall's quote in context. The quote was taken from two letters read to the Royal Society on 19/30 March 1700/1.

> Letter 1
> I have considered ye tables of Sr. Alberghetti intended for shooting bombs out of a Long Gun, & being but for ten degrees of Elevation and under cannot well be applicable to any other but battery; He concludes that a cannon wch he calls Cannone di ducento carries its ball with such velocity yt while it flies 200 paces or 1000 foot, it falls below ye line of direction
> 1 ¾ paces or nearly 9 foot, wch shews yt the time is about ¾ of a second yt ye Ball is flying that space, which yet ye author takes to be nearly a second of time. On ys principle vizt: yt ye fall of ye ball is 1 ¾: paces fallen below ye line of direction while it flies 200 paces, verified, as he asserts, by sufficient experiment, are these tables founded, and calculated according to the theory of Galileo and with exactness and curiosity beyond what ye gross practise of our present Canoniers seems to require, who loose all the Geometricall accuracy of their art from ye unfitness of ye boare to ye ball, & ye uncertain reverse of ye Gunn, which is indeed very hard to overcome, but without it, it will not be so easy to batter at great distances as Sr. Alberghetti supposes. Yet may these tables be of good use for battery, where the works to be battered are considerably above ye level and not very farr beyond point blank of the piece. Besides the theory of Galileo allowing no opposition to ye ball from ye Air is insufficient in great distances.

> Letter 2
> I have by order of ye Royall Society well considered the specimen of the tables of Sigr. Alberghetti designed for the shooting of Bombs out of Long Gunns. I find the Author in all things to follow the Doctrine of projections laid down by Galileo, Torricelli and others & yt he has performed his calculations with more than ordinary exactness: but ye chief design seems to be, to enable the Canoniers to strike any object above or below the Levell without any previous Geometricall working, wherein it would be very easy to commit Errors. It is not to be doubted but ye said Tables may be very serviceable in battering with cannon at a much greater distance then is thought possible by

[31] Hall *Ballistics,* 52 and 56.

> the generality of Practicall Gunners without any more trouble then the bare Inspection of these Tables.[32]

A reading of the whole text suggests that it is necessary to at least modify Hall's interpretation. Halley is concerned with a number of questions. First there are the technical causes of inaccuracy in firing, such as the fitness of the bore. Another is the fact that Galileo's theory is not applicable because of the effect of air resistance. Nevertheless Halley allows that in certain limited circumstances the theory, and the tables on which they are based, might be of value. Also Halley shows that his concern was not just a question of finding a satisfactory theory, but of developing methods that would be easy to put into practice, since the average cannonier could not be expected to carry out complex calculations.

Halley takes a particular interest in the question of the unevenness of the bore of guns as a factor affecting their efficiency and their accuracy. Halley's comments are recorded in an extract from the journal books of the Royal Society for 2 July 1690:

> …the fitness of the shott to the bore of a piece was of great consequence in Gunnery… by observing this, more powder might be saved, than would pay for the turning our great cannon shott, that another great advantage arising from it was that a shott would be made with much more certainty, and a third, that Gunns need neither be so long or so weighty as are now in use and yet do the same, or more execution, which is of exceeding use for the ease of Carriage of artillery by land, & for to ease the sides of shipps at Sea. To confirm this he said he had often seen a brass shott of 14lb weight cast by 2 ounces of powder from a barrel about 10 inches deep, above 550 yards; which could not have been unless the shott had been truly fitted to the bore.[33]

He was not only concerned with technical improvement in cannon production to improve accuracy, but also to reduce costs. On 22 April 1691 he reported to the Society his success in building a small mortar to test the effect on the accuracy and distance of fire of the use of a tampion made of Lignum Vitae (a wood known for its

[32] MacPike, *Correspondence,* 167-8.
[33] *ibid*., 219.

high density and strength, later used for making cricket balls and policemen's truncheons):

> The same made a report of the success of a small Mortar made by him by the Society's order. viz that the Piece being onely 24 lb. and but one single diameter of the shott in length of chase did with no more than 1 ½ ozs of powder cast a solid shott of 12 ¼ lb a full quarter of a mile by help of a Tompion of Lignum Vitae. Without which it did not cast it 100 yards; and softer woods, tho they increased the force, had yet a less effect in proportion to their softness. He said that his Lignum Vitae Tompions being well fitted to lie on the pouder without thrusting it in, but just fitting the chamber, had had a wonderfull effect in point of shooting for five shott in 6 fell within ye compass of less than 5 yards square at a full quarter of a Miles distance.[34]

Such experiments suggest Halley recognised that the theory of the parabola was useless if other technical problems of gunnery were not overcome. But as a more detailed examination of his two contributions to the *Philosophical Transactions* show, he did not just give up on his goal of improving the practice of gunnery. His goal was to bring theory and practice as closely together as possible and to attempt to make use of the theory even if its applicability might be limited and qualified.

A discourse concerning gravity[35]

The first part of the discourse provides a very clear but polemical explanation of Newton's theory of gravity and it is really an introduction to and advertisement for the forthcoming *Principia*. Halley, like Newton, recognised that Descartes' theory of vortices remained the most serious challenge to Newton's system, particularly in France, and in the *Discourse* he expressed his feelings of incredulity that anyone could seriously adhere to a theory that led to such contradictions:

> Nature amidst the great variety of *Problems* wherewith She exercises the Wits of Philosophical men, scarce affords any one wherein the Effect is more visible, and the Cause more concealed than in those of the *Phaenomena* of

[34] MacPike, *Correspondence,* 223 This could be the same mortar that he mentions in the discourse.

[35] Edmond Halley, 'A Discourse Concerning Gravity and Its Properties: Wherein the Descent of Heavy Bodies and the Motion of Projects is Briefly, but Fully Handled : Together with the Solution of a Problem of Great Use in Gunnery', *Philosophical Transactions*, 16 (1686) 3-21.

> *Gravity.* Before we can go alone, we must learn to defend our selves from the violence of this Impulse , by not trusting the *Center* of *Gravity* of our Bodies beyond our reach; and yet the Acutest Philosophers, and the Subtilest Enquirers into the Original of this Motion, have been so far from satisfying their Readers, that they themselves seem little to have understood the Consequences of their own *Hypotheses.Des Cartes* his Notion, I must needs confess to be to me Incomprehensible, while he will have the Particles of his *Celestial matter,* by being reflected on the Surface of the *Earth,* and so ascending therefrom, to drive down into their places those *Terrestrial Bodies* they find above them: This is as near as I can gather the scope of the 20, 21,22, and 23 *Sections* of the last Book of his *Principia Philosophiae;* yet neither he, nor any of his Followers can shew how a Body suspended *in libro aethere,* shall be carried downwards by a continual Impulse tending upwards, and acting upon all its parts equally: And besides the obscurity wherewith he expresses himself particularly, *Sect. 23.* Does sufficiently argue according to his own Rules, the confused *Idea* he had of the thing he wrote. [36]

He also provides clear arguments against other popular hypotheses, such as that the earth is a giant magnet, and the existence of positive levity.

Halley describes Newton's four properties of gravity and shows how the rules of the fall of bodies can be explained from these properties and that the motion of projectiles are mathematically deducible from these rules.[37] He then answered objections to the use of the theory for determining the motion of projectiles, including that of the opposition of the air, which he admitted was considerable in the case of light or small bodies, 'but in great and ponderous Shot, this impediment is found by *Experience* but very small, and may safely be neglected'.[38]

Halley then provides an exposition of eleven propositions on the descent of heavy bodies and the motion of projectiles. He admits there is nothing new that he has contributed except in the tenth proposition, which explains how to hit a target above or below the horizon, a problem that had exercised the ingenuity of mathematicians since Galileo's discovery of the parabolic trajectory. Nevertheless Halley considered

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[36] Halley, 'A Discourse', 3.

[37] He suggests that the Universal Deluge can be explained as a suspension of the second property that the point or centre of gravitation is fixed within the earth.

[38] Halley, 'A Discourse', 8. These are the experiments that he describes later in the piece and that I have quoted above.

that it would be a useful service to draw together all current knowledge, and present it in a more concise and accessible form:

> These *Propositions* considered, there is no question relating to *Projects,* which by the help of them may not easily be Solved; and tho' it be true that most of them are to be met withal, in *Galilaeus, Torricellius* and others, who have taken them from those Authors, yet their Books being Forreign, and not easy to come by, and their *Demonstrations* long and difficult, I thought it not amiss to give the whole *Doctrine* here in *English,* with such short *Analytical* Proof of my own, as might be sufficient to evince their Truth.[39]

Halley continued that the tenth proposition was first solved in Anderson's *Genuine Use and Effects of the Gun* in 1674 but that it required 'much calculation'. It was then that he began to search for an easier solution, and in 1678 he found out the rule and from it the geometrical construction.[40] He notes that Blondel in his *L'Art de Jetter les Bombes* gives the solutions of Bout, Roemer and La Hire , but

> none of them being the same with mine, or in my opinion more easy, and most of them more Operose, and besides mine finding the *Tangent,* which generally determines the angle better than the *Sine,* I thought my self obliged to Print it for the use of all such, as desire to be informed in the *Mathematical* part, of the Art of *Gunnery.*[41]

Halley does not mention here that there was actually some interest by other members of the Royal Society in his solution, and the discourse on gravity gave him the perfect opportunity to showcase it in the context of his wider purpose. In the *Philosophical Transactions* of 6 July 1683 there is a short entry noting that Mr. Tolley brought in a proposition on gunnery obtained from Halley, with its construction and rule, but no demonstration.[42] And in a letter dated 8 April 1686 William Molyneux, founder of the Dublin Philosophical Society, who later worked with Halley at the Chester Mint during the national recoinage, reminded Halley to send the demonstration of the 'rule

[39] *ibid*., 19. He also includes, on 10, as Proposition IV that bodies fall 16ft 1inch per second (acquired velocity 32 ft 2 inches per second) from Huygens *Horologio Oscillatorio* (1673).
[40] *ibid*., 19.
[41] *ibid*., 19.
[42] *Philosophical Transactions*, 4 (1669) 431.

for shooting on ascents and descents with the mortar-piece' that he had shown him last time he was in London. He urged Halley,

> not to detain it from me longer than your next to me. Doubtless you have seen Mons. BLONDEL'S *Art de Jetter les Bombes,* a book wherein there is nothing material more than what was before him in GALILEO ****, except only this business of shooting on ascents and descents: after he had proposed the problem to Messieurs DE L'ACADEMIE ROYALE DES SCIENCES, Mons Buot, Mons. ROMER, Mons. DE LA HIRE, and Mons. Cassini employed their thoughts about it, but I can assure you upon rigid examination, there is not one of their rules holds true in all cases.[43]

Halley's rule introduced velocity (the amount of charge) as a variable to be manipulated by the gunner, as well as the angle of elevation. This involved a key equation that gives the horizontal range at 45 degrees for a given velocity (charge) as $\sqrt{b^2 + h^2} \pm h$ [44] where b is the horizontal distance to the object to be targeted above or below the horizon, and h is the height of the object above or below the horizon. Taking the charge that gives this maximum range at 45 degrees, some proportional manipulation of known values enables the elevation to be found that will hit the desired target, whether above or below the horizon, both with accuracy and with minimum powder. Halley implies that because the angle of elevation is equivalent to that of 45 degrees when shooting towards the horizon, any small error would not greatly affect the accuracy of the shot, since, presumably, he knew that at angles close to 45 degrees of elevation the variation in range is very small. Halley continues that this rule,

> …may be of good use to all *Bombardiers* and *Gunners,* not only that they may use no more Powder than is necessary, to cast their *Bombs* into the place assigned, but that they may shoot with much more certainty, for that a small Error committed in the *Elevation* of the *Piece,* will produce no sensible difference in the fall of the Shot: For which Reasons the *French* Engineers in their late Sieges have used Morter-pieces inclined constantly to the *Elevation*

[43] *Philosophical Transactions,* 4 (1669) 478.
[44] Halley uses bb for b^2 and hh for h^2.

> of 45, proportioning their Charge of Powder according to the distance of the Object they intend to strike on the *Horizon.*[45]

This reference suggests Halley's awareness of French successes with mortar fire that I discussed in my chapter on Blondel.

Halley finally discusses the problem of the resistance of the air, and notes the debate over whether the resistance varies with the velocity or the square of the velocity, both of which were treated mathematically by Newton, as Halley would, of course, have been aware:

> Now these rules were rigidly true, were it not, as I said before, for the Opposition of the Medium, whereby not only the direct imprest Motion is continually retarded, by likewise the increase in the *Velocity* of the *fall,* so that the spaces described thereby, are not exactly as the squares of the times: But what this Opposition of the *Air* is, against several *Velocities, Bulks,* and *Weights,* is not so easy to determine. Tis certain that the weight of *Air,* to that of *Water,* is nearly as 1 to 800, whence the weight thereof, to that of any *Project* is given; but of different matter, the *Opposition* should be *reciprocally* as the weights of the Shott, as likewise that to shott of the same *Velocity* and matter, but of different Sizes, it should be as the *Diameters reciprocally:* whence generally the *Opposition* to shott with the same *Velocity,* but of differing *Diameters,* and *Materials,* should be as their *Specifick Gravities* into their *Diameters reciprocally;* but whether the *Opposition,* to differing *Velocities* of the same shott, be as the *Squares* of the differing *Velocities,* or as the *Velocities* themselves, or otherwise, is yet a harder Question.[46]

Halley then proceeds to describe his experiments with mortars that I have quoted above, where he asserts his confidence that in the case of 'Great and Weighty *Bombs*' the rules may be applied without undue concern for the impediment of the air.

A proposition of general use in the art of gunnery [47]

Very little note has been taken by historians of Halley's work on gunnery. Despite having previously quoted approvingly Halley's realistic remarks about the technical problems that prevented accuracy in firing, A. R. Hall wrote that:

[45] Halley, 'A Discourse', 17.
[46] *ibid*., 20.
[47] *Philosophical Transactions*, 19 (1695) 68-72.

> Blondel and Halley in their essays into the field of ballistics stated in different forms what was already known. The former indeed published designs by Cassini, Roemer, and De la Hire for an improved gunner's quadrant which made possible the allowance for the slope of the ground in assigning angles of elevation, and the latter expressed his results in algebraic form; but Blondel's instruments were never used and Halley's formulae are too involved to be judged an improvement on Torricelli's geometrical constructions, though he must be noticed as the first to remark that the extreme range of a gun on the parabolic hypothesis is always obtained when the axis of the piece bisects the angle between the vertical and the ground, whether this slopes or not.[48]

In contrast, Colin A. Ronan, in his sympathetic and engaging biography of Halley, makes a more positive assessment of Halley's contribution. Unlike Hall he recognises the importance of the context of the first work, written at the time when Halley was working closely with Newton on the production of the *Principia,* and that it was designed as a practical demonstration of Newton's theory of gravitation. He states that a solution to the problem of hitting a target above or below the horizon had been attempted before but that the solutions were cumbersome, whilst Halley

> had laid bare the essentials, showing both how to lay out a mortar in the correct direction to hit a specific target on whatever ground the mortar lay, and also how to adjust its elevation from any position, making use of a metal reflecting plate and a plumb line. He also suggested a standardization of mortars themselves, the bombs they ejected, and the charges of gunpowder used.[49]

Halley's suggestion in the 1695 paper, for using a mirror and plumb line to easily position the mortar so that it bisected the angle between the perpendicular and the object, which guaranteed that the mortar would hit the object, indicates that Halley, quite remarkably given the other demands on his time, had continued to exercise his mind to find a rule that would be easier to use in practice, and he appears delighted with his discovery that his rule could be used without the need for any calculation whatsoever:

> But I was not at that time aware that the aforesaid Elevation did constantly bisect the Angle between the Perpendicular and the Object…Having

[48] Hall, *Ballistics*, 96.
[49] Ronan, *Edmond Halley: Genius,* 80.

> discovered this, I think nothing more compendious, or bids fairer to compleat the Art of Gunnery, it being as easie to shoot with a Mortar at any Object on demand, as if it were on the Level: neither is there any need of any Computation, but only simply laying the Gun to pass, in the middle Line between the Zenith and the Object, and giving it its due Charge.[50]

Using the 'due charge' not only assured accuracy in hitting the target but, as I noted earlier optimised the use of powder. Nevertheless, as before, this still involved finding the amount of powder (velocity) that would cast the shot at an elevation of 45 degrees to a distance of $\sqrt{b^2+h^2} \pm h$.[51] This sounds problematic, but Halley suggested that experiments be made to find the amounts of gunpowder required for these different possible distances, which could be engraved on the mortar for ease of reference. It is in this context that he suggests the benefit of making all mortars as alike as possible in diameter, chase, weight etc, so that the experiments would not need to be repeated for every piece:

> So that it only remains by good and valid Experiments to be assured of the force of Gunpowder, how to make and conserve it equal, and to know the effect thereof in each Piece; that is, how far differing Charges will cast the same Shot out of it; which may most conveniently be engraven on the outside thereof, as a standing Direction to all gunners, who shall from thence forward have occasion to use that Piece: And were this matter well ascertained, it might be well worth the while to make all Mortars of the like Diameter, as near as may be alike of Chase, weight, Chamber, and all other circumstances.[52]

Thus like Blondel he recognised that the effectiveness of the method was dependent on practical experiments and recording of data that would then be, literally, at the gunner's fingertips.

Halley recognised the problems of the theory if applied to the high velocities of cannon shot and in this second work, once more we have a French connection in

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[50] Edmond Halley, 'A Proposition of General Use in the Art of Gunnery, Shewing the Rule of Laying a Mortar to Pass, in Order to Strike any Object Above or Below the Horizon', *Philosophicqal Transactions* 19 (1695) 68-72, on 70.

[51] i.e. the sum of the hypotenusal distance of the object from the gun and the perpendicular height (or minus the drop, if below). *ibid*., 69.

[52] *ibid*., 70.

that he refers specifically to the launching of bombs from mortars at sea, which, as I noted in the last chapter, was not considered practicable until the French used them in the Siege of Algiers in 1682.[53] In is in this context that the introduction to Halley's 'Proposition' should be read:

> It was formerly the Opinion of those concerned in Artillery, that there was a certain requisite of Powder for each Gun, and that in Mortars, where the Distance was to be varied, it must be done by giving a greater or lesser Elevation to the Piece. But now our later Experience has taught us that the same thing may be more certainly and readily performed by increasing and diminishing the quantity of Powder, whether regard be had to the Execution to be done, or to the Charge of doing it. For when Bombs are discharged with great elevations of the Mortar, they fall too perpendicular, and bury themselves too deep into the Ground, to do all that damage they might, if they came more Oblique, and broke upon or near the Surface of the Earth; which is a thing acknowledged by the besieged in all Towns, who unpaved their streets, to let the Bombs bury themselves, and thereby stifle the force of their Splinters. A Second Convenience is, that at the extream Elevation, the Gunner is not obliged to be so Curious in the direction of the Piece, but it will suffice to be within a degree or two of the truth; whereas in the other Method of Shooting he ought to be very curious. But a third, and no less considerable Advantage, is the saving of the King's Powder, which in so great and so numerous discharges, as we have lately seen, must needs amount to a considerable value. And for Sea-Mortars, it is scarce Practicable otherwise to use them, where the agitation of the Sea continually changes the direction of the Mortar, and would render the Shot very uncertain, were it not that they are placed about 45 Degrees Elevation, where several Degrees above or under makes very little difference to the effect**.**[54]

Whilst in 'A Proposition' Halley's aim is straightforward – to communicate his improved method for aiming mortars, as I have noted, the 'Discourse' was more complex in its aims. Although Halley's work on gunnery is only a small and largely unnoticed part of his output it should not be viewed as a discrete subordinate concern but as part of his programme to spread understanding of the practical benefits to be gained from the new science.

[53] Tucker, *Handbook,* 5.
[54] Halley, 'A Proposition', *Philosophical Transactions,* 19 (1695-97) 68-9.

Matters of state

Halley's wish to gain recognition for the national importance of the *Principia* is illustrated by the fact that he presented a copy of the first edition to James II accompanied by a treatise very similar to the 'Discourse' entitled *The True Theory of the Tides, Extracted from that Admired Treatise of Mr Isaac Newton, Intituled, Philosophiae Naturalis Principia Mathematica.* Both works explain Newton's theory of gravity most engagingly and with remarkable clarity. Halley chose to illustrate the utility of the *Principia* with the example of its explanation of the tides, since this question had particular resonance with James II, who took a special interest in naval matters, having held the post of Lord High Admiral with command of the British fleet in the second and third Anglo-Dutch Wars.[55]

The recognition of the esteem of this work is illustrated by the fact that it was reprinted in 1695 in the *Philosophical Transactions*, by popular request

> for the sake of such, who being less knowing in Mathematical Matters; and therefore, not daring to adventure on the Author himself, are notwithstanding, very curious to be informed of the Causes of Things; particularly of so general and extraordinary Phaenomena, as are those of the Tides. Now this Paper having been drawn up for the late King James's Use, (in whose reign the Book [i.e., the *Principia*] was published) and having given good satisfaction to those that got Copies of it; it is hoped the Savans of the Higher Form will indulge us this liberty we take to gratifie their inferiours in point of Science; and not be offended, that we here insist more largely upon Mr Newton's Theory of the Tides, which, how plain and easie soever we find, is very little understood by the common Reader.[56]

Nicely incorporating the example of the trajectory of a gun, Halley explained how the same principles govern motion in the heavens and motion on earth, and swept aside Descartes' vortices with panache:

> The Theory of Motion of the primary *Planets* is here shewn to be nothing else, but the contemplation of the Curve Lines which Bodies cast with a given Velocity, in a given Direction, and at the same time drawn towards the Sun by its gravitating Power, would describe. Or, which is all one, that the Orbs of the

[55] Ronan, *Edmond Halley: Genius,* 87
[56] Reprinted in *Philosophical Transactions,* 19 (1695) 445-457 on 445.

> Planets are such Curve Lines as a shot from a Gun describes in the Air, being cast according to the direction of the Piece, but bent into a crooked Line by the supervening Tendency towards the Earths Centre…So that it appears, that there is no need of solid Orbs and Intelligences, as the Ancients imagined, nor yet of *Vortices* or Whirlpools of the Celestial Matter, as *Des Cartes* supposes; but the whole Affair is simply and mechanically performed, upon the sole supposition of a Gravitation towards the Sun; which cannot be denied.[57]

Halley saw himself as a bridge between those who engaged in scientific discovery and those with economic and political power who could both put those discoveries into practice for the benefit of the state, and to sponsor further scientific work.

Halley's vernacular works on gunnery and the tides show how he was able to repackage the *Principia* to enable the full force of its implications to be appreciated by those who had the power to promote the new science and to facilitate further research and practical applications. As I have already noted, key founding members of the society such as Brouncker and Moray were also high-ranking members of the government; the scary Sir Joseph Williamson, Secretary of State, another of Halley's patrons, had been the *de facto* head of the Restoration government's intelligence system.[58] Newton's friend Sir Charles Montagu, also a member of the Royal Society, was the Chancellor of the Exchequer who engineered Newton's appointment as keeper and then warden of the Royal Mint. The Royal Society became the space where power and knowledge came together for the mutual benefit of both parties and Halley was a supremely adept mediator between the two. Its amateur status provided the state with a more cost-effective and flexible conduit for the discovery and propagation of useful knowledge than the hugely expensive and elitist French Royal Academy.

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[57] Halley, *Philosophical Transactions,* 19 (1695) 445-457, on 448. Controversies continued in Europe at the end of the 17th Century about the foundations of natural philosophy.

[58] See Willamson's entry in the *Oxford Dictionary of National Biography.*

Conclusion

Members of the Royal Society were experimenting on the validity of the parabolic trajectory from at least 1674, but Halley's two works of 1686 and 1695 suggest that his interest in the use of the parabolic theory to improve gunnery practice was further stimulated by the French reputation for expertise in the science of war, and the impact of French military successes, which had led to alarming territorial gains as well as threatening English domination in the control of colonies and trade. The publication of Blondel's *L'Art de Jetter les Bombes* at the time that the French were achieving their most spectacular successes in the use of their new invention of the *galiote de bombes* for bombarding from the sea would have encouraged Halley in the belief that the French were on to something.

Halley's improvements to the practical application of the theory of the parabola provided him with the perfect showcase for illustrating the utility of Newton's *Principia.* His method of using the amount of powder as a key variable and minimising the variability due to minor errors by firing mortars at 45 degrees, or its equivalent when hitting a target above or below the horizontal, is a creative adaptation to the problems of using the parabolic theory. His successful efforts to find a simple rule for hitting a target above or below the horizon illustrate how this practical problem, along with that of air resistance, took on new importance once Galileo had made his breakthrough, and testify to the continued engagement of scientists with the realities of gunnery practice. Halley struggled to find the mediations necessary not only to provide rules that could be easily used by the average gunner, but perhaps even more importantly, would improve the efficiency of cannon and mortars, and optimise the use of materials.

The crucial role that Halley played in facilitating the publication of the *Principia* is well known, but his continued role as promoter and interpreter of Newton's work has perhaps not been so well recognised.[59] Halley did not just appreciate Newton's work as the solution of a genius to the unanswered questions of the nature of motion on earth and the heavens, but also because they could be used as tools to solve practical problems, which he proceeded to do, most notably in his prediction of the motions of comets. The discourse on gunnery was used by Halley to produce a vernacular popularisation that would contribute to the legitimation of the new science by demonstrating its usefulness in solving problems of vital interest to the state.

[59] Halley also wrote a book review in the *Philosophical Transactions* in 1687, which consisted of a straightforward summary of the contents of the *Principia*: *Philosophical Transactions*, 16 (1686-1692) 292-297.

Conclusion

> But the fifth part is brought in only as a temporary measure pending completion of the rest, rather like interest payable until the principal can be had. For I do not hasten to my destination so blindly that I overlook things of use that I come across on my way.[1]

The initial motivation for this thesis was a question: if the theory of the trajectory was useless to gunnery, why did early-modern writers on gunnery include theoretical treatments of the trajectory in their works? The standard answer, that it was merely a rhetorical ploy to increase the authors' credibility in the competitive drive for patronage, appeared to be unconvincing; a deeper understanding of the question involved the examination of the relationship between the claims of these writers and how they related them to the theoretical tools available to them. Each study illustrates the iteration between theory and practice that was crucial for the development of science. The theory of the trajectory was a form of knowledge capital that was passed on to future military writers and theorists, who nevertheless endeavoured to draw off the maximum in interest that they could gain along the way. This recognition of the dual reward of causal enquiry demonstrated both their epistemological insight and historical optimism.[2]

Whilst this was the starting point, the thesis developed into a narrative of the struggle for the new science in a wider sense. It was an epistemological struggle for the discovery of the causes of motion. It was also a struggle to overcome the impediments of matter that interfered with the successful application of theory to practice. It was a political struggle against conservative forces that felt challenged by the threat the new approach to knowledge posed to traditional (particularly religious)

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[1] Bacon, *Novum Organum* in *The Oxford Francis Bacon* XI, 43.

[2] See for example, Bacon's distinction between 'acatalepsy' (that knowledge is unattainable), and 'eucatalepsy' (that is is attainable by the right route). *Novum Organum, The Oxford Francis Bacon* XI, 189.

authority, vested interests and power relationships. And it was a promotional struggle against contemporary critics that were not slow to point out the inadequacies of the theoretical results; thus my protagonists had to struggle to promote understanding of its potential power despite their recognition of the limitations of their theory.

Far from making overblown claims, examination of practical gunnery manuals suggests that gunnery writers were often quite circumspect in their attitude to the question of the true nature of the trajectory, and its applicability to practice; the experienced gunner William Eldred, who at the age of eighty three wrote a manual based on his vast experience and meticulous collection of data from gunnery trials, whilst providing very creditable rules for firing, modestly remarked that:

> I will endeavor to prove the truth of the same as briefly and truly as I can referring my self to others more learned and practiced in the Mathematiks to take some pains to find the Arch or way of a shot, being in my judgement hard to find, but of great use when it is found, in the mean time accept of this table of Randons given.[3]

This thesis has mainly focused on those 'more learned and practiced in the Mathematiks' who took up the challenge of the gunners' question. Examination of the key vernacular texts of these writers to elicit their purpose has revealed a more complex picture than the one presented by Hall, who essentially plucked mathematical results from their literary context, resulting in a rather depressing litany of inadequacy and failure.

Each work demonstrates a programmatic commitment to a new approach to knowledge that aimed to lay certain foundations for improved practice, new discoveries and new inventions. Hall missed this point, and he saw rhetoric solely in

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[3] William Eldred, *The Gunners Glasse* (London: Printed by T. Forcet for Robert Boydel, 1646), 68-9 (after 53), irregular pagination. The ballistics expert Charles Hutton noted that Eldred's 'principles were sufficiently simple, and within certain limits very near the truth, though they were not rigorously so. He has given the actual ranges of different pieces of artillery at small elevations, all under 10 degrees. His experiments are numerous, and appear to be made with great care and caution.' See Charles Hutton, *A Philosophical and Mathematical Dictionary* (London: Printed by J.Davis for J. Johnson; and G.G. and J. Robinson, 1795-96), I, 610.

terms of personal ambition. This thesis has shown that these texts represented a social programme for the transformation of knowledge and learning, based on sure foundations, and with universally applicable rules and methods that would bring increased prosperity to the state. Indeed, during the early modern period, this had become a political as well as a social programme, particularly apparent in the cases of Thomas Digges, Galileo, and François Blondel.

For Hall, it would appear, a real scientist did not engage with the material. Thus, for example, he did not consider Robert Hooke to be a scientist, but rather a 'mechanic of genius'.[4] But all of the characters in the case studies did engage with the real world and had to tackle the problem of bringing theory into conformity with material reality. The evangelists of the new scientific approach to the study of motion were only too aware of the myriad impediments that had to be minimised or eliminated before it could be successful in practice. Hall's thesis is based on a division between scientists, who had little or not contact with practice, and writers of gunnery manuals, who incorporated the theory into their manuals, usually when it was out of date, to no apparent purpose. This, it appears, was the only route from theory to practice. But the innumerable publications on military science during this period were diverse. How, for example, should Blondel's *L'Art de Jetter les Bombes* be categorised? Blondel was a state adviser of considerable expertise and not inconsiderable influence. From Hall's perspective, he did not add anything new to the theory, yet *L'Art* was not really a traditional gunnery manual. It is difficult to know how one would categorise it. Blondel's could be taken as a classic case for the rhetorical argument, but as this thesis has demonstrated, that would be to

[4] Ofer Gal, *Meanest Foundations*, 13.

underestimate his programme and his influence. Most importantly, Hall's approach failed to offer a historical understanding of Blondel's wider purposes.

All the subjects in the thesis were conscious of the double payoff, philosophical and practical, that the scientific study of motion would eventually bring. Yet each one developed this programmatic goal within the context of different national conditions but also increasing European, and indeed, international communication of new scientific knowledge and ideas. But there was a tension between recognition of the benefits of scientific collaboration and communication, and the increasing awareness of national identity and self-interest. Military research sat at the very core of this contradiction. The development of the early modern state encouraged national consciousness and rivalry, in which, unfortunately, the destructive power of cannon warfare played a significant role. To a greater or lesser degree, all the subjects in the case studies were state advisers who, conscious of the enormous potential of the new science, saw their efforts as serving its wider military and economic interests.

The close relationship between science and the state continued into the eighteenth century and is embodied in the work of Benjamin Robins, the central figure of a ballistics revolution, who worked as a theoretical scientist, mathematician, engineer, state military adviser and artillery commander. Military questions continued to provide a strong stimulus to scientific innovation. At the request of Frederick the Great, the mathematician Leonhard Euler translated and extended Robins' *New Principles of Gunnery* (1742) in 1745. In 1774 Turgot urged Louis XVI to have it translated into French for the benefit of the French Navy and Artillery schools; it was translated by Jean-Louis Lombard, Napoleon's artillery theory professor, along with

Euler's *Neue Grundsätze der Artillerie,* in 1783.[5] Brett Steele argues that the 'ballistics revolution' initiated by Benjamin Robins

> challenges important claims in the history of eighteenth century science and technology: that mathematical analysis and scientific experimentation had little interaction and that technology (with the exception of navigation) was not significantly influenced by rational mechanics.[6]

Steele notes that Benjamin Robins' most influential accomplishment was the invention of the ballistic pendulum, which allowed him to 'quantitatively measure both muzzle velocity and (by moving the pendulum at progressively greater distances from the gun) the air resistance of a projectile, the two fundamental parameters in the differential equations of ballistic motion'.[7] It was these two unknown parameters that had hindered the usefulness of earlier mathematical solutions, no matter how sophisticated they became in the hands of Huygens, Newton and Johann Bernoulli. Robins' invention allowed him to develop a rigorous scientific foundation for ballistics. And he combined this with suggestions for design improvements such as decreasing artillery weight and lowering gunpowder charges that bear a remarkable similarity to those adopted by Gribeauval.[8]

Steele confirms that the efforts of engineers such as Bélidor to provide practical range tables for mortar fire highlighted problems with the maintenance of uniform artillery hardware and gunpowder quality, and continues:

> Such limitations, however, far from weakening Galileo's influence on early modern gunnery, inspired efforts to improve the consistency of artillery fire in order to take full advantage of his theory's power. Such work was reflected in the 18th century military interest in mechanical uniformity and interchangeable parts, which culminated in the French artillery reforms of Jean Baptiste Vaquette de Gribeauval.[9]

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[5] Steele, 'Muskets', 369.
[6] *ibid*., 350.
[7] *ibid*., 359.
[8] *ibid*., 356.
[9] *ibid*., 352.

He suggests that Hall was wrong in his assertion that science could not be applied usefully to artillery fire until the technological improvements of the nineteenth century. Rather, he contends that the revolution was caused by the engineering ingenuity of the eighteenth century, particularly the technological innovations of Austria's Prince Joseph Wenzel von Lichtenstein and France's General Gribeauval, which improved the precision of artillery, *and* by the scientific revolution of Galileo and Newton.[10]

The exemplars have been drawn together to form a long-term narrative from which common themes have emerged, but there is scope for further development of an approach that uses the new cannon warfare as its historical thread. To take two examples, Descartes, an experienced soldier and military engineer, investigated the laws of refraction by analogy with the trajectory of an artillery missile and its laws. This was based on his observation that a cannon ball, shot at a certain angle over a river, will not penetrate the water but will be refracted at the same angle to the other side, 'something that was sometimes tried with unfortunate results, when for recreation pieces of artillery were fired towards the bottom of a river, resulting in injury to those on the other side'.[11] Simon Stevin, a significant figure in the development of mathematics and mechanics, also played a crucial military role as teacher and adviser to Prince Maurice of Nassau, discussed in the chapter on Thomas Digges.

The programmatic correspondence with Bacon has been obscured by Kuhn's attempt to identify two separate traditions in the history of science, the mathematical and the Baconian, though he was right in identifying a tension between the abstract

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[10] *ibid*., 354.
[11] Grossman, *Social Foundations,* in Freudenthal and McLaughlin, *Social and Economic Roots, 136.* 'Ce qu'on a quelquefois expérimenté avec regret, lorsque, faisant tirer pour plaisir des pièces d'artillerie vers le fond d'une rivière, on a blessé ceux qui était de l'autre coté sur le rivage.' My translation.

nature of mathematics and the more materially engaged experimental inductive method. The thesis has investigated the attempt to reconcile that tension, something crucial to an understanding of how theory and practice were brought together in complex historical reality.

Rather than focus narrowly on whether or not science was useful to the practice of gunnery, the thesis has focussed on the impetus for discovery generated by the feedback between the practice of gunnery and the development of scientific theory. It has demonstrated that from Tartaglia on, all endeavoured to mediate between practical demands and the theoretical tools available, recognising and attempting to overcome the limitations of a particular theory. Thus there was an iterative process over time as both technology/practice and theory both developed and moved closer together. Steele's research has shown that this process continued and accelerated in the eighteenth century and beyond. The contribution of this thesis to the historiography of this subject is to show that the process that Steele describes began, not in the eighteenth century, but in the sixteenth and seventeenth centuries.

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