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Mechanics and Natural Philosophy before the Scientific Revolution

Edited by
Walter Roy Laird
and Sophie Roux

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PREFACE

This book is the result of a workshop entitled “Mechanics and Natural Philosophy: Accommodation and Conflict” that was held in La Orotava, Tenerife, on January 30–February 1, 2004. The workshop was part of a program on the general theme “From Natural Philosophy to Science” generously sponsored by the European Science Foundation. With the exception of the essay by Edith Sylla, who kindly agreed to the labour of contributing to the volume without the compensatory pleasure of having been in Tenerife, all the papers here were read at the workshop in preliminary form and then thoroughly revised for publication.

In addition to the scholars whose essays follow in this volume, participants at the workshop also included Romano Gatto, Elzbieta Jung, Cees Leijenhorst, Ian Maclean, Antoni Malet, Peter McLaughlin, Pier Daniele Napolitani, Jürgen Renn, and Hans Thijssen. Their contributions to the workshop in Tenerife, whether mentioned in the footnotes or not, have left their mark throughout the arguments of this book. We hope that this volume will continue in another form those stimulating and productive discussions. For we think that the result demonstrates the challenges that the history of ancient, medieval, and pre-Galilean mechanics now presents—challenges to philologists with the skills necessary for editing, commenting on, and translating texts, challenges to historians tracing the interactions both between texts and between texts and practices that resulted in new ways of thinking about mechanics and natural philosophy, and challenges to scholars engaged in delineating the structures of scientific knowledge.

For their efficiency in organizing the workshop and especially for their warm hospitality in Tenerife, we thank Carlos Martín and José Montesinos, Director of the Fondación Canaria Orotava de Historia de la Ciencia. And for their patient and constant support, we thank Hans Thijssen, Chairman of the European Science Foundation program “From Natural Philosophy to Science”, and Cees Leijenhorst, the coordinator of the program.

Finally, this book would not have been possible without the help of Mark Naimark, who translated papers originally written in French and checked the language of papers written by non-native English speakers; and of Marije van Houten-Hettinga, who, through the generosity of the Région Rhône-Alpes (contrat de plan État-Région, Sciences Humaines et Sociales, appel d’offres 2003), assisted in the last phase of a long editing process.

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INTRODUCTION

By the end of the 17th century, modern mechanics – the general science of bodies in motion – had replaced natural philosophy to become the paradigm of the physical sciences, a place it held at least through the 19th century. Modern mechanics was forged in the 17th century from materials that were inherited from Antiquity and the Middle Ages through various and sometimes divergent conceptual traditions that had been transformed in the course of the 16th century partly under the influence of technological innovations and an interest in practical applications. The purpose of the workshop "Mechanics and Natural Philosophy: Accommodation and Conflict" from which this book arose was to articulate the conceptual background to the historical emergence of modern mechanics in relation to natural philosophy. In the workshop we did not pretend to offer a comprehensive account either of natural philosophy or mechanics from Antiquity to the 17th century – or even of the relation between them. Rather, our purpose was much more modest: to present a variety of moments when conflicts arose within one textual tradition, between different traditions, or between textual traditions and the wider world of practice, and to show how the accommodations sometimes made ultimately contributed to the emergence of modern mechanics. More specifically, before the workshop participants were encouraged to consider the following general questions:

1. Problems that are now referred to as mechanical were once treated within a variety of different disciplines, including logic and theology. Did these theoretical contexts have any effect on the formation of mechanical concepts? How did the various types of problems come eventually to be identified as belonging to a single discipline? Was mechanics considered an art or a science? Where and how was it taught? How did mechanics, once considered an Aristotelian mixed science, become identified with the science of nature as such? And in general, what was the status of mechanics in relation to other disciplines?

2. What are the criteria by which we say that a given text belongs to a given conceptual tradition? Not every writer who cited or even commented on the pseudo-Aristotelian *Mechanica*, for example, can be placed within the conceptual tradition of that work; and beyond this simple criterion lie even more complications. Certain words or complexes of words (e.g., "natural" versus "violent motions", *gravitas secundum situm*, *impetus*) are often

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 1–11.

associated with a certain physical theory, as though the words themselves contain the theory in a nutshell. Further, concepts have histories of their own, which may be different from the histories of the words representing them, and the same word can have various meanings in various contexts. Similarly, since the limitations of the mathematical tools at hand imposed certain constraints, we might be tempted to define a conceptual tradition by its mathematical tools. But in the period considered it would seem that the Euclidean theory of proportion was the main, if not the only mathematical tool applied to the analysis of motion and to the science of weights. Lastly, a conceptual tradition might be characterized by the concrete models it appeals to in its explanations (such as the lever, the inclined plane, a thrown ball, a sling, a pendulum, etc.). But the expression "concrete models" is ambiguous: it refers to common experiences as well as to the learned knowledge of practitioners, to figures in books as well as to general physical principles.

3. What were the material routes that these traditions followed and how are the relationships between these traditions to be understood? How were mechanical texts circulated, both before and after the introduction of printing? What were the active and interacting traditions at a given time and at a given place? Were they perceived as conflicting? If they were, why was an accommodation looked for and according to what principles was it eventually found? Do we as historians now see these traditions as conflicting and what criteria do we now apply to determine whether they were?

The chapters in this book all focus on at least one of these questions. But, as often happens in historical studies, the particular materials here are so rich and complex that they cannot be compassed by any preliminary set of questions. We hope that this collection of essays at least begins to answer some of the questions above, while giving rise to new ones that we had not anticipated before we undertook it.

Until about the 17th century, natural philosophy (*physica* or *philosophia naturalis*) was the general science of motion and change – motion being understood as any sort of gradual change, whether change of quality, change of quantity, or change of place. As the name suggests, natural philosophy was concerned mainly with those motions and changes that occur naturally, such as generation, growth, and spontaneous motions such as heartbeat and digestion, the fall of heavy bodies and the rise of light, and the circular motions of the celestial spheres. Throughout Antiquity and the Middle Ages, natural philosophy was dominated by the works of Aristotle, though even his most loyal followers introduced significant novelties into the Aristotelian system. In the *Physica,* Aristotle had established the general principles of motion and change that govern all natural bodies; in his more specific natural works – such as *De caelo*, *De generatione et corruptione*, and the various works *De animalibus* – Aristotle applied these principles to natural changes of all kinds occurring in animate and inanimate bodies: generation, growth, the fall of heavy bodies, and the motions of the stars. But inevitably, within the context of natural philosophy, there also arose questions concerning unnatural or forced motions, such as the motion of projectiles and in general the changes effected by men through the various arts. Partly this was because Aristotle used forced motions and in general the changes produced by art as analogies in order to discover the less obvious causes of natural motions; and partly it was because natural and forced motions are often inextricably combined in daily experience.

In contrast to natural philosophy, the ancient art or science of mechanics was notable for working against or at least outside of nature to effect motion for the use and benefit of mankind. This, at least, was the view taken in the pseudo-Aristotelian *Mechanica* ("Questions of Mechanics" or "Mechanical Problems") (4th century BC), the earliest known theoretical treatment of machines and the earliest attempt to reduce their operations to a single principle. In its introduction, the *Mechanica* suggested that machines of all sorts work against or outside of nature in order to effect changes that are of benefit to men. A machine that moves a large weight with a small power, for example, produces an effect for human benefit, and this effect is not natural, for it violates the Aristotelian physical assumption that a moving power must be greater than the weight it moves. Mechanics and physics thus seem to be in conflict. But the introduction of the *Mechanica* went on to suggest that in mechanical problems, nature provides the subject matter and mathematics the explanation, which is similar to the account Aristotle gave elsewhere of the so-called subalternate sciences – astronomy, optics, music, and of course mechanics itself. In this earliest mechanical work, then, we find the origins of the ambiguous – even paradoxical – relation between mechanics and natural philosophy. On the one hand, mechanics concerns effects that are produced against or outside nature; on the other, mechanics consists in applying mathematics to natural things. In the *Mechanica*, this application takes the form of reducing all mechanical effects to the motions of the balance, which in turn are explained through the marvellous properties of the circle.

In the centuries after Aristotle, mechanics was developed in two general streams, the one more mathematical and theoretical, represented by Archimedes (3rd century BC), and the other more practical and technical, represented by Heron of Alexandria (1st century AD). Although Archimedes was known in Antiquity and the Middle Ages mainly as a designer and builder of instruments and machines, his extant writings on mechanics are entirely theoretical and mathematical. Most notable of these for our purposes is *On Plane Equilibrium*, in which he gave an entirely static proof of the law of the lever that relied on the notion of centre of gravity. Heron of Alexandria, in contrast, while mentioning Archimedes and being obviously inspired by the pseudo-Aristotelian *Mechanica*, wrote extensively on the design and construction of machines. Perhaps his main contribution to the science of mechanics was to reduce all complex machines to what he established as the canonical five simple machines (lever, wheel and axle, pulley, wedge, and screw), each of which in turn he reduced ultimately to the balance. Mark Schiefsky shows in his contribution to this volume that Heron's reduction of complex machines to the five simple machines, and their reduction to the balance, both relied on and attempted to explain the knowledge of machines acquired by mechanical practitioners through experience. Like the pseudo-Aristotelian *Mechanica*, Heron's purpose in general was to explain how with machines one could move great weights with small forces, seemingly in violation of the Aristotelian physical assumption that moving powers must be greater than weights. By offering such an explanation, according to Schiefsky, Heron was in effect explaining away the apparent opposition between machines and nature by modifying natural principles to accommodate machines. Heron also challenged the Aristotelian physical assumption that all forced motion requires a constant mover, using the balance and the inclined plane as cases in point. For a balance in equilibrium, he argued, can be moved by a force as small as one pleases, and a weight on a perfectly smooth horizontal surface likewise. But despite these challenges to its central principles, Heron did not propose to replace Aristotelian natural philosophy with another, new kind of physics.

Although Heron's *Mechanics* was unknown in the medieval Latin West and survives today only in a medieval Arabic translation, an extract from it, which included his account of the five simple machines, was later incorporated by Pappus of Alexandria into Book VIII of his *Mathematical Syntaxis* (3rd century AD), the fourth main source of ancient mechanics. In general, Pappus' purpose was to collect and systematize previous works, although he also tried to fill in the gaps left by his predecessors. In the case of mechanics, he provided a definition of centres of gravity missing from Archimedes, and he attempted to reduce the inclined plane to the balance.

In the Latin Middle Ages, there was, strictly speaking, no science of mechanics. Although there were references to a science of mechanics in the works of Aristotle and other ancient authors, there were no extant treatises on mechanics. Neither the pseudo-Aristotelian *Mechanica* nor Pappus' *Mathematical Syntaxis* were recovered and translated into Latin before the 16th century, and Heron's *Mechanics* remained unknown in the West until very recently. The fate of Archimedes was more complex. His mechanical works exerted considerable influence in the Arabic-speaking world of the early Middle Ages, but although almost all of Archimedes' works were translated from the Greek into Latin in the late 13th century, they had very little influence in the West until the beginning of the 16th century. In the absence of any texts on the ancient science of mechanics, the word *mechanica* was applied solely to the mechanical arts (*artes mechanicae*), which included variously agriculture, metalworking, building, clothmaking, and the like – those arts that provide the necessities of life and are linked with manual labours and dirty hands. As such, they were also characterized as *sellularian* (the Latin translation of the Greek *banausia*, handicrafts) or "adulterine" (because they were adulterated with practice and physical needs) and were explicitly contrasted with the liberal arts and with philosophy in general. And finally, the art of designing and building machines, when it was distinguished at all from the other arts, was called the *scientia de ingeniis*, although this was largely an empty name rather than a coherent body of theory and practice.

The science of weights (*scientia de ponderibus*) is the best candidate for a medieval science of mechanics, although it was never identified with the *scientia de ingeniis* or included with the *artes mechanicae*, since it was considered a theoretical and mathematical science like astronomy and music. From Greek and Arabic works on the unequal-armed balance (the *statera* or Roman balance), it developed in the course of the 13th century, especially in the works of Jordanus de Nemore, into a sophisticated, mathematical treatment of the main problems of statics and a few problems of dynamics, and it held a minor place alongside the other mathematical sciences at the medieval university. The science of weights survived through the 14th and into the 15th century, though little was added to the achievements of the 13th century; only in the 16th century did the science of weights converge with the recovered mechanical traditions of Antiquity to contribute to the development of a new science of mechanics.

Meanwhile, natural philosophy was almost entirely separate from both the mechanical arts and from the science of weights. Topics that we now identify as mechanical, inasmuch as they arose within the Aristotelian corpus of natural philosophy, were discussed using natural philosophical principles and usually with only the most rudimentary mathematics. Several passages on such topics in Aristotle's *Physics* gave rise to an extensive tradition of commentary and criticism in Late Antiquity and the Middle Ages, notably on the motion of projectiles and the relation between movers and things moved. In Book VII of the *Physics*, as part of his introduction to the argument for the Prime Mover, Aristotle gave a set of rules for the relation between movers and things moved and the speeds and times of their motions. By the early

14th century, these rules had been extensively discussed and interpreted in various ways in the Greek, Arabic, and Latin commentary traditions. But in 1328, Thomas Bradwardine, drawing on the medieval theory of proportion, offered in his *Treatise on the Ratios of Speeds of Motions* (*Tractatus de proportionibus velocitatum in motibus*) a strikingly novel and later widely accepted mathematical interpretation now known as Bradwardine's rule. In his contribution to this volume, Jean Celeyrette argues that Bradwardine's intention was not to replace Aristotle's rules of motion with his own, but only to act as a commentator and to offer an interpretation that made these rules clearer and more consistent. Among Bradwardine's successors who accepted and developed further this interpretation in the 14th century, Celeyrette continues, only Richard Swineshead and Nicole Oresme offered significant new insights: Swineshead applied Bradwardine's result to new physical problems, and Oresme tackled the conceptual difficulties implied by stating a proportionality between the speeds on the one hand, and the ratios between forces and resistances on the other. According to Celeyrette, only Swineshead and Oresme offered Bradwardine's rule as a mathematical law of local motion, although neither of them enjoyed a lasting influence. And finally, contrary to the common modern opinion, Celeyrette concludes, Bradwardine's rule was not properly a mathematical function.

Bradwardine's rule was founded on the medieval theory of proportion, and in particular on the idea that ratios could be compounded from other ratios. At the time that Bradwardine wrote, Edith Sylla argues in her contribution to this volume, there were in fact two different ways of understanding the composition of ratios, and Bradwardine chose the one that made his dynamical rule simple and elegant. By the 16th century, however, Bradwardine's way of compounding ratios had been superseded by the simple multiplication of denominations. As a result of this purely mathematical change, then, Bradwardine's rule had disappeared from discussions of motion by the time Galileo took up the subject. Thus, the fate of Bradwardine's rule was tied not to its failure to correspond to the actual behaviour of bodies in motion, Sylla concludes, but to the falling out of favour of a mathematical idea.

The other topic that aroused considerable controversy in the commentary tradition of natural philosophy – a topic that we would now consider proper to mechanics – was the continued motion of projectiles. Since he held that all forced motions needed the continual action of a cause, Aristotle had suggested that projectiles continued to move after they were thrown because of the continual action of the medium through which they move. But even the earliest Greek and Arabic commentators found fault with this explanation and suggested various alternatives, including some sort of moving power impressed on the projectile itself. In the 14th century, Jean Buridan, a master at the University of Paris, gave the most thorough and coherent account of this alternative under the name of *impetus*. Although *impetus* theory directly challenged Aristotle's explanation of projectile motion, Jürgen Sarnowsky argues in his contribution to this volume that there was no distinct *impetus* physics as a stage intermediate between ancient physics and the classical theory of inertia, as some historians of science have suggested. This is because the nature and properties of the impressed force or *impetus* varied greatly from the earlier commentators to Buridan and his successors, so that there never formed around it a distinct and coherent alternative to Aristotelian physics. But although *impetus* was just an ad-hoc solution to particular problems in Aristotelian dynamics, Sarnowsky concludes, it called into question other key Aristotelian principles and thus contributed to the demise of Aristotelian natural philosophy.

Despite the innovations of Bradwardine, Buridan, and Oresme, when the science of mechanics emerged in the course of the 16th century, it did not emerge from within natural philosophy. Rather, it arose largely out of the newly recovered mechanical sources from Antiquity and the Middle Ages – the pseudo-Aristotelian *Mechanica*, the almost complete works of Archimedes, the *Pneumatica* of Heron, the *Mathematical Syntaxis* of Pappus, and the Jordanus tradition of the science of weights. The first of these to be edited, translated, paraphrased, and commented on was the *Mechanica*, and historians of science have recently shown that its influence was to be felt through the whole of the 16th century. Although now it is usually attributed to Strato, a successor of Aristotle, it relied sufficiently on Aristotelian natural principles that in this period it was commonly, though sometimes doubtfully, attributed to Aristotle himself. The attribution to Aristotle challenged commentators to compare its mechanical principles to the principles of Aristotle's natural philosophy found in his other works, and it came to be the main locus for discussions about the general relation between mechanics and natural philosophy.

Christiane Vilain, in her contribution to this volume, examines how commentators on the *Mechanica* treated the opposition of circular and rectilinear motion, which Aristotle had said were incommensurable, and their relation to natural and violent motion, which was a fundamental dichotomy in Aristotle's natural philosophy. In the *Mechanica*, the circular motion of the balance is resolved into two rectilinear motions, one of which is identified as natural while the other as violent or against nature, though it is not at all clear that the natural motion here must be downwards. Vilain shows that 16th-century commentators were divided on this question, Benedetti being the first to consider explicitly the motion of a sling – where the "natural" motion is no longer the downward motion of a heavy body, but the rectilinear and tangential motion of the stone as it is flung from the sling. In this way, she shows how commentators on the *Mechanica* both drew upon and modified the principles of Aristotelian natural philosophy.

One of those who commented on the pseudo-Aristotelian *Mechanica* was Giuseppe Moletti, professor of mathematics at the University of Padua in the 1580s, to whom it gave particular occasion to reflect on the relation between nature and art. In his contribution to this volume, Roy Laird shows how Moletti, in his lectures on the *Mechanica*, took up the suggestion that art both imitates and overcomes nature for human benefit. Drawing on another Aristotelian text, the *De motu animalium*, Moletti went on to suggest that nature itself uses mechanical means in its own works. This implies, according to Laird, that – for Moletti at least – to understand mechanics was also to understand the workings of natural things. With the benefit of hindsight, it seems that from there it would be only a small step to thinking of mechanics itself as the science of nature.

In the 1580s, at the same time that Moletti was lecturing on the *Mechanica* at Padua, the leading natural philosophers at the University of Pisa were discussing several topics that would later come to be of special interest within the emerging science of mechanics, including the fall of heavy bodies, motion in a void, and *impetus*. Mario Otto Helbing, in his contribution to this volume, shows that, contrary to the common opinion, Andrea Cesalpino and Francesco Buonamici were not old-fashioned scholastics treating these topics entirely within the tradition of the medieval commentators on Aristotle, but humanists making extensive use of the newly recovered ancient texts. In his *De motu*, Buonamici drew from the *Mechanica* when he asserted that since the same principles hold for natural motions and for motions that are against nature, mechanics is especially close to natural philosophy. Buonamici also drew on Archimedes: in the *On Plane Equilibrium* he found the concept of centre of gravity, and he devoted one book of his *De motu* to an exposition and criticism of Archimedes' hydrostatical treatise *On Floating Bodies*. Buonamici's most famous student – Galileo – would later take up these same topics in his earliest works on mechanics and motion.

Since there was no single canonical text in the 16th century to define the scope and contents of mechanics, the body of problems and questions that came under mechanical scrutiny was in flux. For all its influence in the 16th century, the *Mechanica* was not a systematic treatise, but rather a collection of miscellaneous questions on a number of applications of mechanical principles, including the balance and lever, pulleys, wheels, and the like. The *Mechanica* did not contain, notably, any account of the inclined plane. For this one has to look to the traditions of Heron, Pappus, and Jordanus. Egidio Festa and Sophie Roux, in their contribution, present a history of the attempts to discover the law of the inclined plane from Heron to Galileo. They show how Heron and Pappus tried to reduce the inclined plane to the balance, and how Pappus was misled by trying to determine the force needed to move a weight on an inclined plane in relation to the force needed to move it on a horizontal plane. Jordanus, in contrast, arrived at the correct solution by appealing to the notion of positional weight; and Galileo also arrived at the correct solution, not through reading Jordanus, however, but by reducing the inclined plane to the bent lever and thus to the balance. Galileo's solution to the inclined plane had implications far beyond the theory of simple machines, since it provided him with a key principle for his new science of motion. Festa and Roux conclude that this solution was arrived at not by new mathematical techniques but rather by the clarification of concepts such as gravity and moment, and that, in general, similarities in method and approach may be the result of a common paradigm rather than direct influence.

As we have already mentioned, some of the topics that we now consider mechanical arose within natural philosophy; others arose from practical experience with actual machines. One of these latter was the pendulum, which Jochen Büttner in his contribution to this volume characterizes as a "challenging object". Because challenging objects by definition do not lend themselves easily to treatment by the means available within existing frameworks of knowledge, they offer the opportunity for conceptual transformations of those frameworks. Büttner shows that the pendulum emerged as a challenging object only at the beginning of the 16th century and largely from its technological applications, in the regulation of clocks, for example, and in machines for the accumulation of power. Galileo took up the pendulum and tried (unsuccessfully) to establish a relation between its isochronism and his newly discovered law of chords; Isaac Beeckman saw planetary motion as analogous to the pendulum; and Baliani used the pendulum as the principle of the laws of motion. These instances show, Büttner concludes, that the pendulum was not only a challenging object, but also an object of shared knowledge, and that the challenge that it offered led to transformations in early-modern mechanics.

If Büttner examines a case of the interaction between the world of texts and the world of practice, the final papers in this volume examine the emergence of mechanics within different institutional and national contexts. In the early 16th century, writing on mechanics was mostly an Italian affair, but by the end of the century, it had developed outside of Italy as well. In his paper, Victor Navarro argues that Spanish authors drew on the same classical and renaissance sources in mechanics as the rest of Europe, and that Spain was not so much the bastion of conservative natural philosophy as is often thought. In the first half of the 16th century, Diego Hurtado de Mendoza, Imperial Ambassador to Venice, not only encouraged the early mechanical work of Alessandro Piccolomini and Niccolò Tartaglia but also translated into Spanish the pseudo-Aristotelian *Mechanica*. Later in the century, the architect Juan de Herrara founded the Academy of Mathematics in Madrid to provide training in theoretical and practical mathematics, especially navigation and artillery. His successors included Juan Bautista Villalpando, whose main interest was static equilibrium as applied to architecture and who drew on the pseudo-Aristotelian *Mechanica*, Pappus, Federico Commandino, and Guidobaldo dal Monte; and Diego de Alava, whose work on artillery was based largely on Niccolò Tartaglia. Although *impetus* theory and the correct law of free fall were taught as part of natural philosophy at Spanish universities by such notables as Juan de Celaya, Domingo de Soto, Francisco Valles, Diego de Zuñigo, and Diego Mas, Navarro concludes that because mechanics was taught only at the Academy and not at the university, there was little opportunity for the sort of contact between mechanics and natural philosophy that elsewhere would give rise to Galileo's new science of motion.

In the final paper in this volume, Geert Vanpaemel describes how the Jesuits, barred from teaching at the universities in the Spanish Netherlands, established themselves at Antwerp and Leuven by teaching mathematics, a subject neglected by the universities. The first of these Jesuit mathematics teachers was Gregorius a Sancto Vincento, who had been a student of Christopher Clavius's at the Collegio Romano. While Gregorius and then Jean Charles de la Faille were concerned mainly with pure mathematics, later Hugo Sempilius and Joannes Ciermans were concerned also with its practical applications, especially in mechanics. Vanpaemel argues that Ciermans considered the action of fluids to be a machine effected by nature without the use of levers or pulleys, and that he implied that to understand the laws of statics as applied to hydrostatics was to understand nature. Nevertheless, Vanpaemel concludes that Ciermans did not espouse a full-blown corpuscular philosophy of nature, because he still allowed room for hidden causes in natural philosophy.

The modern science of mechanics, then, emerged from conflicts between the different textual traditions of ancient and medieval mechanics, between an inchoate mechanics, natural philosophy, and mathematics, and between theoretical considerations and practical experience. To a large extent, modern mechanics is the result of the accommodations that were effected in order to resolve these conflicts and establish it as an independent, mathematical science with a practical bent. The history of mechanics – unlike the history of astronomy or optics – is the history of a variety of disparate topics and problems that were treated in different ways within very different disciplines and conceptual traditions. For this reason it is not enough for the historian of mechanics to trace these various problems and their solutions from one era to another until the right answer was hit upon. Rather, the history of mechanics is to a large extent the history of the relations between different textual and conceptual traditions, between different disciplines, and between theory and practice. We hope that the essays in this volume illustrate some of the ways that this history may be written.

1. ANCIENT AND MEDIEVAL MECHANICS

MARK J. SCHIEFSKY[1]

THEORY AND PRACTICE IN HERON'S *MECHANICS*

INTRODUCTION

In Greco-Roman antiquity, the art or science of mechanics (*μηχανικὴ τέχνη* or *ἐπιστήμη*) encompassed a wide range of manual and intellectual activities, from the building of precision devices such as artillery and automata to the sophisticated theoretical analysis of machines in terms of concepts such as force and weight. As such, mechanics involved many different kinds of knowledge. Two key categories of mechanical knowledge may be distinguished: (1) theoretical knowledge, a set of relations between abstract concepts such as force and weight, sometimes couched in deductive form, and transmitted largely in written texts; and (2) practitioners' knowledge, arising in connection with the productive use of technology and acquired by practitioners in the course of their professional activity. A paradigm example of theoretical mechanical knowledge is the law of the lever, stated and proved by Archimedes as a precise quantitative relationship between forces and weights. But any practitioner who had made use of a lever would be familiar with the fact that it is easier to move a weight if it is placed closer to the fulcrum; a rough generalization of this kind, which can immediately be applied in practice, is a paradigm example of practitioners' knowledge. While it is crucial to distinguish between theoretical mechanics and practitioners' knowledge, there is substantial evidence of a two-way interaction between them in Antiquity. On the one hand, mechanical technology was sometimes developed by applying theoretical knowledge. But this was by no means always the case: new technologies often preceded any theory that could explain them. The growth of technology produced a set of problematic phenomena that called for explanation and thereby stimulated the growth of theoretical mechanics.[2]

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[1] Department of the Classics, Harvard University. I would like to express my thanks to all those who discussed an earlier version of this paper with me at the Tenerife meeting, and especially to Roy Laird and Sophie Roux for their very helpful written comments. The paper was largely written during a sabbatical stay at the Max Planck Institute for the History of Science in Berlin, and I am deeply grateful to Jürgen Renn for his invitation to spend the academic year 2003–2004 there.

[2] For a similarly wide conception of mechanics, see the introduction to Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, pp. 1022.13–1024.2. Pappus indicates that "the mechanician Heron and his followers" (*οἱ περὶ τὸν Ἥρωνα μηχανικοί*) distinguished between the "rational" (*λογικόν*) part of mechanics (involving knowledge of geometry, arithmetic, astronomy, and physics) and its "manual" (*χειρουργικόν*) part

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 15–49.

The *Mechanics* of Heron of Alexandria, a work in three books that survives in its complete form only in an Arabic translation of the 9th century AD, is an especially fruitful source for studying the interaction between ancient theoretical mechanics and practitioners' knowledge. There is every reason to think that Heron was well informed about the technology of his time; his accounts of technological devices and procedures (mainly in Books 1 and 3) have been confirmed by other literary accounts, archaeological evidence, and modern attempts to put them into practice.[3] While the methods and devices that Heron describes are to some extent idealized, it would be rash to deny that his text can yield substantial information about the knowledge of ancient practitioners. As for theoretical mechanics, Heron mentions Archimedes by name some ten times in the *Mechanics*; he is cited as an authority for the proof of the law of the lever, which Heron makes no attempt to demonstrate himself.[4] The second main source on which Heron drew for mechanical theory was the pseudo-Aristotelian *Mechanica*. Though it is not mentioned explicitly in the *Mechanics*, this work provides an important precedent for the core of Heron's theoretical project: the attempt to "reduce" the five simple machines or mechanical powers (the wheel and axle, lever, compound pulley, wedge, and screw) to the circle and, ultimately, the balance (2.1–32).[5]

Heron's account of the five mechanical powers in 2.1–32 provides an excellent illustration of several aspects of the interaction between theoretical mechanics and practitioners' knowledge. First, it illustrates the way in which

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(involving mastery of crafts such as bronze-working, building, carpentry, and painting). For the importance of distinguishing between theoretical knowledge and practitioners' knowledge, see Damerow and Lefèvre, *Wissenssysteme im geschichtlichen Wandel*. On technology as applied theory see esp. Lewis, *Surveying Instruments* on surveying instruments and other fine technology; Russo, *The Forgotten Revolution*, ch. 4 makes a powerful case that the impact of theory on technology in Antiquity has been greatly underestimated.

[3] Drachmann, *The Mechanical Technology*, p. 140, argues that the female screw cutter discussed in *Mechanics* 3.21 was a recent invention at the time when Heron wrote; see his account of a successful attempt to construct this device in *id.*, "Heron's Screwcutter". See also *id.*, "A Note on Ancient Cranes"; *id.*, *Ancient Oil Mills and Presses*, *passim.* Russo, *The Forgotten Revolution*, pp. 130–137, argues that the technology described by Heron dates from the early Hellenistic period, i.e., several centuries before the time at which he lived (assuming this to be the first century AD). But even if this is right, it would not undermine the value of Heron's text as providing evidence of practitioners' knowledge (albeit of an earlier period).

[4] Aside from a single reference to one Posidonius in 1.24 (*Opera*, vol. II, p. 62.28), no other figure is mentioned by name in the *Mechanics*. For Heron's references to lost works of Archimedes see Drachmann, "Fragments from Archimedes".

[5] This, along with other parallels to the *Mechanica*, was noted and emphasized by Carra de Vaux in the introduction to his edition of Heron's *Mechanics*, pp. 22–24.

technology posed a challenge to theoretical mechanics. Each of the five powers made it possible to lift a large weight with a much smaller force; each therefore raised a challenge to the intuitive view, given support by Aristotle's natural philosophy, that a weight can only be moved by a force equal to it. Second, Heron's attempt to reduce the five powers to the balance brings out the importance of models in mediating between theoretical mechanics and practitioners' knowledge. The crucial step in Heron's reduction of each of the five powers is the identification of a similarity between the power in question and the balance; the intellectual operation is that of "seeing" how each of the powers really is a kind of balance. In this way, the balance serves as a model that makes it possible to give an explanatory account of the five powers that traces their operation back to natural principles. Hence, despite the initially paradoxical or wondrous character of the effects they produce, the five powers (like other mechanical phenomena) can be integrated into the explanatory framework of natural philosophy: mechanics in Antiquity, no less than in the Renaissance, was part of the science of nature. Finally, Heron's account of the five powers, along with other closely related passages of the *Mechanics*, also reveals how the application of models could lead to conclusions that were sharply at variance not only with practitioners' knowledge but also with deeply held assumptions of much of ancient natural philosophy, such as the notion that the motion of a body implies the presence of a force that moves it.

THE CHALLENGE OF THE FIVE MECHANICAL POWERS

Before turning to Heron's account of the five powers, it is worth noting that the very notion of a mechanical "power" or simple machine is attested for the first time in Heron himself. Of the five powers discussed in *Mechanics* 2.1–32, three—the wheel and axle, the lever, and the wedge—can be documented from very early times.[6] All three are discussed in the *Mechanica*, which probably dates from the early 3rd century BC.[7] The situation is somewhat different for the compound pulley and the screw. A passage in the *Mechanica* may refer to the former, but the sense is disputed; various sources ascribe its invention to Archimedes.[8] The screw is not

[6] See Heron's remark in *Mechanics*, 2.2 that the lever was the first power to be developed, and that it was discovered by trial and error (*Opera*, vol. II, p. 98.7–28).
[7] The lever is discussed in problem 3 (850a30–b9), the wheel and axle in problem 13 (852a11–21), and the wedge in problem 17 (853a19–31).
[8] Pseudo-Aristotle, *Mechanica*, 853a32–b13 (prob. 18). See Drachmann, "Heron's screw cutter", p. 658; Krafft, *Dynamische und statische Betrachtungsweise*, pp. 44–46.

mentioned in the Aristotelian text at all, and seems to have been an invention of the Hellenistic period; it may even have been developed as a specific application of a mathematical construction.[9] However this may be, Heron is the first author we know of to identify this particular set of machines as making up a distinct group. The Hippocratic treatise *On Fractures*, which probably dates from the late 5th century BC, mentions the winch, the lever, and the wedge as the three devices that enable human beings to achieve the greatest power.[10] In the *Mechanica*, while the lever is indeed treated as a simple machine to which other devices can be reduced, the pulley, wedge, and winch are not accorded any special status.[11] Heron's recognition of the five powers as belonging to a special class is reflected in a terminological innovation, the use of the term δύναμις ("power") to refer to a simple machine. The choice of this term obviously reflects a recognition that these machines could bring about especially powerful effects.

Thus the notion of simple machine seems to have developed in the period between the *Mechanica* and Heron. Why? The reason surely lies, in part, in the great flourishing of technology during this period. The large-scale building projects of the Hellenistic age stimulated the development of new technology and provided extensive opportunities for its application.[12] Moreover, many of the machines developed in this period involved the creative combination of the five powers. This is apparent from Book 3 of the *Mechanics*, for example, where the combination of the wheel and axle, lever, screw, and pulley is characteristic of a number of cranes and presses.[13] A further area of Hellenistic technology that demanded the combination of different machine elements was the building of automata: here the goal was to produce highly complex and varied motions from a single initial movement.[14] The complex nature of ancient mechanical technology required practitioners to combine the wheel and axle, lever, pulley, wedge, and screw in creative ways; this ability went hand in hand with the identification of

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[9] Drachmann, *The Mechanical Technology*, p. 204.

[10] "Of all the contrivances that have been devised (μεμηχάνηται) by human beings, the strongest (ἰσχυρότατα) are these three: the turning of the winch (ὄνος), leverage (μόχλευσις), and wedging (σφήνωσις). Without these, either some one or all of them, human beings cannot accomplish anything that requires great strength" (Hippocrates, *On Fractures*, 31, pp. 3.528.16–530.1 Littré).

[11] See Krafft, *Dynamische und statische Betrachtungsweise*, p. 48.

[12] On the importance of large-scale urban planning in the Hellenistic period see Russo, *The Forgotten Revolution*, pp. 203–209.

[13] See Drachmann, "Heron's Screwcutter"; *id.*, *Ancient Oil Mills*; *id.*, *The Mechanical Technology*, pp. 94–140.

[14] See Heron's own treatise on the subject, the *De automatis*, in *Opera*, vol. I, pp. 338–453.

these devices as belonging to a special class, and was quite independent of any theoretical understanding of their operation.

Heron begins his account of the five powers with a description of their construction and use (2.1–6) that reveals a close familiarity with practitioners' knowledge.[15] The construction of the wheel and axle (2.1), the compound pulley (2.4), and the screw (2.5–6) is described in detail. Two uses of the screw are described: with a wooden beam or τύλος (Fig. 1), and with a toothed wheel (Fig. 2).[16] The account employs a good deal of specialized terminology for the mechanical powers and their parts; a number of these technical terms are explicitly flagged as such using the Greek word καλεῖσθαι, "to be called".[17] A striking feature of Heron's account is the statement of rough, non-quantitative correlations describing the behaviour of the five powers. Thus instead of a precise formulation of the law of the lever as a proportionality between forces and weights, Heron remarks that "the nearer the fulcrum is to the load, the more easily the weight is moved, as will be explained in the following" (2.2). Similarly for the wedge, "the smaller the angle of the wedge becomes, the more easily it exerts its effect" (2.4). In the case of the compound pulley Heron states that "the more parts the rope is bent into, the easier it is to move the load" (2.3).[18] Generalizations such as these would have been familiar to any practitioner

[15] Since the Greek text of these chapters was excerpted by Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, pp. 1114–1130, we can be reasonably certain about Heron's original terminology here.

[16] Note on figures: I agree wholeheartedly with Drachmann's emphasis, in *The Mechanical Technology*, p. 20, on the need to take account of the original manuscript figures, which often deviate greatly from those of modern editions. I have therefore based my exegeses as far as possible on his drawings, which were made on the basis of personal inspection of both the British Museum (B) and Leiden (L) manuscripts. Nevertheless, the figures in the edition of Nix and Schmidt (*Opera*, vol. II) do not in my opinion seriously distort Heron's meaning in the sections of the *Mechanics* with which I am concerned here, and they may provide a helpful orientation for the modern reader unaccustomed to the rather limited drawing techniques that are characteristic of the manuscripts. For these reasons, I include them alongside Drachmann's drawings in a number of cases.

[17] These include: ἄξων, "axle" (2.1); ὑπομόχλιον, "fulcrum"; and μοχλός, "lever" (2.2); μάγγανον, "crossbeam" to which pulleys were attached (2.4); and μονόστροφος, "single-turned", τετράγωνος, "square"; and φακωτός, "lentil shaped" (2.5; each of these is a technical term for a type of screw).

[18] *Opera*, vol. II, pp. 98.25–28, 102.36–37, 102.1–3. For the last of these passages, a close approximation to Heron's Greek text is given by Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, p. 1120.15–16; the Arabic translation of Heron garbles the meaning here (Drachmann, *The Mechanical Technology*, p. 55).

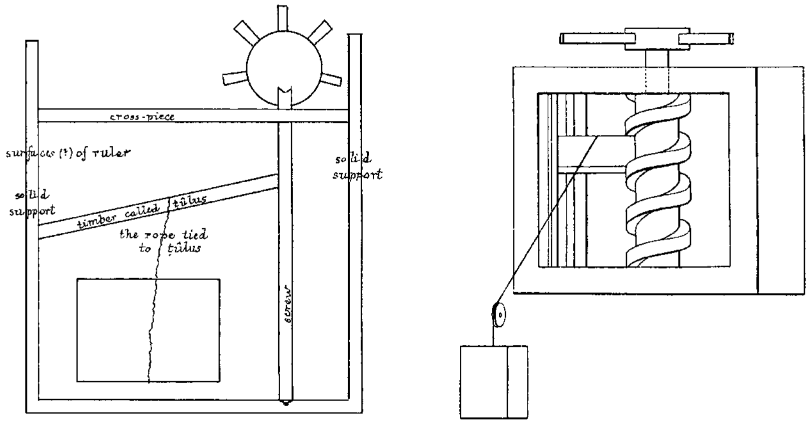

FIGURE 1. The screw used with a wooden beam or τύλος (*Mechanics* 2.5). On the left is Drachmann's drawing (*The Mechanical Technology*, p. 58) made from Ms B; on the right is the figure from *Heronis Alexandrini opera*, vol. II, p. 106.

who had made use of the five powers, and do not presuppose a theoretical understanding of their operation. The explanation of such generalizations, as well as their restatement in exact quantitative terms, is one of the goals of the theoretical account that follows in 2.7–20.

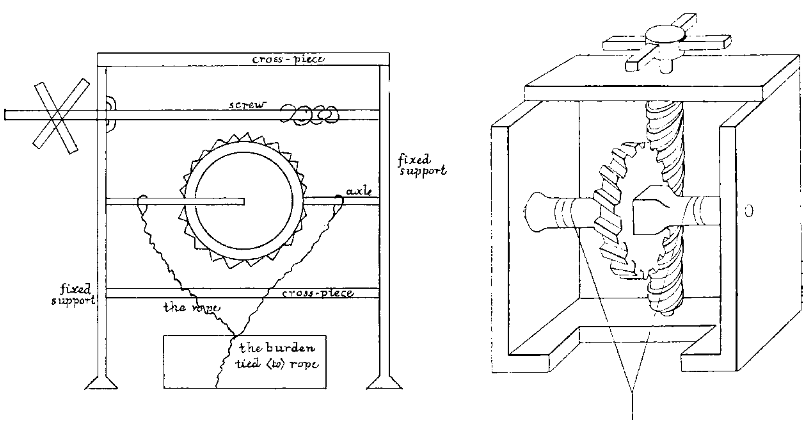

FIGURE 2. The screw used with a toothed wheel (*Mechanics* 2.6). On the left is Drachmann's drawing (*The Mechanical Technology*, p. 61) from Ms B; on the right is the figure from *Heronis Alexandrini opera*, vol. II, p. 110.

The key feature of each of the five powers that called out for theoretical explanation was their ability to move a large weight with a small force. In Heron's text this is expressed in a linguistically standardized fashion, with δύναμις or βία the normal term for "force" and βάρος the normal term for "weight".[19] The notion that a weight can only be lifted by a force that is equal to it was an assumption rooted in practical experience with devices such as the equal-armed balance or the simple pulley; Heron himself states in various passages of the *Mechanics* that to lift a weight without the use of a machine, a force equal to the weight is required.[20] Moreover he consistently writes of forces as measured in units of weight, viz. the talent. In a passage from the *Dioptra*, a work extant in Greek, Heron explains what the assumption of a moving force of 5 talents amounts to: "... that is, the moving man or slave should be one who can (δύνασθαι) move by himself without a machine 5 talents".[21] It was this intuitive relationship between force and weight, grounded in practical experience, which the five powers most dramatically called into question.

Seen from this perspective, Heron's account of the five mechanical powers stands in the same relationship to practitioners' knowledge as the *Mechanica*. That text discusses a wide range of mechanical devices and phenomena

[19] The somewhat complex terminological evidence may be summarized as follows: (1) Pappus, in the sections of his *Mathematical Collection* that contain excerpts from *Mechanics* 2.1–7 (*Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, pp. 1114–1130), uses both βία and δύναμις for the "small force" that can move a "large weight" (βάρος). While δύναμις refers primarily to capacity or ability and βία to physical strength, it is evident that the capacity in question here is that of being able to lift a weight (see Heron, *Dioptra*, ch. 37, in *Opera*, vol. III, p. 308.10–12; below, n. 21); there is no important distinction of meaning between the two terms. (2) Heron's *Dioptra*, ch. 37 (*Opera*, vol. III, pp. 306–312), describes the *baroulkos* or "weight-hauler", a device that uses toothed wheels to move a "given weight" by a "given force" (below, Fig. 10). In this description, which is closely parallel to both *Mechanics* 1.1 and 2.21, the "force" is always δύναμις; the weight moved is normally βάρος, but sometimes φορτίον ("load"). (3) Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, pp. 1060–1068, gives a slightly different description of the same device, which follows the same usage.

[20] See Heron, *Mechanics*, 1.22, in *Heronis Alexandrini opera*, vol. II, p. 58: "If we now want to lift the weight (*ṯiql*) to a higher place, we need a force (*quwwa*) equal to the weight (*ṯiql*)"; *ibid.*, 2.3, p. 98: "If we want to move any weight (*ṯiql*) whatever, we tie a rope to this weight (*ṯiql*) and we want to pull the rope until we lift it, and for this is needed a force (*quwwa*) equal to the weight (*ṯiql*) we want to lift"; and especially *ibid.*, 2.34 (i), p. 180: "For there is no difference between the moving of a weight (*ṯiql*) and the moving of a force (*quwwa*) that is equal to that weight (*ṯiql*)".

[21] Heron, *Dioptra*, 37 (*Opera*, vol. III, p. 308.10–12). The same explanation is given in *Mechanics*, 1.1 (*Opera*, vol. II, p. 4.4–5): "I mean that the man or youth who moves it is someone who can lift five talents by himself without any machine".

drawn from the realm of technology; each is viewed as bringing about a "wondrous" (θαυμαστός) effect that goes "beyond nature" (παρὰ φύσιν), in the sense that it deviates from the normal course of natural events and therefore demands a theoretical explanation. What makes such devices wondrous in particular is their ability to move a large weight with a small force; the paradigm example of such a device is the lever:

> For it seems strange that a great weight (βάρος) can be moved by a small force (ἰσχύς), and that, too, when a greater weight (βάρος) is involved. For the very same weight (βάρος), which a man cannot move without a lever, he quickly moves by applying the additional weight (βάρος) of the lever.[22]

Though Heron's terminology is slightly different, the idea is the same. The five powers bring about effects that at first sight seem wondrous or paradoxical; the problem is to provide a theoretical explanation of how they do so.

THE REDUCTION OF THE POWERS TO THE BALANCE

The core of Heron's account of the five powers is an attempt to reduce each power to a single principle, i.e., to explain by reference to a single principle why each power can move a large weight with a small force (2.7–20). The agenda for this section is set out in the opening sentence of Book 2:

> Since the powers by which a given weight (*ṯiql*) is moved by a given force (*quwwa*) are five, we necessarily have to explain their forms, their uses, and their names, because these powers are all reduced (*mansūba*) to the same nature (*ṭabīʿa*), though they are very different in form.[23]

In 2.7 Heron explains how a small force can balance a large weight in the case of weights placed on two concentric circles; he then goes on in the

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[22] Pseudo-Aristotle, *Mechanica,* 847b11–15. Translations from the Greek are my own unless otherwise indicated. Translations of Heron's *Mechanics* are based on Drachmann, *The Mechanical Technology*, which provides English versions made directly from the Arabic of most but not all the passages discussed in this paper. Translations of passages not in Drachmann are based on the text of *Heronis Alexandrini opera*. In checking both Drachmann's and Nix's versions against the original Arabic, I have made extensive use of a set of tools for the computer-assisted analysis of Arabic texts developed in the context of the Archimedes Project at Harvard University (http://archimedes.fas.harvard.edu).

[23] *Mechanics*, 2.1 (*Opera*, vol. II, p. 94.5–10); Engl. trans. Drachmann, *The Mechanical Technology*, p. 50, modified.

following chapters to argue that each of the five powers is analogous to two circles turning around the same centre. Thus the explanation for each of the five powers is given indirectly, via the explanation for the concentric circles; in this way all the powers are "reduced" or "referred" (*mansūba*) to a single nature. This procedure has a close parallel in the *Mechanica*, and was probably inspired by it. The introduction to the *Mechanica* identifies the circle as the "primary cause" of all mechanical phenomena (τῆς αἰτίας τὴν ἀρχήν), then goes on to make the following claim:

> The things that occur with the balance are reduced (ἀνάγεται) to the circle, and those that occur with the lever to the balance, while practically everything else concerned with mechanical motions is reduced to the lever.[24]

In the remainder of the text the author closely follows the program suggested by this remark.

He first explains how the behaviour of the balance can be explained by reference to the circle (problems 1–2), then shows that the lever can be explained by reference to the balance (problem 3); the rest of the treatise discusses a wide range of mechanical phenomena and instruments, most of which are explained by reference to the lever. The key explanatory strategy is the identification of similarities between mechanical phenomena and the circle, whether directly or indirectly via analogies with the balance or lever. In this way the circle, balance, and lever function as simple models that enable an explanation to be given of all mechanical phenomena. The parallel with Heron extends also to the level of terminology: the term ἀνάγεται, used by the author of the *Mechanica* in the sense of "reduce" or "refer", probably corresponds to *mansūba* in the passage from *Mechanics* 2.1 quoted above.[25] Thus when Heron remarks in reference to the concentric circles that "the ancient authors, who came before us, have also started from this starting point" (2.8), he probably has the *Mechanica* itself in mind.[26]

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[24] Pseudo-Aristotle, *Mechanica*, 847b16, 848a11–14.

[25] This seems clear from a comparison with the parallel version in Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, p. 1116, which states that all five powers "are reduced to a single nature" (εἰς μίαν ἄγονται φύσιν); moreover the extant Arabic translation of Pappus renders ἄγονται as *mansūba* (for a preliminary version of the text see Jackson, *The Arabic Version*). The use of ἀνάγειν to mean "reduce", in the sense of bringing a diversity of cases under a small number of general principles, is Aristotelian; see *Generation and Corruption* 330a25, *Physics* 189b27, *Metaphysics* 983a28, *Prior Analytics* 29b1.

[26] *Opera*, vol. II, p. 114.20–22; Engl. trans. Drachmann, *The Mechanical Technology*, p. 63.

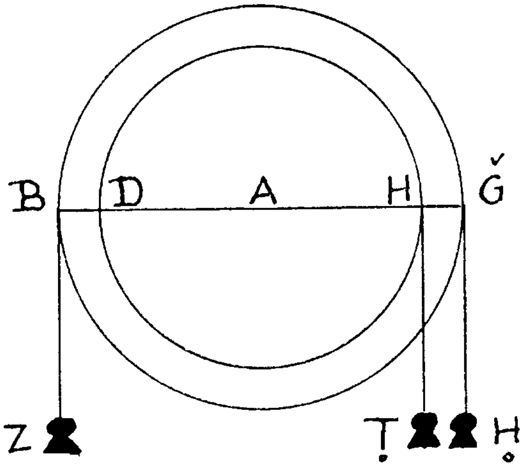

FIGURE 3. Equilibrium on concentric circles (*Mechanics* 2.7). Drachmann's drawing (*The Mechanical Technology*, p. 62) is from the Leiden Ms L.

Heron's account of the concentric circles is given in 2.7 (Fig. 3). We are to imagine two circles free to rotate about the same centre. If two equal weights Z and Ḥ are suspended at points B and Ǧ, it is clear that the line BǦ will remain parallel to the horizon, "... because the two weights Z, H are equal and the two distances BA, AǦ are equal, and BǦ is a balance beam turning on a point of suspension, which is the point A".[27] Now if weight Ḥ is shifted inwards and suspended from the diameter of the smaller circle at H so that it comes to be at Ṭ, the circles will turn in the direction of weight Z. But if the weight at Ṭ is then increased so that its ratio to the weight Z is as the ratio of BA to AH, the imaginary balance beam (now BH) will again be in equilibrium and take up a position parallel to the horizon. To support this claim Heron appeals to Archimedes' proof of the law of the lever in *On the Equilibrium of Planes*.[28] Finally he draws the following conclusion:

> From this it is evident that it is possible to move a great bulk by a small force (*quwwa*). For when the two circles are on the

[27] *Opera*, vol. II, p. 112.30–34; Engl. trans. Drachmann, *The Mechanical Technology*, pp. 61–62.

[28] At *Opera*, vol. II, p. 114.5–7, Heron states that "Archimedes has proven this in his book on the equalizing of inclination" (*wa-ḏālika qad bayyanahu Aršimīdis fī kitābihi fī musāwāt al-mayl*); Drachmann, *The Mechanical Technology*, p. 62, plausibly takes the reference to be to *On the Equilibrium of Planes*.

> same centre and the great weight (*ṯiql*) is on an arc from the small circle and the small force (*quwwa*) is on an arc from the great circle, and the ratio of the line from the centre of the big one to the line from the centre of the small one is greater than the ratio of the great weight (*ṯiql*) to the small force (*quwwa*) that moves it, then the small force (*quwwa*) will overpower (*qawiya*) the great weight (*ṯiql*). [29]

Several points about this procedure call for comment. First it is clear that Heron makes no attempt to *prove* the law of the lever; rather, he accepts it as a result already demonstrated by Archimedes. Second, the analogy with the balance plays a crucial role: both BǦ and BH are explicitly identified with the beam of a balance suspended from point A. In fact, what Heron describes is a kind of compensation procedure that would have been familiar to any practitioner who had worked with a balance with unequal arms. In such balances, the effect on equilibrium of moving a weight closer to the suspension point can be compensated by increasing the weight at that point. This suggests a general equivalence between the addition of a weight to the balance and the displacement of a weight along the beam.[30] Third, Heron describes equilibrium as a relationship between force and weight: a small force (Arabic *quwwa*) balances a large weight (Arabic *ṯiql*). This terminology recurs throughout Heron's account of the five powers, which are consistently described as able to move a large weight (*ṯiql*) by means of a small force (*quwwa*). Equilibrium is understood as an equivalence between a small force and a large weight, rather than between composite quantities (e.g., the product of weight and distance from the suspension point).[31] Thus Heron remarks that in the *baroulkos* or "weight-hauler" (see Fig. 10), a device that uses toothed wheels to lift a weight of 1000 talents with a force of 5 talents, "just as on a balance, the force (δύναμις, sc. 5

[29] *Opera*, vol. II, p. 114.7–16. Engl. trans. Drachmann, *The Mechanical Technology*, p. 62, slightly modified.

[30] See the treatise *On the Balance* attributed to Euclid, which is extant only in an Arabic version, published in Clagett, *The Science of Mechanics*, pp. 24–28. This text first demonstrates the equivalence of (a) adding a certain amount to a weight on a balance and (b) displacing that weight a certain distance along the beam; it then goes on to use this equivalence to prove the law of the lever.

[31] Cf. the Euclidean text mentioned in the previous note, which coins a new term, "force of weight" (*quwwat al-ṯiql*) to express the effect of (a) adding a weight to the balance at a certain distance from the suspension point, or (b) displacing a weight by a certain amount along the beam. Thus a weight suspended at a certain distance from the suspension point can be said to be equivalent to a certain segment of the beam in "force of weight" (Clagett, *The Science of Mechanics*, 27–29).

talents) will be in equilibrium with (ἰσορροπήσει) the weight (βάρος, sc. 1000 talents)".[32]

In the case of two of the five powers, the reduction to the circle is immediate. When the lever is used to lift a weight completely off the ground, as illustrated in Fig. 4 (2.8), the moving force (*al-quwwat al-muḥarrikat*) applied at its end (B) is identified with the force applied to the circumference of the larger circle, the weight (*ṯiql*) to be lifted (Ǧ) with the weight on the arm of the smaller circle, and the fulcrum (D) with the centre of the two circles. Since the ends of the lever trace out arcs on concentric circles as the weight is lifted, the analysis of 2.7 can be applied directly: if the ratio of the length of the longer arm BD to that of the shorter arm DA is equal to the ratio of the weight Ǧ to the moving force applied at B, the lever is in equilibrium; if it is greater than the ratio of Ǧ to B, the force will lift the weight.[33] The reduction of the wheel and axle to the concentric circles is just as direct (2.10; Fig. 5). The wheel corresponds to the larger circle and the axle to the smaller; the weight (*ṯiql*) is hung from the axle and the moving force (*al-quwwat al-muḥarrikat*) applied at the circumference of the wheel.[34]

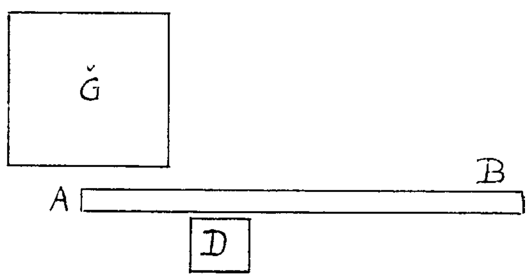

FIGURE 4. The lever, first use (*Mechanics* 2.8). Drachmann's drawing (*The Mechanical Technology*, p. 63) is from Ms L.

[32] *Dioptra*, ch. 37 (*Opera*, vol. III, p. 310.26–7); see below, pp. 34–35. Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, p. 1066, makes the same point in the same language.

[33] On the second use of the lever, discussed in *Mechanics* 2.9, see below, pp. 43–45.

[34] Heron himself remarks in 2.10 (*Opera*, vol. II, p. 120.21–23) that he has included the wheel and axle only for the sake of completeness: "And this is what those before us have already told; we have explained it here, however, just to make our book complete and to give it an orderly composition". Engl. trans. Drachmann, *The Mechanical Technology*, p. 67; see Pseudo-Aristotle, *Mechanica*, 852b11–21.

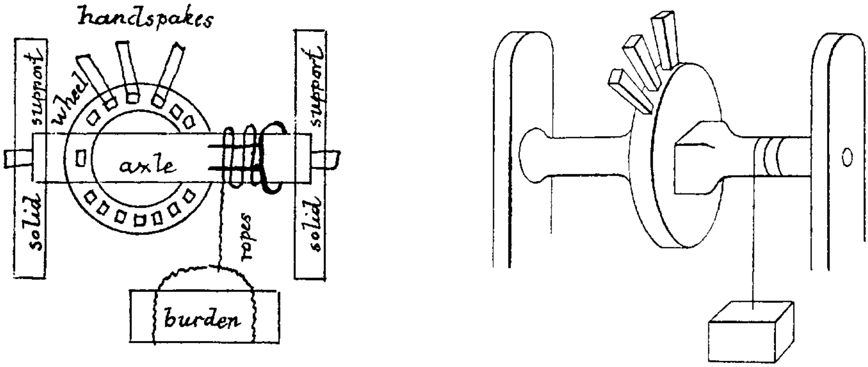

FIGURE 5. The wheel and axle (*Mechanics* 2.1, 2.10). On the left is Drachmann's drawing (*The Mechanical Technology*, p. 51) made from Ms L; on the right is the figure from *Heronis Alexandrini opera*, vol. II, p. 96.

The analysis of the compound pulley, though more complex, nonetheless begins from a direct application of the reasoning of 2.7. The first step is to imagine a weight suspended from two ends of a rope wound around a simple pulley (2.11; Fig. 6). Both segments stretching from the pulley to

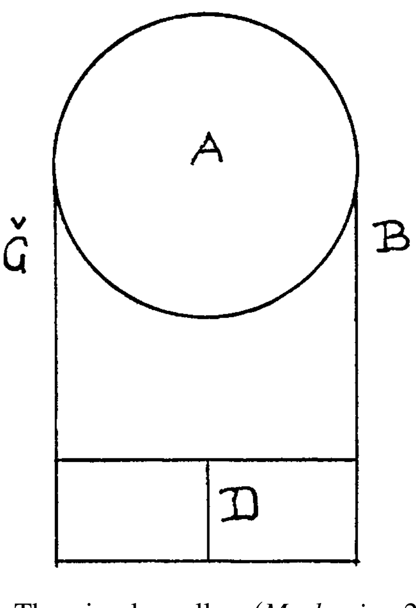

FIGURE 6. The simple pulley (*Mechanics* 2.11). Reconstruction by Drachmann (*The Mechanical Technology*, p. 69) on the basis of drawings in Ms L.

the weight will be equally taut, and each will carry half the weight. If the weight is divided into two equal parts, each one will balance the other, just like the two weights suspended on the circumference of the larger circles in 2.7. This result is then generalized to cover multiple pulleys and ropes, yielding a precise quantitative relationship:

> the ratio of the known weight (*ṯiql*) to the force (*quwwa*) that moves it is as the ratio of the taut ropes that carry the weight to the ropes that the moving force (*al-quwwat al-muḥarrikat*) moves.[35]

For example, in Fig. 7, each of the four lengths of rope stretched between the weight Z and the two pulleys on A carries ¼ the total weight. If we imagine detaching the rightmost section of the weight Z from the sections ǦBṬ, it will hold those sections in equilibrium. Thus a force equal to ¼ the total weight of Z, applied at K, balances ¾ the total weight (ǦBṬ), and a slightly larger force will move it.[36]

In the case of the wedge and screw the similarity to the concentric circles is much less clear. Since Heron claims that the screw is simply a twisted wedge (2.17), I shall concentrate here on the analysis of the wedge in 2.15 (see Fig. 8).[37] The argument is in two stages: (1) First Heron claims that any blow, even if it is slight, will move any wedge. The idea is to divide

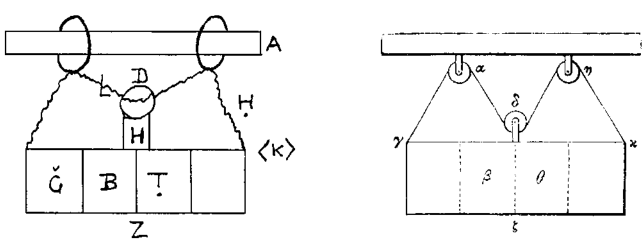

FIGURE 7. The compound pulley (*Mechanics* 2.12). Drachmann's drawing from Ms L is on the left (*The Mechanical Technology*, p. 70); the figure from *Heronis Alexandrini opera*, vol. II, p. 124, is on the right.

[35] *Mechanics*, 2.12 (*Opera*, vol. II, p. 126.1–5); Engl. trans. Drachmann, *The Mechanical Technology*, p. 70, modified.
[36] In general, letting F represent the moving force, W the weight, and n the total number of segments of rope that bear the weight, we have $F:W :: 1:n$.
[37] Note however that the analysis of the screw as a twisted wedge is supported by explicit reference to a practical procedure for making a screw, viz. by winding a right-angled triangle around a cylinder (2.16–17).

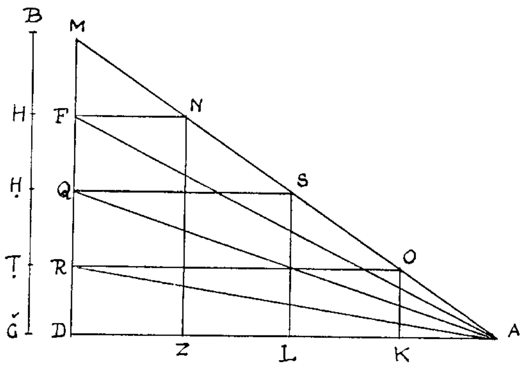

FIGURE 8. The wedge (*Mechanics* 2.15). Drachmann's drawing (*The Mechanical Technology*, p. 72) is from Ms L.

the contribution of a single blow (represented by BǦ) to the movement of the wedge into the contributions of a number of arbitrarily small sub-blows (BH, HḤ, ḤṬ, ṬǦ). The total movement produced by the entire blow is simply the sum of the movements produced by each of the sub-blows. Hence any blow, however small, will move the wedge a certain distance. (2) Second, Heron imagines dividing the wedge into as many sub-wedges (MF, FQ, QR, RD) as the sub-blows (BH, HḤ, ḤṬ, ṬǦ). He argues that each sub-wedge, struck by a sub-blow, covers the same lateral distance AD in the same time as the whole wedge, struck by the whole blow BǦ. The argument is as follows. If the whole wedge is struck by the whole blow BǦ, it will be driven into the load over the lateral distance AD in a given time. But if it is struck by a smaller force, the sub-blow BH, it will be driven into the load over the distance AK, where AK : AD :: BH : BǦ. Now if this same sub-blow is exerted on the sub-wedge RD, it will be driven in over the entire distance AD. The reason is that the displacement of the load (i.e., KO, the distance it is moved along the vertical axis) is the same whether the whole wedge is driven in over the distance AK or the sub-wedge over the distance AD: the same force (BH), acting for the same time, produces the same displacement of the load (KO) in both cases.[38] The upshot is that

[38] Another way of putting the point is as follows. Since the displacement of the load caused by the whole wedge MD when struck by the whole blow BǦ is the sum of the displacements caused by the sub-wedges when struck by the sub-blows, each sub-wedge, driven in by a sub-blow, must displace the load (along the vertical axis) by an amount that stands in the same relation to the total displacement as the sub-blow stands to the whole blow. In order for this to happen, each sub-blow must drive in the sub-wedge over the same *lateral* distance (AD) as is covered by the whole wedge when struck by the whole blow.

the sub-wedge RD will be able to split the load just as much as the whole wedge MD by means of a smaller force; this force, however, will have to act for a proportionally longer time:

> And if that which is driven in is one of the small wedges, if it is hit by many blows and is driven in, then it is driven in as far as the whole wedge is driven in by one whole blow, and this is by a movement corresponding to the blows, I mean by the measure of the blows BH, HḤ, ḤṬ, ṬǦ, and in the same way the ratio of time to time is like the ratio of blow to blow and that of the whole wedge head to the head of one of the small wedges. The smaller now the angle of the wedge becomes, the further will the wedge penetrate by a smaller force than the force that drives in the whole wedge.[39]

Although Heron states that "the ratio of time to time is like the ratio of blow to blow", the meaning must be that the ratio of the times taken by two wedges to displace a load by a certain distance is the *inverse* of the ratio of the forces: a smaller force must act over a longer time to produce the same effect. If we halve the force of the blow and use a wedge which is twice as acute, we will need two blows instead of one, and these blows will take twice as long to split the load by the same amount. Moreover, this more acute wedge will have to penetrate twice as far into the load in order to split it by the same amount; thus the smaller force acts over a longer distance as well as a longer time.[40]

Although Heron does not set out equilibrium conditions for the wedge, this inverse proportionality between forces and times does supply an analogy to the equilibrium between force and weight in the concentric circles. In the wedge, a small force acting for a longer time (i.e., over a longer lateral distance) produces the same effect as a large force acting for a shorter time (distance). Thus just as in the concentric circles, the effect of a small force can equal that of a large one; the difference is that in the circles the effectiveness of a force depends on its distance from the centre, while in the wedge it depends on the distance over which the force acts. Similarly, in the case of the screw Heron notes that a screw with tighter threads will be able to move a larger load by means of the same force, but it will

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[39] *Mechanics*, 2.15 (*Opera*, vol. II, p. 134.21–31); Engl. trans. Drachmann, *The Mechanical Technology*, p. 73, modified.

[40] Alternatively, in Fig. 8, if the force BǦ is applied to the whole wedge MD, the load will be displaced by MD in a given time t. But if the force BH, equal to ¼ BǦ, is applied to the sub-wedge RD, then the load will be moved by ¼ MD in time t and by MD in 4t.

require a greater time to do so.[41] The key idea in the analyses of both the wedge and the screw is thus that of compensation between forces and the times (and distances) over which they act: if we reduce the force, we must increase the time (see further below, pp. 35–41, on this phenomenon of "slowing up").

After completing the reduction of all the powers to the circle, Heron makes what might seem a surprising remark:

> That the five powers that move the weight are like the circles around a single centre, this is clear from the figures that we have drawn in the preceding chapters. But I think that their shape is nearer to that of the balance than to the shape of the circle, because in the beginning the first explanation of the circles came from the balance. For here it was shown that the ratio of the weight hung from the smaller arm to that hung from the greater arm is like the ratio of the larger part of the balance to the smaller.[42]

As we have seen, the analogy with the circle is clear in the cases of the wheel and axle and the lever; the discussion of the compound pulley, too, begins with an explicit reference to the concentric circles (2.11). It is hardly the case that Heron adopts the concentric circles as a starting point only as a nod to tradition.[43] But in fact it is the balance that in one way or another underlies the entire analysis. The balance model is implicit in Heron's attempt to set out equilibrium conditions for the wheel and axle, the lever, and the pulley, and it plays a crucial role in the analysis of the lever in its second use (2.9; see pp. 43–45). In generalizing the analysis of the simple pulley to the compound case (2.12) Heron leaves behind any direct resemblance to the concentric circles. In the case of the wedge and the screw there is no obvious resemblance to the concentric circles at all; the analogy is limited to the point that a small force can overcome a large weight, by making the angle of the wedge smaller or the screw threads tighter. But of course

[41] "And just as it has been explained about the wedge that the one that has a smaller angle moves the weight by less force than the force that moves the weight by a wedge with a greater angle, so we have to say about this that the screw in which the distances between the screw lines are less will move the weight with greater ease than the screw whose distances between the screw lines are greater will move it, because the lesser distance gives a smaller angle" (*Mechanics*, 2.17, *Opera*, vol. II, p. 138.21–29; Engl. trans. Drachmann, *The Mechanical Technology*, p. 76, slightly modified).

[42] *Mechanics*, 2.20 (*Opera*, vol. II, p. 144.24–33). Engl. trans. Drachmann, *The Mechanical Technology*, p. 81, slightly modified.

[43] *Pace* De Gandt, "Force et science des machines", p. 114.

the ability of a small force to overcome a large weight is precisely the feature of the concentric circles that Heron himself explains by reference to the balance.

In making the balance rather than the circle fundamental to his explanation of machines, Heron reverses the explanatory relationship between the circle and the balance from the *Mechanica*; there, as noted above (p. 23), it is the circle that explains the balance. Yet from another point of view the similarities between the two texts are larger than their differences. In the *Mechanica*, as in Heron, the balance is fundamental to the explanation of all other machines. The balance explains the lever, and the lever in turn explains the working of most of the other machines discussed in the 35 problems. For both Heron and the author of the *Mechanica*, the balance is a paradigmatic technological instrument that provides a concrete model for analyzing the relationship between force and weight, and in particular for understanding how a small force can balance or even overpower a large weight. The differences between Heron and the Aristotelian text are due, first, to Heron's recognition of the five powers as a distinct group of simple machines, and secondly to the impact on theoretical mechanics of Archimedes' proof of the law of the lever in *On the Equilibrium of Planes*. Neither of these points concerns the central importance of the balance as a starting point for theoretical reflection.[44]

THE POWERS IN COMBINATION AND THE PHENOMENON OF SLOWING UP

After completing the reduction of the five powers to the balance, Heron turns in 2.20 to the problem of how they can be combined to move a given (large) weight with a given (small) force. The problems involved are different for different powers. The lever and the wheel and axle have to be increased in size in order to achieve greater mechanical advantage; in the case of the

[44] It must be kept in mind that Archimedes' own proof of the law of the lever in *On the Equilibrium of Planes* begins from a set of preliminary assumptions that in part describe the behaviour of weights on the balance; see Renn et al., "Aristotle, Archimedes, Euclid". In any case the differences between the Aristotelian text and Heron are not helpfully characterized in terms of a distinction between a "dynamical" and a "statical" approach (Krafft, *Dynamische und statische Betrachtungsweise*). True, Heron does not appeal to considerations of force and movement in discussing the concentric circles, as does the *Mechanical Problems*. But then Heron makes no attempt to prove the law of the lever at all; he simply accepts it as a given from Archimedes. As we have seen, Heron's analyses of the wedge and screw are based on "dynamic" considerations of the relationship between forces, times, and distances, and he seems to hold that such relationships reflect a basic similarity to the balance.

compound pulley it is the number of pulleys that must be increased. The wedge and the screw, on the other hand, become more powerful with a decrease in size: the angle of the wedge must be made more acute, and the screw threads more tightly wound. Chapters 21–26 of Book 2 discuss the combination of individual wheel and axles, levers, and compound pulleys, each of which is of manageable dimensions, to achieve a large mechanical advantage.

As an example we may consider the use of a combination of wheel and axles to move a load of 1000 talents with a force of 5 talents (2.21; Fig. 9). Given the results established in the reduction, this would require a wheel with a radius 200 times that of its axle. But Heron shows that the same mechanical advantage can be achieved by a combination of three wheel and axles with ratios of 5:1, 5:1, and 8:1, respectively. He first describes the construction of a device in which the force exactly balances the weight to be moved, then states that the same force can be made to set the weight in motion by increasing one of the wheel-axle ratios slightly. The same pattern is followed for the compound pulley and the lever: first a description of what is required to keep the given weight in equilibrium, then an indication of how to make the given force "overpower" (Arabic *qawiya*) the weight. The procedure and the language reflect the continuing importance of the

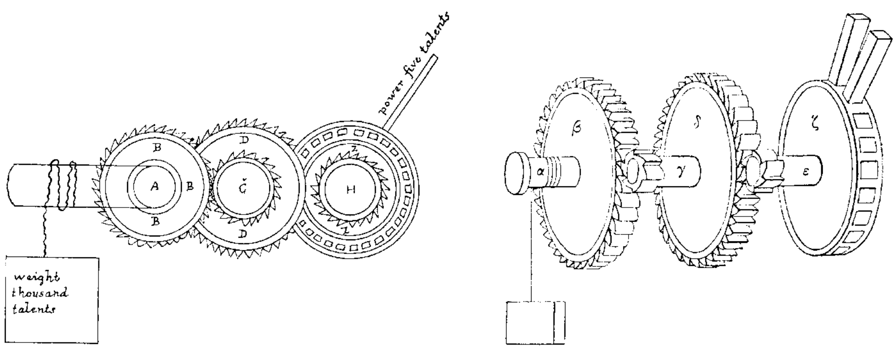

FIGURE 9. Combination of toothed wheels to move a force of 1000 talents with a force of 5 talents (*Mechanics* 2.21). Drachmann's rendering of the figure (*The Mechanical Technology*, p. 82) in Ms L is on the left; the figure from *Heronis Alexandrini opera*, vol. II, p. 148, is on the right. Neither is exactly faithful to the text, which states that the wheel–axle ratios B:A, D:Ğ, and Z:H are 5:1, 5:1, and 8:1, respectively.

balance model in Heron's account. This becomes quite explicit in the Greek version of the description of the *baroulkos* or "weight-hauler" (Fig. 10), a device very similar to the one described in 2.21:

> These things having been done, if we imagine the chest ΑΒΓΔ placed on high, and we tie the weight to the axle EZ, and the pulling force to the wheel ΧΨ, neither side will go downwards, even if the axles are turning easily and

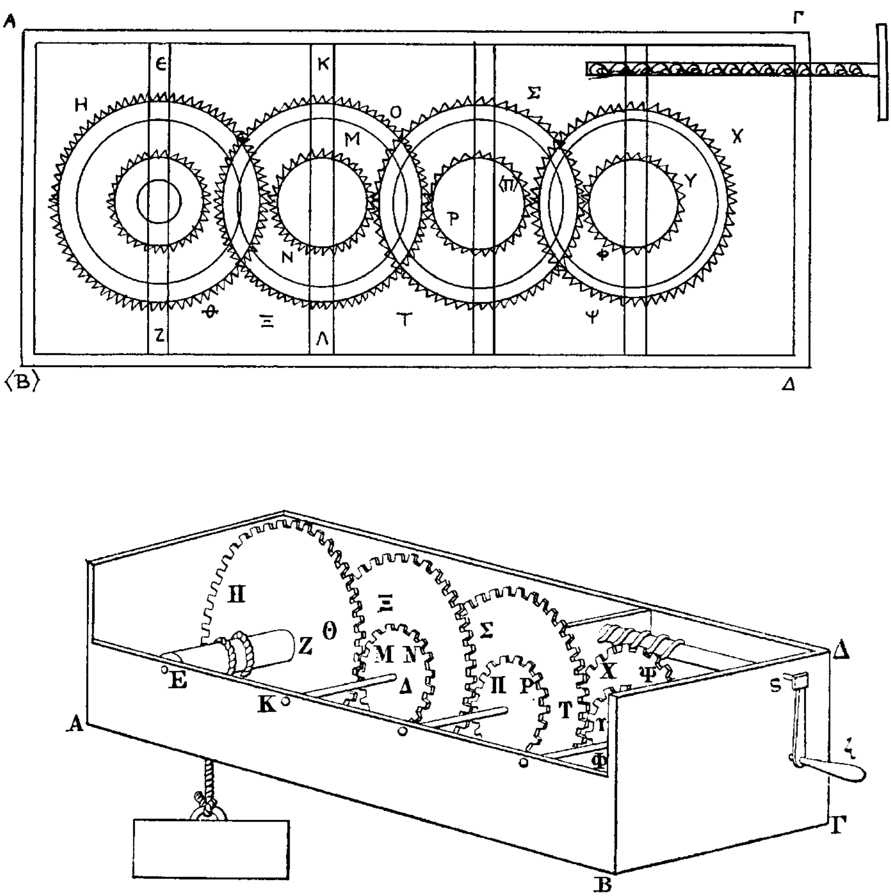

FIGURE 10. The *baroulkos* (Heron *Dioptra* 37; *Mechanics* 1.1; Pappus, pp. 1060–1068). Above, Drachmann's drawing made from the Mynas Codex, a Greek manuscript of the *Dioptra* (*The Mechanical Technology*, p. 25); below, the figure from *Heronis Alexandrini opera*, vol. III, p. 309.

> the engagement of the wheels is fitted nicely, but the force will balance (ἰσορροπήσει) the weight as in a balance (ὥσπερ ζυγοῦ τινός). But if we add a little more weight to one of them, the side where the weight is added will sink (καταρρέψει) and go downwards, so that, if just the weight of one mina is added to the force of five talents, it will overpower (κατακρατήσει) the weight and pull it.[45]

As a final *tour de force* Heron describes how all the powers except for the wedge can be combined to achieve the same mechanical advantage of 200:1 (2.29; Fig. 11).

The discussion of the powers in combination prompts Heron to remark on a further important aspect of their operation, the phenomenon of delay

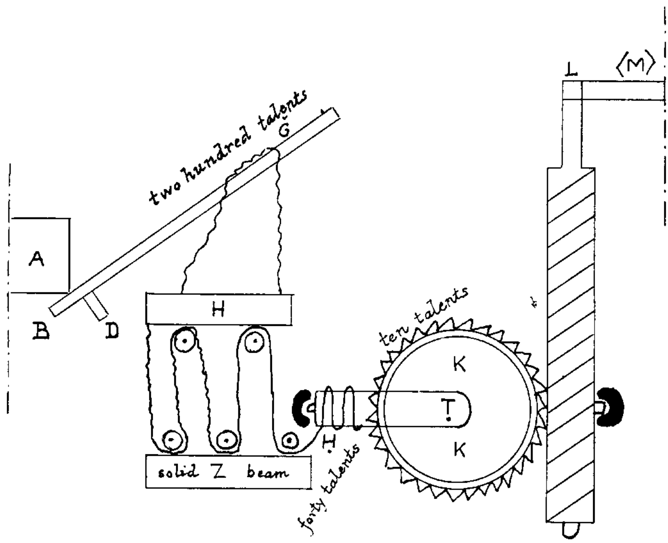

FIGURE 11. Combination of four powers to move a weight of 1000 talents with a force of 5 talents (*Mechanics* 2.29). Drawing of a figure in Ms L (Drachmann, *The Mechanical Technology*, p. 90).

[45] Heron, *Dioptra*, 37 (*Opera*, vol. III, pp. 310.20–312.2); Engl. trans. Drachmann, *The Mechanical Technology*, p. 26, slightly modified. Throughout the passage "force" translates δύναμις and "weight" βάρος. Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, p. 1066.19–31, expresses the same idea in the same language. See also Heron, *Mechanics*, 1.1. On the relationship between these descriptions see Drachmann, *The Mechanical Technology*, pp. 22–32.

or "slowing up". The fullest description of this phenomenon comes in 2.22, in reference to the combination of wheel and axles described in 2.21:

> In these tools and those like them with great force (*quwwa*) there will come a delay, because in proportion of the weakness of the moving force (*al-quwwat al-muḥarrikat*) to the great size of the weight moved (*al-ṯiql al-mutaḥarrik*), in this proportion we need time, and the ratio of force (*quwwa*) to force (*quwwa*) and time to time is the same.[46]

The same relationship between forces and times is asserted in similar language for the compound pulley (ch. 24), the lever (ch. 26), the wedge, and the screw (both in ch. 28). Although Heron's formulation is not as clear as it might be, it is evident from the passage quoted that he is asserting an *inverse* proportionality between forces and times: the smaller the force, the more time is needed ("in proportion of the weakness of the moving force … in this proportion we need time"). The underlying idea is a comparison between the amount of time it takes for two machines of different mechanical advantage to perform a given task. Thus, letting F_1 and F_2 be the forces applied to machines 1 and 2, and T_1 and T_2 the times they require for performing a given task (e.g., lifting a weight a given distance), the following relation holds:

$$\text{(R)}\quad \mathrm{F}_1 : \mathrm{F}_2 :: \mathrm{T}_2 : \mathrm{T}_1$$

This interpretation is clearly supported by Heron's remarks about the wedge and screw in ch. 28:

> But that the delay is also found to take place in those two, that is evident, since many blows take more time than a single blow, and the turning of the screw many times takes more time than a single turn. And we have proven that the ratio of the angle to the angle of the wedge is like the ratio of the moving blow to the moving blow; and so the ratio of the time to the time will be like the ratio of the force (*quwwa*) to the force (*quwwa*).[47]

Heron refers back to his proof in 2.15 that a more acute wedge will produce the same effect as one that is less acute by means of less powerful blows.

[46] *Opera*, vol. II, p. 152.25–30; Engl. trans. Drachmann, *The Mechanical Technology*, p. 85, modified.
[47] Heron, *Mechanics*, 2.28 (*Opera*, vol. II, p. 162.4–11); Engl. trans. Drachmann, *The Mechanical Technology*, p. 89, modified.

But it will take more time to do so, since the many blows take more time than a single strong blow; as the force decreases the time increases, in exact (but inverse) ratio. In order to split a load by the same amount as an obtuse wedge, the tip of an acute wedge must travel over proportionally more distance, which takes a correspondingly longer time (see above, pp. 29–30). It is important to note that this analysis presupposes that each blow takes the same amount of time to move the wedge a certain distance (i.e., the tip of the wedge moves at the same speed whether the blows are weak or strong: "many blows take more time than a single blow"). A similar point follows for the screw, since it is just a twisted wedge; again, it is a crucial assumption that the number of turns of the screw in a given time remains the same (in the text above: "the turning of the screw many times takes more time than a single turn"). In the case of both wedge and screw, then, it is clear that Heron's understanding of the phenomenon of slowing up involves a comparison between machines of different mechanical advantage; moreover, this comparison assumes that the moving forces in the two machines travel at the same speed.

Such a comparison between moving forces also underlies Heron's account of slowing up in the case of the compound pulley (*Mechanics* 2.24, Fig. 12; since the Ms figure is not very clear, the following refers to the figure in *Heronis Alexandrini opera*). Specifically, Heron notes that in order to lift a weight of 1000 talents at β over the distance $\beta\gamma$, a force of 200 talents exerted at δ must be pulled through 5 times the distance $\beta\gamma$ (since each rope in the 5-pulley system $\beta\gamma$ must be pulled over a distance $\beta\gamma$). Similarly, a force of 40 talents exerted at η must be pulled through 5 times the distance of the force at δ (since in order to move the rope at δ by a certain distance, the rope at η must be pulled through 5 times that distance). Having noted this, Heron again remarks that "the ratio of time to time is like the ratio of the moving force (*al-quwwat al-muḥarrikat*) to the moving force (*al-quwwat al-muḥarrikat*)".[48] Thus, the analysis again presupposes a comparison between two moving forces travelling at the same speed. In order to lift the weight a given distance, the moving force at η will have to travel farther than the moving force at δ, and so will take a longer time. This indeed would immediately be evident in practice, since the length of rope that would have to be pulled through the machine to raise a weight a given distance would be much greater in a machine with more pulleys (as would the amount of time needed for the operation). Viewed in this way, Heron's explanation of the phenomenon of slowing up may be expressed as an inverse proportionality

[48] Heron, *Mechanics*, 2.24 (*Opera*, vol. II, p. 158.11–12); Engl. trans. Drachmann, *The Mechanical Technology*, p. 88, modified.

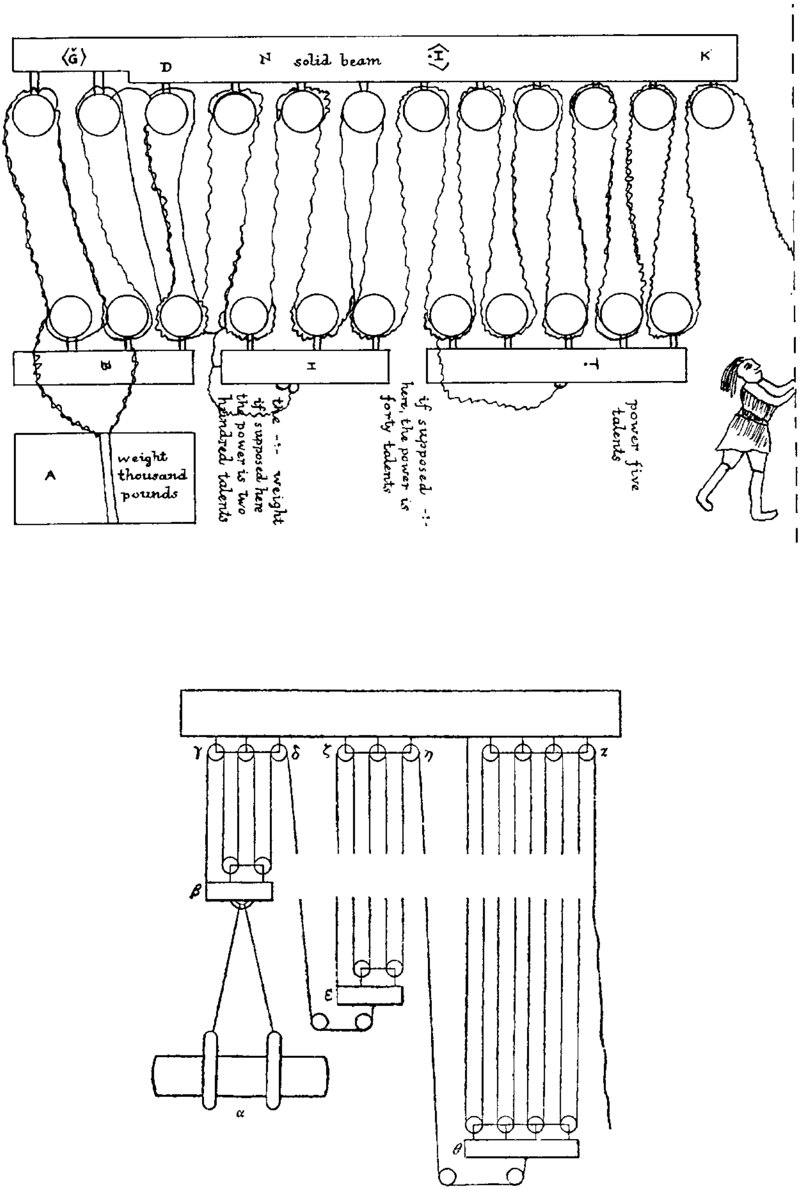

FIGURE 12. Combination of compound pulleys to move a weight of 1000 talents with a force of 5 talents. Drachmann's drawing above (*The Mechanical Technology*, p. 87) is from Ms B; the figure below is from *Heronis Alexandrini opera*, vol. II, p. 156.

between the moving forces and the distances they must traverse in order to move the weight a given distance, i.e.,

$$(\mathrm{R}') \quad \mathrm{F}_1 : \mathrm{F}_2 :: \mathrm{D}_2 : \mathrm{D}_1$$

where D_1 and D_2 are the distances covered by the moving forces, travelling at a constant speed, in *different* times.[49]

Now the operation of the compound pulley can also be analyzed in a way that leads to a different understanding of the phenomenon of slowing up. On this view, the comparison is between the moving force and the weight moved, rather than between two different moving forces. If the moving force is smaller than the weight, it will cover more distance than the weight in any given time (e.g., in Fig. 12 during the time in which the force at δ moves a certain distance, the weight at β will move 1/5 of that distance). Thus the moving force travels more quickly than the weight; in this sense, the "slowing up" that occurs in the machine is the result of the weight moving more slowly than the force that moves it. At any given time the ratio of the distance covered by the moving force to that covered by the weight will be the inverse of the ratio of the force to the weight, i.e.,

$$(\mathrm{R}'') \quad \mathrm{F}_1 : \mathrm{F}_2 :: \mathrm{D}_2 : \mathrm{D}_1$$

where F_2 equals the weight. Put another way, R″ asserts that the product of force and distance traversed is the same for the weight and the moving force; it thus expresses the general principle that the work (understood as the product of force times distance) is the same on both the "input" and "output" sides of the machine: the work in equals the work out.

Some commentators have taken R″ to be the basic principle underlying Heron's analysis of the phenomenon of slowing up. Thus Clagett writes: "A careful study of the examples shows that what is really involved is distance, i.e., that the ratio of force to force is inversely as the distances through which the forces act". He goes on to argue that R″ amounts to a recognition of the concept of "virtual work", according to which the equilibrium between the moving force and the weight is explained as due to the equivalence of the work that would be done on both sides of the machine if the force were to lift the weight by a certain amount

[49] We may also compare the distances over which the *weight* is moved by the two moving forces in the *same* time. In this case, however, we have a *direct* proportionality between the forces and the distances over which the weight is moved, i.e., a smaller force moves the weight over a smaller distance in the same time as a larger force moves it over a greater distance, and the ratio of the forces is equal to the ratio of the distances.

(i.e., the product of force times distance traversed is the same for the weight and the moving force).[50] But care is needed here. Although Heron was presumably well aware from practical experience that the moving force travels faster than the weight moved in a machine of high mechanical advantage, he nowhere states R″ explicitly; instead, his explanations of the phenomenon of slowing up consistently refer to a comparison between different moving forces.[51] In this respect, Heron's account is closely tied to the use of machines in practical situations, where a key issue is the amount of time taken by different machines to complete a given task.[52] Any practitioner with experience using compound pulleys or other such machines

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[50] Clagett, *The Science of Mechanics*, pp. 17–18; Vailati, "Il principio delle velocità virtuali".

[51] The only *prima facie* exception is the passage from 2.22 quoted above (p. 36), which seems to assert that the ratio of the times is the inverse of the ratio between the force and the weight moved. Yet a reference to time would be puzzling if Heron had meant to state or imply R″, since that relationship presupposes that the times of motion are equal for the moving force and the weight (see the next note). And in fact the discussion of the combination of wheel and axles that follows in 2.22 is precisely similar to the discussion of the compound pulley in ch. 24: the comparison is between the times taken by different moving forces applied at the circumference of different wheels to move the weight a given distance (alternatively, it is between the distances they travel in the different times they take to move the weight, assuming they move at the same speed). For example, in Fig. 9 a force of 40 talents applied at the circumference of wheel D must cover 5 times the distance as a force of 200 talents applied at the circumference of wheel B in order to raise the weight by the same distance (the wheel D must turn five times for the wheel B to turn once, and the two wheels have the same circumference). Heron concludes by stating the usual relationship between moving forces: "the ratio of the moving force (*al-quwwat al-muḥarrikat*) to the moving force (*al-quwwat al-muḥarrikat*) is inverse <of the ratio of time to time>" (*Opera*, vol. II, p. 154.9–10). Drachmann, *The Mechanical Technology*, p. 86, translates "the proportion between the moving power and the *power moved* is inverse", but there is no basis in Nix's Arabic text for the variation and Drachmann does not indicate any alternative manuscript readings. Nevertheless, a supplement such as the one he proposes ("of the ratio of time to time") seems necessary. As for the lever (ch. 26), Heron simply appeals to the analogy between the lever and the wheel and axle: "and just as we have proved for these axles that the ratio of the force to the force is like the ratio of the time to the time, so we prove it also here" (*Opera*, vol. II, p. 160.25–28; Engl. trans. Drachmann, *The Mechanical Technology*, p. 89, modified).

[52] To be sure, one can compare the amount of time taken by a given moving power to move a given weight over a given distance to the amount of time it would take for a force equal to the weight itself to do so. That is, we can set F_2 equal to the weight in R′ above and thereby obtain an inverse proportionality between forces and distances ($F_1 : F_2 :: D_2 : D_1$). But here the distances D_1 and D_2 are those covered by the forces in *different* times: given two moving forces travelling at the same speed, one of which is equal to the weight, the force equal to the weight will lift it much more quickly than a force much smaller than the weight. Though this is superficially similar to R″ it is in fact quite different, and does not imply a recognition of a general principle (work in equals work out) such as is implicit in R″.

for lifting weights must have known that it would save time to increase the moving force (i.e., add another slave or an ox) rather than to employ a machine with greater mechanical advantage. The practical context also explains the assumption that the moving forces in the different machines travel at the same speed. The picture to keep in mind is of a workman pulling on the end of a rope that runs through a compound pulley system: even though he always walks at the same speed, it will take him less time to raise a given weight over a given distance if he uses a machine with fewer pulleys.[53] It is therefore wrong to suppose that R″ is the fundamental principle underlying Heron's explanation of the phenomenon of slowing up, or that he views it as the explanation of the equilibrium between a small force and a large weight. Rather, R″ is a kind of spin-off result, one which is implicit in Heron's analysis but plays no important role in his reasoning.[54]

As we have seen, Heron's analysis of the five powers both takes its start from practitioners' knowledge and attempts to explain that knowledge. Nevertheless, Heron concludes his account with an acknowledgment that the relationships between force and weight that he has set out may not hold in practice:

> Since we have now explained for each of these powers that it is possible by a known force to move a known weight, it is necessary also to explain that if it were possible that all the parts made were turned accurately, of equal weight, with parts of the same smoothness, then it would be possible with each of these engines to perform the work we have described in the given proportion. But since it is not possible for human beings to make them perfect in smoothness and uniformity, it is necessary to increase the force on account of what may occur of roughness in the engines, and we must make them greater, and so we increase their size above the proportion we have first given, so that no hindrance occurs herein, and what we find by the use of the engines shall not make out wrong what was correct in our theoretical proof.[55]

Heron acknowledges that he has been discussing machines in their ideal form; to this extent his account is a theoretical one. Yet it should be noted,

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53 Strictly speaking, it is not necessary that the worker walks at a constant speed, only that the speeds vary in the same proportions in the two machines. (I owe this point to Sophie Roux.)

54 See De Gandt, "Force et science des machines", pp. 114–115; p. 127.

55 *Mechanics*, 2.32 (*Opera*, vol. II, p. 170.6–21); Engl. trans. Drachmann, *The Mechanical Technology*, p. 93, slightly modified.

first, that the contrast expressed here is a contrast between ideal and actual physical machines, rather than between a mathematical idealization and its physical realization. And however wide the discrepancy between theoretical analysis and actual practice, Heron's purpose is to *explain* that discrepancy so that a practitioner will not be led to doubt the correctness of the theoretical analysis.

MECHANICS, PHYSICS, AND THE SEARCH FOR PRINCIPLES

The behaviour of the five mechanical powers, in particular their ability to move a large weight with a small force, challenged the basic physical assumption that a weight can be lifted only by a force equal to it. The five powers brought about effects that at first seemed wondrous or paradoxical, and which therefore might be thought to lie outside the scope of physics understood as the knowledge of natural regularities. But Heron's account shows how the balance could be used as a model to provide a theoretical explanation of the behaviour of the five powers. Once the causes of mechanical phenomena were understood, they became a part of physics rather than a challenge to it. This at any rate is suggested by the opening of *Mechanics* 2.33:

> It is now absolutely necessary for those who occupy themselves with the science of mechanics to know the causes that are in effect in the use of each motion, as we have explained for the lifting of heavy objects with natural [i.e., physical] proofs (*al-barāhīn al-ṭabīʿiyyat*), and set out everything that occurs with each one of the powers mentioned. … Now we want to talk of things that the ancients already stated, because of the usefulness they have in this chapter, and we are going to wonder at the things that, when we have proven them, will be contrary to what we had knowledge of before. The beginning (*ibtidā'*, presumably ἀρχή) for the things that we are going to research, we derive from what is clear to us. The things of whose causes we can only talk after the most clear things will, however, even increase our amazement when we see that the things that we apply are contrary to what we have gotten used to and what was certain for us. It is now clear that anyone who wants to find the causes thoroughly, necessarily has to apply natural [i.e., physical] principles (*ibtidā'āt ṭabīʿiyyat*), either one or more, and has to link everything that he researches with them, and that the solution of every single

question is given fundamentally if its cause has been found and this is something that we have already understood.[56]

The mention of "wonder" and "amazement" recalls the opening of the *Mechanica*, a text that emphasizes the wondrous (θαυμαστός) character of mechanical phenomena as taking place in a way that is "beyond nature" (παρὰ φύσιν). But the references to "natural proofs", "natural principles", and the idea that the search for such principles begins from something that is "clear to us" are unmistakable allusions to the opening of Aristotle's *Physics* (184a1–b14). Heron is in fact arguing that mechanical phenomena, despite the fact that they initially seem to violate natural regularities, can be integrated into the study of nature (i.e., physics). Since this passage comes immediately after Heron's account of the five powers and makes direct reference to it, it seems that Heron views his explanations of the five powers as based on such "natural principles".[57]

Yet the application of the balance model also led Heron to conclusions that were in tension with both practitioners' knowledge and deeply held assumptions of much ancient physical thought. One of these assumptions is the idea that a body in motion requires a force to keep it moving, a notion that may be dubbed "motion implies force". That some effort must be exerted to keep a body in motion is manifestly the case in many practical situations because of friction. In Aristotle's natural philosophy, all forced motion requires a mover to sustain it; though this view corresponds to many every-day experiences, it leads to notorious difficulties in the case of phenomena such as projectile motion (e.g., Aristotle, *Physics* 8.10, 266b25ff.). The enduring plausibility of the idea that motion implies force is reflected in the fact that it corresponds to the intuitive understanding of many physics students even today.[58] Its rejection marked an important step towards the development of the concept of inertia in classical mechanics.

As a first example of the way in which the application of the balance model challenged the idea that motion implies force we may consider Heron's discussion of the lever in its second use, viz. where the load rests partly on the ground and turns around a fixed point as it is lifted (2.9, Fig. 13). As in

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[56] *Opera*, vol. II, pp. 170.22–172.18; translation based on the German of Nix.

[57] See *Mechanics*, 2.1 *(Opera*, vol. II, p. 94.8–9), where Heron claims that all the powers can be reduced to a single nature (φύσις, *ṭabīʿa*), and *Mechanics*, 1.34 (*Opera*, vol. II, p. 92.12–15): "In the following we are going to deal with the five powers by means of which weights are moved, explain what they are based on and how the natural effect (*al-fiʿl al-ṭabīʿī*) in them occurs".

[58] See Clement, "A Conceptual Model".

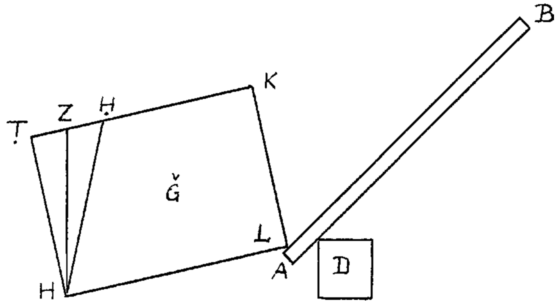

FIGURE 13. The lever, second use (*Mechanics* 2.9). Drachmann's reconstruction (*The Mechanical Technology*, p. 65), made from the figure in Ms L.

the simple case analyzed in 2.8, the moving force applied at the end of the lever corresponds to the force on the larger circle and the load to the weight on the smaller circle. But because the load rests partly on the ground, it requires less force to move it than in the other case. We imagine a vertical line HZ drawn through the corner of the load that remains on the ground. The portion of the load to the left of this line (HṬZ) is in equilibrium with the portion HZḤ to the right of it; then if one imagines the entire portion HṬḤ separated from the load, it will not incline to either side, and it will not take any force (*quwwa*) at all to move it. Thus the lever needs to balance only portion ḤHKL of the load, which diminishes as the load is turned around the point H. Eventually the load will reach a position where the line HZ divides it exactly in two; at that point it will take no force (*quwwa*) at all to move it: "and it is placed in a position that does not need any force, if the imagined surface going through the point H at right angles to the horizon divides the load into two halves".[59] The key step in the argument is a mental operation of dividing the load into different parts, which hold one another in equilibrium like two weights on a balance.[60] Thus, the application of the

[59] *Opera*, vol. II, p. 120.6–9; Engl. trans. Drachmann, *The Mechanical Technology*, p. 65, modified. There are some textual problems in the chapter, but the basic character of Heron's analysis is not in doubt; see Drachmann, *The Mechanical Technology*, pp. 65–67.

[60] It is perhaps because this analysis so clearly appeals to the balance that Heron ends the chapter with a remark that might otherwise seem out of place: "And this effect of the lever can be referred to (*mansūba*) the circle, but it is not the same as the first effect. And that the balance also can be referred to (*mansūba*) the circle is evident, because the circle is a sort of balance" (*Opera*, vol. II, p. 120.10–13; Engl. trans. Drachmann, *The Mechanical Technology*, p. 65). In the final analysis, of course, Heron holds that it is the balance that explains the circle rather than vice versa (2.20; above, pp. 31–32).

balance model leads Heron to the conclusion that no force at all is required to move a body that is perfectly balanced on a point.

To a certain extent, of course, this analysis corresponds to a basic fact of practical experience, viz. that the amount of force required to move a body with a lever used in the way shown in Fig. 13 decreases as the body is lifted. In chapters 20–24 of Book 1, however, Heron goes substantially further than this and directly attacks the notion that a weight (*ṯiql*) can only be moved by a force (*quwwa*) equal to it:

> There are those who think that weights lying on the ground can be moved only by a force equal to them, wherein they hold wrong opinions. So let us prove that weights lying in the way described are moved by a force smaller than any known force, and we shall explain the reason why this is not evident in practice.[61]

As often in Heron, the argument is put in the form of a thought experiment. We are to imagine a smooth, solid body lying on a plane surface. If this surface is tilted to the left, however slightly, the body will slide downwards because of its natural inclination; similarly, it will slide downwards to the right if the surface is tilted to the right. The situation in which the surface is horizontal thus represents a state of equilibrium, in which the body is equally disposed to move to the left and to the right. The conclusion is that the body can be set in motion by a force that is as small as one likes:

> And the weight that is ready to go to every side, how can it fail to require a very small force to move it, of the size of the force that will incline it? And so the weight is moved by any small force.[62]

The idea seems to be that the effect of a force on such a weight is simply to cause it to incline in one direction; it will then continue to move on its own as if it were on an inclined surface. It takes a force to set a body in motion, but not to keep it moving. Heron goes on to suggest that the behaviour of water is analogous because of its fluidity and mobility (1.21). Conversely, the coherence and resistance of solid bodies explains why they do not behave in the same way, and why rollers and other means were invented for transporting loads over the ground.

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[61] *Mechanics*, 1.20 (*Opera*, vol. II, p. 54.10–16); Engl. trans. Drachmann, *The Mechanical Technology*, p. 46, modified.

[62] *Mechanics*, 1.20 (*Opera*, vol. II, p. 56.9–13); Engl. trans. Drachmann, *The Mechanical Technology*, p. 46, modified.

In 1.22 Heron turns to the topic of lifting weights, and makes the familiar claim that the force needed to lift a weight without the use of a machine is equal to that weight.[63] If we imagine two equal weights suspended from the ends of a rope wound around a pulley, they will balance one another; if some weight is added to one side, it will overpower the weight on the other side and draw it upwards, "unless there arises friction in the turning of the block or stiffness in the ropes".[64] This leads immediately to a discussion of the force needed to draw an object up an inclined plane (1.23; Fig. 14). Heron approaches the problem by asking how much force is needed to balance a weight lying on an inclined plane. He considers the special case of a cylinder and imagines a vertical line (AB) cutting the cylinder at the point at which it touches the plane. The weight of the portion of the cylinder to the right of this line (ABC) balances the weight of an equal portion to its left (ABD). Heron concludes that no force at all is needed to move these two portions of the cylinder up the inclined plane; the force necessary to keep the cylinder in equilibrium will be equal to the weight of the other portion of the cylinder (ADBEA), and a slight increase will lift the cylinder. Heron does not attempt

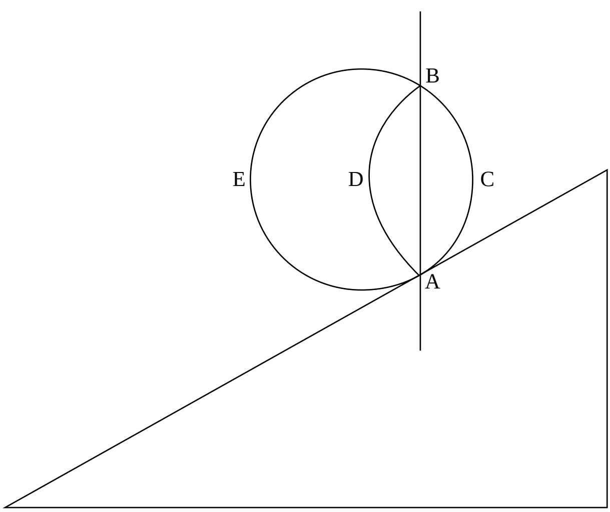

FIGURE 14. Heron's analysis of the inclined plane (modern reconstruction based on the text of *Mechanics* 1.23; there is no figure in manuscripts B or L).

[63] See above, n. 20.
[64] *Opera*, vol. II, p. 60.5–8; English trans. Drachmann, *The Mechanical Technology*, p. 47.

a precise calculation of the weight of the portion of the cylinder that must be held in equilibrium by the force. But his analysis implies that as the plane approaches vertical the portion ADBCA shrinks to zero, so that a force equal to the weight of the cylinder is required to lift it in a vertical direction. As the plane becomes more level the portion ADBCA grows larger, and when the plane is completely level the portion ADBCA is equal to the entire cylinder. The implication (as in 1.20) is that no force at all is needed to keep the cylinder rolling along a level surface, once it has been set in motion.

In these chapters of Book 1 the gap between theoretical results and practical experience is wider than anywhere else in Heron's *Mechanics*; the remarks on friction in 1.21 and 1.22 indicate that Heron was well aware of this fact. But he does not draw out the implications of his analysis of the inclined plane, much less generalize them and introduce a new theory of physics based on the rejection of "motion implies force". While Heron's application of the balance model leads to results that are at variance with basic assumptions of every-day experience as well as practitioners' knowledge, this tension did not lead to a transformation of the conceptual foundations of mechanical knowledge.

CONCLUSION: TRANSMISSION AND IMPACT

The first stage in transmission of Heron's *Mechanics* is marked by the inclusion of extensive excerpts in Book 8 of Pappus of Alexandria's *Mathematical Collection* (pp. 1114–1135 Hultsch). In introducing these excerpts, Pappus draws attention to the difficulty of consulting Heron's original text at the time when he was writing (the late 3rd or early 4th century AD):

> As for the aforementioned five powers, we shall set out a selection from the works of Heron as an *aide-memoire* (ὑπόμνησις) for lovers of learning. In addition we wish to mention the essential things that have been said about the devices with one part, with two parts, with three parts, and with four parts [i.e., the lifting devices discussed in *Mechanics* 3.1–12], lest a person may seek in vain for the books in which these things have been written. For we have come across books that were corrupt in many parts, with their beginnings and ends missing.[65]

Notably, Pappus' excerpts are drawn from the sections of Heron's text that are closest to practitioners' knowledge: the account of the construction and

[65] Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, pp. 1114.22–1116.7.

use of the five powers in 2.1–6, and the descriptions of lifting devices in 3.1–12.[66] Book 8 of Pappus' *Collection* seems to have circulated as an independent manual of mechanics in late antiquity; that it did so in the Arab world is proved by the existence of a self-standing Arabic translation.[67] Although Heron's *Mechanics* also circulated in the Arab world in various versions, none of these seems to have reached the West until the 17th century.[68]

Against this background, the close parallels between Heron's *Mechanics* and a number of early modern texts are especially remarkable. I shall confine myself to three examples. (1) In his *Mechanicorum liber* of 1581, Guidobaldo dal Monte attempts to reduce each of the five mechanical powers or *potentiae* (the lever, pulley, wheel and axle, wedge, and screw) to the balance. (2) In a discussion of the inclined plane in his early *De motu antiquiora* (ca. 1590), Galileo asserts that a body lying on a smooth horizontal surface can be moved by "the smallest of all possible forces"; his reasoning is closely similar to that employed by Heron in 1.20.[69] (3) In his 1597 work *De gli elementi mechanici*, Colantonio Stigliola offers an analysis of the inclined plane that makes use of the balance model in a way very similar to Heron's procedure in 1.23 (Fig. 15).[70] What are we to make of such parallels?

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[66] The Greek text of Pappus ends with the section corresponding to *Mechanics* 3.2, but excerpts of 3.3–12 survive in the Arabic version edited by Jackson, *The Arabic Version*.

[67] For a preliminary edition with translation of the Arabic version, see Jackson, *The Arabic Version*. Eutocius refers to Pappus VIII as the "Introduction to Mechanics" (μηχανικαὶ εἰσαγωγαί) at *Comm. in libros Archimedis de sphaera et cylindro* 70.6; see also the introduction to Jackson, *The Arabic Version*.

[68] For an introduction to the Arabic transmission see the introduction to *Heronis Alexandrini opera*, vol. II, pp. xv–xliv. The great Dutch Arabist Jacob Golius translated the *Mechanics* in the 17th century, but his work seems to have had no impact in scientific circles; see the introduction by Carra de Vaux to *Les mécaniques*, pp. 8–9, and Brugmans, "Specimen mechanicae veterum", repr. in Sezgin, *Hero of Alexandria*.

[69] "A body subject to no external resistance on a plane sloping no matter how little below the horizon will move down [the plane] in natural motion, without the application of any external force. This can be seen in the case of water. And the same body on a plane sloping upward, no matter how little, above the horizon, does not move up [the plane] except by force. And so the conclusion remains that on the horizontal plane itself the motion of the body is neither natural nor forced. But if its motion is not forced motion, then it can be made to move by the smallest of all possible forces" (Galilei, *On Motion*, p. 66).

[70] Stigliola, *De gli elementi mechanici*, pp. 41–42. Specifically, Stigliola's claim is expressed in terms of the concept of "moment" (*momento*): "Il momento della rota appoggiata al piano, al momento della rota sospesa, la ha ragione, che l'eccesso delle portioni del circolo [sc. the area DHFGD], al circolo tutto". The key step, as in Heron, is to recognize that DFE balances DFH; thus it makes no contribution to the *momento* of the circle lying on the inclined plane.

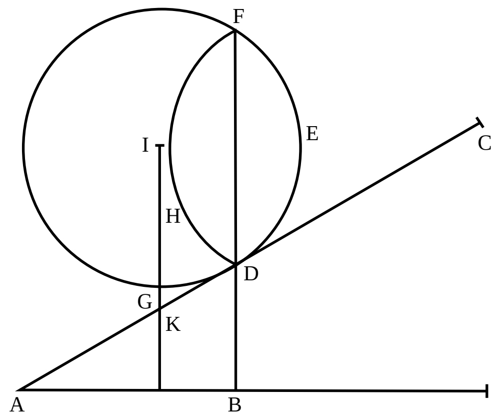

FIGURE 15. Stigiola's analysis of the inclined plane (*De gli elementi mechanici*, p. 41).

It is certainly possible that Heron's *Mechanics* was better known in the early modern period than has generally been recognized; further research may bring to light new avenues by which the transmission of Heron's text facilitated the transmission of ideas.[71] But in fact the parallels we have noted take on even more significance if the *Mechanics* was completely unknown in the Renaissance. Like Heron, the Renaissance authors had both the *Mechanica* and Archimedes at their disposal; they too attempted to analyze the operation of complex machines by reducing them to simpler ones such as the balance or lever. From this point of view, Heron's *Mechanics* provides an independent test case of what can be achieved by building on the conceptual foundations laid down by Archimedes and the author of the *Mechanica*. That different authors working from the same conceptual foundations offer similar analyses of phenomena such as the inclined plane is not in itself surprising. But recognition of this fact is an important step towards understanding the long-term stability of mechanical thinking and the reasons that led to its eventual transformation.

[71] See Russo, *The Forgotten Revolution*, pp. 352–353.

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BRADWARDINE'S RULE: A MATHEMATICAL LAW?

Since the work of Marshall Clagett, it has been generally accepted that Thomas Bradwardine's *Tractatus de proportionibus* (1328) defines a rule for dynamics in mathematical form: this constitutes one of the characteristic elements of what is called the "new physics of the 14th century", a new physics that distances itself from Aristotle's in particular by its systematic reliance on the quantitative.[1] This rule, often called "Bradwardine's Law", became quite widely accepted by the 1340s, both in Oxford and in Paris.[2] By the 1350s, the establishment of this "law" became a "must" when commentators discussed the rules of dynamics in Aristotle's *Physics* VII.5.[3]

This has led an entire tradition of Medievalists to say that the Middle Ages had established a mathematized law of motion. A connection with the 17th-century laws of nature was made, all the more readily as the study of motion had been the principal of the Galilean discoveries. Yet to reject this connection, I may merely note that none of the argumentation in any of the texts I shall discuss is based on an experiment. The only notable exception is found in question 9 in Book VII of Jean Buridan's *Physics* (*Tertia lectura*), in which he mentions observations that do not all seem imaginary. But although they are intended to refute the rules of Aristotle, they could just as well oppose "Bradwardine's Law". In fact, when this rule was cast into doubt at the end of the 14th century, the reasons given were of a mathematical rather than experimental nature: they correspond for the most part to difficulties in understanding the notion of the ratio.[4]

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[1] The date indicated in certain manuscripts is 1328. Crosby cites some thirty manuscripts; several abridged versions, different from each other, were written, but none are dated. The first copy known in Paris is dated 1348 (Ms BN Lat. 625 nouvelles acquisitions); the text is copied in full by Etienne Gaudet in BN Lat. 16621. The modern edition is by Crosby, Thomas Bradwardine, *His Tractatus de proportionibus*; unfortunately, it does not note all the textual variants between the many manuscripts.

[2] As we are discussing the pertinence of the expression "Bradwardine Law", we will put it in quotation marks.

[3] See for example in Buridan, *Super octo physicorum libros Aristotelis*, the version described as *De ultima lectura* of his *Physics* and dated from about 1358, Book VII, question 7 in fols. 107r–108r. In the corresponding question in an earlier version described as *De tertia lectura*, Buridan shows that he knows "Bradwardine's Law" and that he clearly saw that it was incompatible with the rules of Aristotle. See Vatican Ms. Chigi VI-199, fols. 87ra–87va.

[4] It is in his question on the *Tractatus de proportionibus* that Blasius of Parma (late 14th century) refutes Bradwardine's Law. In Italy his argumentation is repeated without major changes by Marliani, *Questio de proportione motuum in velocitate*, pp. 1vb–28rb and by Achillini, *De proportionibus motuum*, pp. 3ra–16vb.

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 51–66.

It is thus on its mathematical form and its use that I shall focus by attempting to answer the following question: Did Bradwardine propose a truly mathematized law of motion, as affirmed by those inspired by the works of Clagett? Along the way I shall look into the pertinence of the frequently used expression, Bradwardine's "function". Like all medieval texts, those of Bradwardine and his immediate successors are long and repetitive: but to speak of them briefly, and in particular with citations taken out of context, risks leading to misinterpretations. For this reason I shall first give the general idea of each author's position before concentrating on the question raised.

The texts that are currently considered as the most important of the early works dealing with our "law" are not all of easy access; some are unpublished, and only three have satisfactory modern editions. Their relative importance is evaluated by their citations among the authors of the 14th century.[5] I have in particular used the scientific notes of a student at the Sorbonne, a future master of theology, Etienne Gaudet, who offers unique testimony regarding natural philosophy and its teaching in Paris in the mid-14th century.[6] In this list can also be found Albert of Saxony's *Tractatus proportionum*, for it is by far the most successful text: it was through this intermediary treatise that "Bradwardine's Law" was first known to the Renaissance.[7] I shall nonetheless not comment on it, for it is probably later than the others, and adds nothing to the text of Bradwardine, of which it is an abridged copy.

THE ARISTOTELIAN ORIGIN OF "BRADWARDINE'S LAW"

I shall roughly summarize what thanks to Marshall Clagett is now well known.[8] For our authors, the problem raised by the rules given in the *Physics* VII.5 and other passages of Aristotle (*Physics* IV.8, *De caelo* III.2)

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[5] The existence of Renaissance editions and the number of manuscripts are indicators that are taken into account, but it is generally felt that these are much less reliable.

[6] These notes, unfortunately particularly difficult to read, have caught the attention of famous medievalists. See in particular Kaluza, *Thomas de Cracovie*, pp. 60–94. They are collected in the manuscript BN Lat. 16621 that Zénon Kaluza dates mostly to the end of the 1340s; note however that later notes also appear.

[7] This treatise is dated from 1351 to 1360. There are more than forty manuscripts, thirteen incunable editions and seven 16th-century editions. There is a modern edition; see Albert of Saxony, *Der Tractatus proportionum von Albert von Sachsen*. See Celeyrette and Mazet, "Le mouvement du point de vue de la cause et le mouvement du point de vue de l'effet dans le *Traité des rapports* d'Albert de Saxe".

[8] Clagett, *The Science of Mechanics in the Middle Ages*, pp. 421–444.

is the determination of the cause of local motion, violent or natural, in the sublunar world; the question of motions of alteration and augmentation presents supplementary difficulties. The motive power and the resistance of the moving object or the medium intervene, but how?

The first idea (Philoponus, Avempace) was that the cause of motion is the excess, in the sense of difference, of power over resistance. But such a hypothesis would lead to the existence of motion in a void, since there would be a cause, which is the motive power alone, and thus an effect, motion.

The response of Averroes was that the cause of motion is not this excess, but rather the ratio of power to resistance, which would eliminate the problem of the void, for in a void the ratio F:R would not exist. By implicitly invoking the proportionality of cause and effect, accepted by all, Averroes let it be understood that speed is proportional, in a mathematical sense, to the ratio F:R.[9] In his commentary 35 on the *Physics* VII, he writes: if it should be that the ratio of the motive power to the moving object is doubled, the speed is necessarily doubled.[10] This phrase will be repeated in most of the texts we will discuss.

BRADWARDINE’S *TRACTATUS DE PROPORTIONIBUS*

As the title indicates, this treatise is presented as dealing with ratios. It is divided into four chapters. The first chapter recalls in classic fashion the definition of ratios and those of the various proportionalities. It ends with a series of properties that will be used for the study of motion in the two following chapters.

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[9] The traditional translation of *velocitas* by “vitesse” or “speed” is problematic. In the 14th-century texts, the word “*velocitas*” is often used for “*motus*” just as “white” could be used for a white object, which is consistent with the fact that a *velocitas* can be treated as a quality, whose contrary quality would be *tarditas*. As the examination of Oresme’s text confirms, the best translation is generally “rapidity”, which has the advantage of not giving rise to an anachronistic reading, even if in certain contexts “speed” may be accepted. I do not wish to enter into this discussion here and shall therefore follow the tradition of using the word “speed”. On this question, see Jean Celeyrette, “La mécanique, une science médiévale?”, available online at the website of the UMR Savoirs et Textes, Langage: http://stl.recherche.univ-lille3.fr. For a more in-depth look at the concept of *velocitas* one can consult the articles of Pierre Souffrin, and in particular Souffrin, “*Velocitas totalis*”.

[10] Averroes, *Aristotelis opera cum Averrois commentariis*, vol. IV, fol. 335ra A–B: “*Contingit necessario ut proportio potentie motoris ad motum sit dupla istius proportionis, et sic velocitas <erit> dupla ad istam velocitatem*”.

(1) Given three magnitudes of the same type, A, B, C, the ratio A:C is compounded of A:B and B:C, noted as A:C = (A:B)(B:C); if A:B is equal to B:C we will say that A:C is the double of A:B.[11] The terminology is justified by the word "compounded", which allows us to consider that A:C is a whole made up of two equal parts (A:B and B:C) that can thus legitimately be called its halves. Similarly, we can say that there are triples, quadruples, etc. of a ratio.[12] This corresponds to our powers of three, four, and so on. This argument is noted in all the texts I shall examine.

(2) No ratio of greater inequality (numerator greater than the denominator) or lesser inequality (denominator greater than the numerator) is greater or lesser than a ratio of equality. Indeed, by taking "multiple" in the previous sense, that of our "powers", a multiple of a ratio of lesser inequality is never a ratio of equality, and as any multiple of a ratio of equality is a ratio of equality, it is never a ratio of greater inequality.[13]

(3) An objection is then raised: if C > A and if B is a third quantity, C:B > A:B by proposition V.10 of Euclid's *Elements*. But if A = B, the ratio A:B is the ratio of equality and C:B, a ratio of greater inequality, is greater than it; thus from 5 > 3 we should be able to deduce that 5:3 > 3:3. Bradwardine responds that the property of the *Elements* invoked applies only to ratios of the same genus, while the ratios of greater inequality, lesser equality, and equality are of three different genera.[14] This obviously does not agree with the characterization of rational ratios by their denomination[15] since the denomination of a ratio of greater inequality is greater than that of the ratio of equality, the number 1.[16] In fact, in his treatise Bradwardine makes use of denominations only to define two equal ratios: the inequality of ratios is not defined.

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[11] The expression used is "*proportio dupla*" while in later texts the terms used are instead "*duplata*" or "*duplicata*". Crosby's edition does not make possible to know if certain manuscripts contained the variants "*duplata*" or "*duplicata*". We note that in response to an objection, Bradwardine refers to Euclid to say that in the above case A:C is A:B *duplicata*.

[12] Bradwardine, *Tractatus*, pp. 78–79.

[13] Bradwardine, *Tractatus*, pp. 80–85.

[14] Bradwardine, *Tractatus*, pp. 84–85.

[15] The denomination is the potentially fractional number that allows us to know the ratio: thus the ratio 3:2 has the denomination 1-1/2 and the name "sesquialter proportion", the ratio 2:1 has the denomination 2 and the name "double proportion". For this notion, in the Middle Ages and the Renaissance, the reference is generally to Jordanus de Nemore (first half of the 13th century). The question of the denomination of irrational ratios is briefly discussed by Bradwardine, *Tractatus de proportionibus*, pp. 66–67, and more in depth by Oresme in his *De proportionibus proportionum*, pp. 138–172.

[16] This will be one of the basic arguments of Blasius of Parma and his successors; see the references given *supra*, note 4.

In the second and third chapters these considerations are applied to motion. In the second chapter, Bradwardine returns to the problem of the cause of local motion. He studies in turn four possibilities and eliminates them one after the other. Reasoning by sufficient division, in the third chapter he adopts a fifth cause, for, he says, we cannot imagine others, and agrees with Averroes's position. The cause of motion is the ratio of power to resistance, with the speed of motion proportional to the ratio F:R that produces it. This proportionality is said to be geometrical, while an arithmetical proportionality would be proportionality to the excess F – R. This is theorem 1 of the chapter, and is what is usually called "Bradwardine's Law". At this stage, the "law" is formally identical to the Averroes's interpretation of Aristotle's rules.[17]

The formulation of this theorem given in Crosby's edition is that the ratio of speeds of motions follows the ratio of the motive power to the resistance, and inversely (*proportio velocitatum in motibus sequitur proportionem potentiarum moventium ad potentias resistivas, et etiam econverso*). This is immediately made explicit as follows: the ratios of the motive power to resistance and the speeds of movements taken in the same order are proportional (*proportiones potentiarum moventium ad potentias resistivas, et velocitates in motibus, eodem ordine proportionales existunt, et similiter econtrario*).[18]

In fact, only the second formulation correctly expresses the proportionality. Contrary to what is stated in the first formulation, it is the speed which follows the ratio F:R and not the ratio of speeds; the ratio of speeds is in fact equal to the ratio of ratios.[19]

Theorems 2–7 which immediately follow are the corrections to the rules of *Physics* VII.5, which are based on the proportionality defined in theorem 1, taking into account the notions of doubling, tripling, etc. of a ratio, as defined in chapter 1. Thus the rule of Aristotle stating that when a motive power is doubled with constant resistance (or when the resistance is halved with constant motive power), the speed is doubled, etc., must be corrected as follows: a doubling of the speed corresponds to a doubling of the ratio

[17] Bradwardine, *Tractatus*, pp. 86–113.
[18] Bradwardine, *Tractatus*, pp. 112–113; the quotation is on p. 112.
[19] It is obviously possible that the first formulation of the law that appears in the Crosby edition is in fact: *proportio velocitatum in motibus sequitur proportionem <proportionum> potentiarum moventium ad potentias resistivas*, and that it would thus be correct; but in the absence of a complete critical apparatus it is impossible to know if this formulation is attested to in certain manuscripts. It must nonetheless be noted that the incorrect formulation appears in other treatises; with the mistake possibly arising there too from scribes who may have thought there was a repetition of *proportionem/proportionum* in their exemplars.

F:R, but this doubling must be understood in the sense of compounded ratios, that is to say, it must correspond to what we know at the square of the ratio. This obliges us to distinguish three cases, depending on whether F:R is greater than, equal to or less than the double ratio; for example, in the first case, the doubling of the motive power corresponds to a ratio 2F:R, which is less than F:R doubled – that is to say squared – and corresponds to a speed less than the double of the initial speed.[20]

The consequence of theorem 1 that received the most comments from the successors of Bradwardine can be expressed this way: if F:R is a ratio of greater inequality, it corresponds to a movement, and thus a speed V. With the proportionality imagined by Averroes, for any ratio F:nR where nR is any multiple of the resistance, there should be a speed V/n, and thus a movement; yet this is impossible if F is less than nR. The result is that this proportionality can only be applied if the ratios considered are ratios of greater inequality. With the proportionality imagined by Bradwardine, any sub-multiple of the ratio F:R remains a ratio of greater inequality, and always has a corresponding movement; in particular the speed V/n comes from a ratio of greater inequality, which compounded n times by itself, gives F:R. Although the discrepancy with Aristotle and Averroes is patent, Bradwardine does not highlight it.

The problems raised by theorem 1

The rules of *Physics* VII.5 use only a doubling, of a speed and of a ratio. And even if "double" is used for any multiple, there is no difficulty in giving a sense to these expressions: Aristotle himself explains that it is a movement twice as fast as given movement, and Bradwardine in his chapter 1 explains, with the help of the composition of ratios, what this expression means: one ratio is the double of another. Theorem 1 is thus very easy to use to correct Aristotle, which is what Bradwardine does in the theorems that follow.

But stating in general fashion proportionality between the speeds and the ratios F:R creates a series of problems that Bradwardine does not deal with; moreover, there is no reason to believe that he saw these problems and implicitly resolved them. Let us try to make a list of these problems. Is it possible to speak of a ratio of a power and a resistance, which are at first glance not of the same nature? Can one speak of the ratio of two speeds, when speeds are not magnitudes? Indeed, what are speeds?

The difficulty is even greater for ratios of ratios. Ratios are not magnitudes but relationships. Only their equality is defined, with denominations,

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[20] Bradwardine, *Tractatus*, pp. 112–113.

and only in the case of rational numbers, while their inequality or their comparison are undefined. Nor is the ratio of ratios of ratios defined, imperative if proportionality is to have a meaning. On the other hand, the non-comparability of ratios of greater inequality and equality, or of lesser inequality, is demonstrated, and its importance stressed, which will not be the case for the treatise of Albert of Saxony. This is indispensable, for only the first two are causes of motion.

The fact that all these difficulties were not dealt with can obviously be explained by the context: in Oxford after John Duns Scotus the systematic use of a quantitative language became commonplace, even for speaking of qualities. But this can also be a clue that Bradwardine's goal is not the elaboration of a general rule, but simply the correction of Aristotle's rules that merely make use of doublings; for such corrections, there is no need to deal with the difficulties we have just listed.

LATER ENGLISH TEXTS

I shall now look at what happened in English texts of the same period or immediately following Bradwardine. I shall deal briefly with the question: Does in every case the motive power exceed the resistance of the moved thing? (*utrum in omni motu potentia motoris excedat potentiam rei mote*). The question is attributed to Kilvington, and is likely to be roughly contemporary to Bradwardine's treatise.[21] Elzbieta Jung-Palczewska has made a detailed comparison of the arguments of Kilvington's question and chapters 3 and 4 of Bradwardine's treatise.[22] She notes 26 identical arguments. In fact, the organization of this part of the question is more or less the same as that of chapters 2 and 3 of Bradwardine's treatise, less the general points on ratios, while the argumentation is analogous, even if the formal presentation is different: Bradwardine's treatise is structured as a work of mathematics (postulates, theorems, etc.), while Kilvington's question is much wordier. The only notable difference is that Kilvington shows that a ratio of greater inequality is infinitely greater than a ratio of equality, rather

[21] This question, which appears in several manuscripts, including Venice Marciana San Marco VI-72, has yet to be published. The attribution to Kilvington is argued by Sylla, "The Oxford Calculators", pp. 437–446. After reconstituting the university career of Kilvington, Jung-Palczewska, "Works by Richard Kilvington", pp. 182–186, proposes the date of 1324–1325, which leads her to attribute to Kilvington the paternity of Bradwardine's Law. Kilvington was well known in Paris in the 1350s, he is abundantly quoted by Etienne Gaudet in BN Lat. 16535, but not much for his works on motion; we know of only one reference (Ms Vat. Lat. 986).

[22] Jung-Palczewska, "Works by Richard Kilvington", pp. 208–213.

than simply stating as does Bradwardine that they are not comparable; he thus does not have to invoke a difference of genus. Let us also note that Kilvington, unlike Bradwardine, insists on the fact that Aristotle's rules are false.

Among the other English texts, we can cite the anonymous treatise *De sex inconvenientibus*, certainly earlier than the Parisian texts – it can be dated to the end of the 1330s or the beginning of the 1340s, for it cites Bradwardine and Heytesbury, but is cited by John Dumbleton.[23] In its fourth question, devoted to the speed of local motion, the first two opinions judged as erroneous in Bradwardine's treatise are noted and their refutations are attributed to Bradwardine and to an almost unknown Oxford master, Adam of Pippelwel. The notion of ratios of ratios (*proportio proportionum*) is mentioned, which is not the case in Bradwardine's treatise, but the definition is not given. In fact, everything seems to indicate that Bradwardine's presentation was quickly adopted in Oxford, and that it became a ritual subject, used in both theoretical questions and in more scholastic exercises such as this last treatise.

JOHN DUMBLETON'S *SUMMA*

More interesting than these cases is the development that appears in part III of Dumbleton's *Summa,* devoted to motion. Let us point out first of all the considerable success of this *Summa*, which can be explained neither by the rank of its author (he probably never finished his studies in theology) nor by the clarity of his explanations. And yet many manuscripts of this work are extant.[24] Etienne Gaudet abundantly copied it, in particular almost the whole of part III, perhaps because Dumbleton taught at the Sorbonne in the years 1346–1347, during which Etienne Gaudet did part of his studies.[25] Dumbleton first presents refutations of erroneous opinions (the very one refuted by Bradwardine), which offer nothing new with respect to those of his predecessors, then a series of postulates or hypotheses necessary for the

[23] In the edition prepared by Bonettus Locatellus, Venice: O. Scot, 1505, we find it at fols 34r–59r.

[24] Sylla, "The Oxford Calculators", pp. 574–588, quotes large extracts from chapter III of the *Summa* established from the majority of known manuscripts. Principal manuscripts are Vat. lat. 6750, Cambridge Peterhouse 272, Cambridge Gonville and Caïus 499/268, BN Lat. 16146. Passages of the *Summa*, including those from Book III, are copied by Etienne Gaudet in BN Lat. 16621.

[25] Weisheipl, "Ockham and Some Mertonians", pp. 199–202, notes that Dumbleton is also listed in the rolls of Oxford in 1340 and again in 1344–1345, then disappears.

establishment of a "law", a demonstration of this law, and finally several applications of it.

With respect to the part devoted to the establishment of the law, it is sometimes difficult to distinguish the hypothesis or postulate on the one hand and the conclusion on the other, as Dumbleton often begins with propositions equivalent to what he wants to demonstrate. At least it is clear that he conceives proportionality indifferently, whether in applying it to speeds and to ratios of F:R, or in applying it to increases in speeds and increases in ratios of F:R. The equivalence between these two proportionalities is true but not evident, although it seems to go without saying for our author.

He presents three demonstrations. To judge their mathematical level, I shall present and comment one of them. It is very short: if a double ratio comes from a speed B and a triple ratio from a speed A, then, *permutatim*, the ratio of A to B is equal to the ratio of 3:1 to 2:1. We note that Etienne Gaudet copies this "demonstration" without comment.[26] What Dumbleton means, and what is at first glance surprising, is that the relation between the double ratio and speed A is the same as that which exists between the triple ratio and B. Without inquiring into the licit character of the assimilation of such a relation (between a ratio and a speed) and a mathematical ratio (which can only be between two magnitudes of the same genus), he interprets this identity of relations as a mathematical proportion, and then exchanges the means; in the *Elements* this possibility is of course limited to the case when the four magnitudes involved are of the same genus. And as to the notion of ratio of ratios, it has here no other definition than that which results from the obvious particular case: if a ratio is the triple of another (in the sense of the composition of ratios), their ratio is 3.

It is clear that this is not an even remotely rigorous demonstration: the mathematical notions and properties called on by Dumbleton to resolve a physical question have hardly more than a metaphoric status. We will nonetheless note, in his defence, that this "exchanging the means" of a proportion without the magnitudes' being of the same genus is not restricted to Dumbleton: it can be found, for example, in a question by Kilvington on *De generatione* (is the continuous indefinitely divisible?), in one of Oresme's questions on the *Physics* (question VII.9), and in Richard Swineshead's *Calculationes*.

The applications that follow the "law" are all numerical illustrations of its second form: the proportionality between the increases in ratios (in the sense of composition) and the corresponding increases in speeds. Dumbleton

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[26] It does not appear in the extracts given by Sylla. In the manuscript that served as her basis, Cambridge Peterhouse 272, this demonstration is at the end of fol. 24va.

expresses this proportionality several times by this formula: to equal latitudes of ratios correspond equal latitudes of motion.[27] Let us examine the first application: if a motion A comes from a double ratio (2:1) and a motion B from a composite ratio of 2:1 and a quarter of 2:1 (a quarter of 2:1 is the ratio that compounded four times by itself gives the ratio 2:1), the increase of the ratio is a quarter of the ratio from which A comes, and the result is that the increase of the speed from A to B is a quarter of the speed of A. The following diagram illustrates this correspondence between the ratios and the speeds:[28]

E---------D--------C---------B---------A
K---------I---------H---------G--------F

The first line represents the latitude of motions (i.e., speeds), the second the latitude of the corresponding ratios. Thus E corresponds to rest, K to the ratio of equality, F to the ratio 16:1, A to the corresponding speed. Let G correspond to the ratio 8:1; the decrease of F to G is the double ratio 16:8, for 16:1 = (16:8)(8:1). But the double ratio is a quarter of 16:1, and thus the decrease of speeds from A to B is a quarter of the speed A, which is also represented by EA. On the diagram, H corresponds to the ratio 4:1, and I to the ratio 2:1. The diagram is systematically used for the other numerical applications.

A few comments. The word “latitude” simply means a possibility of increase (or decrease) – and this increase that normally concerns quantities is generally understood from the beginning of the 14th century to extend to qualities. The representation of latitudes by straight lines is also common, and will be theorized by Oresme in chapter 1 of his *Tractatus de configurationibus qualitatum et motuum*. On the contrary, it would seem that the representation of the correspondence ratios/speeds by these two parallel straight lines as used in the applications can only be found in Dumbleton. In this representation of numerical illustrations of correspondences some believe to be able to see a prefiguration of a function.[29]

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[27] “Equali latitudini proportionis correspondet equalis latitudo motus”. A similar version is quoted by Sylla, “The Oxford Calculators”, p. 577.

[28] This passage appears in Sylla, “Medieval Concepts of the Latitude of Forms”, pp. 265–267.

[29] Thus Sylla, “The Oxford Calculators”, p. 265, note 8, interprets this representation of the correspondence between ratios and speeds as proof that Dumbleton has indeed conceived of a mathematical “function” of F:R to V, a function that would be logarithmic.

QUESTIONS 14 AND 15 OF RICHARD SWINESHEAD'S *CALCULATIONES*

Richard Swineshead is concerned with other problems. The two short texts generally attributed to him (*De motu* and *De motu locali*) appear only as short sketches of what is amply developed in questions 14 and 15 of the *Calculationes*, so we will content ourselves with speaking of these questions.[30] In these texts usually dated from 1345 to 1355, Swineshead does not attempt to demonstrate Bradwardine's "Law".[31] He considers it as established, and uses it to pose and solve new problems, most often mathematically. It is stated as a hypothesis in the following form: motion is measured according to the geometrical ratio of motive power to resistance. In fact it is used as often in this form as in that dealing with increases, without the question of the equivalence being posed.

The goal of question 14 is to determine how power and resistance must vary so that the corresponding variation is of a given type, for example so that the increase in speed is uniform, or so that it is uniformly increasing, etc. To give an idea of the type of argumentation, I shall begin with the first rule given in question 14. If a power increases from F_1 to F_2, with the resistance remaining invariable, the corresponding increase of F:R is the ratio $F_2:F_1$, a ratio that is independent of R. We can see this by writing: $F_2:R = (F_2:F_1)(F_1:R)$ and by conceiving the composition as an addition.

In order for the growth of V to be uniform, that is, for the motion to accelerate in a uniformly difform fashion, the increases in speed must be the same for intervals of equal time. Applying then the proportionality in its second form, this last condition is like saying that the increases in the ratios F:R, that is to say the ratios of type $F_2:F_1$, $F_3:F_2$, etc., corresponding

[30] Some of the rules in the treatise *De motu locali* and in question 14 of the *Calculationes* are copied by Etienne Gaudet in BN Lat. 16621, where they are at first attributed to a certain Robert. Later they are repeated as part of the teaching in Paris of a master Clay, an Englishman who is not otherwise known. In the manuscripts it seems that the different parts of the *Calculationes* are presented as a series of questions (but with a few internal references). But given their complexity, it is difficult to imagine that they could have been written in a short period. The treatise has no modern edition and has been little studied. References are to the edition prepared by Giovanni Tolentini (Pavia: Franciscus Girardengus, 1498), chapter 14, *De motu locali*, fols. 98b–111b, and chapter 15, *De medio non resistente,* fols. 111b–125b.

[31] Maier, *Ausgehendes Mittelalter* 1, pp. 381–399, considers that the *Calculationes* are posterior to 1344 (date of the attested presence of Richard Swineshead among the fellows of Merton College of Oxford), and anterior to 1352, for they are cited in the *disputatio* held by Jean de Casale in Bologna before 1352. But she cited the question 2 and the fact that it was written before 1351 does not imply that the same is true for the others.

to equal times are equal. This is what Swineshead calls uniformly proportional growth.

He then gives the rules stating the properties of the same type for a motive power that decreases or a resistance that increases or decreases with constant motive power. This rule and three others of the same kind can be also found in the short treatise *De motu locali* mentioned above. They are twice copied by Etienne Gaudet, who, in one of the two cases, refers to a certain English master named Clay. We can thus conclude that this part of Swineshead's work was known in Paris, and probably before the end of the 1340s.

In the entire first part of question 14, resistance, constant or variable, intervenes in an abstract fashion. From rule 30 onwards, the resistance is that of a medium whose resistance is non-uniformly difform, but one whose difformity is defined only indirectly, by the following condition: this medium is traversed in a uniformly delayed motion by a moved thing under the action of a constant motive power F_1.[32] Swineshead demonstrates first that such a medium exists, and then that the corresponding constant power F_1 is unique. The rest of the question is then devoted to the problem of knowing if such a medium can be traversed in a uniformly retarded motion by the moved thing under the action of a variable motive power F_2 different from F_1.

The problems become increasingly refined in question 15. For example, in the first conclusion of the second part, Swineshead supposes that a motive power varies in a uniformly difform fashion with respect to time beginning with degree 0, and that the resistance of the medium, constant with respect to time, is uniformly difform from zero from one end to the other. He then establishes that the motion of the moved thing is uniform.

The mathematical level of these questions in the *Calculationes*, if not their clarity, is strikingly different from that of other English texts, including those of Bradwardine. While the reading of the latter authors often leaves an impression of confusion, one cannot but be struck by Swineshead's remarkable mathematic mastery, even if his laconic style does not make reading him easy. The problems he raises are of genuine complexity. To resolve them correctly, or even simply to understand them, we must use modern mathematical tools, and we can thus observe that both the properties accepted and the conclusions established are almost never in error. We can briefly note that he attempted to draw the physical consequences of the law, even if this physics is an imagined one called *secundum ymaginationem*.[33] He seems to have been about the only master to do so.

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[32] I refer here to the numbering of the 1498 edition, which is not found in all manuscripts.

[33] In fact in questions 14 and 15 the physics is "imagined" with two degrees of separation, for the powers and resistances about which one reasons are abstract. It is only in question 11 that we find an imagined physics in which there is a concrete physical problem, that of

NICOLE ORESME

We know that Bradwardine's Law was known in Paris before its first attested publication (1348) because Oresme deals with it at length in his *Quaestiones super septem libros physicorum Aristotelis*, which predates 1347.[34] His major work on the subject, his *De proportionibus proportionum*, dated approximately 1349, is generally considered as one of the most accomplished medieval mathematical texts. Like Swineshead, Oresme sets himself apart from his contemporaries. But he does so in a different way and for different reasons. In his works there are mentioned no other applications than those relating to astrological predictions; in particular, there are no allusions to the applications dealt with by Swineshead.[35] On the contrary, in both his *Quaestiones* and his *De proportionibus proportionum*, he examines in detail the problems posed by the law and not dealt with in the treatises studied, and does so with remarkable rigor. [36]

In his question 7 on Book VII of the *Quaestiones* he deals with the question of the comparison of speeds. Speed (*velocitas*) is defined as the faster motion (*velocius*): that which traverses the same distance in less time, or a greater distance in the same time; so that the proper meaning of "comparison of speeds" is comparison of local motions by means of the faster or slower.[37] Comparisons described as more or less improper are also envisaged, for example, between a local motion and a motion of alteration. Question 8, on the cause of motion, is devoted in classic fashion to the refutation of the erroneous opinions of which we have spoken. In question 9

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falling bodies – although it is true that this fall is supposed to continue to the centre of the Earth. The point is to determine if a stick with weight falling through a channel can reach the centre of the Earth in a finite time. The text, which is a good example of Swineshead's technical virtuosity, was studied and published by Hoskin and Molland, "Swineshead on Falling Bodies".

[34] Entire passages are taken by Pierre Ceffons in his *Commentaire des Sentences* and we know that Ceffons read the *Sentences* in 1348–1349 (see Trapp, "Peter Ceffons of Clairvaux", p. 127, n. 12).

[35] There are no known indications that either of these two masters knew the works of the other.

[36] Oresme's *Quaestiones super septem libros physicorum Aristotelis* are only known by one manuscript, Seville, Biblioteca Colombina, 7.6.30, fols. 1–74vb, and have been partially published by Kirschner, *Nicolaus Oresmes Kommentar zur Physik des Aristotele.* The questions on Book VII of which we speak here remain unpublished. There is a modern edition by Grant of the *De proportionibus proportionum.*

[37] In the Middle Ages this is the only definition of the word "*velocitas*", that of Aristotle. It was not until the 17th century (Wallis, it seems) to have a definition like: "speed is . . .". This is another reason why we feel that "*velocitas*" would be better translated by "rapidity" than by "speed". See *supra*, note 9.

Oresme retains Bradwardine's solution, and explains at length the way in which it is legitimate to speak of the ratio of a power to a resistance, despite the fact that one is an active power and the other passive. The last question is devoted to the correction of Aristotle's rules, which is unremarkable, for a commentary on the *Physics* VII normally concludes with a question on chapter 5, the last chapter.

Of all the difficulties noted above linked to Bradwardine's proportionality, only that concerning the ratio of ratios was not dealt with. How and in what way can one speak of such a ratio when ratios are not magnitudes? The problem is discussed in depth in the *De proportionibus proportionum*. We know that from the additive conception of the composition of ratios, Oresme showed that one can consider ratios as continuous magnitudes, indefinitely divisible, between which it is possible to define a ratio, the ratio of ratios. This ratio is either rational or irrational, so that one can speak of the commensurability or incommensurability of two ratios.[38]

The studies of Oresme, both in the *Quaestiones* and in the treatise, appear all the more remarkable when we compare them, even briefly, with the corresponding questions from the chronologically closest version of Buridan's *Physics* from the early 1350s.[39] Buridan's treatment, quite brief, includes very few theoretical developments: he deals specifically neither with the composition of speeds, nor with the comparison of power to resistance. In the two questions devoted to the cause of local motion he considers as accepted Bradwardine's Law, which constitutes his starting point, and which he unsurprisingly shows is incompatible with Aristotle's rules. He confirms the refutations of these rules based on observations of archery, thrown stones, hauling of boats, etc.

CONCLUSION

What can we conclude from this brief overview? Let me repeat the obvious: the search for a mathematical law of nature is not a medieval problem. The intent of the medieval master is commentary on and explication of the books of Aristotle. When the medieval master appears in contradiction with Aristotle, in general he removes the difficulty with the help of distinctions. If this does not succeed, he can, as we have seen, choose to ignore the disagreement (as Bradwardine does), to stress it (as Kilvington), or even

[38] *De proportionibus proportionum*, pp. 138–263.

[39] The main manuscript of the *Super octo physicorum libros Aristotelis* (*Tertia lectura*) VII.8 et VII.9 is Erfurt 298. For the dating, see Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, 1989, p. 50.

to pretend that Aristotle was incorrectly translated (as Oresme); in another context (the infinite, the void, time, etc.), he could oppose Aristotle in the name of Faith. During the 14th century, we can observe all these attitudes without the choice of one or the other appearing particularly meaningful. As to questioning systematically the Aristotelian framework, we know of only one master who did so, Nicolas d'Autrécourt (late 1330s).

Bradwardine's text, like that of Kilvington, clearly had the goal of critical commentary on *Physics* VII.5. As these rules dealt with the relation of cause and effect in motion, it was normal to begin their examination by the different theses possible. This is the standard framework of medieval discussion. The important point is that here the rules given by Aristotle cannot be saved, even with the acrobatics medieval scholars were so accustomed to and without the option of blaming all on Aristotle's ignorance of Revelation. The solution can thus not arise from an exegesis of Aristotle, nor for that matter from another authority, even if Averroes or even Euclid are called upon; this is why Bradwardine and the others used the process of sufficient division. This is why in the end it seems to us that Bradwardine's text can be situated in the continuity of Aristotelian commentary of the time. His main novelty, and it is important, is that the disagreement with Aristotle is about a question of natural philosophy and for reasons tied solely to its internal coherence and not, as was common at the time, for reasons of faith. All the other texts that we have seen – with the exception of Richard Swineshead's *Calculationes* and Oresme's treatises – participate in a vulgate inaugurated by Bradwardine without bringing any genuine novelty.

In my opinion it is in Oresme and Swineshead that can be found the veritable discontinuity. Only these masters seem to take seriously Bradwardine's general formulation of proportionality, the former by founding it mathematically and the second by applying it to new problems of natural philosophy. These are truly new problems of natural philosophy and not *sophismata*, those scholastic exercises, more often logical, more rarely mathematical, aimed at training advanced students. For example, the question to which I alluded – the fall to the centre of the Earth of a heavy stick – was aimed to test the hypothesis that the whole is its parts.[40] It is only, in my opinion, in these two masters that is implemented a mathematized law of local motion.

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[40] Swineshead imagined that one part of the stick had passed through the centre of the Earth and another had not: if the whole is its parts, the first part resists the falling motion, and by applying Bradwardine's proportionality, Swineshead establishes that the centre of the stick will never reach the centre of the Earth, or in any case not in a finite time, which is contradictory to the notion of natural place. See Hoskin and Molland, "Swineshead on Falling Bodies", pp. 50–182.

Yet neither master had a genuine posterity. The known disciples of Oresme showed little interest in the theory of the *De proportionibus proportionum*; the commentaries that we know on the *Calculationes* from the Renaissance are very incomplete and do not deal with these problems. And when Bradwardine's proportionality is cast into doubt or even refuted, particularly in Italy, no allusion is made to the works of these two masters. The target of the adversaries (Blasius of Parma, Giovanni Marliani) is rather Albert of Saxony's *Tractatus proportionum*.

For similar reasons, I have reservations about the use of the expression Bradwardine "function" with regard to Dumbleton's diagram illustrating the correspondence between speeds and ratios. Their latitudes are represented by parallel straight lines, the degrees noted as numbers. But these are only notations that appear mathematical, which is far from sufficient to be able to conclude that this represents a mathematization. If we reduce the function to its definition, a correspondence between two types of magnitudes such that each of the first type corresponds to one of the second, we could perhaps speak of a function. But a mathematical notion can never be reduced to its definition, and when we introduce it, it is so that it becomes at least a tool, at best an object of study. This is never the case with Dumbleton. And so the expression "function" applied to the medieval context has no set cognitive content, and even less explanatory value; it does however have the disadvantage of leading to anachronistic readings.

EDITH DUDLEY SYLLA[1]

THE ORIGIN AND FATE OF THOMAS BRADWARDINE'S *DE PROPORTIONIBUS VELOCITATUM IN MOTIBUS* IN RELATION TO THE HISTORY OF MATHEMATICS

In the *Libelli sophistarum* published eleven times between 1497 and 1530 for the use of Oxford or Cambridge undergraduates, a shortened version of Thomas Bradwardine's *De proportionibus velocitatum in motibus* is included, along with several other short works mainly on logical topics.[2] That this *Tractatus de proportionibus*, which was probably abbreviated from Bradwardine's longer work at least a century earlier, was still thought to be an important and relevant part of undergraduate education in the early 16th century shows how thoroughly Bradwardine's approach to the proportions of forces, resistances, and velocities in motions had been assimilated into the scholastic curriculum. Indeed, to deal with proportions in the Bradwardinian way was, along with various methods of logic, among the tools of analysis that every Oxford or Cambridge undergraduate was taught even in the early 16th century.[3]

By the 20th century, however, Bradwardine's theory of the proportions of velocities in motions had become alien territory. Thus E. J. Dijksterhuis wrote about Bradwardine's theory:

> Bradwardine now assumes that what really happens is that the proportion in which the velocity changes 'follows' the manner in which the proportion between force and resistance changes; in doing so he says something which in the eyes

[1] North Carolina State University, Raleigh, NC, USA. *Note on terminology*. In an attempt to work within the conceptual structure used by Bradwardine, in this paper I use "proportion" as my translation of the Latin "*proportio*". In medieval translations of Euclid and in the work of Bradwardine, the Greek "*logos*" was translated into Latin as "*proportio*", and the Greek "*analogon*" was translated "*proportionalitas*". In Renaissance translations, however, the word "*ratio*" was used to translate "*logos*". Modern English follows Renaissance translations, understanding by "ratio" a "rational number", "fraction", or "indicated division", and by "proportion" the equation of two ratios, whereas in the medieval understanding rational numbers or fractions were understood not as proportions, but as the "denomination", or "quantity" of the proportion, not at all identical to the proportion itself, which remained a relation between two entities. Following this distinction, I represent the medieval "proportion" with the symbolism a:b, and its "denomination" with the symbolism a/b. In quoting translations or discussions by other authors, however, I normally do not alter their word choice, so "ratio" may appear in quoted translations rather than "proportion" for the Latin "*proportio*".

[2] See Ashworth, "The '*Libelli sophistarum*' ".

[3] For these "analytical languages", see Murdoch, "From Social into Intellectual Factors".

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 67–119.

> of a present-day reader is either entirely indefinite (i.e., if what is meant by 'following' is merely the existence of a dependence) or coincides with the Aristotelian formulation (i.e., if this dependence is defined as a direct proportion). His intention, however, is neither the one nor the other, but to make it clear something must first be said about the terminology of the medieval theory of proportion, which is very strange and confusing when compared with present-day modes of expression … [he goes on to explain the compounding of proportions as Bradwardine uses it].
>
> The mode of expression described above persisted well into the eighteenth century and was a constant source of misunderstanding to later readers, who were always bewildered by the fact that the double of 3 is indeed 6, but the double of the ratio 3:1 is the ratio 9:1 ….[4]

After describing Bradwardine's theory in terms of a logarithmic or exponential equation, Dijksterhuis commented:

> Although this is an instance of an unfounded mathematical formulation of a natural law that is not valid, Bradwardine's argument is by no means destitute of historical importance. It testifies to a tentative search for a mathematical expression of a supposed functional relation in nature. And for us it is instructive as a symptom of the great difficulties that had to be overcome before the phenomena of nature could be described in the language of mathematics. As is quite clear from the above, these difficulties were not only due to the fact that incorrect physical assumptions were the starting-point; they were also caused by peculiarities and defects of the mathematical language. Indeed, the latter was still that of Euclid's *Elements*, a highly specialized, generally rigid mode of expression, which had been greatly restrained by certain demands of exactness due to the influence of philosophical conceptions. Thus, to choose an instance from the subject matter under consideration, it was impossible to speak of a velocity as a ratio between distance and time. … In Euclidean mathematics it is possible only to speak of the ratio between two magnitudes of the same kind, i.e., of two magnitudes,

[4] Dijksterhuis, *The Mechanization of the World Picture*, p. 190.

> each of which is capable, when multiplied, of exceeding the other ….[5]

Following Dijksterhuis, we may take Bradwardine's work on the proportions of velocities in motions as a step of "historical importance" in the search for "a mathematical expression of a … functional relation in nature", even if it involved "a natural law that is not valid".[6] I would like to argue, however, that Bradwardine's "mathematical formulation" was not "unfounded", but had a firm foundation in a theory of proportions and of the compounding of proportions to be found in a pre-Theonine version of Euclid's *Elements* and in the mathematical work of Archimedes and Apollonius as well. While this classical Greek mathematics was, as Dijksterhuis says, "greatly restrained by certain demands of exactness", such as requiring that proportions always involve two magnitudes of the same kind, Thomas Bradwardine built upon it a mathematical theory of the proportions of velocities in motions of an elegance still worthy of our appreciation.

In the last half-century, historians often have not been able to see clearly the virtues of Bradwardine's theory because between his day and ours there occurred a revolution in the understanding of ratio or proportion, as a result of which ratios were identified with indicated divisions or with what, in earlier times, were called their "denominations", "quantities", "exponents", or "sizes" (πηλικότης). Working before the early modern change of conception, Bradwardine, following a classical or pre-Theonine Greek tradition, treated proportions as relations between two quantities and did not apply to them the normal arithmetic operations such as addition, subtraction, multiplication, or division.

COMPOUNDING PROPORTIONS BEFORE BRADWARDINE

In Euclid's *Elements* there are two distinct conceptions of what is called "compounding" proportions, which, to understand the origin and fate of Bradwardine's law, must be kept distinct. Elsewhere I have called these the first and second traditions of compounding. Here I will call the first

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[5] Dijksterhuis, pp. 191–192.

[6] In his paper in this volume, Jean Celeyrette questions the use of the words "law" or "function" in connection with Bradwardine's theory of the proportions of velocities in motions or with its translations into terms of latitudes in the work of John Dumbleton. I think that part of the appeal of Bradwardine's theory was indeed that the mathematical relationship he proposed fit the assumed facts over a wide interval, for example, down to the limit of a proportion of force to resistance of 1:1. I here use the words "rule", "theory", "law", and "function" interchangeably leaving for another occasion debates about their appropriateness or anachronism.

more classical and theoretical tradition the "pre-Theonine tradition" of compounding proportions and the second more practical and calculational tradition the "Theonine tradition".[7]

The first, or pre-Theonine, tradition appears in modern editions of the *Elements* as a not-previously-defined operation found in Book VI, proposition 23, which states:

> Equiangular parallelograms have to one another the ratio compounded of the ratios of their sides.[8]

What is meant here by "compounded of"? What Euclid does to compound two proportions is to take the two proportions of the sides of the parallelograms that he wants to compound, and to equate them to two proportions continuous with each other (i.e., such that the antecedent of one proportion is the same as the consequent of the other). He argues:

> Let a straight line K be set out, and let it be contrived that,
> as BC is to CG, so is K to L,
> and as DC is to CE, so is L to M.
> Then the ratios of K to L and of L to M are the same as the ratios of the sides,
> namely of BC to CG and of DC to CE.

[7] See Sylla, "Compounding Ratios". I am assuming here that Euclid's *Elements*, as it existed before the redaction of Theon (335–405 CE), did not include definition 5 of Book VI. Marshall Clagett refers to this definition as "the so-called Theonine definition 5 of Book VI … repeated in Theon's *Commentary on the Almagest of Ptolemy*" (*Archimedes in the Middle Ages*, vol. II, p. 15). I leave it to specialists in the history of Greek mathematics to determine more certainly the history of the text of the *Elements*, but aside from its transmission through Theon, the definition of compounding in terms of multiplication of the quantities or denominations of proportions was transmitted to the Middle Ages through the commentaries of Eutocius (480–540 CE) on Archimedes. According to Clagett, Witelo (*Perspectiva* I.13) probably picked up the definition from William of Moerbeke's translation of Eutocius's *Commentary on the Sphere and Cylinder of Archimedes*. See Clagett, *Archimedes in the Middle Ages*, vol. II, pp. 13–24. In what follows I trace the history of definitions of compounding proportions mainly through geometry, but, as Clagett shows, a parallel history can be traced through translations of and commentaries on the works of Archimedes and others, as well as through optics and astronomy. As Clagett indicates, and as I suspect, the works *De proportionibus* that Busard published in "Die Traktate *De proportionibus* von Jordanus Nemorarius und Campanus" are probably not by Jordanus and Campanus, but rather by Thabit Ibn Qurra and al-Kindi, to whom they are also attributed in some manuscripts. See Clagett, *Archimedes*, pp. 19–22, esp. note 14.

[8] Euclid, *The Thirteen Books of the Elements*, vol. 2, p. 247. Here "ratio" rather than "proportion" appears because this is Heath's translation and not mine.

> But the ratio of *K* to *M* is compounded of the ratio of *K* to *L* and of that of *L* to *M*; so that *K* has also to *M* the ratio compounded of the ratios of the sides.[9]

If the procedure in proposition 23 of Book VI is taken as the key to the definition of "compounding" proportions, then it is clear that the operation of compounding consists of taking terms in a series and arguing that the proportion of the first to the last is compounded of the proportions between immediate terms, so that, for instance, the proportion of A to E is compounded of the proportion of A to B, of the proportion of B to C, of the proportion of C to D, and of the proportion of D to E. On this understanding, "compounding" needs no demonstration, but is simply obvious to inspection. Compounding continuous proportions is like adding contiguous lines. It is as if the points A, B, C, D, E are marked on a ruler, and the distance from A to E is said to be equal to the distance from A to B, plus the distance from B to C, and so forth up to E.

On this understanding of compounding, proportions are relations between entities of the same sort, and, as relations, are something distinct from numbers or even geometrical magnitudes. In Book V, Euclid had defined a number of operations on proportions, but he had not applied the simple arithmetic operations of addition, subtraction, multiplication, or division to them. He had, however, in definition 17 of Book V, defined an operation called *ex aequali*, which has great affinity to compounding. In the operation *ex aequali*, two paired sets of magnitudes are compounded:

> A ratio *ex aequali* arises when, there being several magnitudes and another set equal to them in multitude which taken two and two are in the same proportion, as the first is to the last among the first magnitudes, so is the first to the last among the second magnitudes.[10]

For this operation *ex aequali* to provide the basis for compounding proportions in the sense that will be used by Bradwardine for the foundation of his law, what is needed is to consider that the terms of the proportions to be compounded should be continuous (as the A, B, C, D, E mentioned above) and monotonically increasing or decreasing. On this understanding the compounding of proportions can be understood as "adding", as in adding shorter segments to make a longer line. This understanding of "adding" proportions could then be exemplified by Euclid's definitions 9 and 10 of Book V:

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[9] *Ibid.*

[10] *Ibid.*, p. 115.

> When three magnitudes are proportional, the first is said to have to the third the *duplicate ratio* [Latin: *proportio duplicata*] of that which it has to the second.
>
> When four magnitudes are <continuously> proportional, the first is said to have to the fourth the *triplicate ratio* [Latin: *proportio triplicata*] of that which it has to the second, and so on continually, whatever be the proportion.[11]

Here, although the Latin translation normally had *duplicata* and *triplicata*, rather than "double" or "triple", some authors understood the meanings as if they were the same, taking the proportion of the first magnitude to the second as half or a third, etc. of the proportion of the first to the last magnitude. Then these halves or thirds could be added together to make the whole proportion of extremes. The simplicity of Bradwardine's function is most apparent when one can say that as the proportion of force to resistance is doubled or tripled, so is the velocity doubled or tripled, and so forth.

In ancient and medieval music theory or harmony, there was a strong tradition of "adding" proportions connected to the adding of musical intervals, as when the musical fifth (corresponding to the proportion 3:2) was added to the octave (corresponding to the proportion 2:1) to produce the interval diapason diapente (corresponding to the proportion 3:1).[12] This "adding" of proportions had appeared already in Boethius's *Fundamentals of Music*. So in Book II, proposition 25 Boethius writes:

> I assert, then, that the diapente rightly consists of the sesquialter, the diatessaron of the sesquitertian ratio … If we subtract the diatessaron from the consonance of the diapente, the interval remains which is called "tone"; if we take a sesquitertian away from a sesquialter ratio, a sesquioctave ratio is left. It follows, then, that the tone ought to be assigned to the sesquioctave ratio.[13]

What Boethius is saying here is that 4:3 subtracted from 3:2 leaves 9:8. He does not say how the mathematics works, but from the diagrams that appear in manuscripts of his work, it appears that he was thinking of an array of the numbers 9 – 8 – 6 in a line with ligatures between the numbers. Then the diapente, in the proportion of 3:2, corresponds to the relation of 9 and 6, the extremes of the line; while the diatesseron, in the proportion 4:3, corresponds to the relations of 8 and 6 at the right of the line, which,

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[11] *Ibid*., p. 114.
[12] See, for example, Marchetto of Padua, *Lucidarium*, p. 177.
[13] Boethius, *Fundamentals of Music*, p. 80.

when taken away from the relation of 9 and 6, leaves the rest of the interval, from 9 to 8.

Similarly in the next proposition, Boethius writes:

> 26. The diapason-plus diapente is in the triple ratio, the bis-diapason in the [quadruple].[14] Since it has been demonstrated that the diapason is duple, the diapente sesquialter, and the duple and the sesquialter joined together create the triple ratio, it is also clear that the diapason-plus-diapente is set down in the triple ratio. If a sesquitertian ratio is joined to a triple relation [i.e., ratio], it makes a quadruple. Therefore if the consonance of the diatessaron is added to the consonance of a diapason-plus-diapente, the quadruple interval of pitches is made, which we have demonstrated above to be the bis-diapason.[15]

Depending on the musical instrument that is taken to be the basis of harmonic theory, the image of adding musical intervals may vary. Later in *Fundamentals of Music* Boethius proposes taking a string and marking it off into seven equal parts. If the string is divided into three parts on one side and four parts on the other, and the two part-strings are plucked, the consonance that will be heard is the diatessaron, corresponding to the proportion 4:3. Similarly the string might be marked off into 5 parts and then divided into three parts on one side and two on the other. When these two part-strings are plucked either simultaneously or one after the other, the tones are in the consonance diapente or 3:2.[16] On a keyboard instrument, it is easy to visualize how the intervals between notes arrayed in a line may be added to make octaves and the like. The two notes always remain two, even when their relation is considered, and the direction of comparison of low note to high or vice versa is irrelevant.

What Bradwardine did in his *On the proportions of velocities in motions* was to take this first "pre-Theonine" tradition of compounding proportions as if it were the only one. The key conclusions in the 16th-century short versions of Bradwardine's work are that

> the velocity of movement follows the geometric proportion of the motor above the power of the thing moved … the meaning … is this: If there are two powers and two resistances and

[14] The text erroneously reads "duple" here.
[15] *Ibid.*, pp. 80–81.
[16] *Ibid.*, p. 160.

> the proportion between the first power and its resistance is greater than the proportion between the second power and its resistance, the first power will be moved more rapidly with its resistance than the second with its, just as one proportion is greater than another ….

According to this opinion, the following conclusions are to be conceded:

> [1] If one power is in a double proportion to its resistance, when the power is doubled (*duplata*) with the resistance remaining the same, the movement will be doubled (*duplabitur*) ….
>
> [2] If any power is in a double proportion to its resistance, that power suffices to move with half that resistance twice as rapidly (*in duplo velocius*) as it moves with the total resistance ….
>
> [3] If one power is in more than double proportion to its resistance, with the power doubled and the resistance remaining the same, the movement will not be doubled….
>
> [4] If one power is in a proportion more than double its resistance, that power does not suffice to move twice as rapidly with half that resistance as with the whole ….
>
> [5] If a power is in less than a double proportion to its resistance, the power doubled will move [the same resistance] more than twice as rapidly….
>
> [6] If a power is in less than a double proportion to its resistance, it will suffice to move more than twice as rapidly with half of the resistance….[17]

Although the sense of Bradwardine's theory of the relation of powers or forces, resistances, and the resulting motions is here broken out into six conclusions, each conclusion appears simple and follows directly from the mathematics of proportions that introduce his work.

The key suppositions and conclusions of the mathematical section (as in the 16th-century abbreviation) are:

> [First supposition:] All proportions are equal whose denominations (*denominationes*) are equal.
>
> The second supposition is this: If there are three proportional terms and the first is greater than the second and the second

[17] Translation modified from that in Clagett, *The Science of Mechanics*, pp. 475–477.

> greater than the third, then the proportion of the first to the last is compounded of (*componitur*) the proportion of the first to the second and of the second to the third.
>
> Third supposition: When there are many terms and the first term is more than the second and the second is more than the third, and similarly with respect to the other terms up to the last proportional term, then the proportion of the first to the last is composed of the proportion of the first to the second and that of the second to the third, and so on for the other terms ….
>
> Sixth [supposition]: When anything is compounded of two equals, that composite will be double (*duplum*) either of the equals; if it is compounded of three equals, it will be triple any of them ….
>
> Seventh supposition: When something is compounded of two unequals, that something is more than double the lesser and less than double the greater ….

There follow certain conclusions:

> The first conclusion to be proved from these suppositions is this: If there is a proportion of greater inequality of the first to the second term and of the second to the third, then the proportion of the first to the third will be a proportion double (*dupla*) the proportion of the first to the second and of the second to the third….
>
> The third conclusion is this: If the first term is more than double the second, and the second term is exactly double the third, the proportion of the first to the third will be less than double the proportion of the first to the second….
>
> The fifth conclusion is this: If the first term is less than double the second and the second is precisely double the third, then the proportion of the first term to the last will be more than double the proportion of the first to the second….
>
> The seventh conclusion: There is no proportion greater or less than a proportion of equality …. Nor is a proportion of greater inequality greater than a proportion of equality….[18]

[18] *Ibid.*, pp. 468–471.

From a comparison of these selected mathematical and physical conclusions, it is obvious that the first and second conclusions about motions are analogous to the first conclusion about mathematical proportions; the third conclusion about motions follows from the third conclusion about proportions, and so forth. In other words, Bradwardine's "dynamic law of movement" follows directly and simply from his mathematics of the comparison and compounding of proportions and not from physics as such.

Thus Bradwardine's *On the proportions of velocities in motions* came as a "package deal". First one learned to accept the mathematics of proportions according to the first "pre-Theonine" tradition and then the mathematics of the relationships of forces, resistances, and velocities was presented as the payoff for having accepted this mathematics. Before Bradwardine, the attempt to translate what Aristotle and Averroes had said about the relations of forces, resistances, and velocities into mathematics had run into several difficulties. A simple mathematical interpretation of Aristotle might say that velocity depends on the proportion of force to resistance on the condition that the force is greater than the resistance, because otherwise there would be no motion. This fit with Aristotle's statements in various passages to the effect that if a certain force moves a certain resistance with a given velocity, then double the force will move the same resistance with double the velocity, or the same force will move half the resistance with double the velocity, and so forth. Mathematically, this understanding has an unacceptable boundary condition, because it implies that there will be a finite velocity as the proportion of force to resistance approaches a proportion of equality. Moreover, if one says that there is no velocity when the force equals or is less than the resistance, then there will be some small velocities corresponding to no proportion of force to resistance (since the simple proportionality would make these small velocities correspond to a force less than the resistance).

The main mathematical alternative to understanding Aristotle to say that velocities are proportional to the proportion of force to resistance was to say that velocities correspond to the difference between force and resistance, or F – R. This formulation had the advantage that it predicts a finite velocity when there is no resistance. Aristotle had used the usual formulation of his view to argue that a vacuum is impossible, since it would imply a proportion of a finite force to zero resistance and hence an infinite velocity, a logical contradiction. By the early 14th century, however, most scholars accepted that God could, by his absolute power, cause a vacuum to exist, and so the mathematics of motion needed to allow for a zero resistance. The formulation F – R accomplished this goal, but it had other serious problems. It implied, for instance, that if two bodies moving with the same velocity were attached together, they would move more quickly, since the differences between the combined forces and combined resistances would add together. Moreover, the formulation velocity proportional to F – R did not "save" Aristotle's statements that when the proportions of force to resistance are the same the velocities will be the same.

The mathematics of the proportions of velocities in motions that Bradwardine proposed provided a new alternative mathematics that matched proportions of force to resistance to all velocities down to zero velocity, thus avoiding the boundary value problems of the standard Aristotelian interpretation, and at the same time it could, with a plausible gloss, save the truth of Aristotle's statements. Moreover, Bradwardine argued for his position by disproving four alternative positions, which left Bradwardine's theory as the only alternative theory left standing.[19]

There were other arguments against the standard Aristotelian interpretation and in favour of Bradwardine's law based on experience. For instance:

> the present [Aristotelian] theory is to be refuted on the ground of falsity, because sense experience teaches us the opposite. We see, indeed, that if, to a single man who is moving some weight which he can scarcely manage with a very slow motion, a second man joins himself, the two together then move it much more than twice as fast. The same principle is quite manifest in the case of a weight suspended from a revolving axle, which it moves insensibly during the course of its own insensible downward movement (as is the case with clocks). If an equal clock weight is added to the first, the whole descends and the axle, or wheel, turns much more than twice as rapidly (as is sufficiently evident to sight).[20]

Bradwardine's fifth conclusion about proportions of velocities, quoted above, explains why, as the power of moving increases from being only slightly greater than the resistance of the mobile, the velocity more than doubles when the power is doubled. But Bradwardine's strategy in arguing for his view was more mathematical than physical. In setting out the mathematical preliminaries to his theory, he systematically expounded one view of proportions and their compounding, while failing to mention alternative approaches. And once he turned to physics, he did not really give the

[19] For Bradwardine's arguments against an "Aristotelian" simple proportionality between forces, resistances, and velocities, see Bradwardine, *Tractatus de proportionibus*, pp. 94–105.
[20] *Ibid.*, p. 99.

Aristotelian view as traditionally understood – that velocities are proportional to forces and inversely proportional to resistances – a full and fair hearing because he implicitly assumed that proportions can only vary as they do in his own approach. This meant that the traditional Aristotelian view had to be divided into two parts, saying velocity varies as force if the resistance is held constant and inversely as resistance if the force is held constant. Having made this division of Aristotle's theory into parts to maintain the pretence that proportions can vary only in the way he proposed, Bradwardine then held it against this theory that it was insufficient, having nothing to say if the forces and the resistances varied at the same time![21] Similarly he argued against the theory that velocities are proportional to (F – R):R on the grounds that the theory could not calculate any velocity larger or smaller than the velocity that occurs when the force is double the resistance, because then (F – R):R is the proportion of R:R or 1:1, and there is no proportion greater or less than a proportion of equality (his seventh conclusion about proportions, quoted above).[22]

Immediately after the appearance of his *On the proportions of velocities in motions* in 1328, Bradwardine's new rule for the relationship of forces, resistances, and velocities in motions was widely and almost unanimously accepted. I believe that this happened because of the effectiveness of the mathematical foundation that he provided for it, together with the striking advance of his theory over the previously considered alternatives. Top thinkers at Paris, as well as Oxford, began to build upon Bradwardine's foundation. At Paris both Nicole Oresme and Albert of Saxony imitated Bradwardine by writing books *De proportionibus* of their own, books which were obviously based on Bradwardine's and included discussions of his dynamical law or rule. At Oxford, Roger Swineshead, John Dumbleton, and Richard Swineshead also included discussions of Bradwardine's dynamics in their works although they did not imitate his title *De proportionibus.*[23] As late as 1509 Alvarus Thomas of Lisbon, working at Paris, published the huge *Liber de triplici motu proportionibus annexis … philosophicas Suiseth calculationes ex parte declarans*, expounding in detail the mathematics and the science of motion that had been founded by Bradwardine and advanced by Richard Swineshead. Two hundred years after Bradwardine's work, Jean Fernel implicitly defended it in his 1528 *De proportionibus libri duo*.

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21 *Ibid.*, pp. 95–99. In refuting the second erroneous position, Bradwardine likewise assumed his own theory of proportions, including the view that no proportion is greater or less than a proportion of equality.

22 Bradwardine, *Tractatus de proportionibus*, pp. 80–81, pp. 92–95.

23 Richard Kilvington may also belong here, unless he wrote his questions on the *Physics* before Bradwardine's *De proportionibus*.

Those who opposed Bradwardine, for instance Blasius of Parma, did so largely for reasons of mathematics rather than physics. The most common complaint was that when a proportion is duplicated (*duplicata*), it is not necessarily doubled, because a "double" is always bigger than its half, whereas a proportion may become smaller by being duplicated (if it is a proportion of lesser inequality) or stay the same size (if it is a proportion of equality). By the later 16th century, instead of writing works about the proportions of velocities in motions as such, mathematicians wrote against the interpretation of compounding proportions that Bradwardine had made the foundation for his work.[24] In the 16th and 17th centuries, indeed, a decision was made in favour of a second, alternative, Theonine approach to the compounding of proportions, which removed the mathematical foundation of Bradwardine's theory. The theory was, therefore, destined to vanish along with the training *De proportionibus* of the *Libelli sophistarum*, irrespective of the discovery of the law of inertia or of other developments in physics that overthrew Aristotelian correlations of speeds with the powers that caused and resisted them. To understand the origins and fate of Bradwardine's law, therefore, it is important to place it within the history of mathematics as well as physics.

THE HISTORY OF THE MATHEMATICS OF COMPOUNDING PROPORTIONS BEFORE BRADWARDINE

Bradwardine did not invent his approach to the compounding of proportions, but selected it from among previously existing traditions. In recent years much important work has been done on the history of changes in the mathematics of ratios or proportions before Eudoxus.[25] Like an archeological site, the soil of Euclid's *Elements* has been sifted to detect remains laid down at earlier historical periods.[26] According to one narrative, Greek mathematicians first had a mathematics of ratios or proportions based on integers.

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[24] In the unpublished fifth day to his *Discorsi*, Galileo discussed the compounding of ratios. See Giusti, *Euclides Reformatus*. Napolitani, *Sull'Opuscolo de proportione composita*.

[25] There have been many contributors to this work including especially D. H. Fowler and W. Knorr. For an interesting philosophical perspective (although not without faults) see Rusnock and Thagard, "Strategies for Conceptual Change".

[26] See, for example, Knorr, *The Evolution of the Euclidean Elements*. As is well known, there are many versions of Euclid's *Elements*, already in Greek and then again in Arabic and Latin. Scholars have disagreed which version of the *Elements* may represent the original Euclidean text and which its edition by Theon. In one of his last articles to be published, Knorr, "The Wrong Text of Euclid", argued that Heiberg had chosen the wrong text and that the Arabic versions are closer to the original than Heiberg's Greek. See also Saito, "Compounded Ratio in Euclid and Apollonius". For my purposes here it is mainly relevant

Once mathematicians became convinced, however, that some magnitudes are incommensurable to others or cannot both be measured by integral numbers of the same units however small, they developed a theory of ratios or proportions that would work for all magnitudes whether rational or irrational, commensurable or incommensurable. The best known example of such a theory is that of Eudoxus, which is thought to be found in Book V of the *Elements*, but another possible example is the extension of the anthyphairetic approach to cases in which the repeated subtractions of one magnitude from the other never end.[27] In the version of the *Elements* in Greek currently best known, that edited by Theon of Alexandria in the 4th century AD, there are inconsistencies, including definitions and theorems, that do not seem to fit with the tenor of the rest of the book. Bits and pieces of a theory of ratios or proportions that works only for numbers are found in parts of the work (notably Books V and VI) that are supposed to apply to geometrical magnitudes (to magnitudes in general as well as numbers in Book V, and to geometrical magnitudes alone in Book VI). In particular definition 5 of Book VI assumes that the quantity of a ratio or proportion can be expressed as an integer or at least as what we would call a fraction or rational number, whereas Book VI is generally agreed to apply the general theory of ratios or proportions developed in Book V to geometric magnitudes including those that are irrational. Already in the 18th century, Robert Simpson had made a convincing argument that the definition of compounding ratios or proportions in the *Elements* Book VI, definition 5, was not authentic – he ascribed it to Theon and called it "absurd and ungeometrical".[28]

This infamous definition 5 of Book VI, interpolated in "Theonine" versions of Euclid's *Elements*, so scorned by Robert Simpson, states, in Heath's translation:

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to know which Latin version of the *Elements* may have been familiar to Bradwardine and to the other medieval scholars to be discussed. The evidence is that Bradwardine and Oresme, at least, read the *Elements* in the Latin version of Campanus of Novarra and along with his commentary.

[27] See, for example, Fowler, *The Mathematics of Plato's Academy*.

[28] Simpson, in Euclid, *The Elements of Euclid*, p. 236. Heath excludes the definition from his translation and remarks in his notes: "it is beyond doubt that this definition of ratio is interpolated. It has little MS authority. The best MS (P) only has it in the margin; it is omitted altogether in Campanus' translation from the Arabic .... There is no reference to the definition in the place where compound ratio is mentioned for the first time (VI.23), nor anywhere else in Euclid; neither is it ever referred to by the other great geometers, Archimedes, Apollonius and the rest. It appears only twice mentioned at all (1) in the passage of Eutocius referred to above ... and (2) by Theon in his commentary on Ptolemy's *Syntaxis*" (Euclid, *The Thirteen Books of the Elements*, vol. 2, pp. 189–190). See above, note 7.

> A ratio is said to be compounded of ratios when the sizes of the ratios multiplied together make some (ratio, or size).[29]

This definition was present in many early manuscripts of the *Elements*, and its presence shaped much subsequent thinking about the compounding of proportions, most usually with the Latin "*denominatio*" taking the place of Heath's "size" – or, in some cases with the word "quantity", or (in the case of John Wallis) "exponent", translating the Greek "*πηλικότης*". Basically, in modern terms, the "denomination" of a proportion is simply the number or fraction that expresses its quantity. The denomination of the proportion of 2 to 1 is 2. The denomination of the proportion of 3 to 2 is 1½.[30] On the Theonine understanding, proportions are compounded when their denominations are multiplied together or, if there is some problem about this, one may multiply the numerators together to form the numerator of the denomination of the compounded proportion, and multiply the denominators together to form the denominator of the denomination.

While translations of the *Elements* from the Greek into other languages mostly include Book VI, definition 5, one of the two main Arabic translations, that of Ḥajjāj, did not include it.[31] The Latin translations of the *Elements* from the Arabic belonging to the "Adelard" family, including the very popular version of Campanus, are mainly based on the "Ḥajjāj" family of Arabic manuscripts and likewise omit the definition, although the Latin translation of Gerard of Cremona, based on the "Isḥāq-Thābit" Arabic translation, includes it as an addition made by Thābit.[32] As indicated above, some scholars have argued that the version of the Greek *Elements* used by the Arabic translators contained a text earlier and therefore better than the Theonine version.[33] I believe that the version of the *Elements* without the interpolated Book VI, definition 5 better represents the Eudoxan approach to compounding ratios or proportions, one that applies to all continuous magnitudes and not just to those that can be represented in numbers as the Greeks understood them.[34]

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29 Euclid, *The Thirteen Books of the Elements*, vol. 2, p. 189.

30 In music theory, if the proportion corresponding to an octave is 2 to 1, there is no special reason why one should privilege one of the extremes: an octave is an octave whether one thinks of it as the proportion of 2 to 1 or as the proportion of 1 to 2. From this perspective it might be said that the denomination of the proportion of 1 to 2 is 2, or it might be said that it is ½.

31 For medieval and Renaissance translations of Euclid, see Murdoch, "Euclid: Transmission of the Elements".

32 Busard (ed.), *The First Latin Translation*, col. 137; for the text, see below, note 44.

33 Martin Klamroth, "Über den arabischen Euklid"; Knorr, "On Heiberg's Euclid".

34 See Sylla, "Compounding Ratios", p. 22.

By the 14th century in Europe, many versions of Euclid's *Elements* were available in Latin, some with and some without Book VI, definition 5. In his *De proportionibus velocitatum in motibus*, however, Thomas Bradwardine worked entirely within the Campanus version of the *Elements*, the version without Book VI, definition 5. Working within this tradition is what allowed him to put forth an understanding of compounding ratios or proportions that avoided the arithmetization involved in Book VI, definition 5, and therefore made his rule for the relations of proportions of force to resistance and velocities seem supremely natural and simple.

Thus Bradwardine established the mathematical foundations for his theory of the proportions of velocities in motions on a pure "pre-Theonine Euclid" without the spurious definition 5 of Book VI. Did Bradwardine merely take up the tradition of compounding he was familiar with to establish the basis for his theory of the proportions of velocities in motions? Although the edition of Euclid that Bradwardine used was that of Campanus, which did not include the spurious definition 5 of Book VI, it is hard to believe that Bradwardine would never have come in contact with it or with some mathematical work derived from it. My sense is that Bradwardine may have avoided mentioning compounding by multiplying denominations for strategic purposes. In Bradwardine's *Geometria speculativa* there is no definition analogous to the spurious Book VI, definition 5, but the Latin term "*denominatio*", corresponding to the Greek word "πηλικότης", translated "size" in the spurious definition, is defined. Instead of compounding by multiplication of denominations, however, Bradwardine has this conclusion:

> The ratio of the extremes is compounded of the ratios of the means.[35]

He explains this conclusion as follows:

> I say that the ratio of A to C is compounded of the ratio of the mean or means taken between A and C. For let B be a mean between A and C, either according to continuous proportionality and similar ratios or according to dissimilar and unequal ratios. It is established that A to C is as much as B to C and as much more as A exceeds B. Therefore A exceeds C according to the ratio of the two received excesses. Therefore that excess contains the other two excesses, wherefore … the ratio contains the ratios, and I call this a ratio being composed

[35] "Proportio extremorum ex mediorum proportionibus est composita" (Bradwardine, *Geometria speculativa*, p. 97).

> of ratios. It is similar also if there are many means, for then the ratio of the extremes is composed of the ratios of the means between themselves and to the extremes. And it is manifest that every ratio can be resolved into ratios in many ways. An example about the double ratio, for it can be resolved into two similar ratios, and they are irrational. It can also be resolved into two rational but not similar ratios; for example, sesquialter and sesquitertia, for 4 thus exceeds 2, namely according to sesquialter, which is 3 to 2, and according to sesquitertia, which is 4 to 3. But if you take double according to 6 and 3, you will find more means and more ratios, and so always by ascending to greater numbers.[36]

If compounding is done using means and extremes, then one could well push aside or ignore the interpolated definition of compounding in terms of multiplication.

Outside of an academic context, in works of commercial arithmetic, multiplication was often used to solve problems involving proportionalities. In works with titles such as *Liber abaci*, the student was taught not only to handle proportions using the rule of three, but also to calculate absolute amounts. This is understandable, because in buying and selling it is necessary to know the price to be paid in money terms and not only that one item is worth twice as much as another. In this applied tradition, one might in effect compound proportions by multiplying their "sizes" or "denominations", in line with the often-mentioned definition 5 of Book VI. If, for instance, one wanted to compound 5:6 with 3:4, one simply multiplied the fractions 5/6 and 3/4 to get 15/24 or, in lowest terms, 5/8. In this second tradition, compounding might come to be understood *as multiplication*, and then the double of the proportion 3:1 might be understood to be 2 times 3/1 or 6/1, rather than 9:1 as in the first tradition.

Moreover, in the various Latin translations of Euclid and in other related works, this alternative approach often crept into even theoretical works. In the translation of Euclid's *Elements* which Busard and Folkerts call "Robert of Chester's (?) Redaction of Euclid's Elements, the so-called Adelard II Version", the spurious definition 5 of Book VI does not appear, but in the proof of Proposition VI.24 (corresponding to VI.23 in Heiberg), the "Robert of Chester" Latin version of the *Elements* uses the words "*producitur*", "*ex ductu*" and "*multiplices*" where one normally expects "*componitur*" or "is compounded".[37] Earlier I suggested that the pre-Theonine conception of

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[36] *Ibid.*

[37] Busard and Folkerts (eds), *Robert of Chester's (?) Redaction of Euclid's* Elements.

compounding proportions might be seen in the proof of this proposition.[38] In the "Robert of Chester" version, despite having the strategy of selecting the lines K, L, and M in the given proportions, the rule that demonstrates the conclusion is stated:

> Do it therefore using this rule: of all three quantities, the proportion of the first to the third arises from the product of the proportion of the first to the second times the proportion of the second to the third. For example, let there be 1, 2, and 3. How much is 1 of 2? Half. How much is 2 of 3? Two of three parts, which, if you multiply it times a half, a third part necessarily emerges, namely the proportion that 1 has to 3. And from Book VI, Proposition 1, and from Book V, Proposition 11, and from Book V, Proposition 22, derive your result.[39]

Thus, in this version K:M equals K:L multiplied by L:M, although the operation of multiplying proportions has not been defined. The three propositions cited in justification are, in Heath's translation:

> VI.1. Triangles and parallelograms that are under the same height are to one another as their bases.
>
> V.11. Ratios that are the same with the same ratio are also the same with one another.
>
> V. 22. If there be any number of magnitudes whatever, and others equal to them in multitude, which taken two and two

[38] Recall that the proposition, in Heath's translation, states: "Equiangular parallelograms have to one another the ratio compounded of the ratios of their sides" (Euclid, *The Thirteen Books of the Elements*, p. 247). In Robert of Chester, the Latin reads: "Omnium duarum superficierum equidistancium laterum, quarum unus angulus unius uni angulo alterius equalis, proporcio alterius ad alteram est que producitur ex duabus proporcionibus suorum laterum duos angulos continencium" (Busard and Folkerts (eds), *Robert of Chester's (?) Redaction of Euclid's* Elements, p. 181). In Adelard I, the proposition is simpler: "Superficies laterum equidistantium quarum anguli unius angulus alterius equales, erit proportio superficiei unius ad superficiem alteram ea que facta est de proportione laterum earum" (Busard (ed.), *The First Latin Translation*, p. 187).

[39] "Age ergo ex hac regula: Omnium trium quantitatum est proporcio prime ad terciam proveniens ex ductu proporcionis prime ad secundam in proporcionem secunde ad tertiam. Verbi gracia. Sint I, II, III. Unus ergo duorum quantum est? Dimidium. Duo vero trium quantum? Due tercie partes. Quas si per medium [*or* dimidium] multiplices, necesse est terciam partem provenire, eam scilicet proporcionem, quam habebat I ad tres. Atque ex prima sexti et XI[a] quinti et ex XXII[a] quinti argumentum elice" (Busard and Folkerts (eds), *Robert of Chester's (?) Redaction of Euclid's* Elements, pp. 181–182).

> together are in the same ratio, they will also be in the same ratio *ex aequali*.

Thus one might say that in the "Robert of Chester" version of Euclid, the two approaches to compounding proportions co-exist in a rather inconsistent way, in the sense that proportions are said to be multiplied, although the justification for this is the *ex aequali* theorem, which makes no mention of multiplication.

In fact, in the Campanus translation of Euclid, at least as published by Ratdolt in 1482, the end of VI.24 reads

> and because *f* to *h* is produced from *f* to *g* and *g* to *h*, as has been said in the end of the exposition of Book V, definition 11, it will follow that *ac* to *de* is produced from the same; whence the proposition follows.[40]

So Campanus, like "Robert of Chester", here seems to understand compounding as meaning the same thing as taking a product, and he relates it to Book V, definition 11. In his exposition of Book V, definition 11, which states that if there are four proportional quantities, the proportion of the first to the fourth is said to be the proportion of the first to the second *triplicata*, Campanus had made use of the concept of denomination and had commented that there can be no more than four terms in a proportionality because there are only three dimensions.[41] In his commentary on definition 10, which states that if there are three proportional quantities, the proportion of the first to the third is said to be the proportion of the first to the second *duplicata*, Campanus had likewise explained *duplicata* as meaning multiplied times itself (*hoc est in se multiplicata*).[42]

In the version of Euclid translated by Gerard of Cremona, there are several very interesting features. In Book V, in definitions 10 and 11, *duplicata* and *triplicata* are explained with the phrase "*cum iteratione*". After definition 18 of the relation *ex aequali*, or in Gerard's terms *equalitas*, there is an

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[40] "… et quia *f* ad *h* producitur ex *f* ad *g* et *g* ad *h*, ut dictum est in fine expositionis 11 diffinitionis quinti, erit ut *ac* ad *de* producitur ex eisdem; quare constat propositum".

[41] Quoted by Grant in Oresme, *De proportionibus proportionum*, p. 327.

[42] Similar definitions appear in Book VII of Campanus's translation. In the Ratdolt edition, the statements about three and four terms are generalized to any number: "Cum fuerint quotlibet numeri continue proportionales dicetur proportio primi ad tertium sicut primi secundum duplicata, ad quartum vero triplicata. Cum continuate fuerit eedem vel diverse proportiones dicetur proportio primi ad ultimum ex omnibus composita". This is followed by a definition of "*denominatio*".

interpolation indicating that Thabit Ibn Qurra reports another reading.[43] Then in Book VI, after definition 2, there is another interpolation reporting the "aggregation" of proportions by multiplication and their separation (?) by division.[44] So here, in the Gerard translation from the Arabic, the alternative approach to compounding by multiplication appears as one found by Thabit in another manuscript. Then in Book VI, proposition 23, where compounding is used, the seemingly inappropriate word "*duplicata*" is used, where other translations had "*multiplicata*" and the whole proposition seems garbled.[45]

Although I lack space here to go through other versions of Euclid available before Bradwardine, it will be worthwhile look briefly at Jordanus de Nemore's *De elementis arithmetice artis*, a work also used by Bradwardine. Book V of *De elementis arithmetice artis* begins with the definitions:

> Every proportion is said to add something over another proportion, which, continued with it, compounds something.
>
> That proportion is called the difference of one proportion with respect to another, which is that by which the one is said to surpass the other.[46]

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[43] "*Dixit Thebit*: in alia scriptura inveni proportio que nominatur equalitas ..." (Busard (ed.), *The First Latin Translation*, col. 118).

[44] "*Dixit Thebit*: In hoc loco inveni in alia scriptura: Dicitur quod proportio ex proportionibus aggregatur, quando ex multiplicatione quantitatis proportionum, cum multiplicantur in seipsas, provenit proportio aliqua. Dicitur quod proportio dividitur in proportiones cum ex divisione proportionum, quando alie per alias dividuntur, provenit proportio quelibet" (*ibid.*, col. 137).

[45] In the proof of an example, the text reads: "Ergo proportio composita ex proportione *bg* ad *gh* et ex proportione *dg* ad *ge* est sicut proportio *k* ad *m*. Ergo proportio *bg* ad *gh* duplicata cum proportione *dg* ad *ge* est sicut proportio *k* ad *l* duplicata cum proportione *l* ad *m*. Sed proportio *k* ad *l* duplicata cum proportione *l* ad *m* est proportio *k* ad *m*. Ergo proportio *k* ad *m* est proportio composita ex proportione *bg* ad *gh* et ex proportione *dg* ad *ge*" (*ibid.*, cols. 155–156). And the text goes on and on using "duplicata" as if it meant something related to but not necessarily the same as "multiplicata" or "composita". In Adelard I, at the crucial points of the proof the inappropriate word "duplicata" appears where one would expect "compounded with" or possibly "multiplied by", for example: "Proportioque *bg* ad *gh* duplicata cum proportione *dg* and *gh*, sicut etiam proportio *k* ad *l* duplicata cum proportione *l* ad *m*" (*The First Latin Translation*, p. 187). In one manuscript instead of "duplicata" there is the word "mutecene" and in another manuscript "repetita". All this indicates that the copyists of Adelard I recognized a problem in the text.

[46] "Omnis proportio super aliam quamlibet addere dicitur proportionem que cum illa continuata ipsam componit. Differentia proportionis ad aliam vocatur illa proportio qua eadem super reliquam habundare dicitur" (Jordanus of Nemore, *De elementis arithmetice artis*, vol. 1, p. 107). In his summary and introduction, Busard says (p. 18), "Book V of the work treats of multiplying and dividing of ratios. In the treatise *Liber de proportione et*

So here we have yet another word used in connection with compounding, namely *continuata* or "continued". This much sounds like the pre-Theonine tradition of compounding proportions.

Jordanus's first proposition of Book V continues in this vein, speaking of what one proportion adds over another, but in a rather enigmatic way, saying that what the proportion A:B adds beyond the proportion C:D is the proportion AD:BC, or A:B = C:D + AD:BC, in this sense of addition.[47] What is going on here? Why is this true? In a way that should be familiar to us, Jordanus defines three new terms, E, F, G, where it is obvious from inspection that the proportion E:G is compounded of the proportions E:F and F:G. He posits, by definition, that E = DA, that F = BC, and that G = DB. Then F:G = C:D, by the rule that if the antecedent and the consequent of a proportion are multiplied by the same quantity (in this case B), the proportion remains unchanged.[48] For the same reason E:G = A:B (in this case the common multiple being D). E is larger than F by proposition II.9.[49] Since the proportion A:B is greater than the proportion C:D (by the assumption of the proposition that A:B adds something to C:D), it follows that E is greater than F. So since the proportion E:G is compounded of the

proportionalitate in two manuscripts attributed to al-Kindi and in two other manuscripts to Campanus we find the following definitions: 3. Now a ratio is 'produced' or 'compounded' out of ratios (when its) denomination is produced out of (their) denominations (i.e., when its denomination is equal to the product of their denominations). 4. A ratio is 'divided' by a ratio, or (to put it another way) a dividing (ratio) is 'removed' from a (ratio) to be divided (when) the denomination of the (ratio) to be divided is divided by the denomination of the dividing (ratio)." For the *Liber de proportione et proportionalitate*, Busard refers to Busard, "Die Traktate *De proportionibus* von Jordanus Nemorarius und Campanus", p. 213. But if the *De proportionibus* edited by Busard attributed to Campanus is by al-Kindi, and the *De proportionibus* ascribed in some manuscripts to Jordanus is by Thabit ibn Qurra, it is not necessary to use a Theonine definition of compounding by multiplication to interpret Jordanus's *De elementis arithmetice artis*. See above, note 7.

[47] "Quod addit proportio primi ad secundum super proportionem tertii ad quartum est proportio que est inter productum ex primo in quartum et productum ex secundo in tertium. Verbi gratia: Sit *a* primus, *b* secundus, *c* tertius, *d* quartus. Atque ex *d* in *a* fiat *e* et ex *b* in *c* fiat *f*. Ducatur autem *d* in *b* et proveniat *g*. Eritque *f* ad *g* sicut *c* ad *d*. Itemque *e* ad *g* sicut *a* ad *b* per viii*am* secundi. Constat autem per nonam secundi quod *e* est maior *f*. Et quia proportio *e* ad *g* constat [*sic*, but the meaning is that it is compounded] ex proportione *e* ad *f* et *f* ad *g*, manifestum est quod dicitur" (Jordanus of Nemore, *De elementis arithmetice artis*, p. 107).

[48] "Si duo numeri unum multiplicent, erit que multiplicantium eadem productorum proportio" (*ibid.*, Book II, proposition 8).

[49] Which states "Si duo numeri ad tertium comparentur, maioris ad ipsum erit proportio maior, ipsius vero ad maiorem proportio minor" (E and F are compared to G in the two proportionalities just established).

proportion E:F and of the proportion F:G, it follows *ex aequali* that the proportion A:B is compounded of the proportion DA:BC and the proportion BC:DB or, reducing the latter proportion to lowest terms, of the proportion DA:BC and the proportion C:D. Q.E.D.

If Jordanus had been thinking of the "adding" of proportions simply as the multiplying of their denominations, he might simply have inspected the proportions A:B and C:D and reasoned that what should be multiplied times C:D to produce A:B must be AD:BC. That instead he goes about defining new terms E, F, G where the compounding of continuous proportions is obvious by inspection shows that he is mainly working in the pre-Theonine tradition.

Another important part of Bradwardine's context is Richard of Wallingford's *Quadripartitum*, written before 1335 and so about the time of Bradwardine's *De proportionibus*, a work that continues this odd co-existence of two approaches to compounding. He begins Part II of the *Quadripartitum*, with definitions that seem to go back to earlier, possibly Arabic, works *De proportionibus*, having a connection to the interpolated definition 5 of Book VI concerning the denomination or size of a proportion:

1. A *ratio* is a mutual relation between two quantities of the same kind.
2. When one of two quantities of the same kind divides the other, what results from the division is called the *denomination* of the ratio of the dividend to the divisor.
3. A ratio [is said to be] *compounded* of ratios when the product of the denominations gives rise to some denomination.
4. A ratio [is said to be] *divided* by a ratio when the quotient of the denominations gives rise to some denomination.[50]

Most of these definitions come from earlier works,[51] where one finds among the definitions or suppositions something very like the infamous Book VI, definition 5 of Euclid.[52] The purpose of these works is to prove the theorem

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[50] Richard of Wallingford, *Writings*, vol. 1, p, 59. Here, with his talk of division and quotients, etc., Richard is taking the second, Theonine tradition of compounding ratios by multiplying beyond what is found in Jordanus de Nemore.

[51] For instance, the works published in Busard, "Die Traktate *De proportionibus*". From the content of these works, it appears that they are *not by* Campanus and perhaps not by Jordanus Nemorarius, given that they include something like Book VI, definition 5. See above, note 7. Biard and Rommevaux in Blaise de Parme, *Questiones circa tractatum proportionum*, p. 25, n. 1, however, provide additional relevant information.

[52] "Proportionem produci vel componi ex proporcionibus est denominationem proporcionis produci ex denominationibus proportionum altera in alteram ductis" (Busard, "Die Traktate *De proportionibus*", p. 205, in the version ascribed to Jordanus). And on p. 213, in the version ascribed to Campanus, which omits the last four words.

of Menelaus or the transversal theorem, following the precedent of Ametus Filius Josephi and Thabit Ibn Qurra. A similar work is also ascribed to al-Kindi, where it is said to concern the sector figure.[53] As John North explains it, what is at issue is to show the 18 acceptable ways of permuting the terms in a relation:

$$1 = \frac{b}{a} \cdot \frac{c}{d} \cdot \frac{e}{f} \tag{1}$$

If this is understood as equivalent to the multiplication of fractions and if the rules for such operations and commutativity are accepted as known, then the entire procedure seems to be trivial and the point of deriving the 17 other expressions from the first is otiose. It follows from this consideration that the theorem is understood to apply to the compounding of proportions, understood as something quite different from fractions, and that denominations of proportions are used only by way of proof. Basically what one does is (a) to go from proportions to simple numbers; (b) to multiply the numbers; and then (c) to take the proportions of the resulting products.

Thus the theorems that follow Richard of Wallingford's definitions given above begin:

> II.1. If the denomination of a ratio of any two extrema be multiplied by [the second] extremum, the other extremum results.[54]

Since the denomination is produced by dividing the antecedent or numerator by the consequent or denominator, it is clear that multiplying the denomination by the consequent will produce the antecedent again.

The second theorem is then:

> Suppose there to be two extrema (to each of which a mean is in some ratio). Then the ratio of the first to the third will be compounded of [the ratios of] the first to the second and the second to the third.[55]

On the pre-Theonine theory of proportions this theorem is obvious (Euclid used it in VI.23 without explanation), but Richard, and the sources on which his work is based, take it upon themselves to prove the theorem, first of all for three homogeneous magnitudes A, B, C. Let (the denomination) of A:B = D, let B:C = E, and let A:C = F. Then by II.1, E·C = B; D·B = A; and F·C = A.

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[53] Richard of Wallingford, *Writings*, vol. 2, pp. 53–54.
[54] *Ibid.*, vol. 1, p. 59.
[55] *Ibid.*, vol. 1, p. 61.

Then F:E :: A:B (because A:B = FC:EC = F:E) and since D is the denomination of A:B it is also the denomination of F:E. But since D is the denomination of F:E, it follows that D·E = F. But this last expression states that the denomination of A:C is the product of the denomination of A:B times the denomination of B:C. Therefore by the third definition the proportion A:C is compounded from the proportion of A:B and the proportion of B:C.

Thus, by converting to denominations, multiplying, and converting back to proportions, the rule for compounding two continuous proportions is proved without ever multiplying proportions as such. On this basis Richard can go on to derive the acceptable modes of compounding from an original mode, starting with

> II.5. When, of six quantities, the ratio of the first to second is compounded of the ratios of third to fourth and of fifth to sixth, there are 360 species of compound, of which 36 are of use, but the remaining 324 are of no use.[56]

Here, by "of no use" Richard means that they do not follow from the first relationship.

I have gone through what these earlier medieval works have to say on the compounding of proportions to show how the pre-Theonine Euclidean theoretical tradition and a second more calculational tradition related to the questionable Book VI, definition 5 co-existed and sometimes intermingled before Bradwardine's *De proportionibus* of 1328. It appears, then, that Bradwardine's exclusion of the Theonine compounding of proportions by multiplication of denominations was a conscious strategy in *On the ratios of velocities in motions* and not inadvertent.[57]

THE INTRODUCTION OF BRADWARDINE'S THEORY OF THE PROPORTIONS OF MOTIONS

Whether or not Bradwardine was familiar with the method of compounding proportions by multiplying their denominations, he chose in his *De proportionibus* not to mention it and to ignore any implications it might have for

[56] *Ibid.*, *Writings*, vol. 1, p. 63.

[57] Of course he uses the notion of "denomination" for the size of proportions. It is perhaps also worth noting that Bradwardine sometimes uses the word "*producitur*" to refer to compounding. For instance, in talking about three terms he says the proportion of the extremes is composed (*componitur*) of the proportion of one extreme to the middle term and of the middle term to the other extreme, but when he comes to two or more middle terms he says "proportio primi ad extremum productiur ex proportione primi …" (Bradwardine, *Tractatus de proportionibus*, p. 76).

the nature of compounding. Relying only on the pre-Theonine approach to compounding, he subtracted one proportion from another by finding a mean between extremes and he added proportions by finding three terms A, B, C, such that the first proportion was the same as A:B and the second the same as B:C.

Indeed, the mathematics of proportions found in Euclid's *Elements*, Books V and VI, provided a ready-made tool for significantly raising the level of mathematics applied to earthly motion. What Bradwardine had to do in *De proportionibus* was to convince his readers of the gains to be had from following consistently the pre-Theonine theoretical approach. One advantage was that his rule apparently gave a measure of velocity *tanquam penes causam* at an instant. In the medieval context, velocity could not easily be measured by the proportion of the distance travelled to the time taken – for medieval authors there was no proportion of distance to time, because proportions exist only between homogeneous quantities, whereas distance is of a quite different sort than time. This meant that they had to hold either distance or time fixed: they could say that (constant) velocities are proportional to distances traversed in the same time, or that velocities are inversely proportional to the time taken to traverse a given distance. Bradwardine could, in contrast, propose to measure instantaneous velocity (*tanquam penes causam*) by the proportion of force acting to resistance, something that was possible because he took force and resistance to be homogeneous quantities (in natural fall, for instance, the gravity of earth in a mixed body could be the force causing the motion, while the levity of air in the body could be the resistance). In ways like this, Bradwardine's dynamics could to be useful in a theoretical context.

In natural philosophy or scholastic mechanics this limitation to the pre-Theonine tradition of compounding proportions could easily seem theoretically justified. For one thing, the pre-Theonine tradition of compounding proportions applies to all magnitudes, whether commensurable or incommensurable, whereas the interpolated definition 5 of Book VI of the *Elements* assumes that every ratio has a size or denomination expressible in numbers or fractions. Since in mechanics the relevant quantities might have no common measure, it would be proper not to assume that multiplication by denominations was always possible. Hence avoiding use of definition 5 of Book VI made sense.

Bradwardine's exclusively pre-Theonine approach also had, for better or worse, additional implications. It followed, for instance, that there is no comparison between proportions of greater inequality, proportions of equality, and proportions of lesser inequality. As the abbreviation of Bradwardine's work quoted in part above put it,

> There is no proportion greater or less than a proportion of equality because [one] proportion of equality is neither more nor less than [another] proportion of equality, all proportions of equality being equal and hence no one being greater than another. Nor is a proportion of greater inequality greater than a proportion of equality, because if so, then a proportion of equality could be increased until it was equal to it (i.e., to a proportion of greater inequality). The consequent is false because however much a proportion of equality is increased, it will always remain a proportion of equality. Consequently, it will be neither more nor less than it is now.[58]

Some scholars might resist this assertion, feeling commonsensically that a proportion, for instance, of 2:1 is larger than a proportion of 1:1. If students were to be taught to use Bradwardine's function, they had, first, to be taught its mathematical presuppositions, particularly insofar as these were unfamiliar. They had to develop the habit of compounding proportions by finding a series of monotonically decreasing or increasing terms, in such a way that the proportion of the extremes would naturally be understood to be larger than the proportions between the intermediate terms. The payoff is that, if the proportions F:R are understood in this way, then to say that velocities are proportional to proportions F:R is no more complicated than saying that velocities are proportional to distances traversed in a given time: when one is doubled, then the other is doubled; when one is tripled then the other is tripled; when one is halved then the other is halved; and so forth in both cases.

John Dumbleton thus expressed Bradwardine's rule by saying that:

> The third opinion of Aristotle and the Commentator, which is to be held, is that motion follows a geometric proportion and is increased or decreased accordingly as the proportion is greater or less in comparison to the previous proportion. Thus one motion exceeds another accordingly as one proportion is

[58] Clagett, *The Science of Mechanics*, p. 471, n. 57. In two places I have deleted Clagett's additions of "[geometrically]", in the belief that they are not necessary if one understands the framework of the first tradition on proportions. Latin (p. 486): "Quod proportione equalitatis nulla est proportio maior vel minor, qua proportio equalitatis non est maior vel minor proportione equalitatis, quia omnes proportiones equalitatis sunt equales, ergo nulla est alia maior. Nec proportio maioris inequalitatis est maior proportione equalitatis; quia si sic, ergo proportio equalitatis potest augeri quousque fuerit equalis isti. Consequens est falsum, quia quantumcunque augetur semper erit proportio equalitatis, et per consequens nec maior nec minor quam nunc est".

> related to another proportion when the unequal motions are produced by those proportions.[59]

A bit later, Dumbleton explained it further:

> It follows next to say how motion follows a proportion and first to explain by example so that those who are not expert in geometry may approach the truth through simple and perceptible examples and may see its cause …. The fourth supposition [is] that the latitude of proportion and motion are equally acquired and lost with each other [*inter se*]. Just as if a space were uniformly acquired in a day, so much a part of the motion is acquired, as of space precisely acquired and so much time lapsed. In the same way, if the latitude of proportion is acquired by a uniform motion of intension, precisely as much of the latitude of proportion will be acquired (which is so to speak the space of the motion of intension), as of the latitude of motion corresponding to it.[60]

Insofar as physics was concerned, some might argue against Bradwardine's rule that double a force should cause double a velocity and not a double proportion of force to resistance. So Dumbleton replied to the argument that 6 ought to produce twice as much in a body acted on as 3:

> Next to solve the argument which is the basis of the opposing position, when it assumes that 3 acts on some body which has a value of 2, and another 3 afterwards acts on the same 2 resisting equally; and that it follows that the two agents acting together will produce twice as much as one produces by itself,

[59] "Tertia opinio Aristotelis et Commentatoris, que tenenda est, talis est quod motus sequitur proportionem geometricam et intenditur et remittitur secundum quod proportio maior est vel minor ad proportionem priorem, ita quod unus motus excedit alium motum secundum quod una proportio se habet ad aliam proportionem secundum quas proportiones fiunt illi motus inequales" (Ms. Cambridge, Peterhouse 272, f. 23va, corrected by comparison to Ms. Vat. lat. 954, in Sylla, "The Oxford Calculators", pp. 576–577).

[60] "Sequitur dicere qualiter motus sequitur proportionem et primo modo exemplari exprimere ut qui in geometrica non sunt exercitati exemplis grossis et sensibilibus veritatem ingrediantur et eius causam videant …. Quarta suppositio quod latitudo proportionis et motus inter se equaliter adquiruntur et deperduntur. Sicut si spatium uniformiter adquiratur in die quanta pars de motu adquiritur tanta de spatio precise adquiritur et de tempore labitur. Ita si latitudo proportionis uniformi motu intensionis adquiritur, quanta pars de latitudine proportionis adquiritur (que est quasi spatium motus intensionis), tantum precise de latitudine motus sibi correspondente adquiritur" (Ms. Cambridge, Peterhouse 272, fol. 24va. Compare Sylla, "The Oxford Calculators", p. 577).

> since neither agent impedes the other. Therefore if the first 3 produces a unit of the latitude of some quality in 2, it follows that 6 will produce twice as much as 3 previously produces. Also 3 produces a certain local motion. Let A be that motion and let that motion be in B medium equal to 2. It follows that 6 produces twice the motion that is A motion, since neither impedes the other.
>
> [Reply:] For these and similar arguments it should be supposed that in every action the total action is the action of the total agent and of any of its parts with respect to that action.[61]

Thus the argument that physically adding two forces that do not impede each other should add their effects was countered by the argument that an agent acts as a whole and not as the sum of its parts. It might be noted that Dumbleton did not propose to test experimentally which theory better fit observation.

Thus Bradwardine's rule for the relation of forces, resistances, and velocities was rapidly accepted because it was elegant and fit the familiar facts of local motion better than the standard Aristotelian view, but, in addition, *De proportionibus* probably became part of the curriculum at various universities because it provided students with a useful instrument of analysis that could be used over a wide range of topics. The mathematics of *De proportionibus* provided solutions to the sorts of puzzles that arose in the practice of university disputations – solutions that were impressive and helped to win arguments. Since the typical students or faculty members in late medieval universities were not expected actually to measure anything or to solve practical problems using their disciplines, they were free to study *De proportionibus velocitatum in motibus* for its own sake or to develop skills in critical thinking. The medieval sub-discipline *De proportionibus velocitatum in motibus*, as represented, for instance, by Richard Swineshead's *Liber calculationum*, typically addressed thorny problems that

[61] "Sequitur solvere argumentum quod est fundamentum alterius positionis cum supponitur quod tria agant in aliquod passum quod se habet sicut duo, et agant alia tria post in eandem duo equaliter resistiva. Sequitur quod ista agentia agunt duplum quam unum per se cum neutrum impedit aliud. Ergo si prima tria agerent unitatem latitudinis alicuius qualitatis in duo, sequitur quod sex agerent duplum ad illud quod prius egerent tria. Item tria producunt aliquem certum motum localem. Sit A ille motus. Et fiat ille motus in B medio assignato per duo. Ergo sequitur quod 6 producunt duplum motum ad A motum, cum neutrum impedit aliud. [Reply:] Pro istis et consimilibus est supponendum quod in omni actione tota actio est actio totius agentis et eius cuiuscumque partis respectu illius actionis" (Ms. Cambridge, Peterhouse 272, fols. 25rb–va, quoted in part in Sylla, "The Oxford Calculators", p. 578).

the application of the Bradwardinian approach itself raised, rather than the empirical adequacy of the results. Addressing such problems could lead to mathematical advances, for instance advances in methods of summing infinite series, while they dealt with thought experiments such as what would happen if a heavy object were dropped through a tunnel dug through the centre of the earth, as discussed by Swineshead in Treatise XI of the *Liber calculationum*.

THE RECEPTION OF BRADWARDINE'S *DE PROPORTIONIBUS VELOCITATUM IN MOTIBUS*

If it was not inadvertent but strategic to develop the pre-Theonine classical approach to proportions, while suppressing the Theonine approach multiplying denominations, nevertheless the approach to compounding proportions that Bradwardine adopted began to enter works on the proportions of velocities in motions in a very natural way. In fact, Elzbieta Jung has argued that Richard Kilvington, in his *Questions on Aristotle's Physics*, had "Bradwardine's function" before Bradwardine's *De proportionibus* of 1328.[62] The basis of her chronology is that Kilvington probably wrote his questions on the *Physics* before his questions on the *Ethics*, which cites the earlier work, and that he wrote both of these works before his questions on the *Sentences*, which cites the questions on *Ethics* and the *Physics*, and which probably results from his lectures on the *Sentences* in the years 1333–1334. By adding to the probable date of Kilvington's bachelor lectures on the *Sentences* the Oxford statutes concerning the prescribed length of theological study (seven years) and James Weisheipl's opinion that "it has not been shown that many masters, if any, taught arts while studying theology", Jung comes to the conclusion that Kilvington's arts lectures likely occurred before 1327, thus before Bradwardine's *On the ratios of velocities in motions* of 1328.[63] As evidence against Weisheipl's opinion it is only necessary to note, however, that, as Jung herself reports, Thomas Bradwardine is supposed to have read the *Sentences* at Oxford around 1332–1333, while we know that his *On the ratios* is dated 1328, less than seven years earlier.[64] It seems more reasonable, therefore, to assume at most that Bradwardine and Kilvington may have been working in parallel and that each knew the work of the other before completing the edited versions of his own work. In fact, from

[62] Jung-Palczewska, "Works by Richard Kilvington". See also her article on Kilvington in the Stanford Encyclopedia of Philosophy http://plato.stanford.edu/entries/kilvington.
[63] *Ibid.*, p. 185, citing Weisheipl, "Curriculum of the Faculty of Arts", p. 166.
[64] *Ibid.*, p. 191.

Jung's reports, it appears that Kilvington may have rejected Bradwardine's function in his questions on *De generatione*, delivered before his questions on the *Physics*, but accepted it by the time of the *Physics*. In his questions *On generation and corruption*, Kilvington argues that in the case of the magnitudes A, B, C, in continuous proportion, the proportion of A to C is indeed *duplicata* the proportion of A to B, but it need not be *dupla* or double.[65] This follows because the three magnitudes may be equal, in which case the proportion of A to C is equal the proportion of A to B; or A may be the smallest of the magnitudes and C the largest, in which case the proportion of A to C is less than A to B and therefore not double. In fact, in several of his arguments, Kilvington seems to be treating proportions as if it is irrelevant whether one speaks of the proportion of A to B or of B to A, as might be the case with musical harmonies. He also seems to be thinking of qualitative interactions in which there are, for instance, simultaneous actions and reactions between fire and water.

If Kilvington's questions *De generatione* should be proved to be before Bradwardine's *De proportionibus*, one could still claim that, although he knows about compounding proportions in the pre-Theonine way, this does not prove he has Bradwardine's law, since Euclid's definitions of a proportion *duplicata* and *triplicata* had been around long before Bradwardine. It is, however, impossible to deny that, within his questions on the *Physics*, in the question "Utrum in omni motu potentia motoris excedit potentiam rei mote", Kilvington rejects the view that velocities are proportional to the excess of the force over the resistance and argues for the view that velocities are proportional to the proportion of force to resistance understood in the Bradwardinian sense, as represented, for instance, by the claim that the proportion of 9:1 is double the proportion of 3:1. Whether the work is before or after 1328, it is in any case significant that Kilvington assumes without argument that proportions are compounded in the pre-Theonine or Bradwardinian sense, and defends a law like Bradwardine's against objections that it means that what Aristotle says about a double force leading to double velocity and the like is false unless one reinterprets the meaning of his text to agree with the "Bradwardinian" view (which, in fact, Kilvington does, saying that by "half of the mobile" means that part of the mobile that has the necessary proportion to the mover, and so forth. Once Jung has

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[65] *Ibid*., p. 219. Note that Jung's eye has slipped in saying, on the basis of Anneliese Maier's work, that Blasius of Parma attributed this opinion to Kilvington. In fact, on the page of Maier's work that Jung cites, Maier has already begun to discuss Giovanni Marliani, from whom the quotation comes. See Maier, *Die Vorläufer Galileis*, pp. 106–109.

published Kilvington's complete works it should be possible to establish the order of Kilvington's and Bradwardine's work more firmly.[66]

Of works that are certainly after Bradwardine's *De proportionibus*, the most widely distributed was Albert of Saxony's *De proportionibus velocitatum in motibus*, which was used as a university text at several universities and was printed at Padua in 1482 and 1484 and Venice in 1494. Albert's work is in a sense something like a new and revised edition of Bradwardine's work. It advocates Bradwardine's dynamical rule in just the same terms as Bradwardine had advocated it. In Albert's terms Bradwardine's rule states that the proportion of velocities in motions depends on the proportion of the proportions of moving powers over their resistances. This is what is usually said: that the proportion of velocities follows a geometrical proportion.[67] When there are three terms in continuous proportionality, Albert calls the proportion of the first to the third "*dupla*" the proportion of the first to the second, rather than "*duplicata*". In the case of four continuously proportional terms he calls the proportion of the first to the fourth "*tripla*" the proportion of the first to the second.[68] Before proving corollaries to Bradwardine's rule, Albert begins with a supposition saying that when there is a series of continuously decreasing quantities, the proportion of the extremes (taking the larger to the smaller) is composed (or compounded) of the proportion of the extremes to the means and of the means to each other, if there are more than one mean.[69] It should be remembered that in works *De proportionibus velocitatum in motibus* such as Albert's, the rules are normally assumed to apply only when the proportion of the extremes is one of greater inequality, which fits with the fact that, physically, motion occurs only when

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[66] In addition to her published articles, I am working from transcriptions of the texts kindly given to me by Jung, for which I give her thanks, and await her further clarifications.

[67] "Proportio velocitatum in motibus attenditur penes proporcionem proporcionum potenciarum <motivarum> super suas resistencias. Et hoc est quod solet dici: proporcionem velocitatum sequi proporcionem geometricam" (Albert of Saxony, *Tractatus proportionum*, p. 63).

[68] "Secunda suppositio: cum fuerit aliqua proporcio ex duabus proporcionibus equalibus precise composita, ad quamlibet illarum est dupla et si ex tribus, ad quamlibet <illarum> est tripla …" (*ibid*., p. 65).

[69] "Si fuerit aliquorum extremorum ad invicem proporcio maioris inequalitatis medio interposito vel mediis interpositis cuius vel quorum ad utrumque extremorum fuerit aliqua proporcio (ad minus quidem proporcio maioris inequalitatis, ad maius autem proporcio minoris inequalitatis), erit proporcio extremi ad extremum composita ex proporcione extremi ad medium (et mediorum inter se, si fuerint plura media), et medii ad extremum. Verbi gracia: sint 4 termini: 8; 4; 2; 1, dico quod proporcio 8 ad 1 composita erit ex proporcione 8 ad 4 et 4 ad 2 et 2 ad 1. Et ista suppositio patet V Elementorum Euclidis" (*ibid*., p. 65; my parentheses).

the force is greater than the resistance. Likewise, the means are supposed to be smaller than the larger extreme and greater than the lesser extreme, and to be monotonically decreasing from the greatest force at one extreme to the least resistance at the other. Some authors complained about the definition of *composita* if it was applied to proportions of lesser inequality, so that the proportion of the extremes would be less than the proportions of the means.[70] In the Bradwardinian approach, however, it is taken for granted that one is dealing with proportions of greater inequality and with middle terms that are less than the larger extreme and more than the smaller one.

Nicole Oresme's *De proportionibus proportionum* extends the Bradwardinian approach because its main goal is to disprove the practicality of astrology by showing that planetary motions are most probably incommensurable with each other. His interest, therefore, is to compare one proportion to another to see the relative frequency of cases in which there is a rational proportion of one proportion to another as compared to all the possible relationships between one proportion and another. Oresme's strategy in *De proportionibus proportionum* only works because he assumes the truth of Bradwardine's rule, because, given Bradwardine's rule, velocities are incommensurable if proportions of proportions, in the special Bradwardinian sense, are irrational. Moreover, his definitions of incommensurability of proportions only apply, if the proportion is, or the proportions are, taken in the Bradwardinian way.

As compared to Bradwardine's and Albert of Saxony's works *De proportionibus*, Oresme's work has several notable features beyond the fact that he extends the theory they have in common to show that heavenly velocities are most probably incommensurable. Unlike Albert, Oresme uses *duplicata* rather than *dupla* for a proportion compounded of two equal proportions, and *triplicata* for a proportion compounded of three equal proportions.[71] On the other hand, Oresme speaks of the compounding of proportions as "adding" and of the taking away of one proportion from another as "subtracting":

> To subtract [*subtrahere*] one ratio from another is to assign a mean between the terms of the greater ratio such that when the mean is related to the smaller term, or the greater term is related to the mean, a ratio is formed that equals the ratio to be subtracted. To add [*addere*] one ratio to another, [first] express one of them in terms [*ponere in terminis*] and then find a third term that is related to the greater term as the ratio

[70] See below.
[71] Oresme, *De proportionibus proportionum*, p. 150.

> that you wish to add; or find a third term to which the lesser term is related as the ratio that must be added.[72]

In two ways, however, Oresme does not sustain the Bradwardinian pretence that his mathematics of proportions is the only one. First of all, he admits that there is another way to compound proportions, namely by multiplying their denominations as in the dubious definition 5 of Book VI of Euclid's *Elements*. He calls this approach "by art":

> If, however, you wish to add a ratio of greater inequality to another by means of algorism [*per artem*], it is necessary to multiply the denomination of one ratio by the denomination of the other. And if you wish to subtract one ratio from another, you do this by dividing the denomination of one ratio by the denomination of the other. The [method] of finding denominations will be taught afterward. Multiplication and division of denominations are done by algorism [*per algorismum*].[73]

Thus the philosophical, so to speak, method of compounding involves subtracting and adding, taking means and extremes, while at the same time there is a practical or algorithmic calculational approach using division and multiplication of denominations. Bradwardine's decision to write as if the latter approach did not exist did not constrain Oresme, though in the entire rest of his discussion he did not use the algorithmic approach.

Oresme's second departure from Bradwardine involves attempting to extend the Bradwardinian approach to compounding proportions from proportions of greater inequality to proportions of lesser inequality. There he faces the difficulty that if, for instance, one considers the proportions of lesser inequality in the series 1, 2, 4, then it seems that the proportion of the extremes 1:4 is compounded of the proportions of 1:2 and 2:4, both of which are larger than the proportion of the extremes. Consequently, Oresme tries to invert the procedures used for proportions of greater inequality, so that one would increase (rather than decrease) a proportion of lesser inequality by inserting a mean between its extremes.[74] Those who opposed the Bradwardinian approach to proportions often did so by arguing that it makes no sense when applied to proportions of equality or of lesser inequality. It would perhaps have been wiser for Oresme to treat proportions of lesser inequality as if they were in reality the same as proportions of greater inequality, only looked at differently, as was the case in musical harmonics.

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[72] *Ibid.*, pp. 142–143.
[73] *Ibid.*, pp. 143–145.
[74] *Ibid.*, pp. 145 sqq.

Oresme, himself, while attempting to extend the Bradwardinian approach, does not intentionally disagree with Bradwardine's view. He states, for instance, that the proportion of proportions is not the same as the proportion of their denominations except in the case of the ratios 4:1 and 2:1.[75] On the other hand, by attempting to extend a Bradwardinian approach to ratios of lesser inequality, Oresme increased its vulnerability in the eyes of later mathematicians.

In his *De proportione proportionum et proportionalitate*, published together with the works of Albert of Saxony, Themon Judeus, and John Buridan in 1518, George Lokert still upheld an interpretation of the composition of proportions like Bradwardine's. Concerning the proportion of proportions, in agreement with Oresme, he writes that the proportion of proportions is not always the same as the proportion of the numbers denominating the proportions – in fact it hardly ever happens except with the proportion of two to one.[76] To the question whether every proportion is "proportionable" to every other, Lokert replies that only proportions of the same kind, such as proportions of greater inequality are proportionable. If a proportion is compounded of two equal proportions, it is double (*dupla*) each of them, and if it is compounded of three equal proportions, it is triple (*tripla*) each of them. Thus one can know the proportions of proportions.[77]

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[75] "Ex istis et aliis leviter patet quod proportio proportionum non est sicut proportio suarum denominationum. Iam enim omnes proportiones quarum denominationes sunt note erunt commensurabiles. Tripla, quidem, est incommensurabilis duple et, tamen, denominatio eius est sexquialtera ad denominationem duple … solum quadrupla et dupla hoc privilegium tenuerunt quod talis est proportio proportionum qualis est proportio denominationum et numquam in aliis reperitur" (*ibid*., pp. 226–229).

[76] "Sequitur ulterius quod non semper est eadem proportio proportionum qualis est proportio numerorum denominantium [*correxi ex* denominationum] tales proportiones. Immo non semper quando proportio numerorum est dupla oportet proportionem proportionum denominatarum a talibus numeris esse talem: licet in aliquibus sit verum: patet de proportione quadruple ad duplam que est dupla sicut numerorum denominantium [*correxi ex* denominantiam] tales proportiones. In aliis vix hoc contingit invenire. Ita non oportet si alique proportiones se habeant in proportione dupla quod numerorum denominantium tales proportiones sit similis proportio. Patet de nonocupla et tripla" (Lokert, *De proportione proportionum*, fol. Aaaiiii va).

[77] "Ad secundam dubitationem dicitur quod solum proportiones eiusdem rationis sunt proportionabiles vel comparabiles secundum rationem proportionis. Dico omnes proportiones equalitatis esse eiusdem rationis, et ita proportiones maioris inequalitatis adinvicem, et similiter minoris inequalitatis adinvicem. Sic ergo nulla proportio equalitatis dicenda est maior minor vel equalis respectu proportionis inequalitatis, nec proportio maioris inequalitatis respectu proportionis minoris inequalitatis …. Patet ex isto si aliqua proportio constituatur ex duabus proportionibus equalibus, talis ad quamlibet illarum est proportio dupla, et si componeretur ex tribus equalibus ipsius ad quamlibet erit proportio tripla, et componatur ex duabus quarum

Lokert does not apply his mathematics of proportions of proportions to dynamics, but he has retained a conception of the proportion of proportions like the one Bradwardine expounds.

As late as 1528, in his *De proportionibus libri duo*, Jean Fernel defended the mathematics of proportionality that had provided the foundation for Thomas Bradwardine's 1328 *De proportionibus velocitatum in motibus* two hundred years earlier. Although Fernel's work appears to be primarily mathematical, he opened it with a statement of the importance of proportions to comparing velocities, whether in the heavens or on earth, and other effects of natural agents.[78] Drawing on the work of Euclid, Jordanus, and Campanus, among others, Fernel emphasized that proportions exist only between things of the same kind, so that, for instance, there is no ratio between a velocity and a number.[79] Although the proportions between weights or between velocities can be denominated by numbers or magnitudes,[80] he said, proportion itself belongs to the category of relation and involves at least two different things compared to each other.[81] A proportion,

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una est dupla ad aliam illius ad maiorem erit proportio sesquialtera et ad minorem tripla. Sic consequenter cognoscuntur proportiones proportionum sicut proportiones quantitatum" (*ibid.*, fol. Aaavi ra)

[78] "Cum naturalis Philosophiae pars optima, quae motuum tum caelestium, tum reliquorum omnium velocitates, agentiumque naturalium varios perpendit effectus, non modo mathematicis demonstrationibus firmate est, sed & quantitatum rationibus passim conspergatur, opportunum & imprimis conducibile visum et horum brevem quandam traditionem facere, ne in philosophica astronomicaque praecepta deductos prorsus deterreant motuum supputationes plurima proportionum abdita prae se ferentes" (Fernel, *De proportionibus*, p. 1r).

[79] "Quo igitur notum id sit de quo nostra futura est disputatio, a proportionis definitione sumendum esse exordium, philosophicus ordo suadet. Hanc sic Campanus tradidit. Proportio, est duarum rerum eiusdem generis adinvicem certa habitudo. Non solum enim numerorum & magnitudinum est proportio, sed & cunctarum rerum quae quovis modo comparari possunt: ut sunt motus in velocitate, pondera in gravitate, soni in acutie, aliaque id genus plurima. Nec demum convenit motum numero comparare, nec magnitudini numerum, ponderive magnitudinem: quod ea diversorum sunt generum: nihilque sit commune, in quo diversa comparari possint. Non enim celeritas numero, (ut motui) competit, nec magnitudini quatenus talis, gravitas (ut ponderi) est propria. Etsi magnitudini & numero quantitas communis sit, alia nihilominus ratione numeri quam continui quantitatem deprehendimus: neque (ob id) numerum magnitudine maiorem minoremve dicemus. Non inconsulte igitur in definitione dictum est, rerum eiusdem generis" (*ibid.*).

[80] "At cuncta quae inter se comparantur, sive in gravitate ut pondera, sive in celeritate ut motus, aequalia aut inaequalia dicuntur: suaeque inaequalitatis denominationem habent a numero vel magnitudine" (*ibid.*).

[81] "Ergo non inscite Euclides sic proportionem definivit. Proportio, est certa habitudo seu comparatio quantitatum generis eiusdem. Habitudo, generis locum habet: quae vox (si res inter se comparatas, ut vulgaris fert opinio, nobis significet) est collectiva, pro duobus saltem extremis inter se comparatis accepta" (*ibid.*, p. 1v).

he said (with reference to Campanus and Bradwardine), is a certain relation (*habitudo*), but this does not mean that it is known or knowable – there may be infinitely many irrational proportions.[82]

Fernel divides his work into two books, the first concerning proportions and the second proportionalities. In Book I, he defines the different species of proportions in a standard Boethian way, including proportions of equality and inequality, multiplex, superparticular, superpartient, rational, irrational, and so forth. Along the way he pauses to deal with the comparison of vulgar fractions, saying that his discussion will not be complete if he deals only with proportions of integers – and drawing no attention to the mathematical nature of fractions as opposed to proportions.

Book II, on proportionalities, then opens with a statement of Bradwardine's rule that might be invisible to a reader not familiar with its significance:

> The fitness of proportionalities is in frequent use not only in astronomical or geometrical computations, but also in philosophical discussions, since it is not possible to conceive that one motion is faster or slower than another without it. When velocities of motions are compared to each other, they are as proportions, for every velocity depends on the proportion of the agent to the resistance, and the comparison of velocities is no other than the comparison or relation of proportions that we call proportionality. Proportionality is therefore a certain relation of more than one proportion, one to the other.[83]

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[82] "Cum certam dicimus habitudinem, non scitam aut scibilem debes accepisse, non omnis enim proportio (ceu mox de irrationali dicetur) cognoscibilis est: verum finitam ut inquit Bravardinus: aut talem qualis est una cum Campano intellexeris" (*ibid.*). In his commentary on of Euclid's *Elements* Book V, definition 3, Campanus writes, "Quod autem dicit certa habitudo. Non sic intelligas quasi nota vel scita, sed quasi determinata, ut sit sensus: Proportio est determinata habitudo duarum quantitatum, ita inquam determinata quod hec et non alia. Non enim est necessarium ut omnis habitudo duarum quantitatum sit scita a nobis nec etiam a natura" (quoted by Molland, in Bradwardine, *Geometria speculativa*, p. 160). Bradwardine comments: "Interest enim geometre totaliter de proportionibus disserere, nam arismeticus non invenit in numeris omnes proportionum modos, quoniam infinite sunt proportiones quas numerorum natura non patitur, quemadmodum testatur Campanus" (*ibid.*, p. 86).

[83] "Proportionalitatum commoditas non modo astronomicis geometricis've supputationibus, sed & philosophicis congressionibus plurimo est in usu, quum ne unum quidem motum altero velociorem aut tardiorem, sine hac liceat deprehendere. Quae enim comparantur motuum velocitates, sunt ut proportiones; omnis siquidem velocitas ex proportione agentis ad resistentiam patientis perpenditur, quarum velocitatum comparatio, non alia est quam proportionum comparatio seu habitudo, quam proportionalitatem dicimus. Est igitur proportionalitas, plurium proportionum unius ad alteram certa habitudo" (Fernel, *De proportionibus*, p. 15r).

Here we can see how natural and even hidden the Bradwardinian rule of dynamics could be when one assumed his understanding of proportions and their variation – modern readers might not have known that the Bradwardinian rule was here at issue had they not been instructed by Anneliese Maier, Marshall Clagett, and others following in their footsteps.[84]

Nowhere in Book II does Fernel support Bradwardine's rule with physical arguments, but he is at pains to refute the authors Bassanus Politus and Volumnius Roldulphus, who had rejected the Bradwardinian understanding of proportionality in the sense of proportions of proportions – an understanding that Fernel ascribes not to Bradwardine in particular, but rather to Euclid, Campanus, Jordanus, and other distinguished mathematicians. To double a proportion, he says, is not to multiply it by two, but rather to multiply it by itself. Similarly to triple a proportion one multiplies the proportion by itself three times, so that triple a proportion of 3 to 1 is the proportion of 27 to 1. In support of this view, Fernel refers to the commentary of Campanus on Euclid, Book V, definition 11.[85] He also relates this understanding of the multiplication of proportions to his previous discussion of the addition of proportions, according to which if the proportion of 3 to 1 is added to the proportion of 3 to 1 the result is 9 to 1, which is double the proportion of 3 to 1. Or, referring to Jordanus, *De elementis arithmetice artis*, Book II, definitions, he says, one may say that in every proportion the proportion of the extremes is composed of the proportion of one extreme to one of the middle terms, of the proportions of those middle terms to each other if there are more than one, and of the last middle term to the other extreme.[86]

After explaining the correct understanding of the proportions of proportions, Fernel turns to a refutation of the false opinion concerning proportionalities that had been proposed by Bassanus Politus and Volumnius Rodulphus. These authors treat proportions as quantities, so that proportions of proportions are just the same as proportions of quantities – which

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[84] See Murdoch and Sylla, "The Science of Motion", in Lindberg (ed.), *Science in the Middle Ages*.

[85] "Quartum documentum, omnis proportio potest quovis modo multiplicari: potest enim duplari, triplari, quadruplari, ac secundum reliquas multiplicationis species augeri. Nec tamen proportionem aliquam duplare est ipsam per 2 vel duplam proportionem multiplicare, quod in numeris fit, sed est ipsum seu suum denominatorem in se quadratè ducere .... Id apertè tradidit Campanus undecima definitione quinti Euclidis" (Fernel, *De proportionibus*, pp. 17r–v).

[86] "Quae antè tradita sunt declarant apertè omnem proportionem extremi ad extremum ex proportionibus extremorum ad media & mediorum inter se in unguem conflari. Idque ab Iordano inter definitiones secundi ad hunc modum traditum est. Quum continuatae fuerunt eaedem vel diversae proportiones, dicetur primi ad ultimum proportio ex omnibus composita" (*ibid.*, p. 18r).

they could not have done had they paid attention to Euclid's *Elements*, Book V. Saying that the proportions of proportions are the same as the proportions of their denominations, these authors erroneously say that the octuple proportion is double the quadruple, and the sextuple double the triple.[87] Hardly any theorem of Euclid on proportions and proportionalities is consistent with the theory of Bassanus Politus according to Fernel. He would not have thought it necessary to say more in refutation of Bassanus Politus had the disputation *De proportionum proportionibus* of Volumnius Rodulphus, delivered at a public session in Rome (published in 1516), which renewed the same errors, fallen into his hands after he had finished his book.[88] Fernel does understand that to add proportions their denominations may be multiplied together (or more exactly their numerators multiplied to make the numerator of the result and their denominators multiplied to make the denominator).[89] If a triple proportion and a quadruple proportion are

[87] "Proportionum proportio, essetne in simplicium proportionum ordinem referenda, multi verterunt in dubium: eorumque pars optima eam simplicis proportionis speciem quandam esse censuit: omnemque proportionem quantitatis rationem habere; quo dilucidius proportionum proportio, ceu quedam quantitatum proportio aestimaretur. Id tamen prorsus dissuasisse visus est Euclides libro quinto *Elementorum*, qui ut proportionem definivit, statim alteram proportionalitatis subiunxit definitionem: inquiens eam esse proportionum similitudinem: satis (me iudice) innuens proportionem à quantitate seiungi …. Proinde nonnulli his delusi cuncta ab Euclide, de simplicibus quantitatibus tradita sunt, de proportionibus ipsis perperam sunt interpretati, omnibus omnia permiscentes. Rursumque in tantum erroris lapsi sunt, ut dicant proportiones omnes eam inter se proportionem habere quam earum denominatores: haud secus proportionum quam numerorum proportiones interstringentes, faciunt proinde octuplam duplam quadruplae, & sextuplam duplam triplae, triplamque duplae" (*ibid.*, pp. 19v–20r).

[88] "Singula incommodo quae hanc fallacem Basani Politi sententiam sequuntur, si referre tentarem, ne magno quidem volumine traderentur: quod vix unico theorematі de proportionibus aut proportionalitatibus ab Euclide conscripto, sit ea positio consona. Pauca haec igitur obiter in capit unum strinxisse sit satis: de quibus nec meminisse statueram, ni absoluto opere in manus fortè incidisset Volumnii Rodulphi de proportionum proportionibus disputatio; qua antiquos renovans errores, Romae congressione publica cunctos, qui etiam perspicacioris iudicii censerentur, planodiam (inquit) canere, et si quam nuper improbavimus opinioni adhaerere coegit. Nec idcirco sanè quod Romanos mathematicos sua (ut inquit) haeresi obruerit, sibi gloriam peperisse credat, sed verius urbi deducus" (*ibid.*, pp. 20v–21r). The work by Rodulphi that Fernel refers to (which I have not seen) is Volumnius Rodulphus Spoletanus [Volumnio Ridolfi], *De proportione proportionum disputatio*. In an earlier paper, I reversed the names as Rodulpho Volumnio based on Clavius, *Euclidis elementorum libri XVI*, p. 218 (although on p. 394 Clavius has Volumnio Rodulpho Spoletano).

[89] "Tertium documentum, quotvis proportiones (ut numeri) simul addi iungique possunt: ex eisque una in summo propter additionem consurgit. Fit autem additio in hunc modum. Oblatorum proportionum primi termini statuuntur, ducunturque extrema maiora inter se, rursumque minor inter se: & que inter productos terminos est proportio, ex primo datis proportionibus exactè componitur …. Visa est nihilominus additio per extremorum multiplicationem

"multiplied" in the improper sense that their numerators and denominators are multiplied, a proportion of 12 to 1 will result, as Euclid, Jordanus, and other celebrated mathematicians would agree, but they would never call the proportion of 12 to 1 triple a quadruple proportion, but rather say that a proportion of 12 to 1 is compounded (*consititui*; *componitur*) from a triple and a quadruple.[90]

In the rest of Fernel's discussion, one of his main concerns is that the false view of the proportions of proportions put forth by Bassanus Politus and Volumnius Rodulphus leads to mistaken results with regard to the proportions of proportions in the sense that it fails to see when these proportions of proportions are irrational (the understanding of which had been one of Nicole Oresme's major achievements in his *De proportionibus proportionum*).[91] For my purposes here, what is important is that by defending what he considered to be the view of compounding proportions held by Euclid, Jordanus, and Campanus, Fernel was implicitly defending the foundations of the Bradwardinian rule for the relations of velocities and proportions of force to resistance. He said nothing about what happens to velocities when forces or resistances increase or decrease, but when Fernel's camp lost the battle on how proportions of proportions were to be understood, as was to happen within the next century, the mathematical foundations of the Bradwardinian approach to the proportions of velocities in motions would be undermined as well.

AUTHORS WHO REJECTED BRADWARDINE'S RULE

Finally, what of authors who rejected Bradwardine's rule? One of the earliest to do so was Blasius of Parma, who, in a commentary on Bradwardine's *De proportionibus*, apparently rejected both Bradwardine's understanding

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expeditior, quam quae denominationum multiplicatione fit, eam idcirco impraesentiarum tradidimus" (*ibid.*, p. 17r). As will be explained below, Bradwardine and many of those following his approach either were unaware of or chose to ignore this approach to compounding proportions by multiplying their denominations, which was embodied in the interpolated definition 5 of Book VI of Euclid's *Elements*, found in many editions, but not in that of Campanus used by Bradwardine.

90 "Vocaverunt nihilominus mathematici brevitatis gratia proportionum multiplicationem, eam multiplicationem quae est inter proportionum extrema: hocque ductu proportiones in unam iunguntur, ac succrescunt extrema proportionis quae ex illis componitur. Ut tandem summatim dicam, si quadrupla per triplam (impropriè tamen & extremorum multiplicatione) multiplicetur, nolunt Euclides, Iordanus, aliisque percelebres mathematici duodecuplam proportionem quae hinc oritur, triplam esse quaduplae: sed id unicum innuunt deodecuplam ex tripla & quadruple constitui" (*ibid.*, p. 21v).

91 Oresme, *De proportionibus proportionum*.

of the compounding of proportions and Bradwardine's rule.[92] In fact there are two versions of Blasius's questions, an early version found in a Milan manuscript, which may have been written while Blasius was in Paris probably before 1382, and a later version (recently published by Joel Biard and Sabine Rommevaux), adding a long eleventh question, probably written in connection with Blasius's teaching between 1389 and 1407 in various universities in northern Italy.[93] In addition to Bradwardine, Blasius criticizes Albert of Saxony and Oresme on mathematical grounds.[94]

Ironically, although Blasius is known for having rejected Bradwardine's law, he seems to assume its truth in many sections of his work. When addressing the question directly of the relation of forces, resistances, and velocities in motions, Blasius concludes:

> Taking velocity for acceleration [i.e., speed], generally in every motion the proportion of velocities depends on the proportion of the denominations of the moving powers to the resistances.[95]

This conclusion is expressly against Bradwardine inasmuch as it refers to the denominations of the proportions rather than the proportions themselves. Earlier, Blasius had expressly rejected the view held in common by Bradwardine, Oresme, and Albert of Saxony:

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[92] Blasius of Parma, *Questiones circa tractatum*. Clagett, *The Science of Mechanics*, p. 443, says Blasius rejects Bradwardine's rule. See Harrison, "Blasius of Parma's Critique of Bradwardine"; Vescovini, "*Arti" e filosofia nel secolo XIV*, p. 296: "non solo, criticando la confusione fatta dal Bradwardine nel suo Trattato delle proporzioni tra due operazioni aritmetiche, la potenza et la moltiplicazione, Biagio contribuisce a demolire anche la regola matematico-fisica dello stesse Bradwardine". See also Biard, "Mathématiques et philosophie".

[93] Biard, "Mathématiques et philosophie", p. 384; Biard and Rommevaux, in Blasius of Parma, *Questiones circa tractatum proportionum*, pp. 47–48; Rommevaux, "Les règles du mouvement".

[94] "Et ob hoc multi nescientes distinguere inter partes componentes et partes producentes in multos errores ceciderunt, ut fuit Magister Thomas Braduardini qui dixit quod ad hoc quod proportio extremi ad extremum componitur ex proportione primi ad secundum et secundi ad tertium, oportebat primum esse maius secundo et secundum maius tertio. Sic enim glosat in respondendo ad quasdam instantias factas per ipsum in tractatu *De proportionibus*. Et in eundem errorem cecidit Magister Albertus de Saxonia in suo tractatu *De proportionibus*, similiter Magister Nicholaus Orem in suo *De proportionibus proportionum* ..." (Blasius of Parma, *Questiones circa tractatum proportionum*, pp. 108–109).

[95] "Prima conclusio: capiendo velocitatem pro acceleratione, generaliter in omni motu proportio velocitatum est attendenda penes proportionem denominationum potentiarum moventium ad resistentias" (*ibid*., p. 154).

> Now follows another position on the proposition, one that the moderns commonly hold to be true. They put forth this conclusion: velocity in motions follows the proportion of the moving power to the resistance, and the proportion of velocities follows the proportion of the proportion of the moving powers to the resistances. This is always to be understood of geometric proportion, and not otherwise. This position seems especially to be that of the Master in his treatise *De proportionibus* …. Although this position is commonly held, it should be rejected ….[96]

The first two reasons that Blasius gives for rejecting Bradwardine's position are physical. First of all, as an agent of alteration assimilates another body, the resistance of the other body becomes less and less, so that the proportion of agent to resistance would become infinite and an infinite latitude would be induced. And, if something like this happens, a finite agent would produce an infinite effect.[97] Blasius's third and last argument against Bradwardine's law is, however, surprising:

> Last, in a question in which it was asked whether given any two extremes, and a medium interposed, the proportion of the first to the last would be compounded of the proportion of the first to the second, etc., it was said and concluded that the proportion of 9 to 1 is precisely double to a quadruple with a half (*dupla ad quadruplam cum dimidio*). But the velocity that arises from the proportion of 9 to 1 is not precisely double the velocity that arises from a proportion of a quadruple and a half (*a proportione quadrupla cum dimidio*). Therefore velocity is not as the proportion of proportions. The antecedent is established because the velocity that arises from a ninefold (*nonecupla*) proportion is precisely double the velocity that arises from a triple proportion, therefore, etc.[98]

Thus, in supposedly refuting Bradwardine's view, Blasius rejects his view of what half a proportion of 9 to 1 is, but not his view that a proportion of 9 to 1 will produce double the velocity that is produced by a proportion of 3 to 1.

In the following question, moreover, Blasius abandons the position he has just taken and assumes the truth of Bradwardine's theory, on the grounds that

[96] *Ibid.*, pp. 152–153.
[97] *Ibid.*, p. 153.
[98] *Ibid.*, pp. 153–54.

that theory is commonly accepted and so his students may want to assume it in their arguments.[99] In fact, throughout most of his commentary, he follows Bradwardine's opinion rather than rejecting it. Only in one extended section does he develop an alternative view, according to which the product of proportions is to be understood differently from the compounding of proportions. Any proportions of greater or lesser inequality may be multiplied times each other producing a product, he concludes. Only proportions of greater inequality, monotonically decreasing from one extreme to the other may be compounded together in such a way that the whole is always greater than the parts.[100]

What is going on here? Did Blasius understand Bradwardine's view? In his sixth question, in fact, Blasius gives a clear description of Bradwardine's view on compounding of proportions under the heading of a view to be rejected.[101] As the truth, he presents the view that if there are a number of means between two extremes, then the proportion of the extremes is *produced*, but not necessarily compounded, from the proportions between the extremes and the means.[102] In defence of his conclusions, he makes use of denominations and their multiplication together. He says that to compound one proportion with another is to multiply their denominations together, something that sounds like the spurious definition 5, Book VI, of Euclid's *Elements*, but which he ascribes to Euclid, Books V and VII and to the author *De proportione et proportionalitate.*[103] The reason why the proportion of the extremes is not always composed of the proportions between the means is that the proportion of the extremes may be smaller

[99] "Tamen quia ista non sunt dici consueta, aliter procedo, me magis consueto et communi modo loquendi conformando. Pro quo pono aliquas suppositiones. Prima: quibuscumque duobus extremis datis habentibus invicem proportio extremi ad extremum componitur ex proportione primi ad secundum et secundi ad tertium .... Omnes iste suppositiones patent ex doctrina V *Elementorum* Euclidis, quas suppositiones admittit Magister iste in tractatu suo *De proportionibus*. Modo sequuntur conclusiones .... Secunda conclusio: proportio velocitatum insequitur proportionem proportionum potentiarum moventium ad suas resistantias" (*ibid.*, p. 166). Blasius goes on to infer corollaries like the conclusions found in the abbreviated version of Bradwardine's work quoted above.

[100] *Ibid.*, pp. 101–111.

[101] *Ibid.*, pp. 98–101. Rommevaux reports that in the early version of his commentary found in the Milan manuscript, Blasius presents the Bradwardinian point of view clearly.

[102] *Ibid.*, pp. 101–111.

[103] "Quarta evidentia: proportionem componi ex proportionibus est denominationem eius ex denominationibus earum invicem multiplicatis produci. Et hoc est auctoris *De proportione et proportionalitate* et Euclidis in V et VII *Geometrie* .... non erat alius proportionem componi ex proportionibus quam denominationes earum proportionum invicem productas producere denominationem totius" (*ibid.*, pp. 102–103).

than some of the proportions between the means.[104] Although to produce and to compound are frequently the same, they are not always so. And this is where Blasius says that Bradwardine, Albert of Saxony, and Oresme err in their work on proportions. At the end of question 6, Blasius repeats that Bradwardine's law is to be rejected for this mathematical reason:

> The last conclusion, irrelevant to our work here (*impertinens facto nostro*): the proportion of velocities in motions does not follow the proportion of the proportions of the moving powers to their resistances. It is proved: the velocity deriving from a proportion of 9 to 1 is double the velocity deriving from a triple proportion, and, as has been proved, the proportion 9:1 (*nonecupla*) is not double a triple proportion. Indeed, as has been proved, the proportion 9:1 is double the proportion 4½:1 (*quadruplam sexquialteram*). And from this root, many conclusions concerning the velocities of motions are to be denied, but on this a general question will be addressed in its place, and I will not treat it further here.[105]

Putting this all together, one reaches the conclusion that Blasius does not reject Bradwardine's law as far as its physical significance is concerned, but he rejects the mathematical foundation Bradwardine had created for it. Even here he is not consistent. In reply to his question 7, whether there is some proportion greater than a proportion of equality, for instance, Blasius says that either answer to the question may be defended and he proceeds to provide the arguments for each side.[106] He then goes on to give rules for adding and subtracting both rational and irrational proportions and concludes that on the basis of his rules many conclusions may be drawn on the assumption that velocity follows a proportion. For instance, if some power moves some mobile with a certain velocity and if the power

[104] "Non quibuscumque duobus extremis datis interposito medio eiusdem generis uno vel pluribus, proportio extremi ad extremum componitur ex proportione primi ad secundum et secundi ad tertium …. Et dicitur in conclusione "componitur" ut fiat differentia inter componi et produci. Patet conclusio quia, si conclusio non staret, sequitur partem adequari toti, et partem esse maiorem toto, et multa inconvenientia alia que adducta fuerunt a principio questionis …. Ex qua conclusione sequitur quod aliud est proportionem componi ex proportionibus et aliud est proportionem produci ex proportionibus. Scias tamen quod sepe coincidunt" (*ibid.*, pp. 101, 108).

[105] *Ibid.*, p. 111.

[106] "Quia questio pro utraque parte contradictionis potest defendi, in primo determinabo questionem pro una parte; in secunda tenebo partem oppositam …" (*ibid.*, pp. 112–124, at pp. 115–116).

augments uniformly while the resistance stays the same, then the velocity will not increase uniformly.[107] The conclusion is inescapable that Blasius understands and accepts, at least part of the time, Bradwardine's law and knows how to draw inferences from it not totally unlike those drawn in Richard Swineshead's *Calculationes*. At the same time he says that it should be rejected (*ipsa est reprobanda*).[108] What he has done, then, is to reject the elegant mathematical foundation that Bradwardine had fashioned for his rule using the pristine Book V of Euclid's *Elements*. This forced him, much like 20th-century historians, to try to achieve the same result by more cumbersome mathematical methods. His motives are mathematical and not natural philosophical: the physical arguments he makes against Bradwardine's view hold equally against his own view.[109]

After Blasius of Parma, his student John Marliani rejected Bradwardine's law for similar reasons.[110] Interestingly, Marliani writes that when he taught Bradwardine or Albert of Saxony on proportions, he was always surprised at what weak arguments they gave for their theory, but he assumed they had some reasons he knew nothing about. But now he has concluded that he has stronger arguments to the contrary, and so he holds the opposite view.[111]

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[107] *Ibid*., pp. 120–124. This conclusion is repeated as part of question 11, pp. 172–173.

[108] *Ibid*., p. 153.

[109] It should be said that Blasius also has physical arguments against Bradwardine. He accepts, for instance, that there may be motion in a vacuum, in which case there would be no resistance. See Biard and Rommevaux edition of Blasius's *Questiones* and Rommevaux, "Les règles du mouvement".

[110] See Clagett, *Marliani*, pp. 125–144. I do not have access to Marliani's work and so must rely on Clagett's description. Marliani apparently rejected Bradwardine's claim that no proportion is larger than a proportion of equality and argued for a dynamical law that held velocities proportional to the proportion between the mover minus the moved in comparison to the moved, or velocity proportional to (F – R)/R. Clagett states that Marliani did not understand Bradwardine's position, but it is possible that Marliani, like Blasius, rejected, rather than misunderstood Bradwardine's mathematics. Maier, *Die Vorläufer Galileis*, pp. 107–110, says that Clagett did not understand Marliani or Bradwardine, but I think she is wrong. Clagett describes the Bradwardinian view accurately enough although he has no special terminology for it. Neither Clagett nor Maier has the perspective I give on the history of the two traditions of compounding proportions.

[111] "Et fateor me semper plurimum admirationis accepisse, cum legerem publice proportiones Barduardini aut Abertoli, quoniam scil. ratione tam deboli nihilque concludenti conclusiones suas demonstrare videbantur …. Profecto, si hi viri sine ratione posuissent conclusiones suas nudas minime illas probantes, magis me … movissent ad eorum opinionem. Credidissem enim illos viros aliquas occultas rationes non a me cognitas habuisse. Sed cum conclusiones suas posuerunt solummodo moti rationibus quas adducunt et nihil moventibus, et ad oppositum ego rationes efficaces habeam mathematicorum etiam et philosophorum dictis magis consonantes, oppositum teneo" (Maier, *Die Vorläufer Galileis*, p. 109, n. 59; reading compared to Clagett,

Another author to try to undermine the foundations of Bradwardine's rule was, ironically, Bassanus Politus, in his *Tractatus proportionum introductorius ad calculationes suisset* published at Venice in 1505, the same year as the publication of Zamberti's translation of Euclid from the Greek. We have already seen that it was Bassanus Politus who, along with Volumnius Rodophus, spurred Jean Fernel to his defence of the Euclid-Campanus-Bradwardine approach in 1528. Politus does not mention dynamics or Bradwardine, but, oddly in a work that is supposed to be an introduction to Swineshead's *Calculationes*, he rejects the terminology of compounding of proportions undergirding Bradwardine's rule. Thus he says that, unlike lines or other quantities that are double the halves of which they are compounded, proportions are not double their component halves, because proportions are compounded by multiplying rather than by adding.[112] If the relations of proportions to each other are understood in this way, Bradwardine's rule can no longer be simply stated as a geometric proportionality.

Marliani, p. 138, n. 30). As Anneliese Maier notes, *ibid*., p. 106, n. 58, Marliani cites Kilvington ("Richardus Clienton") for the point that not every proportion that is *duplicata* or *triplicata* with respect to another proportion is double or triple it.

[112] "Non similiter componitur proportionis quantitas vel denominatio ex proportionum quantitatibus vel denominationibus sicut linea ex liniis, aut numerus ex numeris, aut superficies ex superficiebus, et ita de aliis. Quod patet, nam linea ex liniis componitur per illarum certam invicem additionem, et similiter numerus ex numeris, et ita de aliis. Proportionis autem quantitas vel denominatio ex proportionum quantitatibus vel denominationibus componitur non per illarum additionem invicem factam, sed multiplicationem earundem, ut diximus. Tertia sequitur quod licet omnis linea ex duabus equalibus non communicantibus composita adequate sit utraque illarum precise duplo maior, et duabus talibus inequalibus composita illarum minorem plusquam in duplo maior et earundem maiore minus quam in duplo maior. Non tamen similiter est in compositione proportionum. Huius correlarii prima pars est manifesta. Nam linea bipedalis A adequate composita ex duabus liniis pedalibus non invicem communicantibus B, scilicet, et C utraque illarum est precise in duplo maior, ut patet ex dictis. Secunda pars etiam patet. Nam linea A bi[? 5]pedalis adequate composita ex duabus liniis inequalibus tripedali, scilicet, et bipedali, ut puta C et B ad istarum maiorem, puta ad C, habet proportionem minorem dupla, et ad illarum minorem habet proportionem dupla maiorem, ut patet ex dictis, quare, etc. Secunda pars principalis correlarii quo ad primam partem probatur. Nam proportio nonupla, puta 9 ad 1 adequate componitur ex duabus triplis, ut patet ex illo notato, et infra declarabitur, puta 9 ad 3 et 3 ad 1, que sunt invicem equales, ut patet ex dictis, quare, etc. Quo ad secundam partem similiter ostenditur, Nam proportio dupla, puta 4 ad 2, adequate componitur, ut patet ex dictis, et dicendis ex duabus proportionibus invicem inequalibus quarum una est equalitatis, scilicet 4 ad 4 et altera dupla, scilicet 4 ad 2, et maiorem istarum, scilicet dupla non est minus quam in duplo maior, quia illa non est maior, immo eidem est equalis, ut patet ex dictis …. Ex quo sequitur ultra et ultimo, quod licet in quantitatibus continuis aut discretis valeat hec consequentia: hoc est subduplum ad illud, ergo est eiusdem medietas vel eiusdem medietati equale, non tamen similis consequentia in proportionibus" (Politus, *Tractatus proportionum*, fol. 7rb).

After Politus, Alessandro Achillini also rejected the Bradwardinian approach to the proportions of velocities in motion *tanquam penes causam*. Just as Blasius of Parma, he argues that the proportion of proportions is the same as the proportion of the denominations of those proportions, while admitting that almost all moderns, including Bradwardine, Swineshead, and Oresme believe otherwise.[113] The title of his question, published in 1545, is "Whether the more recent mathematicians have discovered that Aristotle was in error teaching the rules of proportion by which motions are compared to each other".[114] His point is that producing something by multiplication is not the same as producing it by compounding, because the factors in a multiplication may be larger than the product. For this reason, what the moderns such as Paul of Venice, Albert of Saxony, Thomas Bradwardine, and others say, is in error.[115]

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[113] Achillini, *De proportione motuum* in *Achillini* as cited by Grant in Oresme, *De proportionibus proportionum*, p. 69n. In the same note, Grant quotes another opponent of the Bradwardinian-Oresmian way, namely Volumnius, who, in his *De proportione proportionum*, argues, "Campanus, qui in expositione xi diffinitionis quinti, voluit proportionem in duabus quantitatibus esse simplex intervallum et habere naturam lineae; proportionem vero extremorum in tribus terminis habere naturam superficiei; et in terminis quattuor retinere naturam solidi. Hoc idem et adversarii usurpant cum Nicolaus horem in suo libello *De proportionibus* id supponat proportionesque ipsas passim ac si mere essent quantitates dividat, ac regulis quibus et alias quantitates subiiciat et obnoxias esse velit".

[114] Achillini, *De proportionibus motuum*, available online at the Archimedes collection of the Max Planck Institute at http://archimedes.mpiwg-berlin.mpg.de/cgi-bin/toc/toc.cgi?dir=achil_propo_087_la, but there are some errors of transcription, which I have corrected by comparison to the Vatican microfilm available through Manuscripta.

[115] "Corollarium septimum. Proportionem extremi ad extremum ex proportionibus mediis mathematicus componit ut in principio septimi *Elementorum* ponit Euclides. Et secundo *Arithmeticae* supponit Jordanus, non curando quod medium sit maius vel minus extremo vel extremis in quod proportio <e>qualitatis ex dupla et subdupla componatur. Patet a Campano sexto Euclidis propositione 17 quia inter duo et 2 ponit 1 pro medio. Et sic subnonuplam ex duabus subtriplis componit. Et sic quaelibet pars totius est maior toto. Maior enim est proportio 1 and 3 quam 1 ad 9 et hoc est quia dant ipsi totum productive compositum. Et in hoc vide modernos Paulum Venetum, Albertutium, Thomam Bradvardinum, etc. errant ab antiquis mathematicis quia nolunt extremis, exempli gratia 8 et 1, interponere aliud medium nisi minus maiore et maius minore. Cuius oppositum antiqui mathematici faciunt. Ego autem mathematicis concedo proportionum productionem, ut si denominator in denominatorem producatur, componetur proportio productae denominationis ex proportionibus denominatorum producentium hac impropria compositione. Et eam ad multa esse utilem concedo, ut in invenienda figurarum continentia, sed nego quod producentes proportiones productam generaliter componant, eo modo quo partes quantitativae totum integrant. Ideo concedendum est proportionem extremi ad extremum ex proportione extremi ad medium et mediorum invicem, si plura sint media, et extremi ad extremum esse productam, non curando an interpositum sit maius maiore extremo vel minus. Et similiter an medium sit minus minore extremo vel non.

Not only does Achillini think it is an abuse to speak of composition of ratios when the product may be less than the components, but – going beyond Blasius – he argues that in dynamics velocities will vary, not as the proportion of proportions in the Bradwardinian sense, but as the proportion of denominations. If, for example a ratio of 3:1 produces a certain velocity, then the ratio of 6:1 will produce the double velocity, not the ratio 9:1, or heating agents as 8 will produce double the effect of agents as 4, not agents acting as 16 to 1.[116] For this he appeals to natural experiences (*naturales experientiae*), of a stone moved on a moving ship, where the velocities add, or of groups of men working on some machine (*artificiale*), to conclude that eight men, other things being equal, will move twice as fast as four men.[117] Of course, one may doubt whether Achillini actually conducted experiments rather than simply appealing to what seemed plausible.

In the early 17th century Christopher Clavius also rejected the Bradwardinian approach. In his 1611 edition of Euclid, Clavius includes definition 5 of Book VI, in the translation of Zamberti, followed by a long explanation supporting the compounding of proportions by the multiplication of denominations, citing Theon, Eutocius, Apollonius, and Witelo.[118] He then connects

Sed principiis mathematicis et naturalibus repugnat illam productionem nominare compositionem qua unione partium facta excrescit totum ex partibus congregatum" (Achillini, *De proportionibus motuum*, fols. 187rb–va).

[116] "Pono igitur exempli gratia quod inter duo potentiae ut 4 aequaliter calida et aequalis virtutis mediet frigidum, exempli gratia resistentiae ut 1, ita distans quod a quolibet eorum seorsum calefieret a proportione quadrupla, exempli gratia acquirendo gradum ut 1 in hora. Tunc a duobus illis acquirit in hora duos gradus. Ergo octupla est dupla quadruplae, semoto tamen iuvamento accidentali potente uni advenire ab altero. Idem in motu locali in terra simplici triangulari, exempli gratia vacua librae 1 sic quod in ea capi possit libra terrae potente descendere in hoc aere a proportione quadrupla. Tunc in duplo plus pertransibit de aere exempli gratia terra repleta quam non. Tamen non tantum crescit proportio quod decimam sextuplam attingat. Licet enim aer terrae data inclusus resistentiam promoveret" (*ibid.*, fols. 187va–b. See also 189vb).

[117] "Hanc autem proportionum compositionem naturales experientiae convincunt, et non priorem. Patet: moveatur a quadrupla proportio lapis super nave mota a proportione quadrupla ad eandem positionis differentiam. Tunc duplum spacium pertransibit lapis ex duarum causarum congregatione ad id quod pertransiret lapis ab una illarum causarum tantum. Ex quibus tamen congregatis causis non fit super lapide proportio decimasextupla. Unde a quacumque proportione agant quatuor homines circa quodlibet artificiale, non est inventum alios quam homines 8 facere duplam velocitatem illi que a quatuor prioribus provenit, stante tamen temporis paritate, vigoris hominum, et iuvamenti unius ad alterum. Iuvamentum enim accidentale posset interevenire et cetera" (*ibid.*, fol. 185vb).

[118] "Ratio ex rationibus componi dicitur, cum rationum quantitaties inter se multiplicatae, aliquam effecerint rationem. Quoniam denominator cuiuslibet proportionis exprimit, quanta sit magnitudo antecedens ad consequentem … dici solet propterea denominator a Geometris,

the proofs of Theon, Eutocius, et al. to Euclid, Book VI, proposition 23.[119] Earlier, in commenting on Book V, definition 10, Clavius had rejected those who identified a *ratio duplicata* with a *ratio dupla*, from which he went on to deny Bradwardine's rule. If an agent with a force of 10 could accomplish some result, who would think that an agent with a force of 100 rather than a force of 20 would be required to accomplish twice as much?[120] In Clavius,

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quantitas proportionis; ut idem significet quantitas alicuius proportionis, quod denominator. Vult igitur haec definitio, proportionem aliquam ex duabus, vel pluribus proportionibus componi, quando harum denominatores, seu quantitates inter se multiplicatae effecerint illam proportionem, seu (ut vertit Zambertus) efficerint illius proportionis quantitatem, sive denominatorem …" "… Sic etiam in magnitudinibus quibuscunque ordine positis, proportio primae ad ultimam dicetur componi ex proportione primae ad secundam, & secundae ad tertiam, & tertiae ad quartam, etc. donec extiterit proportio; quoniam denominator proportionis primae magnitudinis ad ultimam consurgit ex denominatoribus proportionum intermediarum inter se multiplicatis. Quod quidem primum inductione quadam Theonis Alexandrini, quam hoc loco adducit, confirmabimus: Deinde vero idem duabus demonstrationibus, quarum una traditur ab Eutocio Ascalonita lib 2 Archimedis de sphaera & cylindro, theoremate 4, & in 1 lib. Apollonii Pergaei de conicis elementis, propos. 11. Altera autem a Vitellione lib. 1. prop. 13. suae Perspectivae, comprobabimus" (Clavius, *Commentaria in Euclidis elementa geometrica*, p. 243).

119 *Ibid.*, p. 246.

120 "X. Cum autem tres magnitudines proportionales fuerint; Prima ad tertiam duplicatam rationem habere dicitur eius, quam habet ad secundam: At cum quatuor magnitudines proportionales fuerint; prima ad quartam triplicatam rationem habere dicitur eius, quam habet ad secundam: Et semper deinceps, uno amplius, quamdiu proportio extiterit …. Interpretes nonulli colligunt ex hac definitione, si proponatur plures quantitates continue proportionales, proportionem primae quantitatis ad tertiam, esse duplam proportionis primae quantitatis ad secundam, eo quod Euclides illam vocet duplicatam proportionem huius …. Quod tamen nulla est ratione concedendum. Neque enim Euclides hoc significare voluit, sed docuit tantummodo, proportionem primae quantitatis ad tertiam, appellari duplicatam eius proportionis quam habet prima quantitatis ad secundam; propterea quod inter primam quantitatem, ac tertiam reperitur quodammodo proportio primae quantitatis ad secundam duplicata; quippe cum inter primam quantitatem, ac tertiam interponantur duae proportiones aequales ei proportioni, quam habet prima quantitas ad secundam & sic de caeteris, ut diximus. Non autem intelligit, illam duplam esse huius, ne Theorema proponeret, quod merito quispiam concedere recusaret. Quis enim affirmabit, in his numeris continue proportionalibus 25. 5. 1. proportionem 25. ad 1. duplam esse proportionis 25. ad 5. cum potius eam quis dixerit esse quincuplam? At vero, illam dici huius duplicatam ad sensum expositum, nemo inficiabitur, eo quod bis sit posita, et continue, proportio 25. ad 5 …. Sed auctores, qui proportionem primae quantitatis ad tertiam volunt esse duplam proportionis, quam prima quantitas habet ad secundam, (inter quos auctores est etiam Federicus Commandinus hoc loco: quod valde miror, cum alioquin Mathematicus sit praestantissimus) dicunt in hac defin. requiri terminos inaequales, primamque debere esse maiorem; ita ut definitio haec intelligenda sit necessario de proportione continua maioris inaequalitatis …. Quis autem dixerit unquam, millecuplam proportionem esse triplo tantummodo maiorem proportione decupla, & non potius

then, the Bradwardinian rule for the proportions of velocities in motions was rejected by means of rejecting its conception of the relations of proportions to each other, as well as by reference to physical counter-examples.

John Wallis, towards the end of the 17th century, represents a further step in this progression. In his *A Treatise of Algebra, both Historical and Practical* of 1685, John Wallis devoted a separate chapter to "Composition of Proportions, and other Operations relating to them". His purpose is explicitly to reject the Bradwardinian interpretation of composition. He begins:

> Having said … that what is there said of Fractions, is to be understood of Proportions also: This Chapter might have been spared, had it not been necessary to obviate some mistakes, which are apt to arise from the different sense wherein different Writers do use some words relating hereunto: Euclide in his def 5, lib. 6 hath given us this Definition of … Compounded Proportion…. A proportion is said to be Compounded of other Proportions, when the Exponent of That, is made by the Multiplication of the Exponents of These, one into another. Thus the *Compound of the Treble and Double* (whose Exponents are 3 and 2) is the *Treble of the Double* (whose exponent is 3 x 2,) that is, the *Sextuple* (because 3 x 2 = 6.) Which is manifestly a work of Multiplication.[121]

After some further explication, he goes on:

> But now because *Euclide* gives to this the name of *Composition*, which word is known many times to impart an *Addition*;

trigecuplam decupla esse triplam? … Proportionem autem aliquam tum demum esse alterius, duplam, vel triplam, &c. cum illius denominator huius denominatoris duplus est, vel triplus, &c. ita ut proportio decupla sit dupla quintuplae, & sextupla sit tripla duplae, &c. ostendemus ad finem lib. 9. tractatumque est hoc argumentum copiose a Radulpho Volumnio in Disputatione de proportione proportionum. Nunc satis sit, hoc ipsum communi hominum iudicio ex sensibilibus rebus confirmare. Si igitur Agens aliquod ad Patiens proportionem habeat verbi gratia decuplam, ita ut Agens sit 10. & Patiens 1. quis tam mente captus erit, quis non statim intelligat, si idem Agens augeatur, ut fiat 20. Patiens autem maneat 1. Agens tunc duplo maiorem habere potentiam respectu eiusdem Patientis, quam prius? Quare proportio vigecupla cuius denominator 20. duplus est denominatoris 10. dupla est proportionis decuplae, non autem proportio centupla, ut auctores contrariae sententiae volunt, sed tamen haec proportio centupla dicetur duplicata proportionis decuplae propter multiplicationem denominatoris 10 in se, & propter duas proportiones decuplas, quae inter numeros centuplam proportionem habentes interiiciuntur, ut hic apparet, 100.10.1" (*ibid.*, pp. 216–218).

[121] Wallis, *A Treatise of Algebra*, p. 83.

> (as when we say the line ABC is compounded of AB and BC;) some of our more ancient Writers have chanced to call it *Addition of Proportions*; and others, following them, have continued that form of speech, which abides (in divers Writers) even to this day: And the Dissolution of this Composition they call *Subduction of Proportion.* (Whereas that should rather have been called *Multiplication* and this *Division*). And then move Questions, How can it be, that a Proportion can, by Adding another to it, be made Less?, and that a Proportion made by the Addition of Two may be Less than either of them? … (Which, I confess … is a great Impropriety: for it makes the Part greater than the whole ….) Whereas they should have considered, that though *Composition* (in *Euclide*) do sometimes (not always) signify *Addition*; yet at other times, by *Composition* he means multiplication.[122]

What Euclid says of the composition of proportions is to be understood as composition by multiplication:

> This being premised, it is very manifest (and easy to demonstrate) that, *if between any Two Terms proposed* (as A, F,) *we intersperse never so many intermediate Terms* (as B, C, D E) whatever they be (whether all greater, or all less, or some greater and some less, than either A or F,) *the proportion of the Extremes is compounded of all the intermediates, each with his next consequent.*
>
> That is, $\frac{A}{B} \times \frac{B}{C} \times \frac{C}{D} \times \frac{D}{E} \times \frac{E}{F} = \frac{A}{F}$
>
> For all the intermediate Terms being found both above and below the Line, they do in continual Multiplication destroy themselves; nothing remaining as the result of such continual Multiplication but $\dfrac{A}{F}$.
>
> And hence follows *Euclid's* argumentation *ἐξἴσον* or *ex aequo* …. [123]

So on Wallis's interpretation of composition of proportions by multiplication, the proportions of proportions are the same as the proportions of their exponents:

[122] *Ibid.*, p. 84.
[123] *Ibid.*, p. 85.

> Proportions one to another, are in such Proportion as are their Exponents [i.e., denominations]; That Proportion is Greater, which hath the Greater Exponent, and that Lesser which hath the Lesser; and in such Proportion Greater or Lesser as are the Exponents.[124]

In this context, Wallis distinguishes between "double" and "duplicata", "triple" and "triplicata", so that it is clear when squaring or cubing and when multiplying by 2 or 3 is at issue for proportions. In thus explaining Euclid's meaning on the basis of Book VI, definition 5, Wallis helped to undermine the mathematical foundations of Bradwardine's rule.

CONCLUSION

In this paper, I have attempted to show how the introduction and fate of Bradwardine's dynamic rule was intertwined with the history of mathematics, in particular with ideas about proportions, proportionalities, and, later, ratios. Thomas Bradwardine, though he most likely knew the interpolated definition or calculational route to the compounding of ratios through the multiplication of denominations, chose to exclude it in order to present a consistent mathematical theory in which his rule for dynamics appeared mathematically very simple, deriving from the pre-Theonine version of Euclid's *Elements* as translated by Campanus.[125] In the theoretical atmosphere of the medieval university, the elaboration of an elegant theory took precedence over any question of actual measurements or calculation of absolute quantities. The few individuals who rejected Bradwardine's view in the two hundred years after it was proposed, for the most part, like Blasius of Parma, rejected its mathematical foundation in the pre-Theonine conception of compounding ratios, rather than finding it inconsistent with experience. (With regard to experience, many of the arguments against the Bradwardinian function applied equally to the Aristotelian position.)

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[124] *Ibid.*, p. 87.

[125] M. Clagett and M. McVaugh early on suggested that Bradwardine might have got the idea of correlating arithmetic changes of velocity with geometric changes in the proportion of force to resistance from the theory of al-Kindi concerning the degrees of compound medicines. Al-Kindi's theory was persuasively defended by Arnald of Villanova in his *Aphorismi de gradibus*, a 14th-century copy of which now exists in the Merton College library, although it is unknown how early it came to be there. See Michael McVaugh, "Arnald of Villanova"; and Arnald of Villanova, *Aphorismi de gradibus*. Neither al-Kindi nor Arnald of Villanova, however, defends the correlation of proportions of component parts of hot and cold to resulting effective degrees by reference to the mathematics of compounding proportions.

When Euclid's *Elements* was first printed by Ratdolt in 1482, it was in the translation of Campanus. But the new translation from the Greek published by Zamberti in 1505 was based on the Theonine version and so included Book VI, definition 5. This began to have an effect on those who used the Zamberti translation, although the perspective on compounding ratios found there was resisted by those who continued to prefer the Campanus version. The tide was finally turned by the translation of Commandino, published in 1572, who worked from a Theonine text. Most later work on the *Elements* was based on Commandino's Latin text or on a Theonine Greek text, until François Peyrard found what appeared to be a pre-Theonine Greek text in the early 19th century.[126]

The Renaissance and early modern revolution in the understanding of ratios that occurred in the 16th and 17th centuries had its origins in the belief that Commandino's text represented the "true" Euclidean text and, consequently, that definition 5 of Book VI represented the "true" Euclidean meaning of compounding ratios. Until recently scholars have not studied this second revolution in the understanding of ratios in the same way that they have studied the Greek revolution because, as heirs of the understanding of the conception of ratio that came out of this early modern revolution, they take the understanding of ratios that emerged, involving the arithmetization of ratios and the extension of the concept of number first to rational numbers and then to real numbers, as correct or true.[127] This has meant that, in hindsight, Bradwardine's conception of proportions and the compounding of proportions seems very strange and confusing, to use Dijksterhuis's words, quoted at the beginning of this paper. We have recently begun to study this early modern revolution – for instance, Enrico Giusti's 1993 *Euclides Reformatus* is rich with relevant information – so we are now in a better position to understand Bradwardine.[128]

When new editions and translations of Euclid and other Greek works led to a change in attitude toward proportions or ratios and the concept of number began to be extended beyond the realm of integers, the mathematical

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126 Murdoch, "Euclid: Transmission of the Elements" p. 450. As indicated above (note 26), Knorr argued that the medieval Arabic translations and the Latin translations based on them represent a Greek text of Euclid earlier than the version used by Peyrard, which he argues, may be a preliminary Theonine version.

127 A significant exception to this generalization is Giusti, *Euclides Reformatus*, which provides a marvellous wealth of materials for the early modern transition away from the conception of compounding proportions embodied in Bradwardine's rule. See also Goldstein, "A Matter of Great Magnitude"; Neal, *From Discrete to Continuous*.

128 Giusti, *Euclides Reformatus*. See also the whole issue of *Revue d'histoire des sciences*, 56/2, 2003. Sylla, "Compounding Ratios" may still be worth consulting for background.

foundations upon which Bradwardine erected his theory were undermined, and along with them his dynamical rule, all in separation from advances in physics moving away from the Aristotelian idea that a force is needed for motion and toward the law of inertia, even toward Newton's second law that force is proportional to acceleration, not velocity. Thus Bradwardine's law disappeared as quietly as it had first gained acceptance, more on mathematical than on physical grounds.

JÜRGEN SARNOWSKY[1]

CONCEPTS OF IMPETUS AND THE HISTORY OF MECHANICS

In the history of mechanics, different concepts of an impetus or *vis impressa*, i.e., a force impressed to the mobile, have been very influential. Since late Antiquity, impetus or *vis impressa* offered a solution for certain problems of motion not satisfactorily explained by Aristotelian natural philosophy. Though there is no clear line of influence, the idea of impetus can already be found in the work of the 6th-century philosopher Philoponus; it then recurs in Arabic sources and was seemingly rediscovered by the scholastic philosophers of the 14th century. Afterwards it found wide reception, even expanding into the technical literature of the 17th century.

Though the importance of impetus has sometimes been underestimated, in the view of some historians of science it played a crucial role or even became the very cornerstone of a new physics. For Thomas Kuhn in his *Structure of Scientific Revolutions*, the introduction of impetus marked one of the crises of Aristotelian physics that finally led to Galileo's theory of motion.[2] Hans Blumenberg similarly argued that the concept of impetus, which he defined as a late-medieval equivalent to the concept of inertia, was one of two central elements present in the later Middle Ages that paved the way for the theories of Copernicus.[3] Amos Funkenstein, while denying the equivalence of impetus and inertia, nevertheless saw the positive assumption of an impetus as a starting point for the method of idealization used in Galileo's explanation of free fall.[4] Pierre Duhem, himself a scientist and the first important historian of medieval science at the beginning of the 20th century, had been less cautious. According to him, the progress in dynamics mainly consisted in distinguishing the different elements mixed up in the scholastic idea of impetus, and this would only be accomplished by Leibniz, Huygens, and Newton, whereas the theories of Galileo and

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[1] Historisches Seminar, Hamburg. I wish to thank Sophie Roux and Roy Laird for their thorough and helpful remarks; the faults that remain are, of course, mine.
[2] "Galileo's contribution to the study of motion depended closely upon difficulties discovered in Aristotle's theory by scholastic critics" (Kuhn, *The Structure of Scientific Revolutions*, p. 67); that this refers to the concept of impetus becomes clear by the remarks on Galileo (*ibid*., pp. 119–120).
[3] Blumenberg, *Die Genesis der kopernikanischen Welt*, vol. 1, p. 173.
[4] Funkenstein, *Theology and the Scientific Imagination*, pp. 166–171, 174–175; in general see also *id*., "Some Remarks on the Concept of Impetus". For the problem of equivalence, see also Dijksterhuis, *The Mechanization of the World Picture*, pp. 182–184; Maier, *Zwei Grundprobleme*, pp. 218–219; Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, pp. 400–402.

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 121–145.

Descartes "still implied all the different thoughts which the masters of Paris in the middle of the 14th century mixed up under the name of impetus".[5] But in 1978 Michael Wolff went even one step further. Based on a remark by Alexandre Koyré in his *Études galiléennes*, who had suggested distinguishing between three stages in the early development of physics (and three modes of thought – *types de pensée*), with the "physics of impetus" as an intermediate between Aristotelian physics and the mathematical and experimental physics of Archimedes and Galileo,[6] Wolff wrote a history of the concept of impetus as the history of a certain mode of thought, or rather of a new system of natural philosophy, starting with Philoponus, in which the explanation of projectile motion plays a central role.[7] Wolff rejected any empirical explanation for the changes from Aristotelianism to the physics of impetus, for him, the Scientific Revolution was in fact closely connected with an economic and technical revolution.[8]

With these theories as background, especially those of Blumenberg and Wolff, the aim of this paper is to demonstrate that there was no single concept or theory of impetus, nor even a "physics of impetus" as proposed by Wolff, but rather that impetus was always something like an ad hoc solution for certain mechanical problems dependent on the relevant theoretical (mostly Aristotelian) basis. Thus this paper will review the different concepts of impetus or *vis impressa* in scientific texts from the 6th to the 18th centuries and discuss their use in mechanics and natural philosophy.[9] But first, the problem will have to be developed in its Aristotelian context.

THE PROBLEM OF PROJECTILE MOTION IN ARISTOTLE'S *PHYSICS*

When René Descartes discussed the law of inertia, he was so far from Aristotelian physics that he was nearly at loss about the significance of the earlier discussions: "We are free of the trouble in which the Learned found

[5] Translated from Duhem, *Le Système du monde*, vol. VIII, p. 225; see also the remarks of Maier, *Zwei Grundprobleme*, pp. 218–219.
[6] Koyré, *Études galiléennes*, p. 16. For Koyré, this was clearly not intended to be a model of the history of science in general, and he used it only to explain the developments in renaissance physics.
[7] Wolff, *Geschichte der Impetustheorie*, esp. pp. 29–31, 157.
[8] *Ibid.*, pp. 342–343. Quite recently, Klaus-Jürgen Grün proposed another approach to the concept of "impetus" as a certain mode of thought – see his *Vom unbewegten Beweger zur bewegenden Kraft* – which has not been considered here.
[9] The term "impetus" gains its specific meaning only about 1355; see below and Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, p. 382.

themselves when they wanted to find out the reasons why a stone continues to move for some time after being away from the hand of the one who had thrown it, because for us it is rather necessary to ask why it does not continue to move forever".[10] This statement points to a different concept of motion in Aristotelian and classical physics. One main difference results from two principles concerning the cause of motion formulated by Aristotle in Book VII of the *Physics.* The first one, the principle *OMAM* (quoted after the initials of the Latin version ***O****mne quod* ***M****ovetur ab* ***A****liquo* ***M****ovetur* or similar versions), reads: "Everything that is in motion must be moved by something".[11] Whereas in classical physics – according to the principle of inertia – one has to explain why motion stops and not why it continues, for Aristotle and his followers every motion or change needs a cause. This is reinforced by the second principle, *MMS* (or: ***M****ovens et* ***M****otum sunt* ***S****imul*), according to which "that which is the first movement of a thing... is always together with that which is moved by it (by 'together' I mean that there is nothing intermediate between them)".[12] Thus for Aristotle, motion or change requires an ontologically distinct mover that has to be continuously in contact with the thing moved, which means that motion has to be continuously regenerated.

Though Aristotle himself restricted the validity of the principle *MSM* in the case of the first unmoved mover, which is not in contact with everything it moves but which had to be introduced to avoid an infinite regress following from the principle *OMAM*, his commentators mostly followed in his footsteps and accepted both principles. When Galen criticized the principle *OMAM* and maintained that self-movement in a proper sense is possible, this was first refuted by Alexander of Aphrodisias and later on by Averroes.[13] Averroes argued that Galen had misunderstood the Aristotelian concept of motion *per se* (which meant that the mobile body is moved as such and not in or with another body). Averroes's defence of the Aristotelian principles became very influential in scholastic thought.[14] Thus, for example, his theory of magnetism, designed to fit the principle *MSM*, was widely accepted. For Averroes, the attractive force of a magnet reaches the iron through the medium, which is also influenced, but in a different way; therefore, magnetism does not involve action at a distance.[15] In general, the

[10] Descartes, *Le Monde ou le Traité de la Lumière*, in *Oeuvres*, vol. XI, p. 41.
[11] Aristotle, *Physics*, 241b34; trans. R. P. Hardie and R. K. Gaye.
[12] Aristotle, *Physics*, 243a3–6; trans. R. P. Hardie and R. K. Gaye.
[13] See Pines, "*Omne quod movetur necesse est ab aliquo moveri*".
[14] Weisheipl, "*The Principle* Omne quod movetur ab alio movetur".
[15] Goddu, "Avicenna, Avempace, and Averroes".

Aristotelian principles formed the basis for all subsequent discussions on the causes of motion until the time of Galileo.

But magnetism was not the only touchstone for the Aristotelian principles; there were also the problems of the movers in projectile motion and free fall. In Book VIII of the *Physics* Aristotle asked: "If everything that is in motion with the exception of things that move themselves is moved by something else, how is it that some things, for example, things thrown, continue to be in motion when their movement is no longer in contact with them"?[16] In an effort to save the principles *OMAM* and *MSM*, Aristotle rather hesitatingly advanced two solutions for the problem, both involving the air as the immediate cause of projectile motion. The first one is a theory of mutual replacement (*antiperistasis*), which he probably rejected and did not discuss further,[17] according to which the air comes around behind to push the mobile forward, as was explained by Simplicius in the 6th century.[18] According to the second theory, the different layers or parts of the air are not only moved by the original mover but also receive the power to act as a mover in a double sense, i.e., to move the projectile and to convey its faculties to the next layer or part. Thus the projectile is carried along by the layers or parts of the air until their power to act as a mover ceases. It follows that, contrary to the assumptions both of Hans Blumenberg and Michael Wolff, the central theoretical elements in Aristotle's explanation of projectile motion are not only the principles *OMAM* and *MSM* (and therefore a permanent cause and contact), but he also assumed a double kind of transmission: the first layer of the air not only receives from the original mover the faculty to move the projectile but also the ability to transmit this faculty to the next layer.[19] Therefore, the transmission of – "immaterial" – faculties or forces played a decisive role already for Aristotle and was not "invented" by his later commentators, who only discarded the additional corporeal element, the air. If one wanted to preserve the two principles of motion, the question was rather what were the objects of the transmission: the body and the layers of the air, or only the moved body?

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[16] Aristotle, *Physics*, 266b28–30; trans. R. P. Hardie and R. K. Gaye.
[17] Already introduced in *Physics*, 215a14–17, then mentioned at 267a14–20.
[18] Clagett, *The Science of Mechanics*, pp. 507–508; a translation of the passage from Simplicius can be found in Simplicius, *On Aristotle on the Void*, p. 193.
[19] Blumenberg, *Die Genesis der kopernikanischen Welt*, vol. 1, p. 173, who speaks of "begleitende" and "übertragene Kausalität"; Wolff, *Geschichte der Impetustheorie*, p. 17, who redefines these terms to "Berührungskausalität" and "Übertragungskausalität".

THE *VIS IMPRESSA* IN THE WORK OF PHILOPONUS

Though Aristotle's theory was seemingly in accordance with the special nature of air (i.e., that it is apt to move and be moved), many later authors were not satisfied with the Aristotelian solutions. Johannes Philoponus (John the Grammarian) in his commentary on the *Physics* rejected both of Aristotle's explanations and proposed another theory to fit the principles of motion. Already in Book II, he criticized the idea that the mover has always to be outside of the thing moved, and in Book IV, in the discussion about the possible existence of a void, he rejects the role of the air as a decisive factor in violent motion. Since for him the air resists rather than moves, he supposes that the original mover impresses a kind of incorporeal force in the moved body (and not in the medium). This impressed force (*dynamis endotheisa*) explains why the movement, for example, of an arrow continues after the projectile has lost contact with the original mover, and thus it functions as the immediate cause according to *OMAM* and *MSM*. Since the impressed force is spent by resistance, the projectile motion finally comes to an end.[20] Philoponus tried to illustrate the possibility of transmitting an incorporeal force to a body by another example, this time from optics: when a ray of the Sun falls through coloured glass, a stone facing the glass appears to have the same colour, so that the colour is transmitted to the stone.[21]

It is in the same context that Philoponus uses his concept of an impressed force to explain natural motions and the motion of the heavens: gravity and levity are nothing else than forces impressed by the Creator, and in creation God also impressed an incorporeal force on the spheres of the heavens, which have moved ever since.[22] In another context, perhaps directed against Kosmas Indikopleustes, Philoponus argues against the hypothesis of angels that move the heavens and the elements: "Is there anything more ridiculous than that? It is not impossible, that God who created the Moon, the Sun, and the other stars impressed in them a moving force, as in heavy and light bodies a force to fall down or to rise, as in all creatures the movements caused by the soul incorporated in them, so that the angels do not have to move them violently. Everything that is not moved by nature is in fact moved by force and against nature and is [finally] destroyed. How should

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[20] Wolff, *Fallgesetz und Massebegriff*, p. 41, pp. 45–46, 54–57; see *id.*, *Geschichte der Impetustheorie*, pp. 67–68; Maier, *Zwei Grundprobleme der scholastischen Naturphilosophie*, pp. 120–121.

[21] For this see especially the Arabic version of Philoponus's commentary (Lettinck and Urmson, *Philoponus*, p. 9).

[22] Wolff, *Geschichte der Impetustheorie*, pp. 68–69.

angels have the patience to carry along so many and so heavy bodies for such a long time and with the use of force"?[23]

Philoponus was indeed – as far as we know – the first philosopher to suppose a transmission of forces not in the medium but rather in the moving bodies, to explain violent motion, and he was also the first to transfer the concept of an impressed incorporeal force to celestial and other natural motions. This might have been a revolution, if he had written a new *Physics* in analogy to Newton's *Principia*, but he did not. He mentions his theory quite in passing in the discussion of motion in the void in his commentary on Book IV of Aristotle's *Physics*, and there is no indication – not even in the fragments preserved in Arabic[24] – that there was a more expanded discussion of the problems involved in Books VII and VIII, which are today lost. In the terms of Thomas Kuhn, this was "normal" Aristotelian science, dealing with the puzzle of how to explain the continuation of projectile motion after the first mover had been removed. By analogy, turning to celestial and natural motions, Philoponus found it equally implausible to have to call on angels to explain the motion of the stars or of heavy and light bodies. Like scholastic philosophers he based his arguments on plausibility, arguing from the imaginary case of a void or of angels moving the celestial bodies.[25] Philoponus created no new system of natural philosophy;[26] nevertheless he introduced several new elements into the Aristotelian theory of motion: the transmission of forces into a moving body instead of the medium, the explanation for the continuation of motion without any external cause, and the use of similar concepts for terrestrial and celestial motions, as well as for violent and natural motions. Though there were some modifications in the later theories, these elements reappeared in many later discussions of the Aristotelian theory of projectile motion.

ARABIC VERSIONS OF THE CONCEPT OF AN IMPRESSED FORCE

At first, Philoponus's solution of the puzzle of projectile motion had some impact on the physical doctrine of the Arabs. Aristotle's *Physics* was translated into Arabic several times during the 9th and 10th centuries and

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[23] Philoponus, *De opificio mundi*, I, 12, pp. 28–29; see Wolff, *Geschichte der Impetustheorie*, pp. 69–70.

[24] See the fragments translated in Philoponus, *On Aristotle's Physics 5–8*, pp. 113–135, 135–136; for the context of transmission, see *ibid.*, pp. 3–6.

[25] For the more developed scholastic arguments see Hugonnard-Roche, "L'hypothétique et la nature".

[26] Here I follow Kuhn, *The Copernican Revolution*, p. 119; Maier, *Zwei Grundprobleme*, p. 126, against Wolff, *Geschichte der Impetustheorie*, p. 157.

soon found commentators such as al-Fârâbî, Ibn Sîna (who was known as Avicenna in the Latin West), Ibn Bâjja (Avempace) and Ibn Rušd (Averroes). But the Greek commentaries were also translated and studied by the Arabs, including those of Alexander of Aphrodisias, Themistius, and Johannes Philoponus, though only fragments of the latter's works have been preserved.[27] In this way at least Philoponus's proposition of an impressed force in Book IV of his commentary should have been known. This is supported by the fact that the surviving fragments of the Arabic translation are accompanied by several other commentaries of the 10th and early 11th centuries.[28]

Most of the earlier commentaries are lost, but there are some indications that al-Fârâbî (who died in 950) may have been familiar with the concept. Thus he uses the term "*mail qasrī*" or "violent inclination", which became an Arabic substitute for the impressed force in Philoponus.[29] Finally, there was Avicenna, who at the beginning of the 11th century was obviously well acquainted with Philoponus's solutions, though he does not follow him closely. In his commentary on Aristotle's *Physics* in the context of the *Kitâb al-Shifâ*, the "Book of the Healing", he not only rejects both Aristotelian explanations but also describes three theories that explain the continuation of projectile motion by intrinsic factors. The first of the three comes nearest to that of Philoponus. According to this theory, the continuation of projectile motion is caused by the force that the mobile acquires from the mover and that persists until it is overcome by the natural inclination of the body and by the resistance of the medium. But in that case, Avicenna objects, the force would be more strongly active in the beginning, not, as he derives from experience, in the middle of the movement. He also rejects as "distasteful" a second theory, which supposes elementary parts of motion that have a tendency to follow one another after short periods of rest. His own theory is the third one: "But when we verified the matter we found the most valid opinion to be that of those who hold that the moved receives an inclination (*mail*) from the mover. The inclination is that which is perceived by the senses to be resisting a forceful effort to bring natural motion to rest or to change one violent motion into another".[30] Thus for him the inclination

[27] See Lettinck and Urmson, *Philoponus*, pp. 3–5; Zimmermann, "Philoponus's Impetus Theory".
[28] See also Pines, "Un précurseur bagdadien de la théorie de l'*impetus*".
[29] Pines, "Les précurseurs musulmans de la théorie de l'*impetus*", p. 301.
[30] For the Arabic version, see Ibn Sinâ, *Kitâb al-Shifâ* (published in Teheran in 1885), vol. 1, pp. 154–155; the translation used here is that of Clagett, *The Science of Mechanics*, pp. 511–512; for the context, see *ibid.*, pp. 510–513.

(*mail*) is something different from force; it is rather an instrument of the force of the mover to communicate its action to the thing moved. It is in this context that Avicenna distinguishes between three kinds of *mail*: psychic, natural, and unnatural or violent. Thus, like Philoponus, he extends his concept of inclinations to natural motions as well. The *mail* is the not self-exhausting, but destructible immediate cause of the movement, persisting if there were no resistance – as when operating in a void. But Avicenna does not keep to his differentiation between force and inclination, since he also uses the term "impressed force" as a synonym for "violent inclination", for example, when he compares the transmission of a force into the projectile with the transfer of the heat of fire into water.[31] This inconsistency was very influential in the later Arabic philosophers who discussed their concept of *mail*.

In his theory of projectile motion, Avicenna was followed mainly by the Eastern philosophers, not in Spain, where the sole exception is Al-Bitrûjî (Alpetragius) around 1200.[32] But there were also some modifications. Abu'l-Barakât (who died about 1164), contrary to Avicenna, supposed that the impressed force is not only diminished by resistance but also exhausts itself. Thus projectile motion was possible in a void since it would come to an end after the *mail* had exhausted itself. Self-exhaustion, therefore, was the third factor that acted as resistance in violent motions, beside the opposing natural tendency of the body and the resistance by the medium. Avicenna was also convinced that there could only be one inclination in one moving body. As a consequence, according to him, in the movement of a heavy body thrown upward there had to be a momentary rest after the original violent inclination had been destroyed, and only afterward did the gravity of the body introduce a natural *mail* acting as a cause for the fall. Abu'l-Barakât opposes the assumption of a momentary rest and proposes the possible existence of opposite inclinations in one body, as when an object is pulled violently from different directions. For him, the natural inclination coexists with the violent one, and while at first during the rise of the heavy body the violent is dominant, after the beginning of the fall the natural inclination is stronger, while the violent offers less and less resistance so that the body accelerates.[33] But these are only small modifications since the general thesis of the transmission of forces in violent and natural motions was not questioned.

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31 See Pines, "Les précurseurs musulmans de la théorie de l'*impetus*", p. 302, who points to Avicenna's inconsistency.

32 Maier, *Zwei Grundprobleme*, p. 129; Pines, "Études sur Awhad al-Zaman Abu'l Barakât al-Baghdâd".

33 Clagett, *The Science of Mechanics*, pp. 313–314.

After Avicenna, several other Arabic philosophers in the East favourably discussed the concept of *mail*, and there is also one allusion to it in the *Planetary Theory* of Al-Bitrûjî.[34] In discussing the transmission of moving forces from the outermost sphere of the heavens to the inner spheres in which the forces are more and more diminished, Al-Bitrûjî draws a parallel to projectile motion and points to the diminution of the moving force in an arrow in relation to the distance from the original mover.[35] The influence of the concepts of *mail* on Islamic philosophers obviously continued even into the 17th century.

THE FIRST APPEARANCE OF IMPRESSED FORCES IN THE WEST

Neither the ideas of Philoponus nor the Arabic concepts of *mail* may have reached the Latin West, though the lines of influence are not clear. If there were Latin versions of the relevant texts at all, the allusions to an impressed force or to the concepts of *mail* were not very significant. Philoponus's commentary on Aristotle's *Physics* was translated into Latin only in the 16th century and appeared in print in Venice in 1539, and this translation contained only Books I–IV.[36] It seems that the Greek text was not known, and even then, only a few scholastic philosophers would have been able to read it.[37] As for the Arabic concepts of *mail*, its main source, the *Kitâb al-Shifâ* of Avicenna, was only partly translated into Latin, as *Sufficientia*, with in particular the commentaries on Books V–VIII of the *Physics* missing, while Avicenna treats his concept of *mail* in Book IV only in passing.[38] The same holds true for the short remarks of Al-Bitrûjî in his *Planetary Theory*, which were only summarized in the translation of Michael Scotus and offer no clear hint for a change in the explanation of projectile motion, and for the commentary of Abu'l-Barakât as well, which was never translated.[39]

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[34] Pines, "Études sur Awhad al-Zaman Abu'l Barakât al-Baghdâd", 4, pp. 12–15.
[35] Maier, *Zwei Grundprobleme*, pp. 127–129, based on the different Latin translations.
[36] Philoponus, *Commentaria in primos quatuor libros Aristotelis*.
[37] Wolff, *Geschichte der Impetustheorie*, p. 157, claims that Philoponus was forgotten in the West because of his condemnation of 680, but as for other texts, such as that of Pseudo-Dionysius the Areopagite, whose authors or translators were condemned, it is clear that a Latin translation was a precondition for any significant Western reception.
[38] Avicenna, *Sufficientia*, fols. 13ra–36vb.
[39] Maier, *Zwei Grundprobleme*, p. 128, against Duhem, *Le Système du monde*, vol. VIII, pp. 173–174. Another field of influence was the science of weights, but the role of Jordanus de Nemore is not quite clear; see Giannetto et al., "*Impulsus* and *Impetus*".

But even if there were no literal links, for authors who were not satisfied with the Aristotelian solutions it may have been quite obvious to assume a different explanation of how motion is passed from one body to another and why projectile motion continues. Thus since the 13th century, solutions for the problem different from the Aristotelian approach were discussed, though mostly in a negative form. Anneliese Maier has suggested that the concept of impetus may have been something like a "natural solution" for the earlier scholastics, but that it was never supported in a "doctrinal context" in philosophy, i.e., in relevant passages of commentaries on Aristotle, but rather only in texts discussing the problem without further implications.[40] For example, Thomas Aquinas on the one hand closely followed the Aristotelian explanation of projectile motion in his commentary on Book VIII of Aristotle's *Physics*, and he explicitly rejected the assumption of an internal cause in Book III on *De caelo* because according to him all violent motions must have an external cause:[41] "However, it ought not to be thought that the force of the violent motor impresses in the stone which is moved by violence some force (*virtus*) by means of which it is moved, as the force of a generating agent impresses in that which is generated the form which natural motion follows. For [if] so, violent motion would arise from an intrinsic source, which is contrary to the nature (*ratio*) of violent motion".[42] On the other hand, there are very different allusions to a concept of impetus in a theological context. In his *Quaestiones disputatae*, Thomas turns to the problem of the origin of the soul: whether it exists by creation or is transferred by the sperm. In referring to the principle *MMS*, he discusses the influence of the agent acting by an instrument and compares it to the motion of an arrow, which will continue until the force impressed by the agent (*vis impulsus proicientis*) expires. Also, in another context, he points to the fact that the range of projectile motion is determined by the power of the mover and he compares violent and natural motions in respect to their inclinations towards certain targets.[43] These

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[40] "In den Fällen, wo es nicht um eine doktrinale Erörterung des Problems geht, sondern wo lediglich das Phänomen als solches berührt wird, erscheint offenbar der Hochscholastik die Auffassung im Sinne der Impetushypothese als die nächstgelegene und die dem natürlichen Denken am meisten entsprechende" (Maier, *Zwei Grundprobleme*, p. 140).

[41] See Thomas Aquinas, *In octo libros de physico auditu*, 8, lectio 22, nn. 2510–2521, pp. 535–536.

[42] Thomas Aquinas, *In Aristotelis libros de caelo et mundo*, 3, lectio 7, p. 305; trans. Clagett, *The Science of Mechanics*, pp. 516–517.

[43] Duhem, *Le Système du monde*, vol. VIII, pp. 182–183; Maier, *Zwei Grundprobleme*, pp. 134–137.

elements were to return later in discussions favourable to the concept of an impressed force.

The ambiguity of Thomas returned, at the end of the 13th century, with the Franciscan Petrus Johannes Olivi. In a question on the second book of the *Sentences* of Peter Lombard discussing the principle *MMS*, he refers to a theory according to which there is a kind of "similitude" or "impression" of the mover flowing in the mobile that successively causes the inclination of the moved body towards the target of the motion. This seems to be related to the idea of an influx from the mover into the thing moved and thus a reference to a more general emanation theory, which Olivi probably did not defend himself,[44] though he states the argument that the force of the motor could only act by an impression that is an "actual or formal applying of the mobile to the terminus of motion", not *per se*.[45] This theory reported by Olivi relates to the *species sensibiles* that are proposed in the transmission of light in the medium, and therefore, as in Philoponus's example of the rays of the Sun falling through coloured glass, there is an allusion to optics in the context of the explanation of projectile motion.[46]

In another text, an anonymous question dedicated to the ontological status of motion and ascribed with certainty to him by Anneliese Maier, Olivi deals with the same problems as in the questions on the *Sentences*, but explicitly rejects the inclination theory quoted there and formulates his own theory of motion, according to which local motion is nothing absolute or positive except the thing moved and the space traversed.[47] In the third part of his discussion Olivi turns against the explanation of violent motion by an internal cause – for otherwise there would be no violent motion – and in the fourth he explicitly argues against concepts of an impressed force:

> If indeed local motion were [caused] by some form impressed in the moved body by the mover or somehow caused in it, then it would be necessary that it is in the whole body and

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[44] Clagett, *The Science of Mechanics*, pp. 517–519; Maier, *Zwei Grundprobleme*, pp. 142–153; against Wolff, *Geschichte der Impetustheorie*, pp. 184–191, who claims that Olivi was one of the exponents of the concept of impetus.

[45] I follow the translation in Clagett, *The Science of Mechanics*, p. 518; for Olivi, see Maier, *Zwischen Philosophie und Mechanik*, pp. 291–296.

[46] Maier, *Zwischen Philosophie und Mechanik*, p. 344.

[47] "Utrum motus localis dicat aliquid absolutum supra mobile ipsum quod movetur localiter", quoted in Maier, *Zwischen Philosophie und Mechanik*, p. 290; for the analysis, see *ibid.*, pp. 296–298.

> all its parts moved locally, as the motion of generation and alteration is in all parts of the matter moved at an substantial or accidental terminus. But this is impossible [… because] it is indeed impossible that a motion is impressed if not in the part or in the parts that [the mover] touches substantially or virtually, but the mover does not touch all parts of any one thing moved substantially or virtually. But if it is not impressed in all [parts], it is not in all parts that there will be a form inclining at the terminus of motion, and thus the whole is not mobile.[48]

A positive version of this argument returned in the concept of impetus of Nicole Oresme and his followers beginning in the 14th century, who use it to explain the imagined phenomenon that projectile motion is at first accelerated, but it is clearly in a "doctrinal context" that Olivi formulates his rejection of the concepts of an impressed force. As for Ockham later, it is clear for Olivi that it is not necessary to introduce additional ontological entities to explain projectile motion.[49]

It may therefore be no accident that it was in a theological context – the instrumentality of the sacraments – that there was the first positive assumption of an intrinsic principle in violent motions. When in his commentary on the *Sentences*, Franciscus de Marchia discusses the effect of the sacraments and is enquiring into the possibility of action at a distance, he also extensively refers to the problem of projectile motion, using it as an illustration for the continued action of the sacraments. For him, is it clear that the force that moves the projectile is caused by the original mover, but he poses the question whether this force is situated in the medium or in the body itself. He finally decides that the original mover "leaves" a force in the projectile (a *virtus derelicta*). As in the theory of Abu'l-Barakât, this force expires by itself after some time, like heat in water.[50]

> And if one asks of what sort is a force of this kind, it can be answered that it is not simply permanent nor simply fluent, but almost medial [between them], since it lasts for a certain

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[48] Maier, *Zwischen Philosophie und Mechanik*, pp. 304–305.

[49] See Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, p. 389; Funkenstein, "Some Remarks on the Concept of Impetus", pp. 337–340; it is not possible to treat Ockham's interesting solutions here because in Ockham there is only a negative relationship to impetus.

[50] Maier, *Zwei Grundprobleme*, pp. 195–197; Clagett, *The Science of Mechanics*, p. 520; Wolff, *Geschichte der Impetustheorie*, pp. 192–198.

> time.... It seems preferable that a force of this kind resides in the body which is moved rather than in the medium, regardless of what the Philosopher and the Commentator have said on this matter, because it would be in vain that something should be done by many [causes] which can be done by few – now it does not appear necessary to posit something other than the moving body or the force received in it and the original motor as the effective cause[s] of motion; therefore, the medium is not [the cause]; [also] because in positing this, all phenomena are accounted for better and more easily (*melius et facilius salvantur omnia apparentia*).[51]

In these phenomena Franciscus even included celestial motions, though differently from Philoponus: since his *virtus derelicta* expires by itself, he sticks with the angels or intelligences who move the heavens using impetus as a kind of instrument. As in projectile motion, Franciscus distinguishes between primary and secondary factors. His remarks are not an objection to Aristotle, but are obviously intended as additional explanations. Though speculative and often misleading or confused, Franciscus's discussion was the starting point for a new kind of explanations of projectile motion, by (different) concepts of impetus, but which had much in common with their Arabic antecedents.

THE "CLASSICAL" CONCEPT OF IMPETUS: BURIDAN AND ALBERT OF SAXONY

Although Franciscus de Marchia did not himself transfer his idea of a *virtus derelicta* into the context of the Aristotelian *Physics*, nevertheless, immediately after he had commented on the *Sentences* in 1319/1320, other authors discussed an impressed force in projectile motion.[52] Thus, in their commentaries on the *Sentences*, Franciscus de Mayronis and Humbertus de Garda rejected concepts of an impressed force, while Boneti, in his *Philosophia naturalis*, and Johannes Canonicus, in Book IV of his commentary on the *Physics*, obviously supported solutions like that of Franciscus de Marchia.

It is likely that Franciscus's solution was also known to Jean Buridan, Albert of Saxony, and others, and a certain concept of an impressed force

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[51] The text is quoted in Maier, *Zwei Grundprobleme*, pp. 172–173; trans. in Clagett, *The Science of Mechanics*, p. 529.

[52] Maier, *Zwei Grundprobleme*, p. 161, n. 2, refers to a short *Compilatio super libros physicorum* of Franciscus without any hint to a *virtus derelicta*; for the reception in the 1320s, see *ibid*., pp. 197–200.

thus became very influential as one of the main characteristics of the "Parisian school" of natural philosophy. Though Buridan was the elder, their commentaries on the *Physics* are closely related to each other. Albert, when commenting on the *Physics* in or shortly after 1351, probably used Buridan's *Tertia lectura*, and it seems that Buridan reacted to Albert in his *Ultima lectura*, which can be dated to 1355/1358.[53] It was in this process that the "classical" concept of impetus was formed and the term "impetus" gained its specific meaning.

In Book VII of his *Tertia lectura*, Buridan discusses the problem whether the projectile is moved by an external or by an internal mover, and in his negative arguments at the beginning of the *quaestio*, he refers to the point of view that an internal principle for projectile motion would call the essential difference between natural and violent motions into question. On the other hand, he refutes both Aristotelian explanations of projectile motion and states that the air rather resists than furthers motion. He finally concludes that any projectile is moved by a force impressed by the original mover, and he derives some corollaries, for example, that if there is more matter *ceteris paribus*, a greater force can be impressed.[54] When Albert of Saxony commented on the problems of projectile motion, he transferred the discussion to Book VIII and changed the style of the *quaestio*, nearly renouncing the *sic-et-non* scheme. He asks what the cause is of the continuation of projectile motion after the separation from the original mover and refutes not only both Aristotelian theories but also that of Olivi or Ockham, which explains projectile motion by the influence of the mover.[55] His positive solution is close to that of Buridan: the projectile is moved by a certain moving force impressed in it by the original mover, "which [force] is a quality that innately moves".[56] Albert refers to the experience that a stone is thrown farther than feathers *ceteris paribus* because it contains more matter and thus receives more of the moving force.

Buridan responded to Albert in a more elaborate discussion of the problem in his *Ultima lectura* on Aristotle's *Physics*, also in Book VIII. There, he uses impetus as a technical term and gives the "classical" description of the concept of "impetus":

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[53] For these textual relations see Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, pp. 49–52 and pp. 54–60; Thijssen, "The Buridan School Reassessed". Buridan's earlier commentaries have survived only partially.

[54] Maier, *Zwei Grundprobleme*, pp. 370–378; Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, p. 390.

[55] Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, pp. 391–393.

[56] Albert of Saxony, *Expositio et guæstiones in Aristotelis physicam*, vol. 3, p. 1074.

> Thus we can and ought to say that in the stone or other projectile there is impressed something that is the motive force of that projectile. And this is evidently better than falling back on the statement that the air continues to move that projectile. For the air appears rather to resist. Therefore, it seems to me that it ought to be said that the motor in moving a moving body impresses a certain impetus or a certain motive force of the moving body, [which impetus acts] in the direction toward which the mover was moving the moving body, either up or down, or laterally, or circularly. And by the amount the motor moves that moving body more swiftly, by the same amount it will impress in a stronger impetus .... But that impetus is continually decreased by the resisting air and by the gravity of the stone ....[57]

Buridan and Albert, like Avicenna, did not conceive the impressed force or impetus as a self-expending quality, which means that according to them it can only be diminished by resistance, and like Philoponus and Franciscus de Marchia before them, they also applied their concept to the celestial movements, even going one step further than Franciscus, who limited himself to intelligences. Since impetus does not expire where there is no resistance, it is no longer necessary to call on the concept of intelligences (or angels) moving the celestial bodies: when God created the universe, he impressed an impetus in them, which has moved them ever since. Therefore, Buridan and Albert use a concept developed for violent motions to account for the natural motion of the heavens and also, like Abu'l-Barakât and his Arabic successors, to explain the acceleration in the fall of heavy bodies.[58] In Buridan's commentary on Aristotle's *De caelo*, we read:

> One must imagine that a heavy body not only acquires motion unto itself from its principal mover, that means, its gravity, but that it also acquires unto itself a certain impetus with that motion. The impetus has the power of moving the heavy body in conjunction with the permanent natural gravity. And because that impetus is acquired in common with motion, hence the swifter the motion is, the greater and stronger the impetus is .... Consequently, [the heavy body] is moved more swiftly ....[59]

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57 Buridan, *Questiones*, Book VIII, qu. 12, fol. 120vb; trans. in Clagett, *The Science of Mechanics*, pp. 534–535.

58 Maier, *Zwei Grundprobleme*, pp. 219–223, 266.

59 Buridan, *Expositio et quaestiones in Aristotelis* De caelo, Book II, qu. 6, p. 443; trans. in Clagett, *The Science of Mechanics*, pp. 560–561.

Thus the acceleration of falling bodies is caused by an impetus that is continually increased. Albert of Saxony, following Nicole Oresme and using mathematical formulations, developed a quantitative solution for the acceleration in the fall of heavy bodies and discussed thought experiments that demonstrate the violent character of the impetus in this respect: for example, a heavy body falling down a hole through the earth, continuing its motion past the centre, and ascending again moved by the impetus, and thus oscillating for some time through the centre of the world.[60]

Though the concept of impetus thus had wide-ranging implications, touching upon both violent and natural or terrestrial and celestial motions, it ought not to be forgotten that in the textual tradition of the commentaries on Aristotle's *Physics* and *De caelo*, the question concerning the problem of projectile motion is only one of many, often of more than a hundred others.[61] In many respects, the Aristotelian foundation remained intact, the concepts of impetus being rather an effort to find an explanation of projectile motion that better fitted the principles *OMAM* and *MMS*. Compared to other new developments in medieval physics, such as the discussions of the proportions in motion or the geometrical representation of qualities, the series of concepts of impetus never reached a similar textual independence. Nevertheless, it brought with it serious changes in the Aristotelian principles of motion and in the difference between natural and violent, and terrestrial and celestial motions, even though they were not fully realized.

THE SPREAD OF CONCEPTS OF IMPETUS IN THE WEST

Concepts of impetus at first became a characteristic of certain Parisian philosophers, not only of Buridan and Albert, but also of Nicole Oresme and the youngest of the four, Marsilius of Inghen. Though there were some "doctrinal" differences – for example, Oresme and Marsilius supposed that the impressed force exhausts itself – their writings were often received together. Of course, there were also some critics who clearly opposed the idea of an impressed force, such as the Averroist Theodoric of Erfurt, who in his commentary on the *Physics*, perhaps written in Paris about 1341, denied that impetus could be a quality of one of the possible species (that means *habitus* or *potentia naturalis*) and pointed to the fact that the impression

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[60] Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, pp. 396–398; Drake, "Free fall".

[61] For example, the commentary of Albert has 114 questions, only the last one being dedicated to the concept of impetus; see Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, p. 382, n. 308.

of a force would be an alteration of the moved body, obviously implying that no local motion would result.[62] But Albert became the first rector of the university of Vienna, in 1365/1366, and Marsilius the first rector of Heidelberg, in 1386, and their teachings, together with those of Buridan (and Oresme), spread at the central European universities, in Poland, Italy, and Scotland, and their commentaries became obligatory – at least for a period of time – in Prague, Vienna, Leipzig, Krakow, Freiburg, Cologne, and St. Andrews.[63] At the University of Erfurt, finally founded in 1392, the concept of impetus counted as one of the subjects that a bachelor had to study.[64]

Thus many commentaries on Aristotle's *De caelo* or *Physics* were influenced by the solutions of the "Parisian school" of natural philosophy:[65] Italian authors such as Biagio Pelacani of Parma (Blasius of Parma) or Paulus Venetus (Paolo Nicoletti) – though rather hesitatingly – accepted an impressed force as cause of the continuation of projectile motion,[66] the Scot Lawrence of Lindores positively discussed a concept of impetus in a *dubium* concerning the unmoved mover, while suggesting that this theory had already been put forward by Aristotle,[67] and the Krakow master Benedykt Hesse, relying on Lawrence, also followed one of the concepts of impetus.[68]

Nicolaus Cusanus uses his own concept of impetus when he explains in his *De ludo globi* the motion of a globe that is not regularly shaped, but both convex and concave, and that follows a spiral line. The globe is thrown into a series of circles and – according to the rules of the game – has to come to rest in a circle as close to the centre as possible. The difference of the motions is caused by the difference of the moving force – the stronger it is, the straighter is the line of motion. After some more general reflections concerning the spherical shape of things, the Cardinal returns to the problem, stating that

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[62] See Sarnowsky, "Ein Albert von Sachsen zugeschriebener Physikkommentar", pp. 464–465.

[63] Michael, *Johannes Buridan*, vol. 1, pp. 335–336, 341, 345, 357, 359, 364, 367.

[64] *Ibid.*, pp. 352–353.

[65] I prefer the term "Parisian school" to "Buridanism", which was invented by Markowski in his *Burydanizm*; see Michael, *Johannes Buridan*, pp. 321–389; for a criticism of the term "Buridan school", see Thijssen, "The Buridan School Reassessed".

[66] Maier, *Zwei Grundprobleme*, pp. 271–274; Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, pp. 67–69.

[67] Dewender, "Lawrence of Lindores", pp. 328, 334; for Lawrence in general, see Moonan, "The scientific writings of Lawrence of Lindores".

[68] Wielgus, "Ausgewählte Probleme der '*Quaestiones in libros physicorum*' ".

> The motion of the sphere is diminished and stops though the sphere remains whole and intact, because it is not natural, but an accidental motion caused by force. The motion thus stops when the impetus impressed in it is diminished. If the sphere would be perfectly round, its circular motion would […] never rest, since it is natural and not enforced.[69]

Cusanus thus relates impetus to the course or duration of motion, and he also explains the continuation in celestial motions by the spherical shape of the bodies and by the impetus impressed by God.

In the 16th century, concepts of impetus lost their character as revisions of Aristotle – as was the case already in Lawrence of Lindores – and impetus became a kind of "official" scholastic doctrine.[70] Even the followers of Thomas Aquinas supported the explanation of projectile motion by impetus. Thus the Dominican Domingo de Soto in his commentary on the *Physics*, first published in Salamanca 1545, rejected the Aristotelian solution and decided in favour of an impressed force. Though Thomas had explicitly declared himself against an internal cause of projectile motion in his *De caelo*, Domingo pointed to the references from the *Quaestiones disputatae* cited above.[71] It was in this process that the terms "impetus" and "*virtus impressa*", as we shall see, continually lost their original meaning.

CONCEPTS OF IMPETUS AT THE BEGINNING OF THE SCIENTIFIC REVOLUTION

While concepts of impetus still flourished in the scholastic community, the new astronomy of Copernicus continually gained more and more acceptance. But even in the 17th century, the acceptance of Copernicus's theory was not necessarily connected with the acceptance of new physical solutions.[72] This holds true also for one of the strongest partisans of Copernicus, Giordano Bruno.[73] When he wrote his *La cena de le ceneri* in 1583/1584, Bruno used all possible arguments to defend heliocentrism, and it was in this context that he also referred to a concept of impetus, though not explicitly. To defend

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69 Nicolaus Cusanus, *Philosophisch-theologische Schriften*, vol. 3, p. 242; see Wolff, *Geschichte der Impetustheorie*, pp. 258–259.

70 See Maier, *Zwei Grundprobleme*, p. 294.

71 Duhem, *Le Système du monde*, vol. VIII, pp. 182–183; Maier, *Zwei Grundprobleme*, pp. 134–137.

72 As is demonstrated by the tensions between Galileo and Kepler; see Bucciantini, *Galileo e Keplero*.

73 For Bruno's use of a concept of impetus, see Koyré, *Metaphysics and Measurement*, pp. 8–10.

the motion of the Earth, he referred to the experience of falling bodies on and outside a moving ship and came to the conclusion that the difference between such movements results from the fact that the things that belong to the ship are moved along with it:

> One of the stones carries with itself the virtue [impetus] of the mover, which moves with the ship. The other does not have the said participation. From this it can evidently be seen that the ability to go straight comes not from the point of motion where one starts, nor from the point where one ends, nor from the medium through which one moves, but from the efficiency of the originally impressed virtue [impetus], on which depends the whole difference.[74]

It is evident that Bruno supposes a transmission (or "impression") of forces, similar to that in the concepts of impetus. But he does not use its consequences to describe the actual movement of the two bodies. This is in some respect typical for the use of impetus in the transition to classical physics: a central element of the concept is still accepted, but all the implications of the concept and its context are no longer present.

In general, different concepts of impetus widely circulated in Italy during the 16th century. For example, one of the Pisan professors of Galileo, Francesco Buonamici, still kept to the principles of Aristotelian and medieval science, i.e., its metaphysical foundations and its reliance on sensory experience, but he also refers to impetus in his *De motu* during his discussion of the problems of projectile motion.[75] After having presented a revised theory of *antiperistasis* ascribed to Plato and Simplicius, he introduces the (second) Aristotelian solution as a different concept of impetus, in which the original mover impresses an impetus on the air, which afterwards moves the heavy body. But then Buonamici adduces arguments against this theory from sensory experience, adding

> That is why Philoponus and, after him, Albert [of Saxony], S. Thomas and many others have been of the opinion that the force is not impressed by the original mover in the air, but in the moving thing, as in the stone. And just as a greater or smaller force is impressed in it, it will be moved through greater space or more rapidly. Now and then, this kind of

[74] Bruno, *The Ash Wednesday Supper (La cena de la ceneri)*, trans. Stanley L. Jaki, pp. 123–124.
[75] See Helbing, *La Filosofia naturale di Francesco Buonamici.*

> force will be received more easily and rapidly, sometimes with difficulty and slowly, because of [the factors] assisting the movement, as the form, magnitude, quantity of matter, etc.[76]

For Buonamici, his concept of impetus has everything that is necessary for a good solution of the problems, since it accords with reason and sense experience. But when he discusses the acceleration of falling bodies, Buonamici decides against the idea of a double gravity – the natural and the accidental gravity that is identical to impetus, which is again ascribed to Thomas Aquinas and Albert of Saxony. Thus he quotes the definition of impetus as a quality apt to move and points to the fact that, in the case of falling bodies, the accidental gravity or impetus would be caused by the motion itself. Thus (acceleration in) motion would be caused by motion – and what is supposed to be the effect would become the cause.[77]

Buonamici discussed these problems in a treatise solely devoted to physics or even (in modern terms) "mechanics", and this also became one characteristic of the later debates. This holds true also for one of his contemporaries, Gianbattista Benedetti, who used his concept of impetus both for violent and natural motions:

> Any heavy body moved according to its nature or by force receives in it an impression or impetus of the motion, so that if it is separated from the moving force it continues to move by itself for a certain period of time. For, if it is moved according to nature, its velocity is continuously increased.[78]

It thus does not come as a surprise that Galileo was familiar with these concepts of impetus. In his *De motu*, written during his early teaching career in Pisa, he rejects not only the Aristotelian explanation of projectile motion, but also the whole foundation of Aristotelian physics, stressing its internal contradictions. Like Buonamici he argues that Aristotle is wrong in locating the impression of a moving force in the medium, and like Benedetti, he decides in favour of an impressed force in natural and violent motions. In comparing this *virtus motiva* with a quality like heat, he states that "the impressed force is successively remitted in the thing thrown in absence of the mover, [as] the heat is remitted in the iron in the absence of

[76] Francesci Buonamici, *De motu*, lib. V, c. xxxvi; trans. in Koyré, *Études galiléennes*, pp. 30n–31n.

[77] Koyré, *Études galiléennes*, pp. 43n–44n.

[78] Benedetti, *Diversarum speculationem mathematicarum et physicarum liber*, p. 286, quoted in Koyré, *Études galiléennes*, p. 48; for Benedetti and Galileo, see also Drake, "A further reappraisal of impetus theory".

fire"[79] This comparison offers the reason why Galileo denied the common assumption that projectile motion is at first accelerated and only afterwards slows down: a thing moved cannot accelerate by itself. From this, he also infers – contrary to experience – that the fall of heavy bodies should not be accelerated and should be determined only by the weight of the body, which leads him to accept an Archimedean concept of specific weight.[80] Though Alexandre Koyré has classified it as a "physics of impetus", Galileo's early theory has not much in common with the scholastic theories; it is rather a new development, based on the concept of an impressed force but leaving aside its ontological presuppositions and its Aristotelian context.

THE USE OF CONCEPTS OF IMPETUS IN THE MECHANICAL LITERATURE AND THE NATURAL PHILOSOPHY OF THE EARLY MODERN PERIOD

The change to a more pragmatic use of a concept of impetus also occurred in the mechanical literature of the 16th and 17th centuries. For example, Niccolò Tartaglia tried to explain the motion of a cannonball by the assumption of an impressed force, like many others supposing that its motion is at first accelerated. He was not quite sure if natural and violent elements could exist side by side in projectile motion. At first, he assumed that the motion of a cannonball was composed of two straight lines, one ascending, caused by the impetus of violent motion, the other descending, caused by the gravity of the cannonball. Later on, based on sensory experience, he proposed an additional middle phase of mixed motion, in which the straight rise of the cannonball is bent by its gravity until it finally turns downwards.[81] Tartaglia's considerations found wide reception, especially in literature on artillery, for example in Germany, where Walter Hermann Ryff translated and revised Tartaglia's *Quesiti*, though some of Tartaglia's philosophical discussions – he had referred to the late medieval thought experiment of a heavy body oscillating through the centre of the earth – are left out.[82]

In this more technical context, concepts of impetus still retained their influence, while in physics Galileo and Descartes had already gone very

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[79] Galilei, *De motu*, in *Opere*, vol. 1, p. 310, quoted in Koyré, *Études galiléennes*, p. 62.
[80] Koyré, *Études galiléennes*, pp. 65–67, 70–71.
[81] Tartaglia, *Quesiti et inventioni diversi*, l. 1, qu. 3; see Boas, *The Scientific Renaissance 1450–1630*, p. 221.
[82] Harig, "Walter Hermann Ryff und Nicolo Tartaglia", pp. 34–35.

different ways. As of the 1660s, the members of the Royal Society discussed methods to improve the work in the crafts by introducing some of the inventions and discoveries made during the Renaissance. One of the authors involved in these discussions was Joseph Moxon, who corresponded with scientists such as Robert Boyle, Edmund Halley, and Robert Hooke.[83] In 1696, James Moxon, the son of Joseph, and Venterus Mandey published a work on the theory of machines, "Mechanick Powers: or, the Mistery of Nature and Art unvail'd", in which they used the concept to impetus to explain several mechanical phenomena. At first, their concept of impetus is introduced in very conventional way, as a force that explains the continuity of projectile motion and the acceleration of falling bodies. Mandey and Moxon even discuss the distinction between violent and natural motions and state some general relations:

> A greater impetus is required to move the same weight a greater space than less, whether the whole impetus be produced together, as happens in things projected or thrown, or successively, as when a weight is drawn. Also a greater impetus is required to move a greater weight some space than to move a less weight the same space . . . . By how much more the power is that is moved, by so much the greater and stronger is the impetus produced.[84]

When the impetus is also related to the quantity of motion – namely, that a "greater motion" produces a greater impetus, it becomes clear that the notion used here differs from the scholastic concepts of impetus, which always remained ontological answers to the problems posed by the principle *OMAM*, relating it to the force of the mover. Mandey's and Moxon's impetus is rather like the impulse in modern physics, which is closely related to motion, because it is not only the cause of motion, but also its effect. The terms used in the description of phenomena, such as "impetus" and "natural" and "violent motion", seem to be Aristotelian, but the context has changed considerably. The same holds true for a treatise of Leibniz on the use of the concept of impetus in machines.[85]

This rather unspecific use of the term "impetus" continued in later natural philosophy. For example, Thomas Hobbes, in his *On the Body*, distinguishes

[83] Moxon, *Mechanick Exercises*, p. x (introduction); Wolff, *Geschichte der Impetustheorie*, pp. 356–357.
[84] Mandey and Moxon, *Mechanick Powers*; quoted from Wolff, *Geschichte der Impetustheorie*, pp. 362–363.
[85] See Wolff, *Geschichte der Impetustheorie*, pp. 349–350; Leibniz even translates "impetus" with "Schwung", the German equivalent of "impulse".

between conatus and impetus.[86] Conatus is the impulse of motion at a certain point and time, while impetus is equated with the velocity of the thing moved and is thus defined as the measure of the conatus. In the early 18th century, the Hallensian philosopher Christian Wolff tried to establish a philosophical system that also incorporated the teachings of Newton and other scientists, for example the principle of inertia. It is in this context that he also uses the term "impetus": "A body actually in motion is said to have an impetus insofar as its mass or its coherent matter is moved by a certain grade of velocity".[87] For Wolff, as for Hobbes, the impetus is the quantity of motion, and, it must be stressed, is not its cause, but its effect. It is clear that this notion of impetus has little or nothing to do with the scholastic and early-modern concepts of impetus, where it is conceived as the effect of motion.

Thus the concepts of an impressed force not in the medium but in the projectile itself were developed only as a kind of "puzzle solving" during the course of "normal science", in the terms of Thomas Kuhn. Since this is one of the options of how to deal with the problems of projectile motion within the framework of Aristotelian natural philosophy, it is by no means astonishing that this solution recurred in different contexts of the reception of Aristotle's works, in Alexandria in the 6th century, in Baghdad in the 10th to 12th centuries, in the Latin West from the 13th or rather the 14th century, and even independently from textual traditions. This is witnessed by the fact that Philoponus and some of the Arabic philosophers discussed the problem in the context of Book IV of Aristotle's *Physics* in relation to the question of motion in a void, while for others such as Avicenna, Buridan, and Albert, it became part of the discussion of the causes of motion in Book VII or VIII of the *Physics*. If one takes Aristotelian natural philosophy as a whole, the problem of projectile motion is in fact marginal, but it seems that the very cautious positive reactions of the 13th century, for example in Thomas Aquinas, can nonetheless be explained by a reluctance to question Aristotle's doctrines. It seems that only after alternative concepts had explicitly been supported in a very different context, in the commentary on the *Sentences* of Peter Lombard by Franciscus de Marchia, that the way was open to its transfer into the context of Aristotelian *Physics*. The concepts were now discussed with all their consequences, even for natural and celestial motions, and these were also considered in the commentaries on *De caelo*.

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[86] Thomas Hobbes, *De corpore*, III, xv, pp. 155–156.
[87] Wolff, *Cosmologia generalis*, § 394, p. 289.

But this did not lead to an independent textual tradition: there were no separate treatises on the causes of violent, natural, or celestial motions, as there were treatises on proportions and geometrical representation. Nevertheless, the idea of explaining natural and violent motions by an impressed force spread in the Latin West by the influence of the "Parisian school" of natural philosophy, and finally it became the typical scholastic doctrine that was even attributed to Thomas Aquinas, not only by Domingo de Soto, but also by Francesco Buonamici. It was only then, at the beginning of the Scientific Revolution, that concepts of impetus were discussed in treatises on mechanics and thus became part of an independent textual tradition.[88] Alexandre Koyré is thus in some respect right in attributing something like a "physics of impetus" to Gianbattista Benedetti and the early Galileo, but not to Buonamici, who represented a different tradition. But it must be stressed that if there was something like a "physics of impetus" at all – as an independent theory of motion, based on a certain concept of impetus – it did not develop within the medieval schools and universities, perhaps because of the doctrinal coherence in the reception of Aristotle's writings. And when the concept of an impressed force was finally put into a wider context in the later 16th century, it became a general notion to be used in very different approaches to the problem. This holds true also for the mechanical treatises of the 17th century. Thus in later natural philosophy the term "impetus" lost its specific meaning and was virtually nothing more than "impulse" – the effect and not the cause of motion.

Therefore, although the concepts of impetus played an important role in discussions until Galileo and Descartes laid the foundations of a new physics, there is no sufficient reason to assume a "middle period" in the history of physics, something like a "physics of impetus", covering the period from the 6th to the 17th centuries. There is not even a coherent "theory of impetus", because concepts of impetus varied with the different authors and even the different categories of texts. Rather, the different concepts of impetus were little more than an alternative and ad-hoc solution for one puzzling problem within the complex structure of Aristotelian natural philosophy.

This explains why it is nearly impossible to categorize these concepts – the Arabic and medieval ones in which impetus is a quality, a force, or

[88] There was also another concept of impetus in the context of the *scientia de ponderibus*, which in fact formed an independent medieval textual tradition and which may indeed have influenced the theories of the 16th and 17th centuries, but this is not treated here because its use of impetus is rather unspecific; see n. 39 above.

even an instrument of force, self-exhausting or not self-exhausting, or the Early-Modern ones, in which its ontological status is irrelevant and in which impetus is cause or effect, moving force, or impulse. Nevertheless, these concepts called certain key doctrines of Aristotle into question: the essential difference between natural and violent motions or between celestial and elementary motion. Thus, as Hans Blumenberg has argued elsewhere, other elements of Aristotle's theories may also have lost their original function, and in the end, it became easier to replace the whole system.[89] The different concepts of impetus, therefore, ultimately contributed to the climate of intellectual change beginning in the later 15th century and thus also paved the way for the Scientific Revolution in the 17th.

[89] Blumenberg, "Die Vorbereitung der Neuzeit", p. 132; Sarnowsky, *Die aristotelisch-scholastische Theorie der Bewegung*, p. 427.

2. THE REAPPROPRIATION AND TRANSFORMATION OF ANCIENT MECHANICS

CHRISTIANE VILAIN[1]

CIRCULAR AND RECTILINEAR MOTION IN THE *MECHANICA* AND IN THE 16TH CENTURY

In his *Diversarum speculationum mathematicarum et physicarum liber*, Giovanni Battista Benedetti criticised Aristotle's *Physics*, noting that Aristotle had erred, among other ways, in saying that one could not compare rectilinear and circular motion.[2] Aristotle had indeed denied that the curved could be measured by the straight. And yet, as Benedetti remarked simply, this had in fact been done by Archimedes in his treatise *On the Measurement of the Circle*. But if our interest is first in the more general comparison of these movements, before any question of measurement, we must look instead at the commentaries and criticisms of the Pseudo-Aristotelian *Mechanica*.[3]

It is interesting to compare the designation of certain types of rectilinear motion as "natural" in the beginning of the *Mechanica* with Benedetti's affirmation, in his criticism of this work, of the tangential rectilinear – rather than circular – character of impetus, which without constraint continues in and of itself.[4] In this study I shall attempt primarily to determine the aptness of this comparison. In the *Mechanica*, it is a matter of rectilinear motion of bodies when freed from an imposed circular motion, and of motion that could be described as violent, such as the motion of throwing.[5] We may thus

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[2] Benedetti, *Diversarum speculationum mathematicarum et physicarum liber*, p. 194; Engl. trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 220; this passage, Benedetti's criticism of Aristotle, *Physics*, VII.4, is in that part of the book entitled *Disputationes*.

[3] I shall refer to the recent edition of M.E. Bottecchia, *Aristotele, Problemi meccanici*. I shall also quote the bilingual Greek-Latin edition by Johannes Petrus van Capelle, *Aristotelis quaestiones mechanicae*, in order to provide the Latin text closest to that read by the commentators. There are two English translations, one by D. Foster, in *The Works of Aristotle*, and the other by W.S. Hett in *Aristotle: Minor Works*. M.E. Bottecchia uses the diagrams of previous editors without any commentary, but offers a good deal of information and notes on the Renaissance commentaries.

[4] Benedetti, *Diversarum speculationum*, pp. 159–161; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, pp. 187–189.

[5] The straight line that extends alongside the circle when the body is freed from the constraint that keeps it a constant distance from the centre is the tangent to this circle. But this was not designated as such before Benedetti, perhaps simply due to ignorance of Euclidean geometry.

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 149–172.

ask if the distinction between natural and violent motion is modified or not in the *Mechanica* and its commentaries when compared with the *Physics*. This will be the primary question in this study, and in particular of its first section. I shall also highlight the role of the devices considered: scales and levers on the one hand, wheels and slingshots on the other. The categories proposed in the general analysis of circular motion in the *Mechanica* seem for some commentators to lead in fact to a dynamic content, one that varies depending on the means by which the motion is applied. We can thus ask whether the categories of natural and violent motion are the same for commentators whatever the problem in question. This will be my second line of questioning, which will lead to the law of the lever.

I shall not limit myself to the criticisms suggested by Benedetti himself, which in his *Diversarum speculationum liber* precede his positions on Aristotle's *Physics*,[6] but shall instead try to consider rectilinear and circular motion within the entire body of commentary on the *Mechanica* as treated by other authors during the Italian Renaissance.[7] Benedetti refutes the affirmations attributed to Aristotle and seems keener to deride them than to take them as a guide. I therefore cannot affirm without certain reservations that his reading of the *Mechanica* had modified his way of thinking. He obviously found in the *Mechanica* an inspiration that differed from that of other Aristotelian treatises as well from that of the Archimedean treatises, if only through the examples of mechanical systems other than scales and levers, mechanisms that may have led him to a new way of thinking. I therefore wish to see how other commentators and critics reacted to the fact that the distinction between natural and violent motion is presented differently in the text of the *Mechanica* from how it is presented in Aristotelian physics, since it relies on an analysis of rotational motion into rectilinear components. This research is interesting in and of itself, independent of what may have occurred in the case of a particular author such as Benedetti. Indeed, Benedetti's affirmations seem quite anecdotal compared to his remarks and mathematical analyses taken as a whole and to all the rest of the commentary. The slingshot, his most common example to show that the impetus is in fact

[6] Benedetti had already published in 1554 a critical analysis of the Aristotelian conception of falling motion in his *Demonstratio proportionum motuum localium*; for an English translation see Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, pp. 162–163. See also Koyré, "Jean-Baptiste Benedetti critique d'Aristote".

[7] Nor am I seeking to identify among them the author who influenced Benedetti; according to Rose and Drake, "The Pseudo-Aristotelian *Questions of Mechanics* in Renaissance Culture", p. 92, in Turin he knew only the writings of Niccolò Tartaglia. I shall analyze only the commentaries on the *Mechanica*. Other studies of this type exist; see in particular Festa and Roux, "Le '*παρὰ φύσιν*' et l'imitation de la nature".

always rectilinear and never circular, seems very much a particular case. The movements considered in the Middle Ages were, in contrast, those of millstones or stars, favouring a circular impetus.[8] The change carried out by Benedetti is nonetheless not limited to the mechanism of the slingshot, as we shall see in the last section.

My questions regarding the possible role of the Pseudo-Aristotelian text in the general manner of considering motion are justified by the historical importance of the matter in 16th-century Northern Italy, as demonstrated by the work of Paul L. Rose and Stillman Drake.[9] The difference between the reactions of a humanist and a mathematician will be highlighted in the second section.

CIRCULAR AND RECTILINEAR MOTION IN THE *MECHANICA*

After singing the praises of the circle, which he will use as the sole principle of his mechanics, the author of the *Mechanica* shows that a circular or curved motion can be made up of two rectilinear motions. To do so, he first demonstrates that the composition of two rectilinear motions whose proportions remain constant over time (Fig. 1a) can only generate rectilinear motion, and vice versa. If the result is not rectilinear (Fig. 1b), this implies only that a proportion constant over time cannot be found.[10]

What is important here for the author of the *Mechanica*, is not the construction of a given curve, but the possibility of breaking down a curved trajectory into two rectilinear components. The only condition is that there be no permanent proportion between the two. Thus the movement of the arc BG (Fig. 1b) is made up of lateral movements BD and DG, which have no constant proportion between them over time, for otherwise the result would be the chord BG. It is of no importance that one does not know the particular mode of composition or the variation over time of the ratio of the two components that give rise to a circle: it is enough that it exists.[11]

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[8] For Jean Buridan and Albert of Saxony, circular as well as rectilinear impetus existed; see, for instance, the first experience referred to by Buridan in *Super octo physicorum libros Aristotelis*, fol. cxx, col. b.

[9] Rose and Drake, "The Pseudo-Aristotelian *Questions of Mechanics* in Renaissance Culture". The inventory furnished by this article is impressive: translations and paraphrases accompany the passion of first humanists and later mathematicians for this text. Regarding the influence of the *Mechanica*, see the introduction of Bottecchia, *Problemi meccanici*; Laird, "Renaissance Mechanics and the New Science of Motion".

[10] *Mechanica,* 848b; Capelle, pp. 15–16.

[11] This is what the Renaissance commentators did not always understand: they thought they had found a proportion for certain curves (see, e.g., Moletti, *Discorso*, Milano, Biblioteca

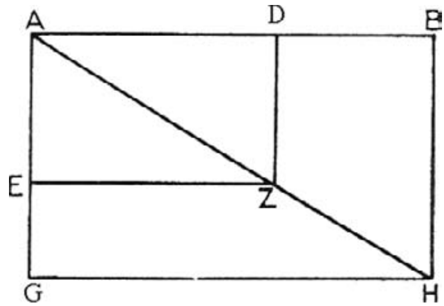

FIGURE 1a.

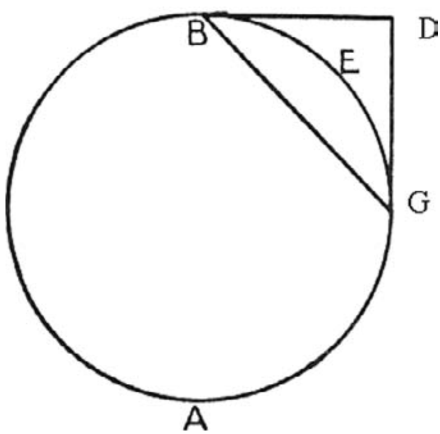

FIGURE 1b.

When we come to the next step in the reasoning,[12] the extremity B of the radius moving on the circle BGED (Fig. 2) is said to be moved "according to nature, but what is oblique [is moved] against nature toward the centre".[13] Later I shall call this movement "lateral" or "sideways",

Ambrosiana Ms. S 103 sup., fol. 161r) or else they demonstrate that the analysis of Fig. 1a is not unique (see, e.g, Benedetti, *Diversarum speculationum*; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, pp. 179–180). But I shall leave this matter for the moment, for it is not this purely mathematic aspect of analysis that interests me.

[12] *Mechanica*, 849a 24 sqq.; Bottechia, pp. 66–69; Capelle, pp. 17–19.

[13] "Hoc igitur in omni radio obtinet, moveturque per circumferentiam, secundum naturam (*κατὰ φὺσιν*) quidem in obliquum (*πλάγιον*), contra naturam (*παρὰ φύσιν*) versus centrum" (Capelle, pp. 16–17). One can see in the original text that the term for "tangent", which is found in the Foster's English translation, as well as in M. E. Bottecchia's Italian translation, must not be taken in the geometric meaning of the word, but in a qualitative sense, as a movement that is lateral, sideways, along the circle.

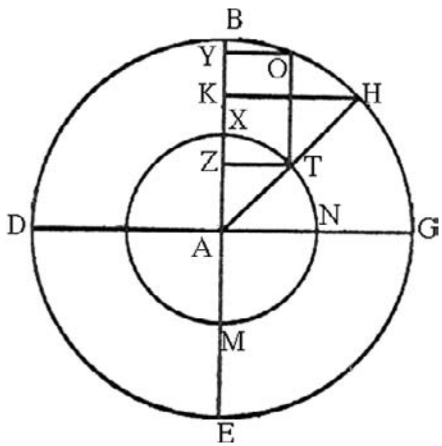

FIGURE 2.

designated by *obliquus* in the Latin text, because it escapes from the line of the circumference, which can be represented as horizontal in Fig. 2 or as vertical in Fig. 3b of the next section, without knowing what was actually the case in the original text.[14] In the absence of the original illustrations from the *Mechanica*, we do not know whether rectilinear motion could take any direction or whether it was always vertical and downward.

I shall now compare the movement on the arcs BO and XT of two concentric circles of different radii. These movements are considered as each being made up of a lateral movement and another that draws in toward the centre. For the same lateral displacement YO = ZT, the movement BY toward the centre in the larger circle is smaller than the corresponding movement XZ in the smaller.

According to the author this smaller displacement explains why movement is easier on larger circles than on smaller ones, the radius AB being able to move more quickly than the radius AX, which undergoes more of a constraint from the centre, to which it is closer.

If on the other hand we compare, as did Pseudo-Aristotle in a second part of the demonstration, the paths of the arcs BH and XT, the ratio of the lateral movements to the vertical (which draw in to the centre) are equal.[15] The author concluded from this that these paths are traversed in the same amount of time,

[14] As Mario Otto Helbing has noted, the original manuscripts did not have illustrations. My diagrams are inspired by the Capelle edition and are not those of the first translations. It is thus possible that my Fig. 2 may not be that of the original text and that the movements leaving the circle were in fact vertical rather than horizontal.
[15] *Mechanica* 849a 32; Bottecchia, pp. 68–69; Capelle, p. 19.

for this is the situation in which the proportion of the natural and non-natural movements are "correct", that is to say, the same for both arcs.[16]

All this serves merely to explain why larger scales are more precise than smaller ones, and does not raise the question of the law of the lever. But one can read a bit later:

> The reason the point farthest from the centre moves more quickly by the same force, and the larger radius describes a larger circle, clearly follows from what we have stated.[17]

The lever itself is dealt with in question 3 of the *Mechanica*, in which it is asked why it is easier to raise a heavy weight with the help of a lever, despite the added weight of the lever itself. The reason is the faster movement of the greater arm with which one exerts a smaller force, while the weight is borne by the smaller arm. This expresses precisely the Archimedean proportion without demonstrating it, with the geometric demonstrations of the first question simply supporting this proportion in a qualitative way.

The argument of the first question is later taken up in several questions in the *Mechanica*. Thus in question 8: "Why are circular forms easier to move (than polygons)?"; in question 9: "Why are objects moved more easily on objects of greater circumference (large pulleys)?"; and lastly in question 12: "Why does a missile thrown by a sling go farther than one thrown with the hand?" The speed of a circle described by a sling is greater than that described by the hand, says Pseudo-Aristotle, for the former is larger than the latter. This last question will particularly inspire Benedetti in favour of rectilinear impetus.

I have noted that the interpretation of these texts relies in part on the orientation of diagrams that do not exist in the manuscripts. In the case of Fig. 2, if the circles are intended to represent scales, with the diameters BE and XM being their horizontal beams, the movement leaving the circle "sideways" is in fact vertical and downward. But if there is a downward movement on one side, there is another, upward, on the other, and both cannot be natural movements of heavy bodies. The answers to questions 9 and 12 that I have just mentioned invite us moreover to imagine that the initial comparison of "sideways" and radial components of portions of arcs cannot be limited to particular positions of point B on the circles, despite

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[16] Thus the faster speed on large circles concerns linear velocity, the angular velocity being the same for all concentric circles subject to the same conditions.

[17] "Quam igitur ob causam ab eadem potentia citius moveatur punctum, quod plus distat a centro, et majorem circulum describat major radius, manifestum ex supra dictis" (Bottechia, pp. 70–71; Capelle, p. 21).

the fact that this is indeed the case for diagrams illustrating the first question given by more recent commentators or editors of the *Mechanica*. When it is a question of a wheel on the ground, as in Question 8, point B is in contact with the ground and the "sideways" movement must be horizontal. One can also mention wheels that turn parallel to the ground, such as potter's wheels. In the case of the sling, no point plays a particular role, and the argument must be valid for every point of the circle and thus for any direction of "sideways" and central movements.

It is thus certain that in this beginning portion of the *Mechanica*, the analysis of motion is original when compared to that of Aristotle's *Physics*. But it would undoubtedly be absurd to read into this a conflict or a contradiction, since the goals of the two texts are different. The preceding remarks invite one rather to avoid imposing rigid categories on matters that in fact can be adapted here to particular situations subject to specific constraints; this already constitutes the beginning of an answer to the questions raised in the introduction. I shall now examine the interpretation that various commentators have given to the analyses of Pseudo-Aristotle.

THE FIRST REACTIONS (1546): PICCOLOMINI AND TARTAGLIA

Here I shall compare the paraphrase of Alessandro Piccolomini, a literary scholar with a passion for the text of the *Mechanica*, with the much dryer commentary of the mathematician Niccolò Tartaglia.[18]

Piccolomini, who had studied at the university of Padua, published commentaries on Aristotle's *Meteorologica*, *Poetics*, and *Ethics*. Diego Hurtado de Mendoza, a friend who himself had made a Spanish translation of the text, no doubt inspired his interest in the *Mechanica*. Piccolomini's loyalty to Aristotle is here combined with his admiration for machines, due to their practical interest for Italian cities of the day. Like others, Piccolomini refers to Vitruvius's *De architectura* Book 10 when necessary. He nonetheless criticizes the earlier translation of the *Mechanica* by Leonico for its lack of rigor, due according to him to the translator's poor understanding of mathematics. The details that he himself brings to the text are nonetheless of a different order, as we shall see.

Piccolomini clearly takes great pleasure in all that the original text contains in the way of qualitative – for him almost literary – considerations. Thus

[18] Piccolomini, *In mechanicas quaestiones Aristotelis*. This commentary is the first known following the first translations, of Fausto in 1517 and of Leonico Tomeo in 1525.

the preamble concerning the "wonder" of the circle, where it is noted in the *Mechanica* that circular motion is made up of two contrary movements, and that the points of a radius describing a circle do not all move at the same speed, are at times recalled in dealing with the questions that follow, which was not the case in the original text.

The particular prestige of the circular form is reinforced when movements are combined to give arcs. Piccolomini's presentation, which on this point follows that of Leonico Tomeo, differs from that shown in my Fig. 1b. The composition of movements BE and EC (Fig. 3a),[19] or BM and MC, nonetheless takes place in the same way, for Piccolomini observes that one obtains the chord BC if there is a constant proportion over time, and that to obtain the arc BC, such a proportion must not exist.[20]

The analysis of circular motion aimed at comparing two concentric motions, imagined in general terms, gives rise first to this commentary:

> for we have already demonstrated that any line that describes a circle, however large, is carried at the same time by two movements, from which it results that one is natural and the other outside of nature and in a certain way violent. We call natural the movement that takes place along the circle, non-natural that which, on the contrary, goes toward the centre. One indeed sees this centre that remains fixed retain and call in [toward itself] all the lines that come from it: it is as if it were imposing on them its law, [that] of not moving farther away from its seat than it pleases.[21]

This somewhat figurative presentation of the constraint exerted by the centre is interesting as an illustration of the adjective "violent": it is as if a hand intervenes to maintain the body on the circle. Consequently, the movement "along the circle" can be characterized as "natural" simple because it is free of this action by the centre.

[19] My figures 3a and 3b are redrawn based on Piccolomini.

[20] The representation of two components of motion by oblique lines rather than perpendicular lines does not modify the problem attributed to Pseudo-Aristotle.

[21] "... quoniam iam demonstravimus quamlibet lineam, quae circulum describit, quantacunque fuerit, duabusmotionibus simul ferri, consequitur ergo alteram sibi esse naturalem, alteram vero praeter naturam, ac quodammodo violentam. naturalis latio illa dicitur, quae secundum ambitum est, non naturalis vero, quae in adversum ad centrum fit. videtur enim centrum ipsum quod manens est, retrahere ac revocare lineas omnes quae ab ipso exeunt: quasi legem ipsis imponat, ne ab eius solio longius removeantur, quam sibi libeat" (Piccolomini, *In mechanicas quaestiones Aristotelis*, fols. 12r–v).

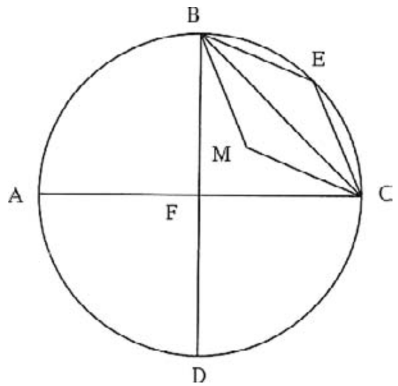

FIGURE 3a.

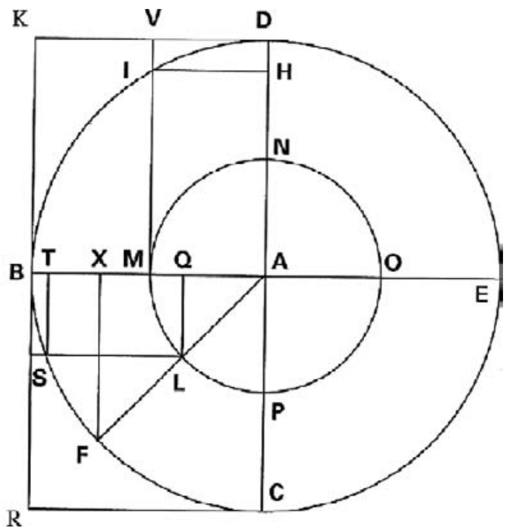

FIGURE 3b.

If the nature of this motion "along the circle" was not defined at the beginning, it would soon be, as in Fig. 3b. Piccolomini considers the movement from point D, which he assumes to be to the right, despite the fact that the nature of the circle requires that the movement can take place either to the right or the left, as has been stated earlier. Point D, "by its nature", will go to the right for the sake of example, and thus toward R:

> AD, whose extremity is D, by its natural motion, which is along the circle, is moved toward the right side … thus if it

> were pushed only by a natural motion, [point D] would move toward R in a straight line.[22]

This passage shows us that the author imagines horizontal rectilinear motions, from D toward R, as natural. The movement that is described as "along the circle" is that which moves away in a straight line, here from D toward K, and farther along from B toward R. The comparison of movement on two circles of different radii is then based on movements from point B, for which the "natural" motions are in this case BG, TS and QL, then XF (Fig. 3b).[23] Circular motion itself is then not truly violent (*non aliquid vero violentum*), as is the motion that attracts toward centre A, but outside of nature (*praeter naturam*).[24]

When he comes to question 8, Piccolomini remarks that mathematics must not be confused with nature:

> Natural bodies, however round or circular, cannot touch the ground at only one point, as would happen if the mass of the material were removed. Bodies of this kind [i.e., circular], however, touch the plane with a smaller part of themselves than a mass of any other shape.[25]

Piccolomini, who imagines himself a mathematician, reasons nonetheless based on concrete bodies not reduced to mathematical abstractions. His commentary on the question 12 of the *Mechanica* is somewhat of the same order, insofar as he calls upon the senses, or perhaps on a sort of common sense, to justify the effectiveness of the sling:

> Which is also manifest to the senses: by using a small force, one can add new motion to whatever body is already in motion, but the initial motion would require a greater force.[26]

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[22] "AD eius extremum quod est D motu suo naturali, qui secundum ambitum est, movebitur in partem dexteram … ita quod si solummodo naturali motione progrederetur, per lineam rectam versus K tenderet" (*ibid.*, fol. 13v).

[23] Piccolomini's commentary shows that the diagram was reversed by the printer.

[24] *Ibid.*, fol. 13r–v. Regarding the expression *praeter naturam* in the beginning of the *Mechanica* and in the texts of this period, see Festa and Roux, "Le '*παρὰ φύσιν*' et l'imitation de la nature".

[25] "Quamvis ergo naturalia corpora, rotunda ac orbiculata, planum in solo puncto contingere nequeant: sicut si moles materialis removeretur, eveniret: huiusmodi tamen corpora, minori sui parte, planum contingunt, quam cuiuslibet alterius figurae moles" (Piccolomini, *In mechanicas quaestiones Aristotelis*, fol. 30v).

[26] "Quod ex hoc etiam sensui manifestum est, quod a modica admotum vi, alicui ponderi, dum est in motu, novus etiam additur motus: quod tamen in mutationis initio maiorem admotum vim postulasset" (Piccolomini, *In mechanicas quaestiones Aristotelis*, fol. 35r).

It is easier to increase the movement of what is already in motion by rotation of the sling. This could already be found in the original text of the *Mechanica*, to which Piccolomini remains faithful. But one can observe through these last two quotations a call to sensory intuition characteristic of this first commentary.

Piccolomini thus shows, I think, the reasons for the success among humanists of the difficult text of the *Mechanica*: one draws from the text an intuition of a movement of rotation made up of constraints and forces rather than rigorous geometric analyses.

It was at the same time, perhaps the same year, that Niccolò Tartaglia criticized the text of the *Mechanica*, in the seventh Book of his *Quesiti* in an imaginary dialogue with Mendoza, the Spanish ambassador who was a friend of Piccolomini.[27] Here we see Tartaglia react at first in a manner as practical and realistic as Piccolomini's: Aristotle, the author of the *Mechanica*, was too much of a mathematician. For to explain why large scales are more precise than smaller ones, it would have been necessary to take into account, according to Tartaglia, the material conditions of manufacture that make the small scales of clockmakers or jewellers very often more precise than larger scales. But he uses these details to say that one must not try to verify physically what one demonstrates in a certain manner by mathematics. Is Aristotle thus rehabilitated? No, for he was in fact not enough of a mathematician, and did not provide the true reason why a force has a greater effect on a longer beam than on a shorter one. The right answer can be found elsewhere, with Jordanus de Nemore, says Tartaglia. This seventh book serves in fact as a motivation for and introduction to Tartaglia's Eighth Book, which concerns the science of weights. His procedure is not Archimedean, for he refers to the work of Jordanus, but in any case, the analysis of movements does not seem to inspire him, and it is not from this that he will draw his famous conclusions about the trajectories of cannonballs. Nor does Tartaglia show any interest in either wheels rolling on the ground or in slings.

We thus note a radical difference in approach in the two authors, Piccolomini and Tartaglia: the former, a university scholar convinced of the importance of mechanics and mathematics, but having practiced neither, and the latter, educated outside the university, and with the skills we know, is not interested in a text attributed to Aristotle. If there is in the later commentaries on the *Mechanica* a genuine modification of the categories of rectilinear and circular motion, this cannot be attributed to mathematicians,

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[27] Tartaglia, *Quesiti et inventioni diversi*; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, pp. 98–143.

nor to a new importance given to geometry in the analysis of motion. It is not this aspect of the text of Pseudo-Aristotle that first attracts commentators, but rather the possibility to rethink natural and violent movement in terms of phenomena other than those of Aristotle's *Physics*, of phenomena associated with concrete practices.

GIROLAMO CARDANO

Cardano was a physician and mathematician. He referred to the *Mechanica* in one of his works, the *Opus novum de proportionibus* of 1570. Before even looking at the propositions of our text, which in fact play only a small role in his work, Cardano redefines the different types of motion. Upward and downward motion seem to be natural, for they bring the body toward its place, whether the centre of the earth or the circumference. But circular motion does nothing of the kind. It is not, however, violent, for any violent motion could not be perpetual. Cardano calls it "voluntary", in accordance with medieval scholastics.[28] It is by such motion that each star is carried around the world.

But the movement of a point on a wheel centred on any point is not of the same sort, since when the wheel is in the vertical plane, the point moves both closer to and farther from the centre of the earth. Nor are the movements that take place during the rotation of the wheel natural, for they are not in a straight line. Natural motion must bring a body to its place as fast as possible and thus in a straight line according to Euclid, says Cardano.[29] The motion of a body linked to a wheel is thus violent, or a mixture of violent and natural.

The reading of the *Mechanica* is thus placed here in a scholastic context more complex than that of Aristotelian physics. Cardano never considers circular motion as such, independent of its situation with respect to the centre of the earth. We shall see that his is thus not a treatment that can apply to the potter's wheel or to the sling mentioned by Pseudo-Aristotle.

It is only much later, after discussing falling bodies in various settings and many other subjects, that Cardano returns to the movement of rotation,

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[28] Cardano, *Opus novum*, prop. 24, p. 24. The will expressed here in the word "voluntary" is that of the angels moving the stars, according to commonly held belief. This is clear, for example, in the way in which Buridan refutes this opinion in his *Questions sur le livre du ciel et du monde*, Book II, question 12, saying that it is not necessary to imagine celestial intelligences to move celestial bodies, since they received at the outset an impetus.

[29] Cardano, *Opus novum*, prop. 23, p. 23.

and criticizes the Aristotle of the *Mechanica*. The wheel in our Fig. 2 and its analysis into movements along perpendicular segments is placed by Cardano in the field of attraction of the centre of the earth, and its points implicitly have weight, which was not at all the case in the original text. The analysis and ratios of horizontal and vertical components serve to show that movement is faster on the arc *fc* than on *af* (Fig. 4a). For *af* the vertical component *ar* is shorter than the horizontal *fr*, while the ratio is reversed in the second part of the quarter circle.[30] The geometry used is the same as that of the *Mechanica*, but it serves to describe a falling movement along a circle, rather than to compare the rotations of various radii.

When it comes to scales, Cardano explicitly reproaches Aristotle for not taking into account the position relative to the centre of the earth in his considerations of the speed of the movement of rotation; this means, as it did in Archimedes, that verticals are not parallel but meet at a point.[31] He invokes neither Archimedes nor Jordanus de Nemore, who guided Tartaglia. With a diagram similar to Fig. 4a, Cardano describes as natural those movements that move toward the centre *e*, and as non-natural *(praeter naturam)* those that, from the side, move away, as if *e* now represented the centre of the world.[32] The following diagram (Fig. 5b) would make one think of Heron of Alexandria[33] if one did not know that his *Mechanica* was at the time unavailable in Europe.[34] We cannot therefore say where Cardano may have drawn his inspiration and his criticisms. He also suggests that the *Mechanica* is not by Aristotle, because it seems to him so very obscure.

Cardano reasons in a sharply different context from the one he criticizes, for he always situates himself in the whole of the earth, with its centre. Hence it is clear that the only natural movements for him are those directed toward this centre, and that circular motion is more or less violent only insofar as it is more or less removed from this direction. He thus remains in agreement with the categories of natural and violent motion found in the *Physics*.

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[30] *Ibid*., prop. 108, p. 101. I have greatly abbreviated Cardano's demonstration, which considers first the small arcs *fg* and *fh* and refers back to several propositions from Euclid's first and third books before generalizing to sectors of circles.
[31] *Ibid*., p. 102.
[32] I have redrawn the diagrams while trying to respect their original style.
[33] See the diagrams in Heron, *Les Mécaniques,* Book I, p. 11 and more significantly Book II, p. 127; for Heron of Alexandria, see also the article by Schiefsky in this volume.
[34] Rose and Drake, "The Pseudo-Aristotelian *Questions of Mechanics* in Renaissance Culture", p. 69.

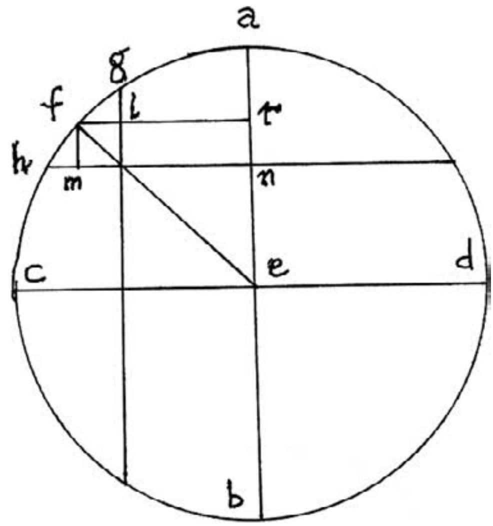

FIGURE 4a.

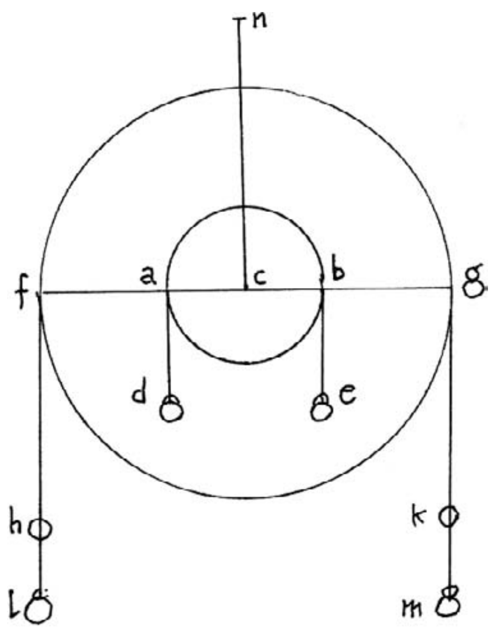

FIGURE 4b.

AN ENTHUSIASTIC READER: GIUSEPPE MOLETTI[35]

Giuseppe Moletti was certainly the commentator of the period the most favourable to the *Mechanica*, and was even more influential than Piccolomini, since he taught in Padua, after Pietro Catena and before Galileo.[36] He followed the teaching of Francesco Maurolico in Messina, before the latter became interested in the *Mechanica*.[37] Moletti engaged in the geometric and dynamic problems raised by Pseudo-Aristotle without reservation and without criticism.

His demonstration of the fact that rectilinear motion can result from the combination of two circular movements is not so much a criticism as a parenthetical remark aimed at addressing Copernicus. Let us turn to his version of the fundamental demonstration, which indeed comes first.

The natural movement of line BE should be along BF to bring it to FG (see Fig. 5a), but this does not take place:[38]

> This posited, I say that point I would be moved more against its natural movement than point B would be. Because the natural movement of the line would be along the straight lines BF and EG, which cannot be because of the violence that point E produces by being in a fixed place; but point B would be moved by its natural [movement] along the arc BR and point I along the arc IN and such movement we shall call with Aristotle the natural movement of the line.[39]

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[35] On Moletti's treatment of the *Mechanica* in general, see Laird, *The Unfinished Mechanics*.

[36] Rose and Drake, "The Pseudo-Aristotelian *Questions of Mechanics* in Renaissance Culture", p. 92. Moletti presented Aristotle's *Mechanica* in his *Expositio* of 1582, but also in the first day of his Italian dialogue of 1576, translated by Laird, *The Unfinished Mechanics*.

[37] Maurolico became interested in the *Mechanica* about 1567, when Moletti had already been gone from Messina for more than ten years. The treatise he devoted to it would not be published until 1613, under the title *De Philosophiae divisione & quaest. mechanicis*. Maurolico shows himself to be more favourable to a physics of weights, as were Tartaglia or Cardano, although in a different manner. His commentary on question 1 is very brief, has no diagrams or demonstrations, and affirms the necessity of referring to Archimedes rather than to the properties of the circle. See Baldini, "Archimede nel seicento Italiano", p. 251, and Moscheo, "L'Archimede del Maurolico".

[38] The diagrams presented here are taken from Laird, *The Unfinished Mechanics*, pp. 95, 115.

[39] "Ciò stante, dico che 'l ponto I si sarà mosso più contra il natural movimento suo, che non haverà fatto il ponto B. Perché il movimento naturale della linea sarebbe per le rette BF et EG, il che non potendo essere per la violenza che 'l ponto E fa con l' essere in quel luogo fermo; però il ponto B si moverà per il natural suo per la circonferenza BR et il ponto I per la circonferenza IN, et tal movimiento chiameremo con Aristotele movimento secondo la natura della linea" (Moletti, *Dialogue*, ed. and trans. in Laird, *The Unfinished Mechanics*, pp. 93–95).

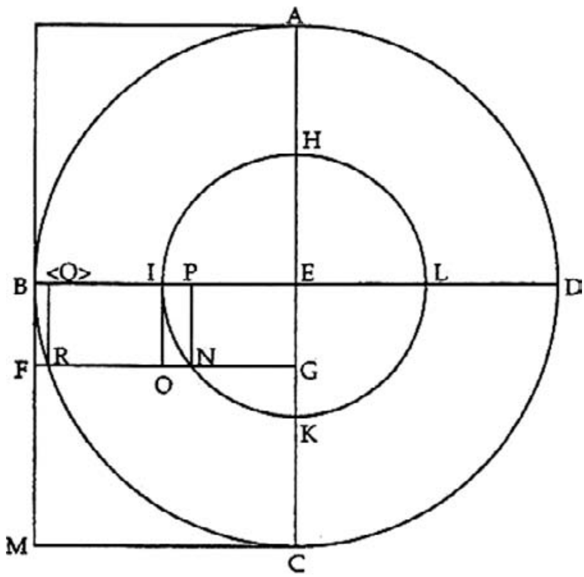

FIGURE 5a.

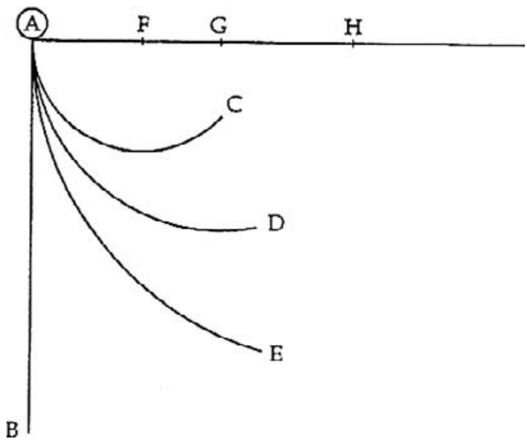

FIGURE 5b.

It is easy to recognize Piccolomini's paraphrase in the "violence" exerted by the centre of the circle. But it seems that Moletti hesitates concerning the natural motion, straight for the radii EB and EI, but curved along the circle for the points B and I; the natural movement of line BI is thus also that which leads it toward both RN and FO, depending on whether it is considered as part of the scheme or not. It seems therefore that the "natural" no longer represents a category properly speaking, but becomes an adjective permitting the description of what is happening. Points B and I are nonetheless considered as diverted with respect to a movement that would otherwise have been rectilinear or natural, and the space FR is said to be traversed by a violent movement. The classical demonstration can now continue: point B is diverted with respect to a rectilinear movement by a space FR and point I by the space ON, greater than FR.[40]

Lastly, I should note that the particular position of Fig. 5a, in which all the natural movements are vertical – which was not the case for Piccolomini – at first has no impact on Moletti's reasoning. The reason for this situation becomes clear when Moletti proposes another demonstration of what he calls "the principle of mechanics", which is that movement is easier on large circles than on small ones. For in this alternative demonstration, weight and the vertical intervene. The largest circle is said to be closer to the natural movement of vertical fall than the smaller, which can be seen in Fig. 5b where the two circles are tangent to the same vertical line. In this Moletti is directly inspired by a passage from Niccolò Tartaglia's *Quesiti*, who with a similar diagram says that the weight is greater on AE than on AC. We can also find this reasoning and a similar diagram, at almost the same moment, in the *Mechanicorum liber* of Guidobaldo dal Monte of 1577.[41]

It is thus clear that only the movement of vertical fall is natural for Moletti in his paraphrase of the *Mechanica.* Although the context in which he situates his circular motion is different from that of Cardano, the result is the same for our purposes: when gravity intervenes explicitly in the systems considered, no non-vertical rectilinear motion can be said to be natural.

MOLETTI, GUIDOBALDO DAL MONTE, AND THE LEVER

As we have seen in the two previous sections, in the commentaries on the *Mechanica* we cannot separate the analysis of circular motion, which is our main interest here, from the equilibrium of weights and their potential motion. As at the outset we asked the question of the relation between the categories of motion and the systems considered, the problem arises as to whether the first question of the *Mechanica* can be understood as an approach to the Archimedean law of the lever and serve to demonstrate it,

[40] Laird, *The Unfinished Mechanics*, p. 94.

[41] Tartaglia, *Quesiti e inventioni diverse*; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 119; Guidobaldo dal Monte, *Mechanicorum liber*; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 271 (first diagram fig. 1).

beyond the simple problem of the precision of scales, which is the explicit objective of the question. The function of the lever itself is raised in question 3 of the *Mechanica*, as we have seen, without a demonstration. The only element proper to the text of the *Mechanica* supporting this law of the lever is that the same force exerted at a greater distance from the centre produces a greater speed, because the proportion of natural movement is greater. Moletti expressed it in this way:

> the same power applied farther from the centre, aided by the natural speed of the circle, will produce so much more force.[42]

This argument is in fact used in the *Mechanica*, and then by certain commentators, but to explain the effectiveness of the sling in the twelfth question. We thus see to what degree the intuition of the link between force and movement is tied to the particular phenomenon considered.

In his *Mechanicorum liber,* Guidobaldo refers to Archimedes' treatise *On the Equilibrium of Planes* and reasons only with weights and verticals directed toward the centre of the world. Scales are nonetheless represented by a circle and the comparison with circles of different curvature intervenes to show, as did Moletti and Tartaglia (see Fig. 5b), that the longer the beam of the scales, the closer the circle it describes is to vertical natural motion, and the freer and hence the "heavier" the weight when compared to a shorter beam.[43]

When Guidobaldo comes much later to the lever, he again uses the geometry of the circle, of two concentric circles, to prove that the displacements of the extremities are proportional to the distance from the centre.[44] He is now able to state, as a deduction from this demonstration and from the Archimedean law of the lever,[45] that the ratio of the force to the weight equals the inverse of the ratio of their displacements. From this condition for equilibrium, he deduces that, in the case of motion, the first ratio must

[42] "Una stessa virtù più lontana dal centro applicata, aiutata dalla velocità naturale del cerchio, tanto maggior forza farà" (Moletti, *Dialogue*, ed. and trans. in Laird, *The Unfinished Mechanics*, pp. 92–93).

[43] Guidobaldo dal Monte, *Mechanicorum liber*; trans. Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 271.

[44] Dal Monte, *Mechanicorum liber*, Prop. IV, fols. 42–43; the demonstrations are omitted in the translation by Drake and Drabkin.

[45] This is demonstrated in the first proposition of his book on the lever, Dal Monte, *Mechanicorum liber*, fols. 38r–v; this demonstration is omitted in the translation by Drake and Drabkin.

exceed the second one.[46] It is thus not deduced from an analysis of the motion of the lever into two rectilinear motions, which would recall that of the *Mechanica*. It certainly seems that such a deduction cannot be made, for to my knowledge no one did it.

We have seen Tartaglia totally reject the analyses of the first question, and Cardano linger over them, while Guidobaldo ignores it. He uses the geometry of the circle in a simple geometrical way, in order to go from the classical static law of the lever to a proportion similar to that of Jordanus' mechanics, and from there to the lever in motion.[47]

When he speaks of the lever, Giovanni Battista Benedetti immediately refers to Archimedes, *On the Equilibrium of Planes*, Book I, prop. 6, because, he affirms, the reason given by Aristotle in the *Mechanica* is not correct.[48] This is perhaps why his critical reading goes into more depth on aspects other than those linked to the science of weights. He makes no reference to any of the authors of whom I have just spoken. Is he aware of them? I cannot say, but he seems to be addressing none of them, except for Tartaglia.

The analysis of motion in the first question of the *Mechanica* thus cannot be used to deduce with rigor a law of equilibrium in the absence of a principle that would link equilibrium to potential motion. The ideas that come from the *Mechanica* that are in fact used by the authors I have cited, especially Moletti, are the relations between distance and speed, then distance and efficacy of the force exerted or weight. They may have allowed Guidobaldo to propose his proportionality: the weights must be as the inverse of the spaces traversed during the movement of the lever. But this is not a deduction, for only the first part was the object of a demonstration.

BENEDETTI AND SLINGS

Giovanni Battista Benedetti criticizes Tartaglia and Jordanus de Nemore on certain points of the science of weights before returning to one of the propositions of the *Mechanica*, with no reference other than to Archimedes.

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[46] This is the proportion expressed by Jordanus de Nemore. According to Guidobaldo (prop. IV, Corrollary; trans. Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 300) it was necessary that the force just exceed this ratio for there to be movement rather than equilibrium.

[47] Jordanus de Nemore, *Liber jordani de ratione ponderis*, ed. and trans. in Moody and Clagett, *The Medieval Science of Weights*, p. 190; note that Jordanus himself used circles in certain parts of his treatise, but never two concentric circles.

[48] Benedetti, *Diversarum speculationum*; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 168.

After discussing the initial analysis of motion, Benedetti denies that the reason large scales are more precise than small ones is their greater speed, for it is the distance alone that counts.[49] This appears reasonable but eliminates the original appeal to potential motion, an appeal that Benedetti does not need, for he relies on the Archimedean demonstration of the law of the lever. It is thus only later, in his criticism of questions 8 and 12, that he seems to have taken advantage, despite his hostility to Aristotle, of a new inspiration. Benedetti's reaction to question 8 of the *Mechanica* is indeed very different from that of his contemporaries. Although he posed first as a partisan of the science of weights, it is here rotation in general, and not just vertical motion, that interests him, beyond any downward attraction. His main argument is already based on slings, although they are not brought up until question 12 of the *Mechanica*. Was he influenced by this original aspect of the ancient text or by his own practical observations? It is hard to affirm the former, but it is equally difficult to think that Benedetti had more opportunities to manipulate slings than others of his time. His official duties in Turin did include practical implementation of matters concerning urbanization, but nothing relating to this outdated weapon! I thus feel that it is indeed the twelfth question of the *Mechanica* that attracted his attention to the sling and allowed him to express in this way that the impetus, which is conserved, is rectilinear and not circular when freed from all constraint. To the three responses to Question 8, Benedetti adds a fourth:

> Fourth, any portion of corporeal matter which moves by itself when an impetus has been impressed on it by any external motive force has a natural tendency to move on a rectilinear, not a curved path. And so, if some portion of the circumference were separated off from the wheel in question, no doubt the separated part would move through the air in a straight line for some length of time. This we can understand by an example taken from slings used for throwing stones. In their case the impressed impetus of motion produces a rectilinear path by a certain natural propensity, when the stone shot out starts its rectilinear path; this path is along the straight line tangent to that circle which the stone previously described at the point at which it was let fly. And this accords with reason.[50]

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[49] Benedetti, *Diversarum speculationum*; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, pp. 180–182.

[50] "Quarta est, quia quaelibet pars corporea, quae à se movetur, impetu eidem à qualibet extrinseca virtute movente impraesso, habet naturalem inclinationem ad rectum iter, non

It is, curiously, an appeal to reason, and not to the senses as in Piccolomini, that ends this text with the observation and functioning of a well-known object. Reason consents to what it sees, or rather completes the observation and decides on a certain affirmation.

The criticism of the text of question 12, the one dealing with slings, is hardly more fundamental than that of the first question; Benedetti remarks with reason that the string held in the hand certainly does not remain radial during rotation, as can be seen in the diagram (Fig. 6), in which points E represent the movement of the hand and points A that of the sling.

In any case, once the body is released, it follows the tangent, all the faster thanks to the impetus it has accumulated during the successive spins of the sling. Notice that once again Benedetti uses a notion of impetus that one does not find in other commentators, who sometimes evoke it in a non-specific fashion. What is specific in the use made of it by Benedetti, in accordance with the Buridanian concept transmitted by Albert of Saxony, is the idea of a quantity accumulated in the body, a quantity that persists in

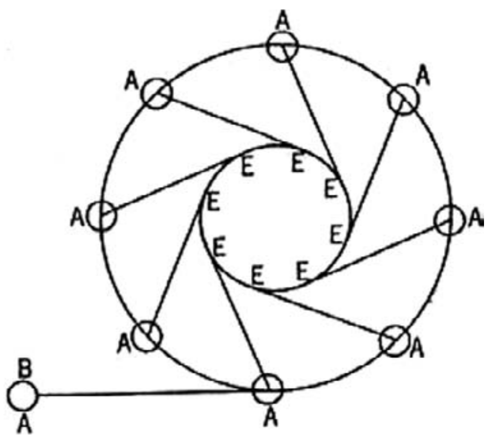

FIGURE 6.

autem curvum, unde si à dicta rota particula aliqua sue circunferentiae disiungeretur, absque dubio per aliquod temporis spatium pars separata recto itinere feretur per aerem, ut exemplo à fundis, quibus iaciuntur lapides, sumpto, cognocere possumus, in quibus, impetus motus impressus naturali quadam propensione rectum iter peragit, cum evibratus lapis, per lineam rectam contiguam giro, quem primo faciebat, in puncto, in quo dimissus fuit, rectum iter instituat, ut rationi consentaneum est" (Benedetti, *Diversarum speculationum*, De mechanica, cap. 14, p. 159; Engl. trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, pp. 186–187. Fig. 5 is taken from this work, p. 189).

itself until destroyed by an outside resistance. This notion is not exceptional at this time in northern Italy.[51]

In his criticisms of the *Physics*, Benedetti directly attacked the Aristotelian category of natural motion for upward or downward motion, for they are not perpetual; he maintains the natural character of circular motion, which alone can perpetuate itself.[52] One considers that the part separated from the whole will return to its natural place by the shortest path, which likely includes the starting point of the tangent, but Benedetti does not return to this, except to say that this is not the natural starting point. Rectilinear motion in general is thus not clearly imagined, except to say that none is truly natural. The "natural tendency" to the straight line does not imply that the motion is necessarily natural. Just as with Moletti, it seems that the word "natural" is at times used as a word from every-day language, at times in its Aristotelian sense. One can clearly sense that Benedetti is more interested in the difference between the part and the whole than in the opposition between natural motion and violent motion, and that these two problems fit together poorly.

This is perhaps why the system of the sling, with the image of the projectile that flies off and keeps its rectilinear impetus, seems more important for his reasoning than does the analysis of the first question of the *Mechanica* and the categories of the *Physics.* A third mention of the sling can be seen with the same argumentation by Benedetti in his letter to Paul Capra de Novare, this time to explain that when a body is in rotation, any part that is freed from the constraint goes off in a rectilinear trajectory.[53] But in another letter to the same person, Benedetti imagines the rotation of millstones and spinning tops to affirm that the various parts of the millstone would also escape in a straight line if they were freed from the constraint that keeps them on a circle.[54] He then affirms that any part separated from the whole moves first

[51] Albert of Saxony's commentary on Aristotle's *De caelo* and *De mundo* was copied or published in Pavia in 1481, in Padua in 1497, and in Venice in 1492 and 1520. This commentary was also part of the Georges Lockert collection of 1516–1518, and was thus easily accessible in northern Italy at this time. Yet Benedetti never cites it explicitly in his *Diversarum speculationum mathematicarum et physicarum liber*, whereas he cites Aristotle, Euclid and Archimedes, Vitellio and Alhazen, Jordanus, and Tartaglia. On the history of impetus and the texts present in Italy at this time, see Sarnowsky's article in this volume, which notes that Philoponos's commentary on Aristotle's *Physics* was printed in Venice in 1539, introducing the notion of impetus for use by Buonamici in his *De motu*.

[52] Benedetti, *Diversarum speculationum*, Disputationes, cap. 25, p. 184; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 217.

[53] Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 230.

[54] *Ibid.*, pp. 228–229.

along a tangential trajectory before moving toward the centre of the world. He thus uses the sling to illustrate a phenomenon that he considers to be general, belonging to any rotating body, although we cannot definitively affirm that he was inspired in this by his reading of the *Mechanica*.

CONCLUSION

Although the attitude of the commentators depends strongly on what they think of Aristotle in general, the analysis of motion on an arc into natural lateral motion and violent motion toward the centre was remarkably well accepted. No commentary indicates a possible problem of compatibility with the Aristotelian corpus itself. In the *Mechanica* it is in fact a question of terrestrial rotational motion, which is not much present in Aristotelian physics in general and which cannot be associated with the circular movement of the stars. The reactions of Piccolomini and then of Benedetti show, in their differences, a common intuition of the constraint due to the rigidity of the rotating devise, without explicitly using the word "constraint", despite the fact that the term exists in the treatise *De caelo*.[55] They all manifest a difficulty in characterizing the rotational motion that is not that of the stars, circular, and perpetual. Benedetti does seem to differ from the other authors I have discussed in his use of impetus and of the sling as an emblematic figure. He does not seem to fit into a continuity that would result in a new status for rectilinear motion.

To the two questions raised in the introduction, I can answer that the *Mechanica* opens the possibility of adapting the categories of natural and violent motion to artificial schemas, and in so doing makes them more flexible, despite the fact that there is no explicit debate on this subject among the commentators. The general categories are not at the outset modified with respect to what is found in the *Physics* with regard to falling and throwing motion, nor are they adapted in turn to one or more machines in particular. The schema conceived in the *Mechanica* is in fact unique: it is the movement of a body subject to a constraint that makes it turn around a centre, with the possibility – real or imagined – for a part to escape from this constraint. The commentators are more or less sensitive to this suggested unification, with its successes and failures, and orient the central problem at times toward the lever, at others toward the rotation of various objects for which the vertical is not a special direction.

If the opposition between violent and natural motion is not clearly modified in its definition, its status is changed: it seems less satisfying since

[55] Aristotle, *De caelo*, III.2, 301a–b.

its compatibility with the opposition between constrained and free motion cannot be obtained without difficulty as soon as one leaves the particular position of the horizontal beam of the scales. The authors seem then to hesitate between a use of the word “natural” as belonging to a system of explanation, to an ontology, and a use that comes to them spontaneously. Piccolomini, for example, asserted that “what is moved along a circle is said to be carried naturally”; and Benedetti, that “any of the corporeal parts … has a natural tendency to rectilinear motion and not curved”.

Thus the pertinence for mechanics of the traditional duality between natural and violent motion is brought into question by the inaccuracies and distortions that occur in the texts we have considered, by the very fact that this duality is not modified from the outset. The scholastic modifications, very clear in Cardano, concerned the circular motion of the stars, but not the rotation of simple machines, which had not yet been considered in this context. Like the solution to the problem of the projectile in terms of impetus,[56] the treatment of rotation in terms of constraints and freedom first arose within an Aristotelian context before entering the modern era.

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[56] See Sarnowsky’s article in this volume.

WALTER ROY LAIRD[1]

NATURE, MECHANICS, AND VOLUNTARY MOVEMENT IN GIUSEPPE MOLETTI'S LECTURES ON THE PSEUDO-ARISTOTELIAN *MECHANICA*

Quoting the poet Antiphon, the introduction to the pseudo-Aristotelian *Mechanica* asserts that: "we master by art what we are conquered in by nature" (*τέχνῃ γὰρ κρατοῦμεν, ὧν φύσει νικώμεθα*).[2] Prompted largely by this suggestion, several 16th-century mechanical writers pondered exactly what it might mean to overcome nature with art and how mechanics could actually be said to do so. Gianni Micheli has collected together a number of passages that suggest that generally they saw that mechanical effects, despite working against nature, can nevertheless be reduced to natural phenomena.[3] In fact, as Egidio Festa and Sophie Roux have shown for the Aristotelian tradition of mechanics at least, mechanical effects are not so much opposed to or contrary to nature – *contra naturam* – as beyond or outside of naturem – *praeter naturam*. The braggarts whom Galileo condemned for vainly claiming to be able to cheat nature were more likely practical engineers than learned mechanics.[4]

That art imitates nature in some sense was already a commonplace by the 16th century: that only by somehow imitating nature with art can we overcome natural difficulties for our own use and benefit. But in the introduction to his lectures on the pseudo-Aristotelian *Mechanica* given at Padua in the 1580s, Giuseppe Moletti suggested not merely that mechanics imitates nature, but also that nature itself uses mechanics in its own works. The implication of this suggestion – although Moletti himself did not draw this out – is that, to the extent that nature itself uses mechanics, a science of mechanics will also be a science of nature. One of Moletti's sources for the relation between nature and mechanics was the *De motu animalium*, in which Aristotle made several references, both implicit and explicit, to the mechanical aspects of voluntary motions, as distinct from natural and violent motions. In what follows I'd like to examine how Moletti used this text and his other sources to suggest that the boundary between art and

1 Department of History and College of the Humanities, Carleton University, Ottawa, Canada.
2 Pseudo-Aristotle, *Mechanica*, 847a21.
3 Micheli, *Le origini del concetto di macchina*, pp. 144–152.
4 Festa and Roux, "Le '*παρὰ φύσιν*' et l'imitation de la nature" in *Largo Campo di Filosofare*; Galilei, *Le mecaniche*, p. 45; English trans. in Galilei, *On Motion and on Mechanics*, p. 147.

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 173–183.

nature, between mechanics and natural philosophy, was not so impervious as some interpreters of Aristotle and writers on mechanics have assumed.

THE NATURE OF MECHANICS

In antiquity, theoretical science – the goal of which is to know and understand what is necessarily and eternally true – was generally seen as distinct from practical art – the goal of which is to make or produce contingent and useful things. Insofar as it was an art, ancient mechanics was obviously not a theoretical science. Further, according to Fritz Krafft, because mechanics was also tainted by the odour of deceit and deception – which was in fact part of the original and more general meaning of the word "*machina*" – it could never have contributed to a science of nature. For how could deceit and deception add to our knowledge of what is eternally true?[5] Although the taint of deception seems to have diminished by the Middle Ages, mechanics was still identified then with the mechanical or sellularian arts – agriculture, weaving, metalwork, and the like – which were explicitly called illiberal and adulterine since they were adulterated with practice and physical necessity.[6] Shakespeare, in *A Midsummernight's Dream*, can still refer to joiners, weavers, and carpenters as "rude mechanicals". In contrast, the medieval science of weights – *scientia de ponderibus* – which was, by analogy to astronomy, optics, and the like, a theoretical science – would not be assimilated to mechanics in the broader sense until the 16th century.

In the course of the 16th century, with the recovery of ancient mechanical texts and their assimilation with the medieval *scientia de ponderibus*, mechanics emerged as a mathematical science, promising rational and mathematical explanations of the marvellous effects produced by art. As a science, mechanics came generally to be understood as one of the intermediate sciences (*scientiae mediae*), since it lay in between mathematics and natural philosophy. On this question as well as on the nature and status of mechanics in general, the introduction to the Pseudo-Aristotelian *Mechanica* was perhaps the most influential text.[7] Mechanical problems, its author

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[5] Fritz Krafft, *Die Anfänge einer theoretischen Mechanik*, pp. 27–28, pp. 237–238, *et passim*; cited in Festa and Roux, "Le '*παρὰ φύσιν*' et l'imitation de la nature", p. 238n, and cited and quoted in Micheli, *Le origini del concetto di macchina*, pp. 29–30.

[6] Ovitt, "The Status of the Mechanical Arts in Medieval Classifications of Learning"; Whitney, "Paradise Restored: The Mechanical Arts from Antiquity through the Thirteenth Century".

[7] On the renaissance tradition of the *Mechanica*, see Rose and Drake, "The Pseudo-Aristotelian *Questions of Mechanics* in Renaissance Culture"; Laird, "The Scope of Renaissance Mechanics".

wrote, are neither entirely physical nor entirely mathematical, but share in both: in mechanics, mathematics provides the "how" (τὸ ὥς) and natural philosophy provides the "what" (τὸ περί). In practice, this was usually taken to mean that in mechanics, as in astronomy, optics, and harmonics, one borrowed conclusions from the pure mathematical sciences of arithmetic and geometry and then applied them as principles in the mathematical study of some narrow aspect of nature – the motion of the stars, vision, consonance, and, in the case of mechanics, weight, power, resistance, and speed. Similarly, natural philosophy provided certain of its own conclusions to be used as principles by mechanics – notably that heavy bodies tend towards the centre of the universe, and that this tendency increases with weight.

In fact, the *Mechanica* assigned to nature an intimate role in mechanics, since its central principle depends on the analysis of circular movement into a natural, downwards component, and an unnatural, violent component towards the centre of rotation. This analysis is claimed to provide the natural reason for the central mechanical principle: that the longer the radius, the more speed and power there will be, since a power moving downwards on the longer radius has more of the natural tendency and less of the violent.[8] Obviously the analysis into natural and violent components strictly holds only for a balance or lever with reference to the horizontal, and only when the powers are weights, i.e., acting from their natural tendency. Nevertheless, the principle of circular movement, generalized, was equivalent to what is now called the law of the lever. In fact, several mechanical writers in the 16th century – including Moletti – asserted that Aristotle had established physically what Archimedes had proved only mathematically. What is significant for us here is that nature is intimately involved in mechanics, though nature is implicitly understood in the narrow sense of the tendency for rectilinear downward motion of a heavy body.

While the *Mechanica* suggested that mechanical effects involved natural movements and tendencies in order to overcome nature, it did not explicitly state that mechanics imitates nature in order to do so. In a well-known passage in *Physics II*, however, while arguing that nature, like the arts, acts for a purpose, Aristotle suggested hypothetically that art and nature might work in the same way:

> If a house, e.g., had been a thing made by nature, it would have been made in the same way as it is now by art; and if things made by nature were made also by art, they would

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[8] Pseudo-Aristotle, *Mechanica*, 849a6–b19.

> come to be in the same way as by nature. Each step then in the series is for the sake of the next; and generally art partly completes what nature cannot bring to a finish, and partly imitates her.[9]

A little later, he added that: "if the ship-building art were in the wood, it would produce the same results *by nature*".[10] Notice that the point is hypothetical: Aristotle does not say that nature and art do in fact act in the same way, but rather that they would if they were to make the same things.

THE MECHANICS OF NATURE

Giuseppe Moletti went one step further, to assert that nature does in fact employ mechanics in its works. As I mentioned before, Moletti lectured on the *Mechanica* at the University of Padua, first in the fall of 1581, then in the winter of 1582, and again in 1585–1586. As the heading of his lecture notes suggests, he seems to have lectured on each of the questions extemporaneously, for what we have of his lectures is only an introduction to the book as a whole.[11] This introduction consists in the usual *accessus ad auctorem* – on the author and title of the work, its scope and purpose, its relation to other arts and sciences, other writers on mechanics, and the like. Section 10 of this introduction is devoted to the question of whether mechanics is found in the works of nature, and whether, if nature were to work in the way of men, it would use mechanics.

The question of whether nature uses mechanics in its works, Moletti begins, is not unpleasant. He first notes that he will sometimes call mechanics an art, and sometimes a science, although he had already devoted an earlier section of the introduction to arguing that mechanics is properly a theoretical science in the full, Aristotelian sense. For despite its practical applications, for which it is often thought of as an art, mechanics properly contemplates the principles and causes of mechanical devices, "which are

[9] Aristotle, *Physics* II.8, 199a13–18; trans. Hardie and Gaye, in McKeon, *The Basic Works of Aristotle*.
[10] Aristotle, *Physics* II.8, 199a27–30; trans. Hardie and Gaye, in McKeon, *The Basic Works of Aristotle*.
[11] Moletti's lecture notes, in Moletti's own hand and in places heavily revised, are preserved in Milan, Biblioteca Ambrosiana MS. S100 sup., fols. 156–196; the heading, on fol. 156r, reads "1581 octobre die 6. hora 14 1/2 in circa repetita 1582 10 februarie. In librum Mechanicorum Aristotelis expositio tumultuaria et ex tempore Josephi Moletii in patavino gymnasio mathematicas profitentis"; the Rolls of the University of Padua list him as having lectured in 1585–1586 on Euclid and Aristotle's *Mechanica* – see Carugo, "L'Insegnamento della matematica all' Università di Padova prima e dopo Galileo", p. 176.

entirely necessary and not contingent and in no way depend on our will".[12] Both the sciences and the arts, he then observes, imitate nature to the extent they can. Indeed, some of the arts, notably mechanics, not only imitate nature in their works, but also conquer it.[13] Mechanics can do this because it derives its means and its principles from nature, notably the power of circular motion, which men have learned from the daily rotation of the heavens. This idea of the origin of mechanics was suggested to Moletti by Vitruvius, whom he cites elsewhere on this point, though not here, and to whom he is indebted for some of the examples that follow. In *De architectura* Vitruvius wrote that "all the machinery of things is born of nature and the rotation of the world instituted by her as teacher and mistress", where the word "machinery" (*machinatio*) reminds one of the commonplace description of the cosmos as the *machina mundi*.[14] For Moletti, though we cannot imitate the rotation of the heavens in its material (since the celestial material, which is neither heavy nor light, is not available to the mechanic), still we can imitate it in its circular figure. Further, we discovered the lever by observing how our arms move in lifting a weight, and by observing a bull using its horns. Horses hooves taught us thebattering ram; chewing taught

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[12] "Et si quis dicet, nonne mechanicus circa factionem versatur, nam construit machinas, eas que molitur ac machinatur, respondetur ipsum praecipere artificibus constructionem machinarum, quarum tantum principia et causas contemplatur, quae omnino necessaria sunt et non contingentia ac nullo pacto a voluntate nostra pendent" (Moletti, *In librum mechanicorum Aristotelis expositio* [hereafter *Expositio*], fol. 199r). In my quotations from the *Expositio* I have silently expanded abbreviations, transcribed consonantal *u* as *v*, and capitalized and punctuated according to modern usage. Where the sense seemed to require, I have inserted letters and words between pointed brackets.

[13] "Non iniucunda contemplatio est quae proponitur nobis discutenda. An scilicet in operibus naturae ars mechanica reperiatur, et ansi natura more hominum operata esset, usa fuisset mechanica arte. Scire convenit me vocare scientiam mechanicam aliquando artem, ut fieri saepe solet, quandoquidem etiam et artes saepe communi nomine appellatur scientiae. Ut igitur melius possumus declarare *Problemata* <*mechanica*> proposita, primo loco sciendum nobis est, quod tum scientiae tum artes inquantum possunt imitantur naturam, imo tunc recte philosophatur quis, quando quadam naturali methodo imitatur ordinem productionemque naturalem. Ob quam methodum Aristoteles divinus existimatus est. Recte quidem etiam artificis effectio procedit quando quadam solertia, ac praestantia ingenii imitatur naturam. Verum hoc admiratione dignum esse putamus, nonnullas esse artes, quae non solum imitantur naturam sed eam quidem in suis effectionibus vincunt, inter quas principem locum tenet mechanica" (Moletti, *Expositio*, fol. 175r).

[14] "Omnis autem est machinatio rerum natura procreata ac praeciptrice et magistra mundi versatione instituta" (Vitruvius, *De architectura* X. 1. 4; trans. Granger, vol. II, pp. 276–277).

us how to grind grain.[15] When nature designed the universe to revolve with several motions in order to give us the seasons (this is also from Vitruvius), it designed it as a sphere from material that was neither heavy nor light, since its motion must be perpetual: thus nature, according to Moletti, used a mechanical consideration and a mechanical method.[16] Now nature does not need gears and wheels and axels in the construction of the celestial spheres because the material is neither heavy nor light. Although our machines are limited and our power finite, nevertheless with them we can overcome

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[15] "Quae imitando naturam postmodo principiis et mediis quae ab ea didicit eam superat. Docet nos natura quotidiana versatione et revolutione universi, ingentem vim motus circularis, quo quidem superatur gravitas gravium et levitas levium. Cognoscit igitur mechanicus mundum circumvolui maxima facilitate, quod quidem evenire intendit tum ratione materiae tum etiam ratione figurae. Materia nempe corporum caelestium neque gravis neque levis est, nam si gravis aut levis esset perennus motus non esset. Haec igitur materiae natura mechanicus vincitur, nam illa materia in conficiendis machinis <non> uti potest quae gravis non sit. Quapropter cum non possit imitati in omnibus naturam eam imitatur in figura circulari, quam machinis tribuit, et in motu etiam circulari aut ex circularibus composito. Si igitur haec scientia mechanica non fuisset in naturalibus rebus, homo non adinvenisset suam artem quam existet. Praeteria homo observans ea quae inseipso reperiuntur adinvenit vectem, nam dum movemus brachium ad elevandum pondus, talis motus est vectis motio. Taurus etiam cornibus docuit adinventionem vectis. Eques calcibus docuit adinventionem arietum. Et ut uno verbo dicam animalia suis motibus suisque instrumentis et organis docent nos artem mechanicam. Commanducatio docuit molitis frumentorum" (Moletti, *Expositio*, fol. 175r–v).

[16] "Est igitur omnino in operibus quidem naturae ars mechanica, quam diligente animadversione talium operum maxima solertia artique sapientes adinvenerunt, quamque etiam non haberemus si naturalis non esset. Eamque existimo naturam abhibuisse in naturalibus rebus construendis. Natura quidem ex cognitione finis operatur, verum maxime distat naturae operatio ab operatione nostra, nam natura simul et semel cognoscit finem, et media quae ad finem conducunt. Ob id quia natura vis quaedam incorporea est. Nos vero quamvis cognoscimus finem saepe tamen caremus mediis quae nos ducere ad finem possunt, quapropter non semper finem consequi possumus. Cognoverat natura necessitatem lucis ac caloris et frigoris aliarumque rerum, tum ad conservationem universi, tum ad perpetuum ortum et interitum rerum efficiendum. Haec consequi non quiverat si fecisset totum caelum ita luminosum ut sol est, et immobile, nam semper res in eodem permansissent statu. Quapropter constituit praeter reliqua caelestia corpora solem autorem luminis et caloris, cui tribuit motum circularem, quo motu una revolutione universum illustrat et imponit calorem suum. Hic quidem motus sine sphaerico corpore minime perennus esse potuisset, quare dedit corporibus caelestibus sphaericam figuram. Verum cognoverat etiam quod si corpora caelestia gravia aut levia constituisset non fuissent perpetua duratura ad motum, et ideo eis tribuit materiam quae neque gravis neque levis est. Hae omnes sunt mechanicae considerationes; quare natura in conficiendis rebus usa est mechanica consideratione ac mechanica methodo. Adhuc si sol tantum motu primi mobilis moveretur, tempora semper fuisset eiusdem temperaturae, et ideo tribuit soli praeter motum diurnum quendam proprium, quo accedit ad capita nostra atque ab eis recedit" (Moletti, *Expositio*, fols. 175v–178r; fols. 176 and 177 are blank).

nature, by which Moletti seems to mean specifically the resistance of heavy things. For nature gave us oxen, which we tame by art and harness to move heavy loads and to plough the fields.[17] If art had not made the vineyard, the olive grove, and cultivated the field, he continues, nature would not have given us the vine, the olive, or grain, since wild grapes, olives, and grain are unsuitable for our uses. And without the wine press, olive press, and mill, we would not have wine, oil, or flour. Similarly, while nature gives us wool, flax, and cotton, art gives us the means to spin yarn and weave cloth, which we learned from the spider. So in general, while nature gives us the raw materials, art makes them useful to man.[18]

Then, turning to the art of building, Moletti offers an allegorical interpretation of the battle of Zeus against the Giants or Titans. The Giants signify mechanics, upon whom Zeus hurls from above lightning – i.e., the light of intelligence – and heavy things – i.e., the greatest delight. If by the last Moletti understands usefulness, then we have here the two main claims of 16th-century mechanics – that mechanics is both a demonstrative science and of the greatest use. Moletti corroborates this allegorical interpretation of the Giants by noting that according to Plutarch, Marcellus called Archimedes

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[17] "Verum quis dicat, velim scire quibus dentatis rotis ac timpanis et axibus in hac versatione et conversione corporum caelestium natura usa sit. Respondetur naturam non indigere istis rotulis dentatis neque timpanis aut axibus, ob id quia potuit et voluit materiam corporum caelestium neque gravem neque levem constituere. Verum si more hominum operata esset indiguisset profecto dentatis rotis, timpanes, et reliquis, quibus utimur nos, propterea quod qualitatibus materiae astricti sumus, atque etiam vis nostra finita et terminata est; imo comparata vi naturae nihil est. Hinc est quod nos non possumus ullo pacto machinam perenni motu mobilem fabricare: at de hoc suo loco. Et quamvis summus materiae addicti, et vis nostra finita, tamen tali vi naturam superamus. Nam nobis natura dedit boves quos cicuramus arte et ingenio, quosque etiam iugo submittimus ut nobis tum ad onera trahenda, tum etiam ad arandam terram aratro iuncto et addito, possint commode inservire" (Moletti, *Expositio*, fol. 178r).

[18] "Natura igitur, si industria humana non fuisset, nobis non dedisset neque vinum neque oleum neque frumentum. Ars quidem confecit vinetum, olivetum, et sevit frumentum. Deinde adinvenit torcularia, trapeta, ac molendina, quibus machinis habemus vinum, oleum, ac farinam, quae namquam nobis subministrasset natura. Producit quidem natura quasdam uvas, quae lambrusce vocantur, quaeque apte non sunt ad vinum exprimendum; pariter natura producit olivas silvestres, quae minime usui nostro commode sunt, neque silvestre frumentum. Quapropter antiquitas ob id quod Baccus, Minerva, et Ceres ostenderunt hominibus atque adinvenerunt modum plantandi recte vineas, oliveta, ac serendi frumentum, et simul adinvenetur instrumenta apta ad haec habenda, eas inter deos connumeravit. Superamus etiam naturam in conficiendis pannis cuiuscunque materiae, nanque nobis natura tantum lana dat aut linum aut gossipium. Mechanica ars adinvenit modum filandi lanam, linum, et gossipium. Praeterea imitando Aracnem adinvenit litia, pectines, texturiam, radium, naviculam atque ea omnia quae ad texendum faciunt" (Moletti, *Expositio*, fol. 178r).

"Briareus", a giant with 100 arms, for his defence of Syracuse.[19] Continuing in this allegorical vein, Moletti then reinterprets the myth of Atlas, who was turned to stone by the head of Medusa borne by Perseus, and the myth of Theseus and the Minotaur, both to illustrate the power of mechanics.[20]

Moletti took the myth of Atlas explicitly from Aristotle's *De motu animalium*, where it is used to illustrate the principle that every motion

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[19] "Nonne etiam superamus naturam arte edificatiore, quae construit domus altissimas, lapidibus ingentibus atque insignis gravitatis, quos ad summitatos domorum maxima facilitate ducit. Apticior quidem magna admiratione dum considero excellentiam ac profunditatem scientiae minus divinae, atque mihi in mentem venit gigantum fabula, quae certe existimo significare nobis mechanicas machinationes, quibus gigantes, qui intelliguntur mechanici, adinvenentur viam elevandi ad quamcunque altitudinem gravia, ac montes montibus superponere caelumque petere, quod innuit constructionem mobilium per se machinarum motus corporum caelestium imitandum. Ob quam inventionem hoc est contra naturam molitionem Juppiter concilio deorum eos fulminavit, ac gravibus rebus subjecit, quod innuit eis dedisse lumen intelligentiae, ac delectationem maximam, qua ducti, continuo moliuntur ac machina<n>tur, artificiosas machinas, quibus superant gravium gravitates ac naturae omnes fere difficultates. Et ob id rebus gravibus subiciuntur, cum circa ea mechanici versantur, qui ut dixi interpraetantur gigantes, proptereaque freti ingenii acumine, naturalibus rebus bellum induxerunt, quando eas suis commodis subiciunt, atque ut illis placet illas tractant. Vere igitur mechanici gigantes sunt, quando efficere audent id, quod fieri non posse existimatur, et praestant quidem ac quod optant conficiunt. Et Plutarchus auctor gravissimus refert Marcellum Archimedem vocasse Briareum, hoc est gigantem centum habentem brachia. Hac scientia facta bene Syracusaro supradicto, idem Plutarco referente, dixisse aiunt, si haberem locum ad quem machinam locaret, possim hanc terram quam partem propellerem" (Moletti, *Expositio*, fol. 178r–v; see Plutarch, Life of Marcellus xvii, in *Plutarch's Lives*, vol. 5, pp. 478–479).

[20] "Et Aristotelis in libello *De animalium motibus* refert fabulam antiquorum de Atlante, qui pedibus terrae insistit, brachiis autem caelum vertit. Quae fabula nobis indicat sphaerae mobilis inventionem et molitionem. Nam Atlas dum terrae insisteret brachiis molitus est machinam caelorum motus imitantem. Hic quidem Atlas a Perseo, qui petasum et talaria Mercurii gerebat etiam Medusae caput, atque in lapidem conversus est. Perseus ingenium indicat, et caput Medusae obiectum; quia quidem ob id quod caelum ascendit ac per omnia permeat habere alos dicunt. Dum hoc scilicet ingenium dum obiectum intuetur, Pers<eo> affigitur Atlas; lapideus fit, hoc est, immobilis, nam hic est contemplationis effectus. Ut Plutarcus etiam refe<r>t de Archimede, qui contemplationibus geometricis affixus, non sensit urbis capturam neque militem super se stantem. Manifesta est Daedali fabula, qui quidem aiunt adinvenisse artem volandi, et hoc innuit adinventionem remorum, velorum, ac artis nauticae. Ac labyrinthus fabricatus est, quod est eum reliquisse artem de navium constructione, in quo inclusus Minataurus erat. Hoc est finis artis, ad quem consequendum, nemo aptus fuit nisi qui filo quodam usus est, hoc est usus est ordine ac studio ordinato: assiduitate, ac quadam pertinatia, quod innuitur ex offa illius, quae faucibus Minotauri Theseus, hoc thesium est constructio, iniciat. Et difficultas quam Minotaurus etiam indicat superatur Thesium ordine, qui filus est assiduo ac tenaci studio, quod offam indicat. Minotaurus quoddam genus navis est, a Daedalo adinventum" (Moletti, *Expositio*, fols. 178v–179r).

arises from something fixed and immobile. According to Aristotle, Atlas could turn the heavens only if his feet were fixed on a fixed earth; but then the earth must be held immobile with an even greater power than Atlas's, which Aristotle suggested was unlikely. With this argument, then, Aristotle proved that an internal mover, like Atlas, cannot account for the motion of the whole cosmos, which leads him to posit an unmoved mover that is not itself part of the cosmos.[21] Moletti has inverted – or at least ignored – Aristotle's original intention in order to read this myth as an allegory for the power of mechanics.

MECHANICS AND VOLUNTARY MOTIONS

The *De motu animalium*, like Aristotle's other works on animals, had a minor place in the curriculum of the medieval and renaissance university, although, like the *Mechanica*, it was included in most of the editions of Aristotle's works that were published in the 16th century.[22] But one accident of its printing history could have brought it specifically to the attention of mechanics. Niccolò Leonico Tomeo made paraphrased translations of several works of Aristotle, including the *De motu animalium*, and these were printed in a small volume together with his translation of the *Mechanica*.[23] A mathematician, then, with no special interest in or expertise in Aristotelian natural philosophy, having picked up Leonico's slender volume of Aristotelian paraphrases for the *Mechanica*, could easily find himself reading the *De motu animalium*.

This was probably how the *De motu* came to the attention of Moletti, who otherwise shows little interest in or knowledge of Aristotle's works besides the *Mechanica*. Moletti first drew on the *De motu animalium* in the second day of his *Dialogue on Mechanics* of 1576.[24] There his purpose was to enumerate and describe the four principles of mechanics, which he listed as the moving power, the resistance, the machine, and the immobile. His entire discussion of the immobile principle loosely follows Aristotle's in the *De motu*, with many of the same examples and instances. Now, Aristotle's concern in the *De motu animalium* was neither with natural, involuntary movements (such as heartbeat, digestion and breathing) nor with violent movements, but with movements that are under the voluntary control of animals. At the beginning of the work Aristotle was at pains to show that

[21] Aristotle, *De motu animalium*, 699a27–700a6.
[22] Cranz, *A Bibliography of Aristotle Editions*, 1501–1600.
[23] Leonico, *Opuscula*.
[24] Moletti, *Dialogue on Mechanics*, pp. 130–137.

such voluntary movements of animals take place as the result of something immobile both within the animal itself and outside of it. To illustrate the internal immobile, Aristotle analyzed the movement of an animal's arm or leg into a circle, identifying the joint as the immobile centre around which the movement takes place. The external immobile he then identified with the ground over which the animal walks or crawls, the water through which it swims, or the air through which it flies.[25] It is entirely possible that Aristotle's analysis of the movements of arms and legs in the *De motu animalium* was the source or inspiration of the principle of circular movement in the later *Mechanica*. Moletti, at least, seems to have been struck by the similarity. In the *Dialogue*, he read the two passages together, for in his discussion there of the *De motu* passage he calls the immobile centre of movement of shoulders and elbows the *hypomochlion* or fulcrum, a word that is used of levers in the *Mechanica* but that does not appear in the *De motu animalium*.[26]

Later in the *De motu animalium*, in his discussion of the role of *phantasia* in the motion of animals, Aristotle compared the movement of animals to that of *automata* or automatic puppets, where the sinews and bones of the animal are like the cables and pegs of the puppet.[27] And in the *Mechanica* itself there is one problem that explicitly suggests that some voluntary movements of animals have mechanical explanations. Question 30 asks why a man when standing up leans forward and moves his feet under his body.[28] Although the solution in the *Mechanica* vaguely argued that the head and the feet should be in a vertical line in order to stand up, several later commentators explicitly invoked Archimedean centres of gravity. Such passages in the Aristotelian literature suggest that there is a parallel between the voluntary movements of animals and the motions produced by machines, though beyond Moletti I do not know of any mechanical writers that drew explicitly on the *De motu animalium*.

In sum, Aristotle in the *De motu animalium* analyzed the movement of limbs into circular motion and compared voluntary movements to the movements of automata. The author of the *Mechanica*, in similarly analyzing mechanical movements using the circle, was perhaps following him here. If this was the case, then mechanics truly was discovered within the motions of natural things. But the *Mechanica* built nature into mechanics in yet another way. The natural tendency of heavy bodies to fall to the centre

[25] Aristotle, *De motu animalium*, 698a14–699a11.
[26] Moletti, *Dialogue on Mechanics*, p. 137.
[27] Aristotle, *De motu animalium*, 701b2–10.
[28] Pseudo-Aristotle, *Mechanica*, 857b21–858a2.

of the earth is an essential part of the analysis of mechanical movement that leads to the central Aristotelian principle of mechanics, the principle of circular movement. And finally, Moletti went even further, to suggest that nature itself uses mechanics in its works. This implies – though Moletti did not say so – that to understand the necessary principles and causes of machines such as the lever is also to understand the necessary principles and causes of the movements of arms and legs, and perhaps of other natural things as well. From here it is but a small conceptual step to seeing such principles and causes to be universal in nature and to be precisely what mechanics imitates in order to effect its works. Mechanics is thus on the verge of becoming the science of nature.

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MECHANICS AND NATURAL PHILOSOPHY IN LATE 16TH-CENTURY PISA: CESALPINO AND BUONAMICI, HUMANIST MASTERS OF THE FACULTY OF ARTS

Galileo matriculated at the Faculty of Arts of the University of Pisa on September 5, 1580 (not 1581, as almost all his biographers, including Favaro and Drake, indicate) to do studies in natural philosophy and medicine; he left the University in 1585 without completing his studies. He was later named lecturer in mathematics at this Faculty in 1589, where he would teach until 1592, when at age 27 he obtained the chair in mathematics in Padua.

Our direct knowledge of this Tuscan period of Galileo is quite poor, and his writings from this period still pose serious problems for historians of science (in particular the *Juvenilia* and the texts on motion published for the first time by Alberi and then by Favaro in the first volume of the National Edition under the title *De motu antiquiora*).[2] Even less well known is the cultural context of this Tuscan period of Galileo, and in particular the thinking of the professors teaching at the University of Pisa when he was a student and later a lecturer in mathematics from 1589 to 1592.

In his *Études galiléennes*, Alexandre Koyré had the merit of drawing attention to Francesco Buonamici's immense work, *De motu*. A certain number of studies were later devoted to the cultural context of Galileo's university education by Eugenio Garin, Nicola Badaloni, Charles Bertrand Schmitt, William Wallace, and more recently by Michele Camerota and me.[3] We must nonetheless recognize that our knowledge of the works of the masters of the Studio Pisano of this period and our understanding of their natural philosophy are today very fragmentary. Almost no-one studies these authors anymore, and we thus remain ignorant of the content of the immense Aristotelian literature of the late Renaissance, for which there nonetheless exist excellent catalogues.[4] In such conditions, the image given by Koyré of Buonamici as a medieval thinker can prevail, as can more generally the idea that the emergence of mechanics as a science in the

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[1] Fonds National Suisse de la Recherche Scientifique.
[2] Galilei, *Le Opere*, Alberi ed., vol. 11; Galilei, *Opere*, Favaro ed., vol. 2.
[3] Garin, "Galileo e la cultura del suo tempo"; Badaloni, *Il Tempo*, *periodo pisano nella formazione di Galileo*; Schmitt, "The Faculty of Arts in Pisa at the Time of Galileo"; Wallace, *Galileo and his Sources*.
[4] Lohr, *Latin Aristotle Commentaries: II.*

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 185–193.

16th century was totally alien to the natural philosophy of the masters of Pisa.

In this paper I propose to examine several texts of these professors, in particular Buonamici's *De motu*, and to show that on the contrary, the rebirth of ancient mechanics was already strongly present in many debates on natural philosophy in Pisa between 1580 and 1592.[5]

THE PROFESSORS OF NATURAL PHILOSOPHY IN PISA

It is useful to first recall the didactic activity of these professors of natural philosophy. Teaching, in Pisa as in other European universities, was propaedeutic to medicine, and included lectures and disputations. Over a three-year cycle extraordinary professors of natural philosophy were supposed to read in their lectures the following texts of Aristotle: *De generatione et corruptione, Meteorologica*, and *Parva naturalia*; ordinary professors were entrusted, again over a three-year cycle, with the *Physics*, *De cœlo*, and *De anima*. It is also important to recall that two teachers would concurrently explain the same text by Aristotle following different commentaries (e.g., *De cœlo* would be explained by one teacher following the commentary of Simplicius while the other followed the commentary of Averroes). They developed highly different philosophical orientations, going from Averroism to Neoplatonism.

The exercise of the disputation was linked to the lectures: a disputation opposed a professor and his students to his competitor and his students on various *quaestiones*, that is, problems such as the possibility of motion in a vacuum, the cause of acceleration of heavy falling bodies, and the cause of projectile motion. The Pisan disputations (*disputationes circulares*) were known for their vehemence; we still have the testimony of this from Claude Bérigard, who taught in Pisa from 1627 to 1636.

As shown by Charles Schmitt, the University of Pisa was an important institution in Europe, and had professors of great renown. Their activity was not limited to teaching, and they were often members of the other great cultural institution of the Grand Duchy, the Accademia Fiorentina. Among these masters we can find

> Andrea Cesalpino (1509–1603), one of the most famous physicians of his time, and a founder of modern botany. His work on natural philosophy entitled *Peripateticarum quaestionum libri V*, published in Venice in 1571, contains, among

[5] See also Helbing, *La Filosofia naturale di Francesco Buonamici.*

other points, an explanation of tides as being caused by the movement of the Earth.

Girolamo Borro (1512–1592), the author of a *De motu gravium et levium*.

Francesco Buonamici (1533–1603), a student of the great Hellenist Pier Vettori, a member of the Accademia Fiorentina, and the author of the aforementioned *De motu*.

Jacopo Mazzoni (1518–1598), who taught in Pisa from 1588 as ordinary professor and a competitor to Buonamici and who also taught Plato. He is the author of *In universam Platonis et Aristotelis philosophiam praeludia*, published in Venice in 1597.

These works are collections of questions, and it is interesting to note that the two works of Borro and Buonamici have as their main point a group of questions arising from a debate on the motion of the elements, *De motu elementorum* or *De motu gravium et levium*. Buonamici informs us explicitly of this fact:

> The occasion for writing this volume was given by the disputation that took place at the Academy of Pisa between our students and the students of our colleagues on the subject of the motion of the elements.[6]

As we have already shown elsewhere, it can easily be established that a good deal of Buonamici's immense treatise is aimed at criticizing the theses defended by Borro in his *De motu gravium et levium*.[7] Hundreds of pages of *De motu* are in fact devoted to combating the Averroist opinion on the motion of the elements as defended by Borro, and to supporting, on the contrary, the opinion of the Greek commentators of Aristotle (Alexander, Themistius, Simplicius). The problem was to determine the cause of natural motion, for example the fall of inanimate bodies. Is it a matter of an external cause – the medium or the place? – or an internal cause – the body's shape, substance, or its own gravity?

Borro's opinion, which follows and develops Averroes's position, is that the motion "totum elementum a propria illius forma per se primo movetur";

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[6] "Occasio vero scribendi voluminis ab ea controversia sumpta est, quae in Academia Pisana inter nostros collegarumque auditores exorta est de motu elementorum" (Buonamici, *De motu*, p. 3).

[7] Helbing, *La Filosofia naturale di Francesco Buonamici*, pp. 55–57, pp. 135–189, pp. 208–215.

the elementary body has the active principle of its motion in itself, that is to say in its own form.[8] On the contrary, Buonamici holds that "principium quod efficiat motum non sit formae tribuendum"; for him, the cause is external, from the medium.[9] Yet the medium does not function as the active principle; it is indeed the cause, but in the sense that, if there were no resistance from the medium, the bodies would be immediately displaced to their natural place. In other words, the resistance of the medium prevents the instantaneous displacement (*mutatio*) and creates a successive displacement over time (*motus*).

It is precisely in this context that we find developments of certain interest for the new science of the 17th century. We will recall three of these developments:

(1) The discussion of the thesis of Avempace and Philoponus, who had held that in a vacuum, bodies fell at a determined speed;

(2) The precious testimony of Borro of an experiment he conducted in the presence of several other persons, by dropping from the window of his house a piece of wood and a piece of lead of the same weight.

(3) Buonamici's highly detailed discussion of the theory of *impetus*, either with regard to the motion of projectiles, or to the accelerated motion of naturally falling bodies.

This was the very discussion by Buonamici on the *impetus* that Alexandre Koyré had the merit of examining more than sixty years ago in his *Études galiléennes*. But apparently having read only a few pages of *De motu*, he too hastily judged that Buonamici's natural philosophy was that of an epigone of medieval physics.

And yet, contrary to what Koyré wrote, Francesco Buonamici, a student of Vettori and Strozzi, imbued with classical culture, and himself a remarkable Hellenist, followed a philosophical orientation that completely diverged from the medieval tradition. His enormous treatise aspired to construct the whole of natural philosophy as a "philosophy of motion" and aimed to rediscover the pure thought of Greek Antiquity by using new ancient sources that were unknown or little known in the Middle Ages and that did not reappear until the 16th century, such as Proclus's commentary on Euclid, several works of Archimedes, Plutarch, and Philoponus, as well as the *Mechanica* ascribed at the time to Aristotle.

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[8] Borro, *De motu gravium et levium*, p. 167.
[9] Buonamici, *De motu*, p. 340.

THE EMERGENCE OF MECHANICS IN THE 16TH CENTURY

Fairly recent studies by W. Roy Laird, Michele Camerota, and me have shown that the introduction of the *Mechanica* in the 16th century contributed decisively to promote, alongside astronomy and optics, a science with the name of mechanics, one that dealt mathematically with a physical subject, and that, by integrating the medieval *scientia de ponderibus*, would by the end of the century comprise a vast corpus, including ancients such as Aristotle, Archimedes, and Pappus, as well as contemporaries such as Maurolico, Guidobaldo dal Monte, Giuseppe Moletti, and Michel Varro.[10]

With regard to the *Mechanica* we must recall that it has three parts:

(1) An introduction to which the translator Leonico Tomeo gave the title “quae sit artis mechanicae facultas”;

(2) The exposition of the principles of the science of mechanics, among which can also be found the rule of the diagonal in the composition of motions (when a body has uniform motion and at the same time is displaced by another uniform motion perpendicular to the first, there will result a single composite motion along the diagonal); and

(3) The discussion and solution, using these principles, of 35 problems, dealing mainly with simple machines (the balance, the lever, the pulley, etc.).

The *Mechanica* does not, however, deal with the inclined plane, nor is the notion of centre of gravity found there at all. As we know, the classical sources of these notions are to be found in Archimedes and Pappus.

And yet, when we return to the works of our professors in Pisa at the end of the 16th century, we can remark that apart from Borro, who does not cite the *Mechanica*, all the other authors have a very good knowledge of it and use it in their treatises on natural philosophy. Moreover, they consider mechanics to be a science, that is a *scientia media*, part mathematics and part physics, and attempt to define it better. This attitude is hardly surprising, in particular in Tuscany. It was to a Tuscan writer, Alessandro Piccolomini, that we owe the first important paraphrase of the *Mechanica*, which was later translated into Italian. Another famous writer, an eminent representative of the Accademia Fiorentina, Benedetto Varchi, mentions “il divino libro delle *Meccaniche* di Aristotele”.[11]

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[10] Laird, *The Unfinished Mechanics of Giuseppe Moletti*; Camerota and Helbing, *All’alba della scienza galileiana*.

[11] Varchi, *Lezioni sulla pittura e la scultura II*.

Even if the presence of references to the *Mechanica* and to other works of mechanics is limited, these traces are precious. Moreover we observe certain attempts to use this "new science" to resolve questions of natural philosophy. Let us examine in this light a text by Cesalpino and a few aspects of Buonamici's complex treatise.

CESALPINO AND THE *MECHANICA*

Cesalpino explicitly cites the principle of the composition of motions in the *Mechanica* in the fourth book of his *Quaestiones peripateticae* in his question 5.[12] He states that

> Any mixed motion takes place following the diagonal of the two motions that compose it, as is proven in the *Mechanica*. The diagonal is indeed situated between two lines that cut each other; that is why, when an object falls and at the same time is displaced sideways, there is produced a motion resulting from the motions that take place on the two lines, so as to traverse their diameter; one can see this in the rain that falls at the same time the wind is blowing and in the throwing of a stone; indeed, at the same time gravity deviates the stone from the straight line along which it had been thrown.[13]

Cesalpino's language certainly does not possess the rigor of that of a mathematician, but the application of the principle of the composition of motions to raindrops that fall in the wind and to the motion of projectiles is clear. This text also shows that, contrary to a widely held belief among historians of science, the impossibility of the composition of a natural motion and a violent motion was not a dogma among all Aristotelians.

BUONAMICI AND ANCIENT MECHANICS

Buonamici's *De motu* makes even freer use of the *Mechanica*. One could even say that scattered through the ten books of the professor from Pisa, almost the entire text of this work can be found. This great scholar, who

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[12] Pseudo-Aristotle, *Mechanica,* 848b 11–26.

[13] "Omnis motus mixtus secundum diametrum fit componentium, ut in Mechanicis ostenditur. Diameter autem inter duas lineas secantes se invicem cadit, ut dum quippiam feratur deorsum, simul movetur ad latus, motus autem resultat ab utraque linea descendens adeo ut diametrum constituat, quemadmodum videre licet in pluviae descensu interim vento ad latera, et in lapidis proiectione, simul enim gravitas lapidem inclinat a linea recta secundum quam proiectus est" (Cesalpino, *Quaestiones peripateticae*, II qu. 5).

read and spoke fluent Greek, at times obliges us to make great efforts to understand his commentaries on certain passages of the *Mechanica*. Here, for example, is how he explains and develops the famous incipit of the *Mechanica*, in which Aristotle affirms that things the causes of which we are ignorant appear to us like miracles. Working as a philologist, Buonamici calls upon an analogous affirmation by Plato in the *Theaetetus* and, while attempting also to explain this obscure passage from this work ("for wonder is the feeling of a philosopher, and philosophy begins in wonder"),[14] states that

> As Aristotle says in the *Mechanica*, when in nature something happens of which we do not know the cause, it is a wonder, from which springs in the mind the desire to search, which is the origin of philosophy. This is why Plato in the *Theaetetus* affirms that wonder is proper to philosophers, and that the poets sang of this wonder in the person of Iris, the daughter of Thaumas; which I interpret in this way: Iris is the movement of reason by which the mind goes up from causes to effects and from effects descends to causes.[15]

Thus according to Buonamici, at the beginning of the *Mechanica* Aristotle expresses the same thought that his master Plato had suggested in a cryptic way in the *Theaetetus* through the myth of Iris: the wonder at the origin of philosophical searching is personified by Iris, the messenger who rises from humans to the gods and descends from gods to mortals; Iris, the daughter of Thaumas, indicates the double process of the mind that seeks to discover causes from effects and to understand effects by causes.

Let us now examine how the master of the Studio Pisano defined mechanics, which he situates among the theoretical sciences, as a *scientia media* between pure mathematics and natural philosophy. After establishing the status of two other *scientiae mediae* (optics and music), he says that

> The other sciences, astronomy, nautics, and mechanics are closer to natural science, because they deal with substances, that is to say the sky, the winds, and masses of wood and iron as they move. Mechanics is especially close to the natural sciences because it establishes in the same manner the

[14] Plato, *Theaetetus*, 155 d.

[15] "… est vero miraculum, quando quippiam fit in natura, cuius sane causa ignoretur, unde efficit in animo desiderium inquisitionis, quod initium philosophandi fuit; quocirca testatus est Plato proprium esse philosophi eiusmodi affectum, et a poëtis decantatum Irin Thaumatis filiam, quod ego sic interpretor, gyrum rationis quod mens ipsa modo ab effectis procedit ad caussas, mox a caussis regreditur ad effectus" (Buonamici, *De motu*, p. 626).

> principles of motions that are natural and motions that are against nature. Nonetheless, given that it considers the causes by taking them from the nature of figures, it must be rightly considered a mathematical science.[16]

By saying that mechanics establishes not only the principles of motions against nature, but also natural motions, Buonamici seems indeed to consider that the field studied by mechanics is not limited to that of simple machines.

Moreover, in addition to the Aristotelian *Mechanica*, the Pisan master is aware of other works of ancient mechanics that, thanks in particular to the works of Commandino, had made their appearance during the second half of the 16th century, and by this means of the notion of the centre of gravity (absent, as I have just noted, in the *Mechanica*).

In the fifth book of his *De motu*, Buonamici uses the notion of centre of gravity in his discussion of the determination of the centre of the Earth, by making use of Archimedes' *De planorum aequilibriis sive de centris gravitatis planorum*. And it is remarkable to observe that he also takes on in this context the famous experimental determination of the centre of gravity (*ponderis medium*, *centrum gravitatis*) given by Pappus in his *Collectiones mathematicae*, whose Latin translation by Commandino appeared in Pesaro in 1588:

> The centre of gravity is determined by equilibrium in such a way that it produces no movement either one way or the other by excess and that all the parts possess the same force; it is a question, as Pappus says, of a point of a body such that, if one imagines this body suspended from this point and one moved it, it would always keep the same position.[17]

Moreover, several chapters of the fifth book are devoted to the exposition and critique of Archimedes' hydrostatics.[18] As we have remarked elsewhere,[19]

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[16] "Ad naturalem reliquae magis accedunt, Astrologia, Nautica et mechanica; quippe quod aliquas substantias, caelum scilicet aut ventos, aut moles ligneas ferrasve tractant sub ea ratione qua moventur; praecipue mechanica quae item principia motum naturalium et contra naturam assignat. Sed quod etiam caussas accipit a figurarum natura desumptas, optimo iure mathematica perhibetur" (*ibid.*, p. 88).

[17] "… ponderis [medium] autem, unde ratio sumitur aequilibrii, ut in neutram scilicet partem superata agitetur, sed undique tantundem valeant omnes, et denique punctum auctore Pappo intrapositum, unde si grave cogitetur appensum, si moveretur, inter movendum posituram eandem profecto servare" (*ibid.*, p. 457).

[18] Thurot, "Recherches historiques sur le principe d'Archimède".

[19] Helbing, *La Filosofia naturale di Francesco Buonamici,* p. 356.

this is the most important testimony to the knowledge of Archimedes in Tuscany, at about 1587, the date of the dedication by Buonamici of his *De motu* to the Grand Duke of Tuscany.

CONCLUSION

The examination of the works in natural philosophy by the masters of the Studio Pisano at the time when Galileo was a student and professor of mathematics at this university allow us to establish that (1) the principal *quaestio disputata* dealt with by Borro, Buonamici, and Galileo, the latter in his *De motu antiquiora*, was that of *De motu gravium et levium*; (2) in the context of this disputation, they attempted to decide the question by experiments of falling of heavy objects; and (3) ancient mechanics, notably the *Mechanica*, attributed at the time to Aristotle, as well as Archimedes' statics and hydrostatics, were involved in this debate. It is quite surprising and distressing to observe that the works of Borro, Cesalpino, and Mazzoni, unmentioned by Clagett, remain unrecognized by historians of science of the 16th and 17th centuries, despite their importance in understanding a major part of Galilean science.

EGIDIO FESTA AND SOPHIE ROUX[1]

THE ENIGMA OF THE INCLINED PLANE FROM HERON TO GALILEO

The law of the inclined plane states that the ratio between a weight and the force needed to balance this weight on a given inclined plane is equal to the ratio between the length and the height of this plane. With the peremptory tone for which he is known, Descartes affirmed that this law was known to "all those who write about mechanics".[2] Yet the problem of the inclined plane appears neither in Aristotle nor in Archimedes, and while writers such as Heron of Alexandria, Pappus of Alexandria, Leonardo da Vinci, Girolamo Cardano, and Colantonio Stigliola do indeed formulate it, they do not find the solution. At the end of the 16th century, the writers stating this law can be counted on the fingers of one hand: Jordanus de Nemore (and in his wake Niccolò Tartaglia), and later, within the space of some ten years, Michel Varro, Simon Stevin, and Galileo Galilei.

In the present article, painstaking work on the texts and an epistemological reflection will allow us to resituate Galileo's demonstration of the law of the inclined plane in a long history of mechanics. The brief version of Galileo's *Mecaniche* states the law of the inclined plane without giving the demonstration, writing instead: "it would be a bit more difficult study; let us thus set it aside here".[3] This is not a rhetorical clause employed by Galileo to get around providing a demonstration he was unable to supply: he had already provided the demonstration a few years earlier in the 23 chapters *De motu*.[4] If he omits the demonstration in the short version, it is likely

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[2] Descartes to Mersenne, July 13, 1638, in Descartes, *Oeuvres*, vol. II, p. 232.

[3] Galilei, *Les mécaniques*, short version, p. 13, ll. 5–10. The short version does not appear in Galilei, *Le opere*. Hereafter, we use the following abbreviations: "s. v." for "short version" and "l. v." for "long version".

[4] This is the name given to the manuscripts written in the hand of Galileo during his Pisan period, presented in Galilei, *Le opere*, vol. I, pp. 250–419. According to the indications given by Viviani, *Quinto libro degli elementi di Euclide*, pp. 104–105, these were several small notebooks (*quinternetti*) found together in a folder bearing the title *De motu antiquiora*, i.e., the older studies on movement; a first incomplete edition was presented under the title

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 195–220.

because of the nature of this version: it was probably an oral lesson and it would have been normal, given the level of the students, to omit mathematical demonstrations. This was not the case in the long version, where after having exposed Pappus's error, Galileo gives a detailed demonstration of the law of the inclined plane.[5]

We shall begin by examining the texts of Heron and Pappus so as to understand the reasons for their failure to resolve the problem of the inclined plane. We shall then turn to the first successful demonstration of the law of the inclined plan, which provided by Jordanus de Nemore in his *De ratione ponderis*; unlike Pappus, Jordanus does not relate the force needed to move a body on an inclined plane to the force needed to move it on the horizontal: he compares it, so to speak, directly to the forces on two different inclined planes by reducing them to vertical movements. Thus are united the elements needed to understand the originality of the demonstration proposed by Galileo in *De motu*: like Jordanus, Galileo isolates the vertical movements; but he does this in a totally different fashion, by introducing a bent lever. We shall then show that the demonstration of the long version of the *Mecaniche* is geometrically similar to that of *De motu*, but that the introduction of the notion of *momento* shows an important conceptual and terminological reworking. Finally, we shall indicate the epistemological and historiographic conclusions that appear to arise from this study.

ANCIENT MECHANICS: THE FAILURE OF THE DIRECT APPLICATION OF THE MODEL OF THE BALANCE

How is it that a man who does not have the strength to raise a given weight can succeed in doing so when he pushes it up an inclined plane? This could have been the 36th or 37th question asked by Pseudo-Aristotle

Sermones de motu gravium in the edition by Alberi, *Opere di Galileo Galilei*, vol. XI, pp. 83–125. Among the texts making up the writings of *De motu*, the chronological order of writing generally recognized today is as follows: an outline of projected work in Galilei, *Le opere*, vol. I, pp. 418–419; the *Dialogus*, *ibid.*, pp. 367–408; an *Essay* in 23 chapters, *ibid.*, pp. 251–340; a reworking of chapters 1 and 2 of this *Essay*, *ibid.*, pp. 341–343; an *Essay* in 10 chapters, *ibid.*, pp. 344–366; notes entitled "*Memoranda*", *ibid.*, pp. 409–419. On the vicissitudes of these manuscripts, see primarily Favaro, *Galileo Galilei e lo studio di Padova*, p. 3; Fredette, "Galileo's *De motu antiquiora*" pp. 321–350; Camerota, *Gli scritti* De motu antiquiora *di Galileo Galilei*, pp. 19–62; Giusti, *Elements for the relative Chronology of Galilei's* "De motu antiquiora"; Fredette, "Galileo's *De motu antiquiora*: notes for a reappraisal", pp. 169–177.

[5] Galilei, *Les mécaniques*, l. v., pp. 59–63, l. 80–185. The long version is published in Galilei, *Le opere*, vol. II, pp. 149–190.

in his *Mechanica*: it would have been all the more interesting since the inclined plane cannot be so easily reduced to the balance as can other simple machines.[6] But there are only 35 mechanical questions, and the only authors of Antiquity who attempted to solve the enigma of the inclined plane were Heron and Pappus of Alexandria.[7] Their answers are interesting, despite their ultimate failure.

Hero of Alexandria: an intuition that falls short

One might be surprised that in an article devoted to the reading of Galilean text, we should analyze a text still unknown in the 15th century, Heron of Alexandria's *Mechanics*.[8] This is because Heron's attempt to find the law of the inclined plane is the first we know about, because we find it as such in Leonardo da Vinci and Stigliola, and because it employs an intrinsically interesting intuitive approach.

"Let us propose to draw upward a weight posed on an inclined plane", writes Heron.[9] One may suppose that the inclined plane is well polished and that the weight is a cylinder, represented in Fig. 1 by a right circular cross section.

As the circle touches the inclined plane (DB) at only one point (L), its natural tendency to roll downward will not be hindered. Let us consider the

[6] In the following discussion, we take "model of the balance" to mean the idea that all mechanical systems (and in particular the inclined plane) can be understood from the starting point of weights balanced on a balance; we note that this idea does not prejudge the manner in which this balance is itself explained.

[7] Heron's list of simple machines, adopted by Pappus, does not include the inclined plane, undoubtedly precisely because it cannot be reduced to the balance (Heron, *Les mécaniques*, II 1, p. 115; Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII 31, vol. III p. 1115). The specificity of the inclined plane also appears in Pappus insofar as it constitutes, along with the Delian problem and a problem of how to construct a toothed wheel with a given number of teeth to fit another toothed wheel with a given number of teeth, as one of the three problems that had not been resolved by the Ancients that were later resolved (Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, p. 1028. These three problems are resolved in Heron, *Les mécaniques*, I 1, 11, and 23, resp., pp. 59–62, 72–74, 91–92).

[8] This work of Heron, written in the first century of the common era, is known to us by an Arabic translation by Qostâ ibn Lûkâ in the 9th century; in the 17th century, Golius brought back from the Levant a 16th-century copy of this translation; the first full translation and edition of this copy was that of the baron Carra de Vaux in 1893. For more on Heron, see more generally Schiefsky in this volume.

[9] Heron, *Les mécaniques*, I 23, p. 91. The context of this problem is interesting. I 20 establishes that, contrary to received opinion, the smallest force can move a body on a horizontal plane; I 21 analyzes the difference of behaviour between solids and liquids on an inclined plane; I 22 shows that a weight suspended from a pulley is moved by a power equal to it.

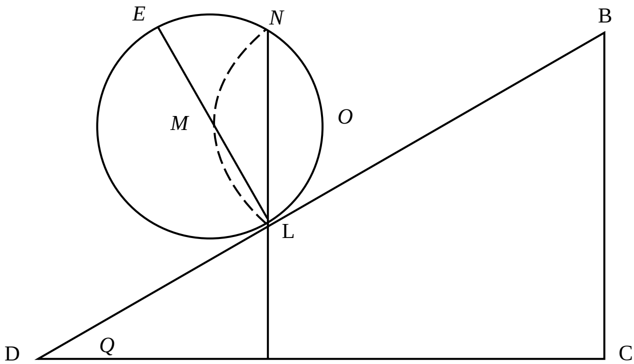

FIGURE 1. Heron, *Mechanics*, I 23. Known manuscripts include no illustration. Our figure is inspired by Leonardo and Stigliola.

plane passing through L perpendicular to the inclined plane: it divides the cylinder into two equal parts. On the other hand, a second plane passing through L perpendicular to the horizon divides the cylinder into two unequal parts, the smaller, LON, above, and the larger, LEN, below; because the lower part is larger than the upper part, it draws the cylinder downward.[10] Here a common experience receives an intuitive explanation: it is harder to raise a weight on a steeper plane because the greater the inclination with respect to the horizontal the larger the lower part.

Heron goes a bit farther, but in addition to the fact that this extra step shows the limits of his physical intuition, it does not lead to a procedure that would allow him effectively to calculate the force necessary to raise a body on a given inclined plane. To the lune LON, he associates the lune LMN symmetric to it with respect to LN: according to him, they are in balance on the inclined plane. He consequently concludes that the cylinder will be in equilibrium on the inclined plane if the supporting force is equal to the weight of the cylinder less the weight of the part whose section corresponds to the two lunes. This force is not explicitly calculated.[11]

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[10] Heron, *Les mécaniques*, I 23, pp. 91–92. The manuscript included no illustrations. Our figure is inspired by that found in Stigliola, *De gli elementi mechanici*, p. 41; in Da Vinci, *Il codice atlantico*, fol. 338r b.

[11] Cohen and Drabkin, *A Source Book in Greek Science*, pp. 199–200, propose an illustration that allows a rapid calculation of F/P = (area of the circle – the two lunes)/area of the circle. By using the angle of inclination a of the plane BDC, we finally obtain $F/P = 2(a + \cos a \sin a)/\pi$. We can immediately see that Heron's result, false in general, is correct for $a = \pi/2$ ($F = P$) and for $a = 0$ ($F = 0$).

As we can immediately see in our Fig. 1, Heron thus gives an intuitive explanation for the reason a body goes down an inclined plane, as well as the reason why the steeper the plane, the more it goes down. The limits of this explanation appear when Heron affirms that the two lunes LON and LMN balance each other: without making it explicit, he reasons as if they were on the pans of a balance whose axis would be the perpendicular passing through the point of contact, whereas in fact they are on an inclined plane. It is undoubtedly the omnipresence of the model of the balance that explains that this same intuition, the same illustration and the same mistake are found in Leonardo da Vinci and Stigliola.[12]

Pappus of Alexandria: the failure of the direct application of the model of the balance

Contrary to Heron's *Mechanics*, Book VIII of Pappus's *Mathematical Collection* was known at the end of the 16th century, in particular in Federico Commandino's Latin translation, finished before his death in 1575 and finally published by Guidobaldo dal Monte in 1588.[13] The analysis of the inclined plane given in proposition 9 of Book VIII was nonetheless known before 1588: in 1577, Guidobaldo dal Monte indicated in his *Mechanicorum liber* that Pappus had reduced the inclined plane to the balance, without explaining how; in 1581, Filippo Pigafetta inserted into his Italian translation of the *Mechanicorum liber* a translation of this proposition, under the title "Problema di Pappo Alessandrino nell'ottavo libro delle raccolte matematiche".[14] As we shall see, Galileo knew of Pappus's proposition when he wrote the long version of his *Mecaniche*.

Pappus poses the problem of the inclined plane in these terms:

> Given a weight drawn by a given force on a plane parallel to the horizon and given another inclined plane forming a given angle with the underlying plane, find the force by which the weight will be drawn on the inclined plane.[15]

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[12] Leonardo da Vinci noted that the farther the perpendicular to the horizon passing through the point of contact of the sphere and the inclined plane from the perpendicular to the horizon passing through the centre of the sphere, the heavier the body and the faster it descends (Da Vinci, *Il codice atlantico*, 338r b and 355r a, and Da Vinci, *Les manuscrits de Léonard de Vinci*, A 21v and A 52r). Stigliola, *De gli elementi mechanici*, "De rotte vettive", proposition 2, pp. 41–42

[13] On Commandino's translation and the vicissitudes of its publication, see Rose, *The Italian Renaissance of Mathematics*, pp. 209–212 and pp. 224–225. Passalacqua, "Le collezioni di Pappo".

[14] Dal Monte, *Mechanicorum liber*, proposition 2, p. 124. Pigafetta 1581, p. 121.

[15] Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII. 9, vol. III, pp. 1055–1059.

Beginning with the idea that a given force is necessary to move the body on the horizontal plane, he proposes thus to evaluate as a function of the latter the force necessary to immobilize the body on the inclined plane. The instantiation of the problem is thus as follows (Fig. 2).

The horizontal plane is MN, the inclined plane MK, the angle of inclination KMN. We are given the weight A and force C necessary to move the weight on the horizontal plane MN. We suppose that weight A is a homogeneous sphere concentrated in its centre E. We place the sphere on the inclined plane, with L being the contact point. The radius EL of the sphere is thus perpendicular to the inclined plane. We draw from E the line EH parallel to MN, we note H as the intersection of the inclined plane and EH, we call G the intersection of the sphere and EH, F the intersection of EH and the perpendicular passing through L. The question is to know what weight B will supply the necessary force D to maintain the sphere on the inclined plane MK.

Elementary geometric considerations show that we know the ratio FG/EF. The angle ELF is equal to the angle EHL (they are complementary to the same angle FLH) and the angle EHL is equal to the angle of inclination KMN (by construction). The angle ELF is thus equal to the angle KMN. Consequently the triangle LFE is given, thus the ratio EL/EF, which is equal to EG/EF. The final result is that we know the ratio FG/EF, which (by construction) is equal to (EG/EF) – 1.

We can thus arrange it so that weight B (and thus force D) is such that A is to B (and thus C to D) as FG is to EF. In these conditions, if we suspend weight A at E and weight B at G, A and B balance. GE thus acts like the beam of a balance fixed on the inclined plane KM by the fulcrum FL. But the weight of the sphere is A, situated at its centre E. Thus B is the weight which, placed at G, supplies the force D able to maintain the sphere on the inclined plane.

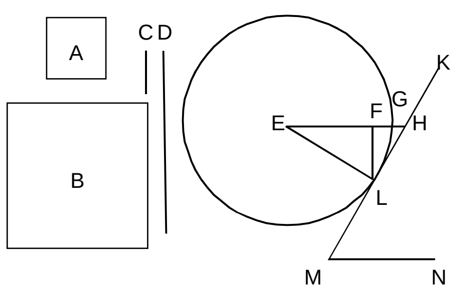

FIGURE 2. Pappus, *Mathematical Collection*, VIII, prop. 9. Our figure is inspired by Pigafetta.

Now, to raise it, we must add C to D. In other terms:

$$F_{\text{total}} = C + D = C + C/(EF/FG) = C(1 + EF/FG).$$[16]

To conclude, Pappus does what he calls "the construction and the proof from an example".[17] A body requiring a force C of 40 men to be moved on a horizontal plane could be raised, on an inclined plane of 60°, by a force seven and a half times as great, or by a force of 300 men.[18]

We know that Pappus knew of Heron, but we do not know if he knew of his analysis of the inclined plane. In any case, it is interesting to note the similarities, but also the differences, between the two analyses. In both Pappus and Heron, only the components of the weights perpendicular to the horizontal plane are taken into account. Figure 2 allows us to understand that the reasoning of both Pappus and Heron are thus physically in error for a similar reason: just as Heron's lunes do not know that they are on an inclined plane, the balance GE being always parallel to the horizon, sphere E does not know that it is on an inclined plane. To express this more clearly in the case of Pappus, the calculation is formally correct, since the angle ELG is equal to the angle of inclination KLM, but it corresponds to nothing physically, because this same angle ELG does not intervene in the manner in which equilibrium is achieved. We can imagine that in both cases the error arose from the fact that the model of the balance was not adequately adapted to the particular problem of the inclined plane, but was so to speak directly applied to it.

The differences between the two approaches are no less significant: Pappus imagines himself to be both a better mathematician than Heron and closer to professionals.[19] On one hand, indeed, his proof is a canonical example of Euclidean reasoning, respecting what is announced in the introduction of Book VIII of the *Mathematical Collection*: "I have found

[16] If we call a the angle KMN equal to ELF, we have D/C = EF/(EG – EF) = sin a/(1 – sin a), from which F_{total} = C + D = C/(1 – sin a). For a right angle, this gives $F_{total} = 1/0 = \infty$, or in other words, it requires an infinite force to raise any body vertically. For a zero angle, this gives F_{total} = C, which fits the initial hypothesis.

[17] On the different functions of numerical examples and practical applications in Pappus, see Cuomo, *Pappus of Alexandria*, pp. 180–186, and for the particular case of the inclined plane, pp. 184–185.

[18] To calculate the ratio EF/FG, Pappus refers to the *Canon*, a manual table given in the first book of Ptolemy's *Mathematics*. By using the value 0.866 for sin 60°, the formula established by Pappus allows him to conclude that the total force must be 7.46 times greater than C.

[19] On the representation of mathematics that Pappus could have had, see Cuomo, *Pappus of Alexandria*.

it appropriate to expound [these theorems] in a more concise and clearer manner and to establish [them] by a better reasoning than that of those who have previously written on this subject".[20] On the other hand, as the final numerical example indicates, he wants to satisfy an audience interested in the practical applications of the results obtained. It is undoubtedly this desire that leads him to take as his starting point the principle that a given force is needed to move a body on a horizontal plane, a principle that Heron had denounced as false.[21] This principle is testimony indeed to the concern for giving a method of calculation for those who use inclined planes: for craftsmen and engineers, pushing or pulling a material body on a horizontal plane, however smooth, requires that a certain effort be made; when they want to know the force needed to raise a weight on a given inclined plane, they can start from that which is needed to move it on a horizontal plane. Thus Pappus calculated the force required to move a weight on an inclined plane from the force needed to move it on a horizontal plane, whereas Heron did not include this force, since for him it was non-existent.

The problem of the inclined plane constitutes a particular challenge for the Archimedean as well as for the Aristotelian tradition: it cannot be immediately reduced to the balance, which are the fundamental model for these two traditions, whatever their other differences. This is why Heron and Pappus fail: they directly apply the model of the balance, the former doing so rather intuitively, the latter believing he can do so geometrically. As we will see, the response to this challenge did not come from a return to the direct experience of practitioners, but, in the case of Jordanus, from the exploitation of another mechanical system and, in the case of Galileo, from the adaptation of the model of the balance to the specificity of the problem considered.

JORDANUS DE NEMORE: A NEW USE FOR AN OLD MECHANICAL SYSTEM

At the beginning of the last century, a certain number of historians held that Galileo's knowledge of the texts of Jordanus de Nemore inspired him in his demonstration of the law of the inclined plane.[22] We would certainly agree

[20] Pappus, *Pappi Alexandrini collectionis quae supersunt*, VIII, vol. III, p. 1028.

[21] On the principle that the smallest force is sufficient to move a body on a horizontal plane, see Festa and Roux, "La moindre petite force".

[22] See, for example, Duhem, *Les origines de la statique*, vol. I, pp. 251–252; Caverni, *Storia del metodo sperimentale in Italia*, vol. IV, pp. 21–23, echoed by Vailati, "Il principio delle velocità virtuali", pp. 16–17, who affirmed furthermore that the texts of Jordanus served for the advanced study of mechanics.

that in the second half of the 16th century the notion of positional gravity was known; it is found, for example, in the works of Cardano, Scaliger, Tartaglia, and Benedetti.[23] Nor can it be contested that the Jordanian demonstration of the law of the inclined plane was accessible at that time, in particular through Tartaglia's *Quesiti et inventioni diverse* (1546 for the first edition) and the edition he prepared of *De ratione ponderis* (published posthumously in 1565).[24] But no external clue allows us to affirm that Galileo had read this work by the time he wrote *De motu*, and more particularly his demonstration of the inclined plane; only an examination of the texts will allow us to determine if it is necessary, or even plausible, to suppose that Jordanus inspired Galileo on this point.

The demonstration of Jordanus de Nemore

The demonstration of the law of the inclined plane developed by Jordanus aimed to evaluate the positional gravity (*gravitas secundum situm*) on an inclined plane. The notion of positional gravity results from an elementary physical observation: a body acts not only according to its weight, but also to the position at which it is placed. More precisely, the "suppositions" placed at the beginning of *De ratione ponderis* give two complementary determinations. First, the positional gravity of a body is physically characterized by the effects it produces: according to supposition 6, a body is lower in position than another when in going down it makes the other rise.[25] Second, positional gravity is evaluated geometrically: according to suppositions 4 and 5, the positional gravity of a body is inversely proportional to the obliquity of its descent, and this obliquity is measured by the ratio between the given segment of the descent and its vertical projection.[26] Bringing these two determinations together — which Jordanus does not do — we can say

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[23] On the notion of *gravitas secundum situm*, see Moody and Clagett, *The Medieval Science of Weights*, pp. 94–95, 123–124, 150–151; Galluzzi, *Momento*, pp. 70–73.

[24] The first text of the Jordanian tradition to be published was the *Liber de ponderibus* by Petrus Apianus in Nürnberg in 1533, but the analysis of the inclined plane can be found only in *De ratione ponderis*, proposition 10. This last proposition was published the first time in 1546 by Tartaglia, *Quesiti et inventioni diverse*, Book VIII, proposition 15, with an important addition (given below in note 33 in the republication of the second edition [1554] of the *Quesiti*). In 1547–1548, Tartaglia was accused of plagiarism by Ludovico Ferrari, and perhaps for this reason prepared an edition of *De ratione ponderis*, which was published posthumously in 1565 by his Venitian publisher, Curzio Troiano, under the title *Jordani opusculum de ponderositate, Nicolai Tartaleae studio correctum*.

[25] Jordanus de Nemore, *Liber Jordani de ratione ponderis*, supposition 6, p. 174: "Minus grave aliud alio secundum situm, quod descensum alterius sequitur contrario motu".

[26] *Ibid.*, supposition 4: "Secundum situm gravius esse, cuius in eodem situ minus obliquus descensus ([A body] is heavier positionally when, in a given position, its descent is less

that the positional gravity of a body on an inclined plane is the part of its gravity that is effectively responsible for its descent along the plane and that must be counterbalanced so that the body remains immobile.

The problem of the inclined plane is explored in propositions 9 and 10. Proposition 9 states that for a given inclined plane, the weight is the same everywhere (Fig. 3).[27] This follows immediately from the fact that whatever the points D, E, chosen on the inclined plane, the right triangles DFK and EGM are equal, and thus so are their slopes.

In proposition 10, Jordanus wishes to demonstrate that if the weights are placed on planes whose slopes have the same ratios as the weights, they descend with the same force,[28] or in other words, that they have the same positional gravity.[29] Placing himself in the particular case of planes

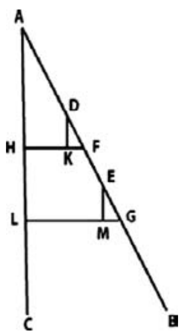

FIGURE 3. Jordanus, *De ratione ponderis*, prop. 9. Our figure is inspired by Moody and Clagett

oblique)". *Ibid.*, supposition 5: "Obliquiorem autem descensum, in eadem quantitatem minus capere de directo (A more oblique descent is that which, for a given distance, there is a smaller vertical component)". The expression "*minus capere de directo* (take less in the vertical)" simply means "descend less according to the vertical", or, as we translate it, "have a smaller vertical component"; by using the angle of inclination, l the part given for the descent and h its projection on the vertical, the obliquity is $l/h = 1/\sin a$.

[27] *Ibid.*, p. 188: "Equalitas declinationis identitatem conservat ponderis. (The equality of the declination conserves the identity of the weight)". The meaning of "declination" is given in proposition 10; see below, note 30.

[28] *Ibid.*, p. 190: "Si per diversarum obliquitatum vias duo pondera descendant, fueritque declinationum et ponderum una proportio eodem ordine sumpta, una erit utriusque virtus in descendendo (If two weights descend by paths with different declinations and if the declinations and the weights are directly proportional between them, the weights will have the same force when they descend)".

[29] Two bodies that have the same gravity descend with the same force: according to supposition 1, the force (*virtus*) of a body is its power to tend towards the bottom (*potentia ad

of different slope but with the same height, he calls the "proportion of declinations" the ratio between the lengths of these planes.[30]

Let ABC be a horizontal line, BD a vertical line and two inclined planes DC and DA of the same height but of different slopes (Fig. 4). We place *e* and *h* two weights, whose ratio is the same as that of DC to DA – in other terms, the ratio of the slopes. We must show that *e* and *h* will have the same force in descending.

As in the case of the demonstration of the principle of the lever, we suppose that there is no balance: "If that is possible, then *e* descends to L and thus pulls *h* to M".[31]

To simplify the geometric part of the demonstration, we use the vertical DB as a line of symmetry. We thus draw a line DK, symmetric to DC with respect to the vertical, and we then place on this line DK a weight *g* equal

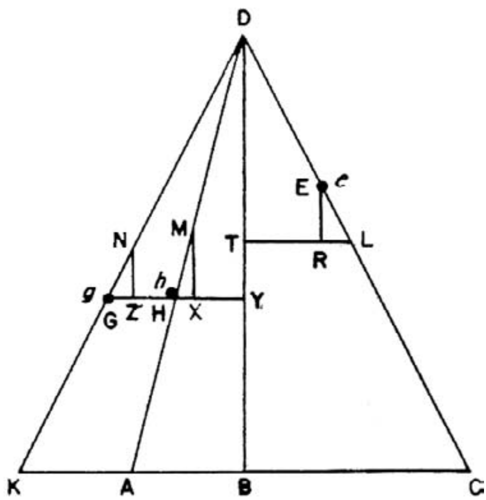

FIGURE 4. Jordanus, *De ratione ponderis*, prop. 10. Our figure is inspired by Moody and Clagett

inferiori tendendi) and to resist contrary motion (*et motui contrario resistendi*); according to supposition 2, a heavier body descends faster (*quod gravius est, velocius descendere*).

[30] *Ibid.*, p. 190: "Proportionem igitur declinationum dico non angulorum, sed linearum usque ad æquidistantem resectionem in qua æqualiter sumunt de directo (By 'proportion of the declinations', I do not mean that of the angles, but that of the lines taken to their intersection with the horizontal when they have the same vertical component)".

[31] *Ibid.*, p. 190: "Si igitur possibile est, descendat e in L, et trahat h in M".

to weight *e*. We set a segment NG equal to EL and thus to MH; then let NZ, MX, ER, be the vertical projections of NG, MH, EL.

Next comes the geometric demonstration.

> Given the similarity of the triangles NGZ, DYG, DBK, we have NZ/NG = DY/DG = DB/DK.
> Likewise we have MX/MH = DB/DA.
> We conclude by an elementary manipulation of the ratios MX/NZ = MH/NG × DK/DA.
> But by construction, MH = NG; whence finally MX/NZ = DK/DA = g/h.[32]

The equality of the ratios MX/NZ and DK/DA shows that we can replace movements on inclined planes by vertical displacements. Jordanus goes no further. But we can spontaneously give a physical interpretation of the inverse proportionality between weights *g* and *h*, and the vertical displacements NZ and MX: "that which suffices to raise *h* along XM also suffices to raise *g* along ZN" or, as Tartaglia writes, "the force or the power of *h* on the plane DA is equal to the force or the power of *g* on the plane DK".[33] This interpretation is not explicit in Jordanus, but his reasoning as follows seems to require it. In fact, he continues, since *e* cannot raise *g* (indeed, by construction, planes DC and DK have the same slope and weights *g* and *e* are equal to each other), *e* cannot raise *h* either. Thus *e* and *h* will remain in equilibrium.[34]

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[32] *Ibid*., p. 190: "Quia igitur proportio NZ ad NG sicut DY ad DG, et ideo sicut DB ad DK, et quia similiter MX ad MH sicut DB ad DA, erit MX ad NZ sicut DK ad DA, et hoc est sicut g ad h".

[33] Tartaglia, *Quesiti et inventioni diverse*, fol. 97v: "E pero [*h* e *g*] si vengono ad egualiar in virtu, over potentia, E per tanto quella virtu, over potentia, che sara atta à far ascendere luno de detti dui corpi, cioe à tirarlo in suso, quella medesima sara atta, over sofficiente à fare ascendere anchora l'altro, adunque sel corpo *e* (per laversario) è atto, E sofficiente à far ascendere il corpo *h* per fin in M, el medesimo corpo *e* saria adunque sofficiente à far ascendere anchora il corpo *g* a lui equale, E inequale declinatione, la qual cosa è impossibile per la precedente propositione (Thus, if [*h* and *g*] come to be equal in force, or in power, for in as much as the force, or the power, which will be apt or sufficient to make the other rise, thus if the body *e* (according to the opponent) is apt and sufficient to make the body *h* rise to *m*, this same body *e* would be by this fact sufficient to make the body *g* also rise to equal to itself, and equal in inclination, which, according to preceding proposition, is impossible". Duhem fills in this gap by applying here an "implicit postulate" by which that which can raise a weight P by a height *h* can also raise *n*P by a height *h/n* (Duhem, *Les origines de la statique*, vol. I, pp. 142, 147). This postulate is also made explicit in proposition 6 of *De ratione ponderis*, in *ibid*., p. 182.

[34] *Ibid*., p. 190: "Sed quia *e* non sufficit attollere *g* in *N*, nec sufficiet attollere *h* in *M*. Sic ergo manebunt".

An ancient mechanical system and a new model

After having set certain physical definitions, Jordanus presents a "purely formal" mechanical proof; in one decisive point, it even avoids any involvement in a physical interpretation. It is nonetheless incomprehensible if we do not identify the underlying mechanical system: if *e* and *h* were not tied by a line passing over D (or better yet, through a pulley attached at D), there would be no sense in supposing that *e*, when it descends to L, would draw *h* to M. We do not know if certain manuscripts of Jordanus de Nemore contained illustrations representing such an arrangement; in his notebooks, Leonardo da Vinci did associate analyses of the inclined plane clearly inspired by the Jordanian tradition with drawings of this system.[35] We will note in passing that the presence of this set up in a given technological system does not provide a sufficient condition for its use in the science of machines: the "Jordanus set up", if we may so name it, was usual in Antiquity, whether to raise loads or to prevent them from descending too quickly; yet neither Pappus nor Heron exploited it to analyze the inclined plane.[36] If Jordanus succeeded in resolving the problem of the inclined plane, it is first of all because he was able to see in an ancient scheme a new model.

We must also underline the fertility of the equation $MX/NZ = g/h$. In this equation, the segments MX and NZ, which express the variations of level, are the vertical projections of the oblique segments NG and MH respectively, equal to each other. As in the case of the balance, here too the heavier weight corresponds to the smallest vertical displacement. As has often been stressed, what characterizes Jordanus's demonstrations regarding the balance and the inclined plane is precisely the implementation of a method – that of vertical displacements – that will later lead to what has been called the principle of virtual work.[37] Jordanus's equation, which states explicitly the equilibrium condition as a function of vertical displacements, also contains implicitly

[35] This arrangement is, for example, represented when it is a matter of comparing weights on unequal slopes, in Da Vinci, *Il codice atlantico*, 354 vc et 375va, and in *Les manuscrits de Léonard de Vinci*, E 59r, E 75r, G 77r, G 79 r. The texts accompanying some of these representations attest to a knowledge of the Jordanian tradition; we can compare, for example, the statements of propositions 9 and 10 of the *De ratione ponderis* with Da Vinci, *Il codice atlantico*, 354 vc: "La equalita della diclinazione osserva la equalità de' pesi. (La proporzion de' pesi (po) posti in obliquiotà della medesima proporz). Se la proporzioni de' pesi e dell'obliqua, dove si posano, saranno equali, ma converse, (i pesi) essi pesi resteranno equali in gravità e in moto".

[36] See, for example, the drawing illustrating proposition 9 of Book III of Heron's *Mechanics*, reproduced in Heron, *Les mécaniques*, p. 277.

[37] See first Vailati, "Il principio delle velocità virtuali", pp. 20–21.

the law of the inclined plane, as it would be established by Galileo. Let us write this equation in terms of positional gravity, that is to say in the form $g\times$ NZ $= h \times$ MX; since we have NG = MH, we can just as easily write $g \times$ NZ/NG $= h \times$ MX/MH. The positional gravity is thus correctly expressed as a function of the height/length ratio of the plane in question.

However brilliant it is, Jordanus's solution remained isolated; in particular it is found neither in the *Liber de ponderibus*, nor in Blasius of Parma's treatises on statics. At the end of the 16th century, at least three solutions to the problem of the inclined plane emerged in less than ten years: those of Varro (published in 1584), of Stevin (published in 1586), and of Galileo (written between 1589 and 1592). To characterize the demonstrations of Varro and Stevin with respect to those of Jordanus, we could say that the former both have a common point with the latter: Varro considers vertical components, and Stevin uses the same mechanical system.

To evaluate the ratio of forces needed to raise a weight on different inclined planes, Varro proposes a figure in which the inclined planes have the same length and make up the radii of a circle; thus, he continues, the ratio sought is equal to the ratio between the tendencies to fall, evaluated by the vertical projection of the inclined planes (their lengths are equal, but their angles of inclination are different, as are the vertical projections). At the beginning of the treatise he set out the equality between resistance to lifting and the tendency to fall.[38] The only problem is thus the evaluation of the tendencies to fall for the vertical projections. Varro immediately draws this from the initial definition of the line of inclination (*linea nutus*) as the straight line that goes from the place where the movement begins to the place to which it tends to move, that is to say, in the case of a body with weight, the vertical straight line.[39] Given the knowledge of the time, we can wonder if such an affirmation could have any demonstrative value: a body on an inclined plane cannot, in fact, move *vertically* downwards; and how can we know how far its movement, if it could take place, would take it?

Stevin uses the mechanical system we have seen used by Jordanus: he considers a triangle whose sides are inclined planes of the same height and whose hypotenuse is parallel to the horizon. The resemblance to Jordanus stops there, for we know that Stevin refused to explain equilibrium in terms of potential movement: if a necklace of identical and evenly-spaced

[38] See Varro, *De motu tractatus*, conclusions I–III, p. 15, in Camerota and Helbing, *All' alba della scienza galileana*, pp. 274–275, and their comments in *ibid.*, pp. 139–148.

[39] Def. IV, in *ibid.*, p. 250: "Linea autem recta quae est ab eo loco a quo motus fieri incipit ad illum ad quem tendit illius vis quae motus efficit, nutus dicetur".

beads surrounding our triangle were not in equilibrium, it would acquire a movement that "would have no end, which is absurd". For reasons of symmetry, he continues, we can eliminate the part of the necklace that is under the inclined plane; the remaining parts on the top and the slope are thus in equilibrium. Now the number of beads on a plane is proportional to its length, whence the conclusion that the "powers" of the beads on a plane are inversely proportional to the length of the planes.[40] Stevin's proof does not correspond to the canons of Euclidean geometry, but relies instead on a fruitful intuition, in which we can see a first step in the revelation of the parallelogram of forces.

GALILEO (1): THE MODEL OF THE BALANCE IS ADAPTED TO THE INCLINED PLANE IN *DE MOTU*

Galileo's *De motu* in 23 chapters is a criticism of the Aristotelian theses on movement developed from the notions of Archimedean hydrostatics and, of course, from the principle of the lever.[41] Galileo thus refers to the balance.[42] But as the title indicates, this is not a treatise on simple machines like the *Mecaniche*, but rather an essay on motion. In chapter 14, "in quo agitur de proportionibus motuum eiusdem mobilis super diversa plana inclinata (in which is examined the ratios between movements of the same moving object on different inclined planes)", it is not a matter of using the law of the inclined plane to explain the function of the screw as it will be in the

[40] Stevin, *La statique*, I theorem 11, proposition 19, p. 448. The *De Beghinselen der Weeghconst* was published in 1586, the Latin translation by Willebrordus Snellius in 1605, the French translation by Albert Girard in 1634.

[41] Thus Archimedes is said to be more modern than Aristotle ("Archimedes Aristotele est multo recentior"), in Galilei, *Le opere*, vol. I, p. 303. On Galileo's training in Pisa, see Schmitt, "The Faculty of Arts at Pisa"; Helbing, "Mobilità della Terra"; Camerota and Helbing, "Galileo and Pisan Aristotelianism". On the criticism of Aristotle and the relation between *De motu* and the teachings of the Collegio Romano, see Dollo, "Galileo e la fisica del Collegio Romano".

[42] See in particular chapter 6, "in quo explicatur convenientia quam naturalia mobilia cum libræ ponderibus habent" (*Le opere*, vol. I, p. 257). There is a "convenientia", that is, an analogy, between the natural movement of a body and the movement of a weight on a balance: in the first case, it is the excess gravity of weight with respect to the gravity of the counterweight that makes it move; in the second case, it is the excess of gravity of the body with respect to the gravity of its medium (see in particular chapters 7 and 8, in *ibid*., pp. 260–273). This analogy is wrong: in balances with unequal arms, weight is not the only factor that must be taken into account. In fact, Galileo mixes here under the term *gravitas* two notions which we distinguish from each other: the specific weight of a body, which is involved in hydrostatics, and its weight, which is taken into account on a balance.

Mecaniche, but rather to answer two questions concerning the movement of a body along an inclined plane. First, a qualitative question: why does a moving object descend very fast vertically and ever more slowly on an inclined line the smaller the angle of inclination? Second, a quantitative question regarding speed: how much faster is the moving object on the vertical than on the inclined path?[43] The resolution of these questions supposes a demonstration of the law of the inclined plane.

The first question

Galileo begins by recalling two of the physical hypotheses to which he subscribes at the time:

i) The force with which a body falls is equal to the force required to raise it.[44]
ii) The ratio of the forces required to raise a body vertically and on a given inclined plane is equal to the ratio between the respective gravities on these planes.[45]

If we know the gravity, we can answer the two initial questions. At the time of *De motu*, Galileo consistently assumes that the speeds follow the ratios of the gravities.[46]

Thus Galileo's purely geometric demonstration aims to determine the ratio between the gravities of a body on different inclined planes.[47]

To do so, the gravity of a body on an inclined plane is reduced to the gravity of a body suspended on a bent lever.[48] Galileo imagines (Fig. 5)

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[43] The term most frequently used in this chapter of *De motu* to designate the measurement of motion is not *velocitas*, but rather *celeritas*, which we translate here as "speed"; as we indicate below, if there is a problem, it concerns not the term used, but its meaning.

[44] Galilei, *Le opere*, vol. I, p. 297: "Prius hoc est considerandum, quod etiam supra animadvertimus: scilicet, quod manifestum est, grave deorsum ferri tanta vi, quanta esset necessaria ad illud sursum trahendum; hoc est, fertur deorsum tanta vi, quanta resistit ne ascendat". For previous occurrences of this hypothesis in *De motu*, see, for example, Galilei, *Le opere*, vol. I, pp. 274–275. Regarding this idea, see Camerota and Helbing, *All' alba della scienza galileana*, pp. 139–148.

[45] *Ibid.*: "Sed tunc sciemus quanto minor vis requiratur ad sursum trahendum mobile per bd [inclined plane] quam per be [lesser inclined plane], quando cognoverimus quanto ejusdem mobilis erit gravitas in plano secundum lineam d, quam in plano secundum lineam be".

[46] See, for example, *ibid.*, pp. 295–296: "velocitates mobilium, in medio in quo moventur, gravitates; et proportiones consequenter velocitatum, gravitatum proportiones, sequ[u]ntur". Indeed, speed and motion cannot be distinguished from each other: gravity, the cause of motion, is also the cause of speed (*ibid.*, pp. 260–261).

[47] *Ibid.*, p. 297: "Procedamus itaque ad inquisitionem talis gravitatis".

[48] Heron of Alexandria attributes the explanation of the bent lever to Archimedes (Heron, *Les mécaniques*, I 33, pp. 108–110); on it application by Jordanus de Nemore, see Moody and Clagett, *Liber Jordani de ratione ponderis*, pp. 136–137.

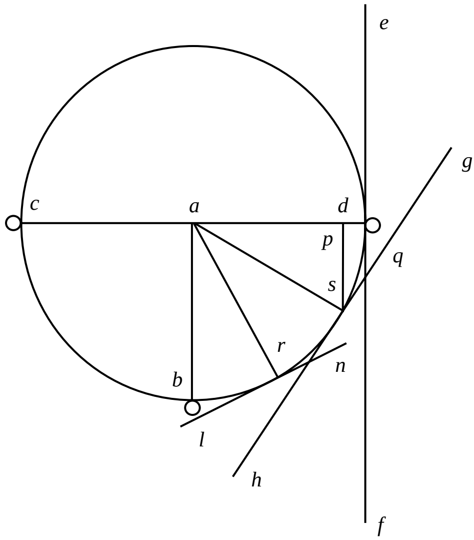

FIGURE 5. Galileo, *De motu*, chap. 14.

balance *cad* whose arms *ca* and *ad* are equal. We place at *c* and *d* two identical weights, then we hold *a* in place while turning *d* toward *b*: the suspended body is thus at one of the ends of a bent lever.[49] At each point of the quarter circle *db*, it is as if the moving object were at the tangent at this point, the descent following the gravity of the mobile object at this point. The successive positions of the bent lever thus correspond to a series of inclined planes.[50]

If the gravity of the moving object at *r* is thus less than at *s*, and at *s* less than at *d*, then the speed of the object at *r* is less than at *s*, and at *s* less than at *d*. Now – and it is here that the law of the bent lever comes into play – the gravity at *r* is less than the gravity at *s*, and at *s* less than at *d*:

[49] Galilei, *Le opere*, vol. I, p. 297: "Et intelligatur libra cd, cuius centrum a, et in puncto c pondus æquale ponderi alii quod sit in puncto d. Si itaque intelligamus, lineam ad, manente puncto a, moveri versus b …".

[50] *Ibid*., p. 297: "… in primo puncto d descensus mobilis erit veluti per lineam ef; quare per lineam ef descensus mobilis erit secundum gravitatem mobilis in puncto d. Rursus, quando mobile erit in puncto s, in primo puncto s suus descensus erit veluti per lineam gh; quare mobilis per lineam gh motus erit secundum gravitatem quam habet mobile in puncto s. Et rursus, quando mobile erit in puncto r, tunc illius descensus in primo puncto r erit veluti per lineam tn; quare mobile per lineam tn movebitur secundum gravitatem quam habet in puncto r".

> The weight at point *d* counterbalances the weight at point *c*, since the distances *ca* and *ad* are equal; but the weight at point *s* does not counterbalance the weight at point *c*. Indeed, if at point *s* we draw the perpendicular line to *cd*, the weight at *s* is, with respect to the weight at *c*, as if it were suspended at *p*; now, at *p* the weight weighs less than the weight at *c*, since the distance *pa* is shorter than the distance *ac*.[51]

We thus find here the answer to the first question initially asked: if a moving object on an inclined plane descends with a smaller force when the angle of inclination is smaller, it is because the smaller the angle, the smaller its weight.[52]

The second question

The answer to the second question easily follows. First, the ratio between the speeds by *ef*, the tangent at *d*, and by *gh*, the tangent at *s*, is equal to the ratio between the gravities at *d* and at *s*, which is itself equal to the ratio between *da* and *pa*.[53] Second, the triangles *asp* and *sqp* being similar and *as* being equal to *ad*, the ratio between *da* and *ap* is equal to the ratio between *qs* and *sp*, that is to say to the ratio between the oblique downward segment (or the length) and the vertical downward segment (or the height).[54] By calling l and h the length and the height of the inclined plane, V_l and V_h the

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[51] *Ibid.*, pp. 297–298: "Si itaque ostendamus, mobile in puncto s minus esse grave quam in puncto d, erit iam manifestum quod illius motus erit tardior per lineam gh quam per ef: quod si, rursus, ostendamus, in r mobile adhuc minus esse grave quam in puncto s, erit iam manifestum quod tardior erit motus per lineam nt, quam per gh. Atque iam manifestum est, mobile in puncto r minus gravare quam in puncto s; et in s, quam in d. Pondus enim in puncto d æqueponderat ponderi in puncto c, cum distantiæ ca, ad sint æquales: sed pondus in puncto s non æqueponderat ponderi c. Ducta enim linea ex puncto s perpendiculari super cd, pondus in s, respectu ponderis in c, est ac si penderet ex p; sed pondus in p minus gravat quam pondus in c, cum distantia pa sit minor distantia ac".

[52] *Ibid.*, p. 298: "Manifestum est igitur quod mobile maiori vi descendet per lineam ef quam per lineam gh, et per gh quam per nt".

[53] *Ibid.*: "Et quia tanto facilius descendit mobile per lineam ef quam per gh, quanto gravius est in puncto d quam in puncto s; est autem tanto gravius in puncto d quam in s, quanto longior est linea da quam linea ap; ergo mobile eo facilius descendet per lineam ef quam per gh, quo linea da longior est ipsa pa. Eandem ergo proportionem habebit celeritas in ef ad celeritatem in gh, quam linea da ad lineam pa".

[54] *Ibid.*: "Est autem sicut da ad ap ita qs ad sp, hoc est obliquus descensus ad rectum descensum: constat igitur tanto minori vi trahi sursum idem pondus per inclinatum ascensum quam per rectum, quanto rectus ascensus minor est obliquo; et, consequenter, tanto maiori vi descendere idem grave per rectum descensum quam per inclinatum, quanto maior est inclinatus descensus quam rectus".

speeds depending on the length and the height of the inclined plane, P_l and P_h the gravities depending on the length and height of the inclined plane, we have:

(1) $$P_l/P_h = h/l$$

or

(2) $$V_l/V_h = h/l$$

The analysis of the inclined plane thus first leads Galileo, in a text already obscured by the ambiguities of the term *gravitas*, to construct a new understanding of this term: it is a propensity to descend, variable according to the inclination of the inclined plane, and consequently constant all along a given inclined plane.[55] By a physical hypothesis according to which speeds are like gravities, (2) then results from the extension of (1) to speeds. Obviously this extension is not legitimate if we understand "speed" in its classical sense: when a body is moving and accelerating as it descends an inclined plane, its speed is not constant. To save this extension, Pierre Souffrin supposes that the term *celeritas* refers here to what he calls a holistic measurement of speed. To say that the ratio of the speed along the plane to the speed along the vertical is equal to the ratio of their lengths would be equivalent to saying that the ratio between the spaces traversed in the same time along the plane and along the vertical is equal to the ratio between the height and the length of the plane, which is correct.[56] What remains, however, is that the notion of a holistic measurement of speed does not allow us to take into account the variations of *celeritas* on the inclined plane, and is thus unsatisfactory.[57]

We now have the elements needed to evaluate what Galileo may owe to his reading of his predecessors. According to his testimony, no philosopher had

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[55] On the meaning of *gravitas* in *De motu*, see note 42 above. Galluzzi, *Momento*, p. 187, remarks that this understanding is hardly compatible with the hydrostatic model that permeates *De motu* and, pp. 195–196, formulates the hypothesis that Galileo became aware of the necessity of a lexical reorganization in these pages.

[56] Among the many studies that Pierre Souffrin devoted to the holistic measurement of speed, the most explicit for our text is Souffrin, "Sur l'histoire du concept de vitesse". See also Souffrin, "Motion on inclined planes".

[57] On the theory of accidentally accelerated movement in *De motu*, see Galluzzi, *Momento*, pp. 182–187; Damerow, Freudenthal, McLaughlin and Renn, *Exploring the Limits*, pp. 138–144.

dealt with the question of the motion that he analyzes in chapter 14 of *De motu*; this declaration refers, to all appearances, to the question of the speeds of a mobile object on variously inclined planes, a question proper to the science of motion.[58] There is no obvious reason to doubt this testimony, for in general Galileo does not hesitate in his writings to cite the authors he opposes or those who inspire him; more importantly, the examination of the texts seems to confirm this. Galileo is in fact as far from the purely intuitive process of Heron, Leonardo and Stigliola as from the geometric analysis of Pappus.

The author to whom he is the closest is Jordanus de Nemore: like him, he attempts to evaluate the force of the body as it descends the inclined plane (a force called *gravitas* in *De motu* and *virtus in descendendo* in the *De ratione ponderis*), and there is a starting point that in each case allows a resolution of the problem. As we have seen, the equation of Jordanus contains the Galilean law of the inclined plane. Nonetheless, the geometric construction proposed by Galileo – the use of a bent lever with one fixed end and another whose successive positions follow a series of inclined planes – has nothing to do with that proposed by Jordanus. In this sense, although it is not materially impossible that Galileo had read the *De ratione ponderis* when he was in Pisa, the details of his demonstration prevent us from concluding, as did Caverni and Duhem, that he owes his demonstration of the law of the inclined plane to this reading.

GALILEO (2): THE *MECANICHE* INTRODUCES THE NOTION OF "*MOMENTO*"

In the long version of the *Mecaniche*, Galileo begins by criticizing Pappus's demonstration; he did not do this in *De motu*, either because he did not know this demonstration or because he judged that it did not allow the resolution of the problem at hand, that of the speed of a body on the inclined plane. The criticism aimed at Pappus is that the principle he adopted as his starting point doomed him to failure. Since, according to Galileo, no force is needed to move a body on a horizontal plane, there is no sense in seeking to evaluate with respect to this force the force required to move a body on an inclined plane. What must be done instead is to suppose as given the force that moved the weight vertically and to evaluate with respect to *this* force that which will move the weight on the inclined plane.[59] Galileo thus merely makes explicit, in his criticism of

58 Galilei, *Le opere*, vol. I, p. 296: "Quaestio, quam nunc explicaturi sumus, a philosophis nullis, quod sciam, pertractata est: attamen, cum de motu sit, necessario examinanda videtur illis, qui de motu non mancam tractationem tradere profitentur".
59 Galilei, *Les mécaniques*, l. v., p. 60, l. 5–17.

Pappus, the procedure that he used in *De motu*; we shall show more generally that, if the demonstrations of *De motu* and the *Mecaniche* are identical from a geometric point of view, there is nevertheless a terminological and conceptual clarification in the *Mecaniche*.

Geometric identity…

Let us recall the process followed in the *Mecaniche*. We consider, as in *De motu*, a balance whose arms AB and BC are equal and from whose ends we suspend two equal weights: the system is in equilibrium on the vertical support BI (Fig. 6).

If we hold AB fixed while BC can turn, we have a bent lever as in *De motu*. When BC turns, the equilibrium is broken, and according to the principle of the lever demonstrated at the end of chapter 2 of the long version, the *momento* of the weight varies according to its distance from the point of support.[60] At F, for example, it is as if the weight were suspended from K, and its *momento* decreases by the ratio of BK to BC; at L, the *momento* decreases by the ration of BM to BC, etc.

Galileo makes explicit the reasons why we can go from a bent lever to an inclined plane, which he did not do in *De motu*:

i) If the body that descends follows a single path, it does not matter whether it is suspended from a bent lever or supported by a circular inclined plane such as CFLI.[61]

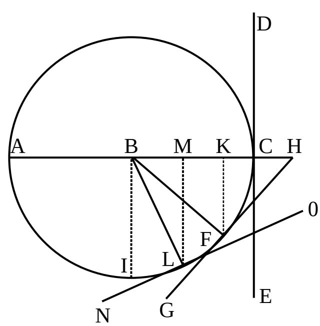

FIGURE 6. Galileo, *Mecaniche*, chapter on the screw.

[60] *Ibid.*, pp. 35–38, l. 73–163. In the case of the bent lever, the distance is measured by the projection of the radius in the horizontal direction, here on the diameter AC this particular case is examined in l. v., p. 35, l. 59–72.

[61] *Ibid.*, p. 61, l. 131–140. In *De motu*, Galileo passes directly from the bent lever to the inclined plane, without any consideration at this point of anything related to motion. In this

ii) When the moving body is at a given point of the circumference, it is as if it were at the tangent to this point; thus its inclination and hence its *momento* are the same on the inclined plane and on the tangent.[62]

We then return to the elementary geometric considerations of *De motu*. Given that the triangles BFK and KFH are similar and that BF = BC, we have BK/BC = KF/FH, from which we draw the ratio between the *momento* of descending the inclined plane and the *momento* of descending vertically: it is equal to the ratio between the height of the plane KF and its length FH.

What remains is merely to extend this result to the situation where a supporting force is required. How to do this is immediately apparent with the principle, already given in the study of the balance, by which there is no notable difference between the power of a weight to support another and its power to move it.[63] From this comes the final conclusion: the ratio of the force to support a weight is equal to the ratio of the height and length of the plane, which comes back to the relation noted as (i) above:

$$P_l/P_h = h/l$$

This result, as well as the construction and geometric structure of the demonstration, are thus identical to those of *De motu*.

… and conceptual differences

Three differences nonetheless separate the analysis of *De motu* and that of the *Mecaniche*:

(1) *De motu* evaluated the speed of bodies descending an inclined plane; the *Mecaniche* refrain from any consideration of speed. This may be explained by the fact that the *Mecaniche* are not intended to deal with motion. It is undoubtedly also due to the fact that Galileo had no satisfactory result to propose. In *De motu*, the conclusion was that the speeds (or the spaces traversed in the same time, according to

page of the *Mecaniche*, Galileo seeks to justify the assimilation of the circularly inclined plane by introducing notions supposing a movement (the body considered "moves" and follows a certain "path", l. v., pp. 61–62).

62 *Ibid.*, pp. 61–62, l. 150–155.

63 Referring to a balance on which two weights are in equilibrium, Galileo remarks that by adding the smallest of weights to one, the balance would move, which is obvious. What is much less obvious is to oppose Guidobaldo dal Monte by affirming that one must make "no difference between the power that a weight has to support another and the power it has to move it" (*ibid.*, pp. 38–39; see also s.v. p. 6, l. 22–28).

the interpretation of Souffrin) are as the gravities: this result does not take into account the trivial observation that a body accelerates when it descends an inclined plane. And if we admit that the speed does indeed increase, how can we obtain this result from a factor such as *momento*, which is constant on all points of a given inclined plane? [64] Perhaps Galileo had already understood at the time of the *Mecaniche* that the evaluation of the speed of a body descending vertically or along a plane constituted a problem that was important to solve.

(2) *De motu* brought together under the name of gravity two distinct quantities, weight and the effectiveness of weight; the *Mecaniche* distinguishes them by calling *gravità* the weight, and *momento* the effectiveness of the weight, that is to say the force required to support it or move it. As we have seen, this *momento* is evaluated by the static moment of a bent lever, as a function of the weight and the distance from the centre; contrary to weight, it is thus variable according to the inclination of the plane on which the body is placed. The term *momento* allows in this way a conceptual clarification.

(3) Whereas, as we have just recalled, any reference to speed is absent from the *Mecaniche*, *momento* appears there as the *momento di discendere (momento* of descending) and as *impeto* to go downward, that is to say, as the cause of downward movement, the motive force that draws a body toward the centre of the universe.[65] The importance of the notion of *momento di discendere* for the later developments in Galilean science has already been noted. As Paolo Galluzzi has shown, Galileo no longer considered speed as proportional to the weight of the body, but he has nonetheless not given up studying, from the basis of statics, the characteristics of the motion of a body on an inclined plane, and in particular its speed. A certain number of letters and fragments indicate in fact that, during his entire stay in Padua, Galileo attempted to found a science of accelerated motion "*senza trasgedire i termini meccanici*", and that he based this on the notion of *momento* of speed, constructed by analogy with the notion of *momento* of gravity, and not yet designating

[64] Galluzzi, *Momento*, p. 216.

[65] The use of the expression "*impeto* to go downwards" as a double for the term *momento* already appears in the definition of *momento*, see Galilei, *Les mécaniques*, l. v., p. 34, l. 22–24. According to Galluzzi, *Momento*, p. 206, *impeto* represents so to speak the physical consequences and the concrete effects of variations of the effectiveness of weight abstractly recorded by *momento*. For other occurrences of *impeto*, see Galilei, *Les mécaniques*, l. v., pp. 40, 45, 59, 61, 62, 66.

an instantaneous speed.[66] Thus, for Galluzzi, the notion of *momento* of descending is a milestone in the long series of metamorphoses of the notion of *momento*. Maurice Clavelin gives even more decisive conceptual importance to the notion of *momento* of descending. For him, this is the equivalent of dissociating the "gravific" function of gravity (accounting for weight as the pressure exerted by a body at rest) and the "motor" function of gravity (accounting for movement toward the centre). Once this dissociation is accomplished, it is impossible to suppose that gravity as weight can measure gravity as motive force; thus the latter would become a physical entity unto itself requiring a specific analysis, which would be indispensable for arriving at the idea that there must be a law of falling bodies that applies to all bodies.[67]

To repeat the various stages of the evolution examined by Galluzzi and Clavelin would take us far beyond the purpose of the present article. To conclude our examination of the *Mecaniche*, let us merely stress that, in addition to a conceptual effort that this work shares with *De motu* and which allows for a physically effective mathematical treatment of the problem of inclined plane, the *Mecaniche* is a testament to the conceptual work that followed this mathematical treatment. It is not only a matter of reflecting on a proof and on a figure to draw out their physical significance, but also to invent a terminology that will allow the stabilization of new concepts.

CONCLUSION

The law of the inclined plane constitutes a significant advance of Galilean science. It will be used, for example, to demonstrate that the degrees of speed acquired on the planes, with different slopes but the same height, are equal when the mobile object arrives on the horizontal plane, and that

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66 For an overview of this period, see Galluzzi, *Momento*, pp. 261–308, who comments on the letters to Guidobaldo dal Monte from November 29, 1602 and to Sarpi from October 16, 1604 in Galilei, *Le opere*, vol. X, pp. 97–100 and pp. 115–116, the fragments found in *ibid.*, vol. VIII, p. 378ff. and p. 417ff., and their repetition in the *Discorsi*, in *ibid.*, vol. VIII, pp. 222 and 262.

67 Clavelin, *La philosophie naturelle de Galilée*, pp. 173–175, remarks in particular that the *gravitas secundum situm* of Jordanus does not lead to this dissociation, but merely indicates the ratio of the total weight to the reduced weight (*ibid.*, p. 174, n. 143). Considering the intellectual conditions that would allow the emergence of this new conception of gravity, Clavelin, "Le copernicianisme padouan de Galilée" also holds that the notion of *momento* of descending is the hint of a "silent Copernicanism" in Galileo's mechanics during the Paduan period; for reasons which we will not present here, we do not share this interpretation.

their value depends only on the height of the plane.[68] The first edition of the *Discorsi* (1638) made of this statement a principle; an objection from Viviani led Galileo to give a demonstration using, among other elements, the law of the inclined plane.[69] He formulated it at this time in the following manner:

> The moments (*i momenti*) or speeds of a same moving object vary with the different inclination of the planes … so that the *impeto*, the power (*il talento*), the energy (*l'energia*) or, as we wish to say, the moment (*il momento*), of descending, are decreased in the mobile object by the plane on which they are supported and they descend.[70]

Because Galileo would later be able to articulate this law to obtain certain results in the science of accelerated motion for which he is famed, it may appear natural for the historian to trace it back to its beginnings; but as we have just seen, this is a complex task. More precisely, in the light of the questions raised in this volume, the three following points can serve as a conclusion:

(1) The geometric techniques are identical in all the demonstrations: it is a matter of comparing similar triangles and manipulating proportions. The advances do not come here so much from a greater or lesser mathematical sophistication as from a work of conceptualization, both before and after the geometric demonstration. Before the geometric demonstration, by the mental manipulations that allow the reduction of the inclined plane to mechanical systems already understood. After the geometric demonstration, in particular by work on the language, allowing the sedimentation or crystallization in a precise terminology of what had heretofore been mixed together, for example by the distinction between *gravità* and *momento* that characterizes the *Mecaniche*.

[68] Galilei, *Le opere*, vol. VIII, p. 218: "I gradi di velocità d'un mobile descendente con moto naturale dalla medesima sublimità per piani in qualsivoglia modo inclinati, all'arrivo all'orizzonte son sempre eguali, rimossi gli impedimenti". The statement of this principle can already be found in the *Dialogo*, in Galilei, *Le opere*, vol. VII, p. 47.

[69] Galileo to Benedetto Castelli, December 3, 1639, in Galilei, *Le opere*, vol. XVIII, p. 126. Viviani wrote down this demonstration for Galileo who was already blind, and had it inserted in the first edition of the *Opere* of Galileo (Bologna, Manolessi, 1656, pp. 132–134). On the role of this principle in Galileo's new science of motion, see Laird, "Renaissance Mechanics and the New Science of Motion".

[70] Galilei, *Le opere*, vol. VIII, p. 215.

(2) The problem of the inclined plane constitutes a challenge to the Archimedean tradition as well as to the Aristotelian tradition: it cannot be immediately reduced to the balance, the fundamental model of these traditions, whatever their other differences. As we have seen, the failures of Heron, Pappus, Leonardo da Vinci, or Stigliola come from a direct application of this model; the success of Jordanus, Stevin, or Galileo comes from the fact that they relied on another model or managed to adapt the model of the balance to the problem at hand, by means of the bent lever. In light of this inventiveness, the distinction often made since Duhem among different mechanical traditions is not pertinent: faced with a new problem, mechanical physicists make the most of the tools they have to hand. This conclusion could be verified historically in a certain number of 16th-century authors, who did not consider Pseudo-Aristotle, Archimedes, or Jordanus as fathers of opposing traditions, but as representatives of a common knowledge that was to be revived.

(3) The few certainties that we have regarding the material paths followed by these texts are rare, and often negative: we know, for example, that the Pseudo-Aristotelian *Mechanica* was not directly known in the medieval Latin world, and that Heron's *Mechanics* was unknown in 16th-century Italy. In such conditions, how can we interpret the identity between two or more demonstrations? We will offer here two elementary rules. First, it is indispensable to establish that the two demonstrations are in fact identical: contrary to what has often been written, for example, the demonstration of the law of the inclined plane proposed by Jordanus is not identical to that of Galileo. Second, when this identity has been established, historians must distinguish as best they can (and for this, they surely need as much tact as patience) what cannot be explained without genuine knowledge of a text from what may be a mere coincidence. Let us suppose, for example, that Galileo (1) and Galileo (2) are two distinct individuals and it is possible, but not established, that the latter knew the works of the former: the identity of their geometric demonstrations of the law of the inclined plane would lead us to think that the latter had indeed consulted the works of the former. Conversely, the identity of the intuitive analyses of the inclined plane given by Heron, Leonardo da Vinci, and Stigliola is inconclusive, for there is a weak identity, sufficiently explained by the omnipresence of the model of the balance until the end of the 17th century.

3. MECHANICS IN NEW CONTEXTS

JOCHEN BÜTTNER[1]

THE PENDULUM AS A CHALLENGING OBJECT IN EARLY-MODERN MECHANICS

> Next I come to those other questions, pertaining to pendulums, a subject that may seem very dry, especially to philosophers who are forever occupied in the more profound speculations about physics.[2]

It is an inherent characteristic of early modern science that a variety of phenomena that did not belong to the canon of objects studied in traditional mechanics, such as the trajectory of a projectile, the oscillation of a pendulum, impact, and the curve of a hanging chain, commenced to attract the interest of scientists. These new phenomena entered the intellectual horizon of the early modern scientists mostly from outside the dominating academic traditions, namely from contemporary technology. Attempts to integrate the immense body of practical knowledge that had accumulated from the application of these phenomena in contemporary technology into the existing theoretical frameworks of mechanics, however, regularly led to internal problems. Most of the new phenomena simply turned out to be too complex to be successfully tackled by means of the existing theoretical frameworks of mechanics and, as a consequence, triggered their conceptual transformation. A history of science that aims to explain the dramatic conceptual transformation and social reorganization of mechanical knowledge in the early modern period—a transformation that eventually resulted in what has been termed the "mechanization of the world picture"—has hence to deploy an approach that acknowledges the essential role that the new mechanical phenomena played in this process.[3]

The new mechanical phenomena under discussion have elsewhere been referred to and described as the "challenging objects" of early modern science.[4] The present paper adopts this notion and identifies the pendulum

[1] I am indebted to Sophie Roux and Roy Laird, who, as editors of this volume, provided me with many helpful comments.

[2] Galilei, *Two New Sciences*, p. 96.

[3] The expression "mechanization of the world picture" was coined by Dijksterhuis, *The Mechanization of the World Picture*.

[4] The notion of challenging object and a description of the function of challenging objects within the transformation of the science of mechanics in the early modern period has been developed by a team at the Max Planck Institute for the History of Science, in particular by P. Damerow and J. Renn, in the context of research on the long-term development of mechanical knowledge. The approach sketched in the introduction of this paper has in similar

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 223–237.

as a particular instance of such an object. In the discussion of the pendulum the existing notion of a challenging objects will be elucidated and refined. At the same time the overall usefulness an approach acknowledging the relevance of challenging object for the historiographical problem of the transformation of mechanics in the early modern period will be pointed out.[5]

The challenging objects of early modern science are characterized by the following five essential attributes: their novelty to theoretical reflection in the period, their technological background, their complexity as objects of theoretical reflection, their role as a shared knowledge resource, and finally their tendency to stimulate the conceptual reorganization of knowledge. Below it will be asked one by one whether these characteristics actually applied in the case of the pendulum. Even though in some cases the obvious answer may seem to be no, after close re-examination this answer, as will be shown, nonetheless has to be revised. For instance, the theoretical reflections on the pendulum, at least according to standard accounts, do not seem to depend on a technological background. However, reassessment of the historical evidence based on the assumption that the pendulum is in fact a challenging object eventually will compel the acceptance of that very assumption.

THE PENDULUM AS A NEW CHALLENGE IN EARLY MODERN SCIENCE

The objects that challenged early modern science were typically new to theoretical reflection in that period. The identification of the pendulum as a challenging object hence requires that it became an object of theoretical interest only in the early modern period. In fact, throughout the entire corpus of Aristotle's works no reference to a pendulum is made.[6] Likewise

form already been enunciated for instance by Renn, "Editor's Introduction". The role of a particular challenging object in the history of mechanics in the early modern period, the trajectory of a projectile, has been discussed in Büttner et al. "The Challenging Images of Artillery".

[5] A detailed account of the role of the pendulum in the history of science has been given in Ariotti, "Aspects of the Conception and Development of the Pendulum". Ariotti, however, limited himself to discuss the history of the discovery of only a few particular and allegedly important properties of pendulum motion. As will be argued in this paper, Ariotti's discussion needs to be supplemented by additional information in order to offer a comprehensive account.

[6] The absence of the Greek word for "pendulum" in Liddell and Scott, *A Greek-English Lexicon*, suggests that no word for "pendulum" existed in the classical Greek language. This lack of a special technical term for the pendulum is indicative of the fact that the pendulum was not part of the ancient theoretical canon of mechanics.

pendulums are not mentioned in theoretical texts on mechanics until the second half of the 14th century. A direct reference to a pendulum can, however, be found in Nicole Oresme's *Le livre du ciel et du monde*.[7]

In a section of his book Oresme compares the motion of a pendulum to the motion of a stone that, according to his famous thought experiment in falling through a hole in the earth passes the earth's centre and then changes direction and begins to fall towards the centre again, thus displaying an oscillatory motion. As Buridan had done previously with the vibration of the string of a cither and the swinging of a bell, this oscillatory motion was then explained by Oresme by the concept of impetus. This explicit reference to a pendulum by Oresme seems to indicate that pendulums were actually not new to theoretical reflection in the early modern period.

However, the introduction of the pendulum in scholastic theories apparently did not challenge these theories as a whole, as their relative stability over the next centuries indicates.[8] As will be discussed below for the case of the pendulum, new phenomena characteristically became challenging objects only after they had become part of complex technical environments subject to the experience of practitioners, a complexity that was mirrored in the theoretical accounts that the early modern scientists devised for these objects. Oresme's treatment of the pendulum is, however, devoid of any such complexity. As far as his investigations are concerned, the pendulum should hence not be considered a challenging object, all the more so as Oresme compared its motion to a purely fictitious arrangement in the context of a thought experiment.

The first substantial theoretical reflections on the pendulum that could legitimately be seen as reflections on the pendulum as a challenging object are obviously Leonardo's ponderings on the matter. Two instances from Leonardo's *Codex Madrid* will be put forward here as examples of his

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[7] See Nicole Oresme, *Le livre du ciel et du monde*, Book. I, ch 18, fols. 30a–30b, p. 145.

[8] In fact, in view of its incoherent and disjointed nature and its marginal status within scholastics, not even the impetus theory as a whole challenged scholastic theory, as J. Sarnowsky shows in his contribution to this volume. Galileo himself seems to denounce the scholastic perception of pendulum motion as a phenomenon that provides an argument in favour of impetus explanations of violent motions as crude and restricted compared to his own perception of pendulum motion as a complex challenging phenomenon when in the *Discorsi* he has his spokesman Sagredo say: "... from such common things, or I might even say such base ones, you draw new and curious knowledge that is often far beyond my imagining. A thousand times I have given attention to oscillations, in particular those of lamps in some churches hanging from very long chords, inadvertently set in motion by someone, but the most that I ever got from such observations was the improbability of the opinion of many, who would have it that motions of this kind are maintained and continued by the medium ..." (Galilei, *Two New Sciences*, p. 98).

fundamental theoretical considerations concerning the pendulum, examples that exceed aspects of the practical utilization of pendulums in technological contexts. On folio 147 recto of *Codex Madrid I*, Leonardo compares the motion of a pendulum with that of a projectile, trying to find a theoretical explanation for the fact that in the case of pendulum motion the violent upward part of the motion is shorter than the downward part, whereas in the case of projectile motion the violent upward part of the motion is unexpectedly longer than the downward part. A number of entries concerning pendulum motion are also scattered over the two successive folios 182 and 183 of *Codex Madrid I*, among them entries that document Leonardo's attempt to give a quantitative description of the damping of pendulum motion.[9] The pendulum thus turned into a challenging object only at the advent of the early modern period.

THE TECHNOLOGICAL ROOTS OF THE PENDULUM

The challenging objects of early modern science were by and large anchored in contemporary technology, which facilitated their taking on a role as mediatory instances between early modern science and its technological context. An identification of the pendulum as a challenging object of early modern science hence suggests that the pendulum was actually applied in contemporary technology and that the practical experience gathered in these technological contexts to a great extent guided the course of the respective theoretical reflections.[10] Traditional accounts of the role of the pendulum in the history of science, however, contradict these assumptions.

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[9] See Leonardo da Vinci, *I codici de Madrid*. The passages in Leonardo's works relating to the question of pendulum motion have not yet been critically reviewed, nor have they been systematically compiled. The few scattered references to occurrences of pendulums in Leonardo's work in the literature are, as a rule, restricted to examples of pendulums in the drawings of technical devices and machines, be they real or imagined (see also notes 16 and 18 below).

[10] The present section owes greatly to Lefèvre, "Galileo Engineer", in which the author addresses the extent and specifically the way in which the emergence of modern science has to be "seen in the context of the world of craftsmen and engineers". Congenially Lefèvre discusses the specific case of the pendulum to show how "the technical possibilities of a given time shape the specific form in which problems can be investigated". I am furthermore in dept to W. Lefèvre for helpful hints and discussions in the drafting of the present paper. An author, who discusses the role of technological sources for Galileo in particular, is L. White. White incorporated the pendulum even in the concluding remarks of his paper: "It is exactly Galileo's environment of technical innovations like suction pumps and pendula which makes the tonality of his new science historically intelligible" (White, "Pumps and Pendula: Galileo and Technology", p. 110).

According to Vincenzo Viviani, Galileo's last and most faithful disciple and his first biographer, Galileo discovered a fundamental property of pendulum motion, the pendulum's isochronism, as a student while observing the swinging of a lamp in the dome of Pisa.[11] Serious doubt has since been cast on Viviani's account, for instance by Antonio Favaro's observation that the particular lamp today referred to as the "lampada di Galileo" did not exist in the cathedral of Pisa at the time Galileo was a student there.[12] However, these doubts concerning the alleged discovery of the isochronism of the pendulum have not affected a basic assumption underlying most accounts of the role of the pendulum in the history of science, namely that Galileo had to discover its isochronism in order to realize its "potential as a tool adaptable for time measurement".[13] Consequently the first indisputable application of the pendulum as a timing device in medicine, the pulsilogium, even though described in a publication by S. Santorio, is usually attributed to Galileo.[14]

The application of challenging objects, however, as a rule predates their theoretical reflection and such a reverse dependency relation between technical application and theoretical reflection may also prevail in the case of the pendulum. Indeed, a reassessment of the historical evidence indicates

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[11] A characteristic property of pendulum motion is its period, that is the time it takes the pendulum to complete one full oscillation. The assumption that this period does not depend on the initial displacement has become known as the "isochronism" of the pendulum. The "isochronism" of the pendulum holds, according to classical mechanics only approximately. The full solution of the equation of motion of a pendulum, which requires the use of elliptic integrals, shows that the period does indeed depend on the displacement of the pendulum.

The story of Galileo's discovery as a student of the isochronism of the pendulum in the dome of Pisa is told in Viviani's biography of Galileo, see Viviani, *Racconto istorico*.

[12] See Favaro, "Ancora sulla cosidetta 'Lampada di Galileo' "; Drake, "Renaissance Music and Experimental Science", has tried to supersede Viviani's account with a similarly imaginative story. He proposed, without adducing any evidence, that Galileo's interest in pendulum motion might well have been triggered by his father's musical experiments. In view of the historiographic approach taken in this paper, this is as unsatisfactory an account as Viviani's.

[13] Bedini, *The Pulse of Time*, p. 7. Such an assumption about the relation between theoretical reflection and practical application of a particular phenomenon is quite a common premise in history of science. It finds exemplarily expression for instance by Koyré: "The Cartesian and Galilean science has, of course, been of extreme importance for the engineer and the technician; ultimately it has produced a technical revolution. Yet it was created and developed neither by engineers nor by technicians, but by theorists and philosophers" ("Galileo and Plato", p. 401, n. 5).

[14] Galileo himself never claimed to have invented the pulsilogium. The first such claim was made by Viviani, *Racconto istorico*, p. 603. For a more recent statement concerning the invention of the pulsilogium, see for instance Bedini, *The Pulse of Time*, p. 7: "He [Galileo] proceeded to develop a device which became known as the pulsilogium".

that the pulsilogium, mentioned by Santorio in his *Methodi vitandorum errorum omnium qui in arte medica contingunt* published in 1602, was not an invention by but rather a source of inspiration for Galileo.[15]

The practical application of the pendulum in the early modern period was, however, not restricted to its use as a timekeeper. Pendulums were used as parts of various machines serving different functions, as illustrations for instance in Jaques Besson's *Theatrum instrumentorum et machinarum* of 1569, Fausto Veranzio's *Machinae novae* of 1615, or in a Spanish manuscript from around 1570, *Los veintiún libros de los ingenios* show.[16] Galileo himself at some point in his career had to review the proposal for a machine that evidently suggested the use of a pendulum as a device for "accumulating power".[17] These examples indicate that pendulums were

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[15] Those who claim that the pulsilogium can ultimately be traced back to Galileo's discovery of the pendulum isochronism commonly cite the second edition of Santorio's book, which appeared 1603. The first edition, however, apparently had already been published in 1602 (see Recht, "Life and Work of Sanctorius", p. 736), i.e., contemporaneously to Galileo's first allusion to the isochronism of pendulum motion in a letter to Guidobaldo at the end of the same year. In view of the accumulating evidence that the pendulum was already used as a timekeeper in clocks in the 16th century, the priority question concerning the invention of the pulsilogium becomes less important for answering the question of the relation between technical application and theoretical reflection on the pendulum of Galileo. Feldhaus, "Beiträge zur Geschichte Der Uhr" and "Das Pendel bei Leonardo da Vinci" for instance has identified two drawings in Leonardo's manuscripts that suggest that Leonardo already intended to use the pendulum as a regulator for a clockwork. Feldhaus furthermore maintains that a clock built around 1580 in Osnabrück by a certain Jost Bodeker probably used a pendulum mechanism. E. Morpurgo apparently claims that Benevenuto Volpaia (1486–1533), from a Florentine dynasty of instrument makers, had also employed pendulums as regulators for clocks; see Bedini, *The Pulse of Time*, p. 5, note 13.

[16] Of special interest is a manuscript version of Jacques Besson's book of machines that includes a list of mechanical principles absent from the published versions. This manuscript version has been discussed by Keller, "A Manuscript Version of Jacques Besson's Book of Machines". For a discussion of the particular machines in Besson that employ pendulums, see Foley et al. "Besson, da Vinci, and the Evolution of the Pendulum". See also Veranzio, *Machinae novae*, plate 44; Turriano, *Los veintiún libros de los ingenios*, vol. 3, fol. 331v. According to Victor Navarro in this volume, this book, usually attributed to Juanelo Turriano, is actually a work by Pedro Juan Lastanosa (see p. 251 n. 40 above). These examples of applications of pendulums in early modern technology have been extracted from a database of early modern machine drawings compiled by M. Popplow and W. Lefèvre, in which machines are analyzed and categorized according to their different components. This categorization facilitates immensely the identification of a particular component, such as the pendulum. See http://dmd.mpiwg-berlin.mpg.de/home (14.4.2007). For an excellent introduction into the topic of machine drawings see Lefèvre (ed.), *Picturing Machines*.

[17] See "A proposito di una macchina con gravissimo pendolo adattato ad un leva", in Galilei, *Opere*, vol. VIII p. 571.

indeed part of contemporary technology at the time when a substantial theoretical reflection on the properties of the pendulum motion set in. From a historiographic perspective that acknowledges the importance of the challenging objects as mediators between early modern science and the accumulated shared knowledge of the practitioners and engineers of the time, more detailed studies of the role these objects played in contemporary technology and of the social context within which they were placed remain, however, a desideratum. In this respect an investigation of the nature of the relation between the practical knowledge gathered from experience with these objects in technological contexts and the emerging theoretical knowledge about the very same objects requires particular attention.[18]

THE CHALLENGING COMPLEXITY OF THE PENDULUM

In technological contexts challenging objects did not occur as isolated phenomena but rather as phenomena exhibiting a multiplicity of properties that in turn were often connected in intricate and complex ways to other mechanical phenomena. This background in complex technological environments was, as a rule, mirrored in the complexity of the theoretical accounts that early modern scientists devised of these objects. The complexity of these accounts can, in turn, be understood mainly as a result of the following three interdependent factors. First, the immense practical knowledge about the challenging objects served as an empirical basis for and provided the early modern engineer-scientist with determining factors in the form of a wide range of properties that their theoretical accounts had to address. Secondly, the practical needs connected to the application of these objects led to an increased demand for quantitative instead of qualitative theories, hence adding an additional element of complexity. Thirdly, the connection of these objects to other mechanical phenomena present in a technological context entailed the formation of complex theories capable of integrating knowledge about many different mechanical phenomena.

As it turns out, in the long run it was this very complexity that doomed to failure almost any attempt to integrate these objects into the existing theoretical frameworks of early modern mechanics. In fact, a suitable

[18] Worth mentioning is a study by Foley et al. “Besson, da Vinci, and the Evolution of the Pendulum”, in which the authors, by rebuilding and empirically testing one of the machines depicted by Besson that uses a pendulum, have tried to reconstruct the practical knowledge that could be gained from its use, and also to investigate whether this particular application of a pendulum brings about the advantages ascribed to it in a manuscript version of Besson’s book.

example of how the complexity of the phenomenon of pendulum motion provided an insuperable challenge to early modern scientists is provided by Galileo's own enquiry into the subject. Yet in order to appreciate fully how the pendulum with its challenging complexity presented Galileo with an array of problems, none of which he was able to solve satisfactorily, one cannot rely exclusively on Galileo's published writings, where he attempted to conceal these problems, but one rather has to consult his correspondence and working notes.

Galileo's famous letter written to Guidobaldo dal Monte in November 1602, shows that he was at that time in possession of a proof of the so-called law of chords, that is, the proposition that the natural descent of a moveable body along all inclined planes that are inscribed as chords in a circle and that contain either its lowest or highest point take the same time. Galileo furthermore informed Guidobaldo that he was searching for a proof of the alleged isochronism of pendulum motion; that is of his assumption that the period of a pendulum is independent of its initial displacement.[19] In order to find such a proof Galileo devised an ingenious method for establishing a relation between the pendulum motion and the motion of fall along inclined planes. Galileo's idea begins with the observation that the motion of a pendulum may be identified with the swinging of a body along a concave spherical surface, since both motions effectively take place under the same geometrical constraints. This last motion in turn can be approximated by the falling and rising of a body along a sequence of conjugate inclined planes. In the most simple case, the motion along an arc is then approximated by the motion along the chord subtending this arc, a case for which Galileo, with his law of chords, had already proven that the isochronism holds. Since the approximation of an arc by a single chord was obviously too crude to draw from it any consequences regarding the time of the motion along an arc, Galileo laboriously proceeded to consider ever more fine-grained polygonal approximations. His excessive efforts to prove the isochronism of swinging pendulums along this line, which are documented by a series of folio pages of his notes on motion, did not, however, produce a conclusive result.[20]

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[19] This letter to Guidobaldo has been focused upon in the countless historical studies pertaining to Galileo's theory of motion; for a recent discussion, see Damerow et al. "Hunting the White Elephant".

[20] Galileo approximated the quarter arc of a circle by a sequence of as many as 16 chords and laboriously calculated the corresponding times of motion along these chords. A diagram of the 16 chord approximation is featured in mss Gal. 72, fol. 166r. The results of his calculations of the times of motion along these chords are listed on fol. 183r. Digital

Manuscript evidence furthermore indicates that Galileo at about the time of the composition of this letter to Guidobaldo was also aware of the law of fall, i.e., the quadratic dependence between the time of fall and the distance fallen as well as the so-called pendulum law, i.e., the quadratic dependence of the period of a pendulum and the length of its suspension. The realization of the challenging structural similarity between the isochronism of the pendulum motion and the law of chords on the one hand, and between the pendulum law and the law of fall on the other, must have strongly suggested the existence of a close relation between pendulum motion and accelerated motion along inclined planes. Galileo even tried to approach the theoretical problem of this relation in an experiment. A folio page of Galileo's notes on motion documents that Galileo had timed the period of a real pendulum as well as the rolling of a ball down an inclined plane. The alleged relation between pendulum motion and accelerated motion along inclined planes suggested to him that the period of the pendulum should be related to the time of fall over a vertical distance equal to the length of that pendulum. Galileo's theory of naturally accelerated motion, in fact, allowed him to calculate the latter time from his experimental data. However, even though his measurements were quite accurate from a modern point of view, the outcome of his considerations concerning the experiment did not present him with a decisive clue regarding his actual problem.[21]

In hindsight it is obvious why both Galileo's attempts, that is, his attempt to find a proof for the isochronism of the pendulum based on his theory of motion along inclined planes as well as his attempt to find experimental support for the suggestive similarity between the two types of motion, were destined to fail. The alleged isochronism of the pendulum simply does not hold according to classical mechanics, and the similarity of the isochronism of the pendulum and the law of chords on the one hand, and of the pendulum law and the law of fall on the other, is merely coincidence. Historically, the integration of pendulum motion into a general theory of motion required refined theoretical concepts as well as complex mathematical tools, both of which were not part of the theoretical framework

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reproductions of these folio pages are part of an electronic representation of Galileo's notes on motion, accessible from the websites of the Max Planck Institute for the History of Science, http://www.mpiwg-berlin.mpg.de (5.4.2005) and of the Istituto e Museo di Storia della Scienza in Florence, http://galileo.imss.firenze.it/ (5.4.2005). Galileo's efforts, however, remained without a tangible result with respect to his original questions.

[21] The bulk of Galileo's notes concerning his pendulum-plane experiment are featured in Ms. Gal. 72, f. 189v. The pendulum-plane experiment has been discussed by Hill, "Pendulums and Planes". An extensive re-examination of the experiment and its context will be included as part of my forthcoming Ph.D. thesis.

available to Galileo.[22] However, though Galileo could not meet his own ambitious goals, his attempts to solve the challenging complex puzzle of pendulum motion did provide immense stimulus for the genesis of his theory of naturally accelerated motion.[23]

THE PENDULUM AS A SHARED KNOWLEDGE RESOURCE

Characteristically, the challenging objects of modern science did not represent a challenge for merely one individual scientist. Rather, they served as a shared knowledge resource that presented a similar challenge to a whole group of intellectuals. It can in fact be shown that a large number of early modern scientists, including Jeremiah Horrocks, Niccolo Cabeo, Marin Mersenne, Marci von Kronland, René Descartes, Giovanni Battista Baliani, Isaac Beeckman, and others, accepted the challenge posed by the pendulum and tried to integrate this phenomenon into their mechanical theories, often independently of each other and some of them long before Galileo mentioned and discussed pendulum motion in any of his publications.[24] The following cursory look at one of these attempts to integrate the pendulum into the body of mechanical knowledge will help to reinforce the concept of the pendulum as a shared knowledge resource.

A number of passages in Beeckman's *Journal* are concerned with pendulum motion, among them two entries made between the 23rd of November and the 26th of December 1618.[25] The first of these entries, on folio 104 recto and verso, refers to the motion of chandeliers that hang from long ropes in churches.[26] In this entry, however, Beeckman does not consider

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[22] According to classical mechanics, the period of a pendulum is given by an elliptic integral. Depending on the historiographic position taken, the study of elliptic integrals is supposed to have begun either with John Wallis's study of the arc length of an ellipse in the second half of the 17th century, or a century later with the contributions by C. G. J. Jacoby and G. Fagnano. For the history of elliptic integrals, see Ayoub, "The Lemniscate", and Cooke, "Elliptic Integrals and Functions". What is clear, however, is that Galileo did not have at hand the necessary mathematical tools for a full integration of the phenomenon of pendulum motion into the existing framework of mechanics.

[23] An extensive discussion of the role of the pendulum for the conceptual development of Galileo's theory of motion will be included in forthcoming my Ph.D. dissertation.

[24] See Wilson, "On the Origins of Horrocks's Lunar Theory", p. 92; Cabeo, *In quatuor libros meteorologicorum aristotelis commentaria et quaestiones*, book I, p. 93 and pp. 98–99; von Kronland, *De proportione motus*, ff. B1–D4.

[25] Remarkably, these entries in Beeckman's *Journal* fall into the period of intense collaboration with Descartes shortly after the two men had met for the first time on 10 November, 1618.

[26] I am indebted to Sophie Roux for drawing my attention to this particular passage.

the type of motion commonly referred to as pendulum motion, namely an oscillation of the pendulum bob (i.e., in Beeckman's case a chandelier) in a fixed plane, but rather the continuous circular motion of the chandelier below and around its point of support. In an earlier passage Beeckman had tried to argue that perpetual circular motion without a motive force, i.e., "circular inertia", is possible provided that every part of the moving body moves at the same speed, a requirement that is met when the orientation of the moving body remains constant with respect to the surrounding space. This, according to Beeckman, is the case not only for the annual rotation of the earth around the sun in the Copernican universe, but also for the observed rotation of chandeliers. Except for accidental factors such as air resistance, the rotation of chandeliers hence represents for Beeckman a perfect mechanical analogy of the annual rotation of the earth around the sun, substantiating empirically that the latter motion is indeed conceivable.

This entry by Beeckman points to two essential characteristics of challenging objects. On the one hand it exemplifies how the early modern engineer-scientists did not merely try to find within the scope of their mechanical theories explanations for phenomena the objects displayed, but that they often attempted, mainly on an empirical level, to establish connections between different phenomena, even in the absence of strong theoretical explanations for them.[27] Considering the fact that scientists such as Horrocks or Hooke similarly used a rotating pendulum as a mechanical analogy for planetary motion, this entry, on the other hand, points to the fact the pendulum served as a shared knowledge resource. As such it serves to explain the appearance of similar ideas with different authors without resorting to the assumption of a direct influence between them. This last aspect becomes particular apparent in Beeckman's second entry concerning pendulums.

In the second entry, which is on folio 105 verso, Beeckman discusses a number of properties of pendulum motion. He alludes to the alleged isochronism of pendulum motion, states a qualitative pendulum law for the relation between the length and the period of a pendulum, and discusses the influence of the length as well as the surrounding medium on the damping of the pendulum. Moreover, based on the dynamical concepts of

[27] A well-documented example of an attempt to establish a connection between two challenging objects is Galileo's mistaken identification of the catenary with the shape of the projectile trajectory; see Damerow et al. "Hunting the White Elephant". This tendency to establish mechanical analogies between different challenging phenomena, as discussed in the previous section, contributed substantially to the complexity of the theoretical accounts of challenging objects.

the contemporary theoretical framework of mechanics, Beeckman tries to provide an actual argument for the isochronism of pendulum motion.[28] These considerations in Beeckman's *Journal* are in fact so close to Galileo's well-known reflections concerning the pendulum that it may be asked whether Galileo's ideas had reached Beeckman by the end of 1618, or whether alternatively this passage must be considered evidence that the pendulum did indeed serve as a shared knowledge resource for scientists of the early modern period.

Direct contact between Galileo and Beeckman in or before 1618 can be excluded with great certainty; indirect contact, for instance via Descartes or Mersenne, as a way of transmission is just as implausible.[29] Last but not least, Beeckman's own claim of being very experienced with pendulums lends great plausibility to the thesis that his practical interest in pendulums was not inspired by Galileo. But even in cases where a direct exchange of ideas had admittedly played a role, such as for instance between Beeckman and Descartes, a historiographic approach that interprets the pendulum as a challenging object retains explanatory power.[30] In view of the fact that

[28] In his *Journal*, Beeckman introduces the pendulum's isochronism: "And I am inclined to believe that the same lamp hanging from the cord of the same length has its own time in which it traverses some part of the circle, and that this is always the same". Further down the page he tries to provide a dynamical explanation of the phenomenon: "... therefore the greater the number of particles in a part of a circle, the greater is the inclination of the descent. Hence, the speed of the greater is related in this way to the slowness of the smaller; and as much as the space decreases so much does the slowness increase. Therefore the time is always equal". Beeckman's qualitative pendulum law takes the following form: "And [the longer the length, the] longer [*diuturnius*] it takes; the shorter [the length], the less time it takes". In the context of a discussion of damping, he also considers the influence of air: "If we imagine this happening in a vacuum, with only the inclination towards the centre of the earth retained, it will correspond perhaps more exactly with what has been said: for the slowness of the motion is strongly affected by air such that it straight away becomes much slower as a result". Finally Beeckman himself associates his theoretical considerations with practical experience: "For I think that I am experienced in these things which hang from long ropes" (Beeckman, *Journal*, p. 260). The original entry is partly in Latin and partly in Dutch. My special thanks go to Peter McLaughlin for his help in preparing the translation given above.

[29] In his letter to Mersenne, 11 October, 1638 (*Oeuvres*, vol. II p. 388), Descartes himself claims that he had never met nor corresponded with Galileo. According to Garber ("On the Frontlines of the Scientific Revolution", pp. 142–145), Mersenne had not yet had personal contact with Galileo in 1618 and was furthermore obviously not very familiar with the latter's scientific work at that time.

[30] Mersenne and Descartes repeatedly exchanged opinions about questions regarding pendulum motion, for instance, in letters written on 8 October, 13 November, and 18 December 1629 (Mersenne, *Correspondance*).

none of the scientists mentioned had a satisfactory theory of the pendulum motion at hand, let alone a sustainable idea of how pendulum motion could be integrated into the existing body of mechanical knowledge, direct transmission provides an explanation for the appearance of the pendulum in the theoretical writings of these authors, but not for the importance they generally attach to it.

THE PENDULUM AS A STIMULUS FOR CONCEPTUAL REORGANIZATION

The challenging objects of early modern science, due to their complexity that could not be adequately represented in the existing conceptual frameworks, had an inherent tendency to activate a cognitive dynamics that eventually brought about major conceptual reorganizations. Viewing the pendulum as a challenging object of early modern science hence suggests that the attempts to integrate it into the body of theoretical knowledge of mechanics had profound repercussions on the conceptual organization of that very knowledge.

Indeed, Baliani's theory of motion, published as *De motu naturali gravium solidorum* a few months before Galileo's celebrated *Discorsi e Dimonstrazioni Matematiche intorno a Due Nuove Scienze*, provides a remarkable example of how the pendulum had advanced from one challenging mechanical phenomenon among many others in Leonardo's writings to the central element of an Euclidean-Archimedean theory of natural motion, Baliani's own version of the "new science". Baliani's theory of naturally accelerated motion is striking in its resemblance to Galileo's. The theorems Baliani puts forward are indeed a subset of the theorems found in Galileo's *Discorsi*, a similarity that can undoubtedly be attributed to the intellectual contact between the two men. There is also, however, a significant difference between the two theories of naturally accelerated motion. Whereas Galileo tried to found his theory on an assumption about the augmentation of degrees of speed in naturally accelerated motion, Baliani based his theory entirely on assumptions about pendulum motion, among them the isochronism of pendulum motion and the law of the pendulum. Starting from these assumptions as principles, Baliani constructed his theory of mechanics as a theory in which the pendulum had advanced from explanandum to explanans.

However, Baliani's contribution to the history of science has often been discounted as mere imitation of Galileo's achievements. Consequentially the decisive difference in the foundation of the two theories, which bears witness to the potential of the pendulum as a challenging object to alter conceptual foundations, went almost completely unnoticed.[31] Hence it comes as no surprise that the historians of science have likewise failed to notice that it was precisely this conceptual difference between their theories that provided a remarkable stimulus for Galileo's thoughts. A marginal note in Galileo's personal copy of the *Discorsi*, as well as in his copy of a letter he wrote to Baliani, document that Galileo intended to reverse Baliani's proof of the law of fall in order to fill the gap in his first edition of the *Discorsi* to provide the missing proof of the law of the pendulum.[32] However, his death prevented him from including his findings in a revised edition of the *Discorsi*, in which the pendulum would most likely have played an even more prominent role.[33]

CONCLUSION

In the preceding it has been argued that the pendulum conforms to all essential characteristics of the challenging objects of early modern science and that it should therefore be considered to range among them. Comparison with other challenging objects that have been studied more carefully will then help to see the role of the pendulum in the history of science from a new perspective and improve our understanding of it. Conversely an improved understanding of the pendulum and the other challenging objects of early modern science will contribute to our understanding of the emergence and development of the modern sciences in which they played such an important role. A historiographic approach sensitive to the characteristic properties of challenging objects of early modern science, such as for instance their place in contemporary technology and their function as shared knowledge resources, requires, however, to a great extent, a reorientation of the focus of interest of historians of science. Hence, before a comprehensive account

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[31] Drake, for instance, emphasizes that: "He [Baliani] did not arrive at the laws of falling bodies independently of Galileo ...", but fails to mention that the proofs of those laws start from completely different assumptions and that hence the conceptual structure of the foundations of the two theories of motion are entirely different (Drake, "Baliani", in *Dictionary of Scientific Biography*, vol. 1, 424).

[32] The marginal note is in Galilei, Ms. Gal. 79, f. 62 r. The letter is contained in Galilei, Ms. Gal. 74, f. 35–36. Despite the enormous interest that has been paid by historians of sciences to Galileo's investigation of pendulums, both the marginal note as well as the letter have to my knowledge only been mentioned in a single article in this context (see Büttner et al. "Galileo's Unpublished Treatises").

[33] The pendulum developed its full potential in influencing the conceptual organization of early modern science only after Galileo's death, for instance in the work of Christiaan Huygens; see Yoder, *Unrolling Time*.

of the pendulum as a challenging object of early modern science can be ventured, a lot more attention will have to be devoted to neglected topics such as the practical knowledge of early modern engineers, to largely disregarded sources such as early modern machine drawings, and to the contributions of scientists, such as Baliani, who have up to now been seen only as marginal figures.

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MECHANICS IN SPAIN AT THE END OF THE 16TH CENTURY AND THE MADRID ACADEMY OF MATHEMATICS

In 16th-century Spain, as is well known, a substantial number of texts on natural philosophy, most of them related to university teaching, were written and published. To classify those texts simply as Aristotelian or scholastic would not do them justice, without making an effort to subject them to a more critical analysis with appropriate hermeneutical criteria, as Charles Schmitt and others have recommended. In these texts, the various trends of the late-medieval science of motion, both in its kinematical and dynamical aspects, are well represented.

Moreover, the theory of impetus was vigorously supported by authors of the first half of the century, such as Juan de Celaya, Domingo de Soto's professor in Paris and later rector of the University of Valencia. In the second half of the century, the theory was still considered as the best explanation of the movement of projectiles by authors as varied as the physician Francisco Valles, the theologian and philosopher Diego de Zuñiga and the Valencia professor of natural philosophy, Diego Mas.[2]

It should be emphasised that, on the whole, 16th-century Spanish achievements (whether by Spaniards or by authors living in Spain) in fields such as geography, cartography, terrestrial magnetism, astronomy, navigation, and natural history were notable. Although on many occasions the imperative of secrecy imposed by the government limited the spread of science, it did not completely end it, and through various channels Spanish science became part of the European patrimony of knowledge. There were also notable Spanish contributions in technology, as Nicolas Garcia Tapia and others have pointed out.[3]

The scientific and technical activity in early modern Spain, however, is not well known to non-Spanish historians of science, and the Iberian

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[2] See Navarro-Brotons, "De la filosofía natural a la física moderna"; and *id*., "La filosofía natural".

[3] See López Piñero, *Ciencia y técnica en la sociedad española*; *id*. (ed.), *Historia de la ciencia y de la técnica en la Corona de Castilla*; Vernet and Parés (eds.), *La ciencia en la història dels Països Catalans*; Navarro-Brotons, "Espanya i la Revolució Científica"; Navarro-Brotons and Eamon (eds.), *Beyond the Black Legend*.

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 239–258.

Peninsula is still to a large extent considered as the principal bastion of late scholasticism. The present study tries to evaluate in a more thorough way than has been done before the activities relating to the science of mechanics in late 16th- and early 17th-century Spain.[4] "Mechanics" is here understood according to its contemporary meaning, i.e., not only as theory of simple machines, but also as the discipline dealing with practical problems, e.g., from ballistics. I shall attempt here to integrate more or less well known information with other data less well known or little studied, in order to provide a new synthesis and interpretation of the subject. To do this, I focus on certain relevant personalities and the various contexts of their activities, principally the Mathematics Academy of Madrid, although I shall also refer to other institutions.

In the field of mechanics, Spanish authors – especially engineers, architects, cosmographers, and mathematicians – had access to the same ancient, medieval, and renaissance sources as scholars in the rest of Europe. As elsewhere, Spanish humanists played major roles in the recovery and diffusion of ancient sources. In the case of mechanics, an especially prominent figure, both in Spain and Italy, was Diego Hurtado de Mendoza.[5]

After studying Latin, Greek, and Arabic in Granada and Salamanca, Hurtado de Mendoza continued his studies in civil and canon law at Salamanca. After completing his studies there, he moved to Rome, where he assisted the lectures of Agostino Nifo (ca. 1526/1527)[6] and became familiar with the Averroist trends in Aristotelian philosophy.[7] Mendoza had a brilliant diplomatic career. In 1536, Charles V instructed him to join Eustace Chauys, the imperial ambassador in England. In 1539, Mendoza was appointed the imperial ambassador to Venice. In 1545, he participated in the Council of Trent as Charles V's representative. In 1547, he was appointed ambassador in Rome, and governor and commander in chief of

[4] At least among Spanish historians of science or non-Spanish historians interested in Spanish history.

[5] On Hurtado de Mendoza, see González Palencia and Mele, *Vida y obras*; Spivakovsky, *Son of the Alambra*.

[6] According to Ambrosio de Morales: "You had eminent teachers, such as Agustin Nypho, Montesdoca and others. You listened to their lectures on logic, philosophy and mathematics" (quoted in González and Mele, *Vida y obras*, vol. III, p. 471); see also Spivakovsky, *Son of the Alhambra*, p. 38.

[7] His library included various works of Pietro Pomponazzi as well as those of Nifo; on Mendoza's library, see González and Mele, *Vida y Obras*, vol. III, Apéndice CXIX, pp. 481–564.

Siena and other cities in Tuscany. Mendoza returned to Spain in 1553 and was installed in the court as an advisor. In 1568, he was banished to Granada as a result of a violent dispute with the Count of Leyva. Mendoza died in Madrid in 1574.

During his stay in Venice, Mendoza was a frequent visitor to the nearby Paduan academies and was friendly with intellectuals such as Pietro Bembo, Alessandro Piccolomini, and Giovanni-Battista Memmo.[8] Probably as a result of his meeting with Mendoza, Piccolomini extended his interest to mechanics, and in a work published in 1542 he spoke enthusiastically of machines and of the pseudo-Aristotelian *Mechanica*. "Aristotle himself has written of them in his short though excellent book", wrote Piccolomini. "Of this book, which by reason of the great corruption of its text is very obscure and has been explained by no one, I have made, at the persuasion of Don Diego Hurtado de Mendoza, a paraphrase".[9] Later, while at the Council of Trent in 1545, Mendoza made his own translation of the *Mechanica* into Spanish and discussed it with other delegates to the Council between sessions.[10]

While in Venice, Mendoza built an extraordinary library that included important Latin and Greek codices along with various early printed books. Greek scholars such as Nicolás Sophiano, whom Mendoza employed in Venice, also obtained Greek and Latin manuscripts in eastern countries. Mendoza's library contained a large number of works on philosophy and mathematics, including texts on ancient and medieval mechanics. Mendoza

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8 Rose, *Italian Renaissance of Mathematics*, p. 53.

9 Piccolomini, *Della Institutione*, p. 59v, quoted by Rose and Drake, "The Pseudo-Aristotelian *Questions of Mechanics* in Renaissance Culture", p. 82; repr. in Drake, *Essays*, p. 144. Piccolomini published his Latin commentary, *In mechanicas quaestiones Aristotelis paraphrasis paulo quidem plenior*, at Rome in 1547. The work was reprinted in Venice in 1565 and translated into Italian by the metallurgist Vanoccio Biringuccio.

10 The translation has been edited in Foulché-Delbosc, "Mechanica de Aristóteles" who dates the translation to 1545. The Library of El Escorial preserves three manuscript copies of Mendoza's Spanish translation, one of them with marginal and textual corrections in Mendoza's hand (Ms. f-III-15). The second (Ms. f-III-27) appears to be a clean copy of the first. The third manuscript (Ms. K-III-8) contains only the first section of the translation and the introduction, where Mendoza names the duke of Álava as the patron to whom the translation was dedicated. For a description of the Ms. f-III-15, see Zarco Cuevas, *Catálogo de los manuscritos castellanos de la Real Biblioteca de El Escorial*, vol. I, p. 142; Graux, *Essai sur les origines du fonds grec de l'Escurial*, includes facsimiles of pages of Ms f-III-15 and Ms. f-III-27. Mendoza's translation was part of a projected series of Aristotelian translation and studies. Mendoza's *Paraphrasis de phisico auditu en 14 quadernos* is preserved in Escorial as well.

often loaned his books to other scholars; among the most distinguished users of the library was Conrad Gesner. [11]

At Venice, Mendoza became the friend and pupil of the mathematician Niccolò Tartaglia.[12] In Tartaglia's *Quesiti ed inventioni diverse*, published at Venice in 1546, Mendoza appears as the pupil to whom the science of statics is expounded. Mendoza may have brought Tartaglia a manuscript of the Moerbeke Archimedes in the autumn of 1539. The manuscript from which Tartaglia published the 1543 edition and from which he copied the Jordanus *De ratione ponderis* and the pseudo-Archimedean *Scientia de ponderibus* is preserved at the Library of El Escorial.[13]

As I have indicated, Mendoza's translation of the *Mechanica* is conserved in three copies, with a dedication to the Duke of Alba on the principles and utility of the work. According to Mendoza, "the intention of Aristotle was to introduce the mathematician to the practice and use of this subject, especially geometry … . What moved our author to write was the difficulty

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[11] On Mendoza's Library, see González Palencia and Mele, *Vida y obras*, vol. I, Part III, pp. 253–363. On Gesner and Mendoza's Library, see Gesner, *Biblioteca universalis*, pp. 12–13; Gonzalez and Mele, *Vida y obras,* vol. I, Part III, p. 260; Spivakovsky, *Son of the Alambra*, p. 133. Another user of his library was the humanist Juan Páez of Castro, who was also in Trent for the Council, where he became friends with Mendoza and participated in the "Aristotelian Academy" organized by him. In a letter to his patron, the historian Jerónimo of Zurita (who became Chamber Secretary of Felipe II and organizer of the future Archivo de Simancas), Páez informed him that "I have the house full of ever so many printed books of Don Diego that I want, and manuscripts too, and his sketchbooks …. Now we understand the *Mechanics* of Aristotle, making great demonstrations. He has it translated into Spanish and has written a glossary; I believe I can help him somewhat. Always he says: 'Let us study, Señor Joan Páez' " (Páez de Castro to Zurita, July 6, 1545, in Dormer and de Uztarroz, *Progresos de la historia en el reyno de Aragón*, p. 461; see also González and Mele*, Vida y obras*, vol. I, Part III, p. 315; for the English trans., see Spivakovsky, *Son of the Alambra*, p. 132). Paéz de Castro wrote a *Memorial sobre los libros y utilidad de la librería y orden y traza que en ella se ha de tener* directed to Philip II around the beginning of his reign. The project of Páez was to include not only books, but natural objects and instruments, as well as antiquities. The project of Paéz is considered decisive in the construction of the Library of El Escorial and its collections. The text of this project is preserved in the Library of El Escorial, Ms. 1-II-15, fols. 190v–195v. The manuscript was edited by Nasarre (see Páez de Castro, *Memorial*); see also fragments in Graux, *Essai*. On the Páez project, see Vicente Maroto and Esteban Piñeiro, *Aspectos*, pp. 39–47; Gómez López, "Natural Collections in the Spanish Renaissance". On Paéz, see Solana, *Historia de la Filosofia Española*, vol. II., pp. 203–210.

[12] From the letters of Tartaglia to Cardano we know that Tartaglia met Mendoza for the first time in 1539, see Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 23.

[13] Drake and Drabkin, *Mechanics*, p. 23, who refer incorrectly to the National Library of Madrid: the cited manuscripts are preserved actually at the Library of El Escorial.

that he is aware lies in joining practice with theory in the mathematical sciences".[14]

In his will, Mendoza named Philip II as his heir, and his books and manuscripts became part of the library at El Escorial. Mendoza's library, including his collection of books on mechanics and related topics, and his translation of the *Mechanica*, as well as his presence as an interlocutor in Tartaglia's work and his relationship with Italian and Spanish scholars, contributed to the diffusion and spread in Spain of interest in mathematics and mechanics.[15]

JUAN DE HERRERA: ARCHITECTURE AND MECHANICS

Mechanics was a significant preoccupation among the mathematicians and cosmographers associated with the Council of the Indies and the so-called Academy of Mathematics of Madrid, individuals who developed an extensive repertoire of activities, including nautical astronomy, cartography, and civil and military engineering. One of the most important instigators of this activity was Philip II's famous architect, Juan de Herrera.

Herrera was born about 1530 in Mobellán, near Santander in the north of Spain. He belonged to the gentry and, like other 16th-century *hidalgos*, he sought his fortune at court. Herrera entered the service of Prince Philip in 1547, accompanying him to Italy and the Low Countries. Returning to Spain in 1551, Herrera enlisted in the army in the guard of Ferrante Gonzaga, with whom he went to Flanders. Between 1553 and 1563, Herrera was in the service of the Emperor and Philip. In 1563, Herrera and Juan de Valencia were appointed assistants of the Royal Architect Juan Bautista de Toledo. At present it is not clear what Herrera's qualifications were for this post and we know very little about his education. Although in later life he became a famous intellectual, he never claimed to be highly educated and always referred to himself as self-taught. We can be sure, however, that by 1562 Herrera was a skilled draftsman with a special interest in machines and

[14] Mendoza in Foulché-Delbosc, "*Mechanica de Aristóteles*", p. 368; quoted in English translation in Rose and Drake, "The Pseudo-Aristotelian tradition *Questions of Mechanics* in Renaissance Culture", p. 86; repr. in Drake, *Essays*, vol. III, p. 147.

[15] As Rose and Drake, "The Pseudo-Aristotelian *Questions of Mechanics* in Renaissance Culture", p. 87; repr. in Drake, *Essays*, vol. III, p. 149, observed: "As a friend of Bembo and Piccolomini and pupil of Tartaglia, Mendoza acted as a bridge between principal phases of the Renaissance tradition of the *Mechanica*".

scientific instruments. That year he completed a set of scientific illustrations for a copy of a medieval astronomical treatise, the *Libro de las Armellas*.[16]

Between 1563 and 1567 Herrera completed his architectural training as an assistant to Juan Bautista de Toledo, a sophisticated designer whose experience in the Italian tradition included his affiliation with the Roman circles of Sangallo and Michelangelo.[17] By 1570, after the death of Juan Bautista de Toledo, Herrera was functioning as the royal architect for the Escorial. In 1579, was appointed Royal Architect and "Aposentador de Palacio" (Royal Chamberlain). Besides his work as an architect, Herrera undertook a considerable amount of scientific and technical work, functioning as Philip's advisor on those subjects, screening projects, making criticisms and corrections, and forwarding promising schemes. Herrera was consulted on every scientific and technological matter from fortifications to botanical illustration, and made direct contributions on several subjects.

As an architect, Herrera followed Vitruvius' model much more than that of his Italian colleagues, above all in reference to the importance of mathematics, mechanics, and technological questions. In Herrera's view, architecture "presupposes something of all the arts and sciences … particularly geometry, arithmetic, perspective, music, astrology, gnomonics, and mechanics".[18] And, as Wilkinson-Zellner as observed, "the idea that building belonged with technology was based on the belief that all these subjects derived from mathematics, specifically from geometry and the geometrical laws of mechanics".[19] According to this view, building was a branch of the science of mechanics. As I have already indicated, however, Herrera was also interested in and practiced other disciplines mentioned by Vitruvius, including astronomy and scientific instrumentation. At the same time, his myriad interests and the particular importance he accorded to the mathematical disciplines and their applications, was well reflected in his extraordinary library and his collection of objects and instruments.[20]

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[16] The *Libro de las armellas* was included in the *Libro del saber de astrología* by Alfonso X, El Sabio. For Herrera biographical data, see Arcaute, *Juan de Herrera, arquitecto de Felipe II*; Wilkinson-Zerner, *Architect to Philip II of Spain*. Documents relating to Herrera and some of Herrera's works, including his work on mechanics mentioned below, are published in Herrera, *Discurso del Sr. Juan de Herrera*. A collection of papers is published in Aramburu-Zabala and Gómez Martínez, *Juan de Herrera y su influencia*.

[17] On Juan Bautista de Toledo, see Riera Blanco, *Juan Bautista de Toledo y Felipe II*.

[18] Herrera, *Institución de la Academia Matemática*, p. 15.

[19] Wilkinson-Zellner, *Juan de Herrera*, p. 18.

[20] See Cervera Vera, *Inventario de los bienes de Juan de Herrera*. See also Herrera, *Discurso*, pp. 410–468.

Herrera's library included an excellent collection of works on mechanics, civil and military engineering, artillery, and architecture, as well as numerous works on mathematics, astronomy, and natural philosophy, with special attention to the Lullist tradition. He also owned an impressive collection of scientific instruments and of the tools to make them. Most were for nautical and astronomical applications, but the collection also included other items, such as a "balance to demonstrate Archimedes' ideas". Texts on mechanics included the *Quesiti* of Tartaglia, the *Mechanicorum liber* of Guidobaldo dal Monte (both the 1577 edition in Latin and the 1581 Italian translation by Filippo Pigafetta), a summary of Pigafetta's *On plane equilibrium* of Archimedes, the collected works of Archimedes edited by Federico Commandino, the Latin and Italian versions of the pseudo-Aristotelian *Mechanica* edited by Alessandro Piccolomini, and a manuscript of a vernacular translation of the *Mechanica* (*las Mecánicas de Aristóteles en romance manuscrito*). The architectural works included various versions of works by Vitruvius and Alberti as well as texts by Serlio, Vignola, and other authors not mentioned by name in the library inventory. Among the engineers of Antiquity, Herrera possessed various editions of the works of Heron of Alexandria; in addition, he had a good collection of works (both printed and in manuscript) by Renaissance engineers and of texts on the military arts and artillery.[21]

Among Herrera's surviving writings is one titled *Architecture and Machines*, a work aimed at explaining the foundations of mechanics. The text, probably written between 1563 and 1564, is dedicated to Philip II. In the work, Herrera points out that the admirable effects of machines, invented out of practical necessity, can be explained by geometry and natural philosophy. The effects of machines, he explains, "result from the circle, which nature endowed with properties that could not be placed in others, and in these are many oppositions … because if the circle moves, one can see that the centre is still, and mobile and fixed are opposites". Furthermore, he observes, going up and going down are opposites and the circular line is also both convex and concave. As is evident, Herrera is only paraphrasing the first pages of the pseudo-Aristotelian *Mechanica* and states, in agreement with that work, that "all the machines in the world are founded on the balance" (*en la romana están fundadas todas las máquinas del mundo*).[22] He subsequently sets out

[21] See Cervera Vera, *Inventario de los bienes de Juan de Herrera*; the quotation is on p. 168, n. 679. See also Herrera, *Discurso*, pp. 410–468.

[22] "The facts about the balance depend upon the circle, and those about the lever upon the balance, while nearly all the other problems of mechanical movement can depend upon the lever" (Pseudo-Aristotle, *Mechanica* 848a11–15; trans. Hett, *Aristotle: Minor Works*, p. 335).

the principle of concentric circles, which he applies to cranes. He also cites Archimedes ("*siracusano*") and the latter's supposed affirmation (according to Plutarch and Pappus), "Give me a place to stand, and I will move the earth". Herrera confines himself, however, to stating the basic principle of statics: the proportionality between the weights and the lengths of the arms of a lever (in modern terminology, the equality of static moments).[23] I shall not go into the problem of the two traditions – Archimedean versus the pseudo-Aristotelian – as Herrera, like other Renaissance authors, does not appear to make any conceptual distinction between them.[24] As we have pointed out, Herrera had a manuscript copy of the *Mechanica* translated into "*romance*", which was probably a copy of the Spanish translation made by Diego Hurtado de Mendoza.[25]

THE ESTABLISHMENT OF THE ACADEMY OF MATHEMATICS

In 1571 the Council of the Indies, the monarchy's supreme advisory board for the administration of the New World, underwent a profound reform begun by its chairman, Juan de Ovando. An important result was the creation

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[23] The manuscript, preserved in the Archivo general de Simancas, Casas y Sitios Reales, leg. 258, f. 488, was first edited in Llaguno and Ceán Bermúdez, *Noticias de los arquitectos y arquitectura*, vol. II, p. 125. It was transcribed without commentary by Arcaute, *Juan de Herrera*, pp. 36–38, and by Godoy, in Herrera, *Discurso*, pp. 169–174. A facsimile edition appears in Cervera Vera, *El manuscrito de Juan de Herrera indebidamente titulado Arquitectura y Máquinas*. See also, in García Tapia, "The Manuscript '*Architectura y Machinas*' ", a facsimile of the text and an analysis of Herrera's work. In his very interesting analysis of the manuscript, Garcia Tapia has failed to recognize the pseudo-Aristotelian *Mechanica* as the Herrera's main source.

[24] Even if Herrera based his explanation on the Pseudo-Aristotelian *Mechanica*, this does not imply that Herrera was not familiar with the others traditions, but he did not notice the conceptual differences between them. The melding of the Archimedian tradition and the Aristotelian was common among 16th-century authors. Thus, Guidobaldo dal Monte considered Aristotle's principles to have been the starting point for Archimedes. See Rose and Drake, "The Pseudo-Aristotelian *Questions of Mechanics* in Renaissance Culture" p. 90; repr. in Drake, *Essays*, vol. III, p. 150. See also, on the melding of traditions, Micheli, *Le origini del concetto di macchina*, pp. 118–119. On mechanics more generally in the Renaissance, see Laird, "The scope of Renaissance Mechanics" and the introduction by Gatto in Galilei, *Le mechaniche*.

[25] As far as I know, there is no other Spanish translation of the *Mechanica*. Although the work in question might have been the Italian translation of Piccolomini's *Paraphrasis*, the latter appears explicitly in the inventory (see Cervera Vera, *Inventario,* p. 167) under the author's name, as we have already indicated. Thus, listed under number 656 is "Mecánicas de Aristóteles de Alejandro Piccolomini, en latín" and, under number 670, "Piccolomini sobre las mecánicas de Aristóteles en italiano, con un cuaderno manuscrito de lo mismo". In Herrera's inventory, works in Italian are always distinguished from works in Spanish, the latter being referred to as in "romance".

of the post of Chief (or Major) Cosmographer-Chronicler of the Indies. Ovando appointed to this position his assistant, Juan López de Velasco, who attempted to develop Ovando's program of producing an accurate and thorough geographic description of both Spain and the New World, including geographic coordinates calculated by astronomical methods. At the same time, the Council took on the urgent necessity of improving navigational charts and instruments. That task involved several initiatives, one proposed by Juan de Herrera. After the annexation of Portugal, Philip II, from Lisbon, approved Herrera's initiative along with another, also from Herrera: the creation of an Academy of Mathematics in Madrid.[26] In his proposal, Herrera argued that the dearth of good mathematicians in the kingdoms of the Hispanic monarchy necessitated the creation of an academy to train theoretical and practical arithmeticians, geometers, astronomers, theoretical musicians, cosmographers, pilots, architects, designers of fortifications, engineers, and machinists ("entendidos en el arte de los pesos y en todo género de máquinas"), gunners and instrument-makers, plumbers and water graders, clock experts, perspective experts and also sculptors and painters with practice in perspective. The academy would also take on the task of training the sons of noble courtiers as mathematicians. For each matter or activity Herrera recommended an appropriate group of texts, which displays his intimate familiarity with the mathematical disciplines and their applications. Likewise, Herrera instructed that the classes be taught in Spanish.[27]

For mechanics in particular, Herrera recommended the first seven books, plus books eleven and twelve, of Euclid's the *Elements*, Archimedes's *On the Equilibrium of Planes*, the *De centro gravitatis* of Commandino, the *Liber de ponderibus* of Jordanus de Nemore, the pseudo-Aristotelian *Mechanica* in the Piccolomini version, and Guidobaldo's *Treatise on Mechanics*. "To get to know what a machine is", he recommended the works of Vitruvius, Valturius, Vegetius, Heron, and Tartaglia; and for the lectures in the Academy, he preferred Archimedes's *On the Equilibrium of Planes*, "the

[26] See Vicente and Esteban, *Aspectos*, pp. 72ff., on the reform of the Council of Indias and the foundation of the Academy of Mathematics. See also Esteban Piñeiro and Jalón, "Juan de Herrera y la Real Academia Matemáticas"; Navarro-Brotons, "The Teaching of the Mathematical Disciplines in Sixteenth-Century Spain".

[27] The Herrera's text has 20 pages; printed in 1584. It has been reedited by José Simón Díaz and Luis Cervera Vera from a copy preserved in the Bibliothèque Mazarine of Paris. On the Academy of mathematics, see also Vicente and Esteban, *Aspectos*, p. 70ff. and Esteban and Jalón, "Juan de Herrera".

most important parts of the mechanics of Aristotle", and the works of Jordanus and Guidobaldo.[28]

Juan Bautista Lavanha, a Portuguese cosmographer of noble descent who had studied in Rome, was chosen to run the academy and, in December 1582, Lavanha was appointed by a royal patent letter "to take charge of matters relating to cosmography, geography and topography in our court and elsewhere as ordered, and to teach mathematics".[29] At the same time, Pedro Ambrosio of Ondériz was appointed Lavanha's assistant and made responsible for translating scientific texts. Ondériz, who had studied classical languages and, having studied two years in Portugal (sent there by Herrera), also had good training as a mathematician and cosmographer, translated into Spanish Euclid's *Optics* and the pseudo-Euclidean *Catoptrics*; Books XI and XII of the *Elements*, Teodosio's *Sphaerica* and Archimedes's *On the Equilibrium of Planes.* As for artillery, Herrera stated that this was "subaltern to mechanics", of which "the most important parts will be learnt in the theory".[30]

In 1591, the Academy was subordinated to the Council of the Indies, although Herrera continued to be in charge of its activities. The post of Cosmographer-Chronicler was divided in two: Onderiz was named Cosmographer Major of the Indies and Juan Arias de Loyola was appointed Chronicler. Lavanha moved to Portugal, where he took up the position of Cosmographer Major. Arias and Onderiz were in charge of teaching in the Academy until 1595, when, following the death of Onderiz and the departure of Arias, Julian Firrufino of Milan was appointed to teach all the lessons. Firrufino had taught artillery in Burgos and Seville. After the death of

[28] In a letter of Herrera to the secretary of the Spanish ambassador to Venice January (1584), Herrera requested a series of scientific books, including works by Heron (in Commandino's edition), Guidobaldo ("las mecánicas en vulgar italiano", i.e., the *Mechanicorum liber* [Pesaro, 1577] translated into Italian as *Le mechaniche* by Filippo Pigafetta [Venice, 1581]), Piccolomini's *Paraphrasis* of the pseudo-Aristotelian *Mechanica* and, if the "mecánicas de Herón se han traducido en vulgar se prodrían enviar (if Heron's *Mechanics* has been translated, it should be sent)". In the same letter Herrera informed the ambassador of the creation of a new chair of mathematics in the court, from which we can suppose that the books requested were destined for the Academy, although we cannot affirm this with certainty. See an edition of the letter in Vicente and Esteban, *Aspectos*, pp. 119–121. Vicente and Esteban, *Aspectos*, p. 90, suggest that perhaps Herrera ordered them either for the library of El Escorial or for his own library, since they all appear in the inventory of his books.

[29] For a transcription of the royal patent letter on the appointment of Lavanha, see Vicente and Esteban, *Aspectos*, p. 115. On Lavanha, see Sánchez Pérez, "Monografía sobre Juan Bautista Labaña"; Teixeira de Mota, "O Cartógrafo Joâo Baptista Lavanha".

[30] See Vicente and Esteban, *Aspectos*, pp. 90ff. and p. 116, for a transcription of the royal patent letter on the appointment of Ondériz.

Herrera in 1597, on the initiative of the Count of Puñoenrostro, an artillery general and member of the Council of War, the lessons and the number of professorships at the Academy were expanded, although the new professors did not receive appointments. Thus, Juan Cedillo Diaz taught trigonometry, Juan Angel lectured on Archimedes's *On Floating Bodies*, and the soldiers Rodriguez de Muñiz and Cristobal de Rojas taught military tactics and fortification.[31] In addition to cosmography and the *Elements* of Euclid, Firrufino also gave lessons in artillery, for which he prepared a text titled *Descripción y tratado muy breve y lo más provechoso de la artillería* … (Brief and Profitable Description and Treatise on Artillery) which is preserved in a manuscript of 1599.[32]

The teaching program of the Madrid Academy of Mathematics contributed to the diffusion of knowledge in these disciplines among Spanish elites, especially those linked to the court. If the interests of the humanist and statesman Hurtado de Mendoza and other humanists were oriented to the recovery of the classical legacy and the union of theoretical with practical knowledge, the utilitarian motive was even more noticeable in the Academy. Nevertheless, this did not prevent some members of the Academy from also being interested in the theoretical aspects of mechanics and involving themselves in contemporary discussions about the foundation of mechanics and its epistemological status. To these more theoretical aspects of mechanics I devote the following paragraphs.

BOTWILD NERICIUS, GARCÍA DE CÉSPEDES, JUAN BAUTISTA VILLALPANDO: MECHANICS AND STATICS

Among those interested in such matters, particularly nobles, courtiers and knights, was a Swedish diplomat, Botwild Nericius, who had studied at the Collegio Romano and had maintained a correspondence with Clavius. Nericius went to Madrid before 1586 and died in Spain after 1599. In this correspondence, chiefly about mathematical matters, Nericius informed Clavius that members of the Madrid Academy were interested in his mathematical works.[33]

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[31] The information of the activities of the Academy between 1597 and 1600 here described come from the prefaces of de Rojas, *Teórica y práctica de fortificación*, and of Ginés Rocamora, *Sphera del universo*. There is no evidence of the fact that the new teachers received an official appointment, see Vicente and Esteban, *Aspectos*, pp. 137ff.

[32] Firrufino, *Descripción y tratado muy breve y lo más provechoso de la artillería*.

[33] On Nericius, see Baldini and Napolitani (eds.), *Christoph Clavius. Corrispondenza*, vol. II, parte II, pp. 75–76. For the letters from Neritius to Clavius, see *ibid*., vol. IV, letter 136

Moreover, Nericius was engaged in a debate with Guidobaldo dal Monte (through Clavius) on statics, particularly on the demonstration of proposition 4 of the section "On the Balance" of Guidobaldo's *Mechanicorum liber*: "A balance parallel to the horizon, having its centre within the balance and with equal weights at its extremities, equidistant from the centre of the balance, will remain stable in any position to which it is moved" (Libra horizonti aequidistants aequalia in extremitatibus, aequaliterque a centro in ipsa libra collocato, distantia habens pondera; sive inde moveatur, sive minus; ubicumque relicta manebit).[34] Guidobaldo based his demonstration on Pappus' definition of the centre of gravity of any body as "a certain point within it, from which, if it is imagined to be suspended and carried, it remains stable and maintains the position which it had at the beginning, and is not set to rotating by that motion", and on Postulate 2: "The centre of gravity of any body is always in the same place with respect to that body".[35] Nericius considered this to be a circular argument. According to Nericius, from Pappus' definition, one can deduce that, to say that bodies in a balance are in equilibrium is equivalent to saying that they are equidistant from the horizon. In his extended demonstration of proposition 4, Guidobaldo had shown that, given the convergence of the direction of the weight toward the centre of the earth, the weight is heavier in the point tangent to the right line toward the centre of the earth than in the extremity of the horizontal diameter of the balance. Similarly, Guidobaldo affirmed that weights increase their heaviness as they approach the centre of the world.[36] From these conclusions, that is, accepting that the heaviness varies with the distance from the centre of the earth and converges toward it, Postulate 2 now would not be valid and the only possible definition of *aequeponderare* would be that proposed by Nericius.[37]

This argument was initiated, apparently, in the Madrid Academy between supporters and critics of Guidobaldo. We do not know who initially defended

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(February, 1597), pp. 17–19; letter 139 (October, 1597), pp. 27–28; letter 149 (December, 1598), pp. 68–69; letter 153 (April, 1599), p. 77.

[34] Guidobaldo dal Monte, *Mechanicorum liber, De libra*, proposition. IV, f. 5r; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 261.

[35] Guidobaldo dal Monte, *Mechanicorum liber*, f. 1r–v; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, p. 259.

[36] Guidobaldo dal Monte, *Mechanicorum liber*, fols. 5r–30r; trans. in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, pp. 261–294.

[37] For this debate, see Gamba and Montebelli, *Le Scienze a Urbino nel tardo Rinascimento*, pp. 213–250, which includes a letter from Guidobaldo to Clavius (July, 1598) on the subject. See also, in Baldini and Napolitani, *Christoph Clavius. Corrispondenza*, vol. IV, Letter 149, pp. 61–64. See also the letters quoted above from Neritius to Clavius.

Guidobaldo's position, but one of the mathematicians associated with the academy, Andrés García de Céspedes, gave a favourable appraisal of the Italian author in a work published some years after the polemic took place.[38]

When the court moved to Valladolid, the Madrid mathematics chair was discontinued; it is not known whether its activities were continued in Valladolid. Firrufino died in 1604. In 1607, the chair recommenced its work, again in Madrid. Its new holder was the Cosmographer Major of the Indias, Andres Garcia de Céspedes, who succeeded Juan Cedillo Diaz in 1611.

Garcia de Céspedes was a distinguished cosmographer, astronomer and engineer. He wrote extensively on these subjects, and two of his works were published: the *Libro de instrumentos de geometria* (1606) and the *Regimiento de navegacion* (1609). The latter is a veritable *summa* of all the knowledge and techniques developed in Portugal and Spain relating to the art of navigation, as well as incorporating contributions by authors from other countries.[39]

The *Libro de instrumentos nuevos de geometria* (1606) includes a magnificent treatise on hydraulics, the most comprehensive on the subject published in Spain during the Renaissance.[40] The work also includes a chapter on ballistics, especially the question of the angle of shot for the greatest distance. García de Céspedes states that "artillery can be called the machine of machines", adding that its scant progress could be attributed to the fact that practitioners of the art were not mathematicians or philosophers. He then states that many artillerymen know what the angle of the greatest range is but could not give a reason for it.[41] García de Céspedes resolves the trajectory into two parts, one straight and the other curved, although he advises that the straight part is only "appreciably straight" and that it is impossible for one to calculate the length of the part that is perfectly

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[38] See the Letter 149 from Neritius to Clavius (Decembre, 1598), in Baldini and Napolitani, *Christoph Calvius. Corrispondenza*, vol. IV, pp. 61–64, on the commencement of the debate in the Academy of Mathematics of Madrid.

[39] On García de Céspedes, see Picatoste Rodríguez, *Apuntes para una biblioteca científia española del siglo XVI*; Vicente and Esteban, *Aspectos*, pp. 422–431 sqq.; Navarro-Brotons, "Astronomía y cosmografía"; *id.*, "La astronomía".

[40] The structure of this part dedicated to hydraulics is similar to *Los veintiún libros de los ingenios y máquinas*, including sections on methods for finding water, kinds of waters, forms of transportation and means for carrying water, techniques and instruments for levelling, and arches and methods for making them. *Los veintiún libros de los ingenios y máquinas* was traditionally attributed to Juanelo Turriano and has been edited as such, but Garcia Tapia, *Ingeniería y arquitectura en el Renacimiento español*, pp. 158–159, showed that it is actually a work of Pedro Juan Lastanosa.

[41] Céspedes, *Libro de instrumentos*, fols. 43vff.

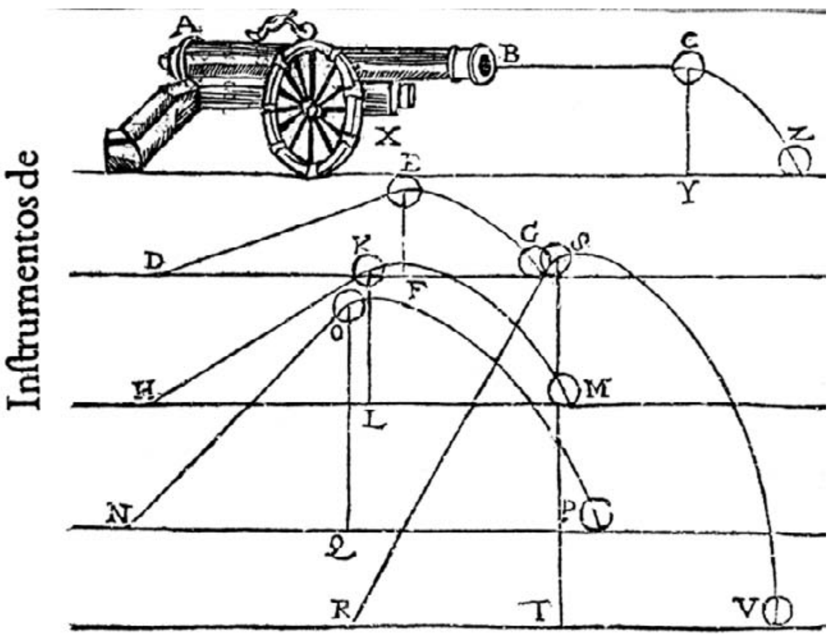
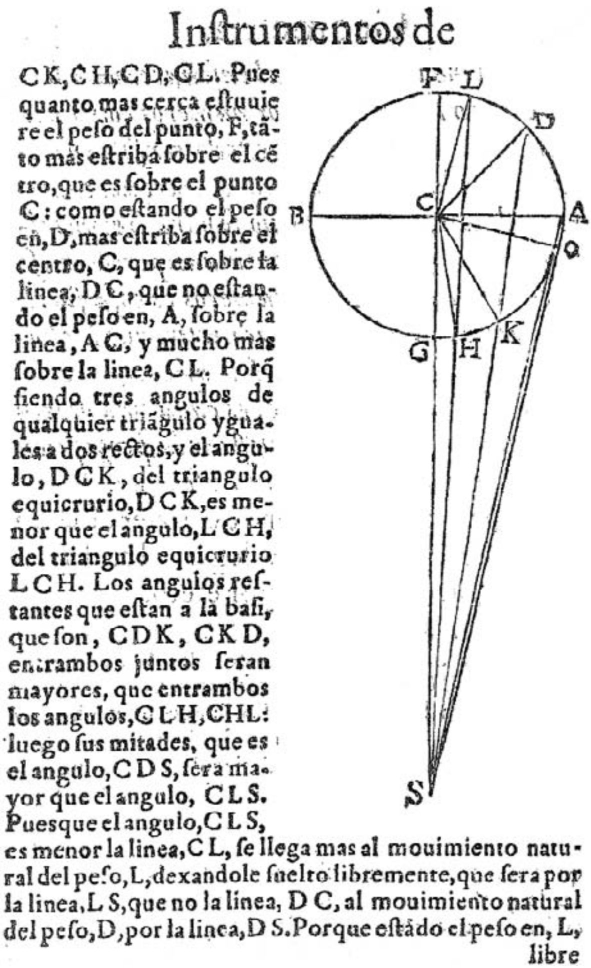


FIGURE 1. Trajectory of the projectiles according to A. García de Céspedes, *Libro de instrumentos de geometría* (Courtesy of the Instituto de Historia de la Ciencia y Documentación “López Piñero”, Valencia)

straight (see Fig. 1). He tries to compose the two impulses, one of the ball and the other of gravity, and to explain the power of the shot according to its angle of impact, he uses Guidobaldo’s *Mechanicorum liber*. First, in order to demonstrate that “the potential that throws or puts pressure on the angle closest to the right angle will be heaviest and have the greatest effect”, García de Céspedes reproduces literally Guidobaldo’s demonstration that the weight is heavier at the point tangent to the line drawn to the centre of the earth than at the extremity of the horizontal diameter of the balance (see Fig. 2).[42] Then, basing his argument on Guidobaldo (although without following Guidobaldo’s text literally) Céspedes expounds on the indifferent equilibrium of a balance of equal arms, which requires that the weights be fixed in the arms of the balance, because, in that case, the balance is not considered with two weights that rest upon the centre of the world, but as

[42] *Ibid.*, fols. 56r–58v.



Inſtrumentos de

CK, CH, CD, CL. Pues quanto mas cerca eſtuuiere el peſo del punto, F, tãto mas eſtriba ſobre el cẽtro, que es ſobre el punto C: como eſtando el peſo en, D, mas eſtriba ſobre el centro, C, que es ſobre la linea, DC, que no eſtando el peſo en, A, ſobre la linea, AC, y mucho mas ſobre la linea, CL. Porq̃ ſiendo tres angulos de qualquier triãgulo yguales a dos rectos, y el angulo, DCK, del triangulo equicrurio, DCK, es menor que el angulo, LCH, del triangulo equicrurio LCH. Los angulos reſtantes que eſtan a la baſi, que ſon, CDK, CKD, entrambos juntos ſeran mayores, que entrambos los angulos, CLH, CHL: luego ſus mitades, que es el angulo, CDS, ſera mayor que el angulo, CLS. Pueſque el angulo, CLS, es menor la linea, CL, ſe llega mas al mouimiento natural del peſo, L, dexandole ſuelto libremente, que ſera por la linea, LS, que no la linea, DC, al mouimiẽto natural del peſo, D, por la linea, DS. Porque eſtãdo el peſo en, L, libre

FIGURE 2. The properties of the weights in a balance according to A. García de Céspedes, *Libro de instrumentos de geometría* (Courtesy of the Instituto de Historia de la Ciencia y Documentatión "López Piñero", Valencia)

one body whose centre of gravity is a point that gravitates toward the centre of the world. On the other hand, if the weights are suspended freely on the balance, an indifferent equilibrium will not exist.[43]

[43] *Ibid.*, fols. 60r–62v. In the list of works of García de Céspedes included in the *Libro de instrumentos*, there is a *Libro de mechanicas donde se pone la razón de todas las machinas*, the manuscript of which I have been unable to find.

One of the most important Spanish contributions to mechanics was by a Herrera student, Juan Bautista Villalpando, whose contribution is contained in the treatise *In ezechielem explanationes et apparatus urbis ac templi hierosolymitami* published by Willalpando jointly with Jerónimo Prado in Rome in three volumes (1596–1604). The work was dedicated in part to recreating the design of Solomon's Temple.[44] Born in Cordoba in 1522, Villalpando studied mathematics and architecture under the "learned teaching of Juan de Herrera", with whom he shared the same idea of architecture.[45] As we have seen above, according to Herrera, architecture "presupposes something of all the arts and sciences, particularly geometry, arithmetic, perspective, music, astrology, gnomonics and mechanics". Villalpando repeated these ideas in several places in his work, although he stressed in particular the importance of mathematics for its certainty, nobility, and utility: "There is no art in the Republic, nor any activity of craftsmen nor any work of farmers, that is not based upon mathematics … . Mathematics opens the way, stimulates the genius and illuminates the minds of those students of almost all the most notable arts and disciplines".[46] In writing this defence of mathematics, Villalpando was in all likelihood influenced by Clavius and the mathematicians of the Collegio Romano, with whom he was acquainted during his stay in Rome.

The text on mechanics referred to here concerns "the centre of gravity and the line of direction", in which Villalpando establishes 21 propositions, including a proposition concerning "the polygon (or area) of support": "A heavy body that rests on the ground and covers a certain area remains in equilibrium when the vertical line drawn through the centre of this area passes through the centre of gravity; or, otherwise, when a vertical line drawn through the edge of this area passes through the centre of gravity or leaves it on the same side of the area, the heavy body will necessary fall".[47]

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[44] Volume II of *In ezecheliem explanationes* has recently been published in Ramírez et al., *El templo de Salomón* with Spanish translation and the engravings, dedications, and salutations of the other two volumes of Prado's and Villalpando's work, and with several studies by Juan Antonio Ramírez et al.

[45] For Villalpando's biographical data, see the Ramírez et al., *El templo de Salomón,* vol. III, p. 345 ("Apéndice Documental n.1"). See also Taylor, *Arquitectura y Magia*, pp. 52–53, which is a Spanish version with some additions of *id*., "Architecture and magic".

[46] "Nulla est enim Republica ars, nulla artificiorum exercitatio, nulla agricolarum opera, quae non aliquo Mathematici adminiculo fulciatur…et studiosis omnibus ad omnia paene optimarum artium ac disciplinarum studia viam aperit, ingenia exacuit, mentes illustrat" (Prado and Villalpando, *In Ezechielem*, vol. III, Pars II, Proemium, p. 250).

[47] Prado and Villalpando, *In Ezechielem*, vol. III, Pars II, Book I, ch. VI, pp. 319–328. Proposition VII, on the polygon of support, is on p. 322; for an English translation, see Dugas, *A History of Mechanics*, p. 102.

In relation to architectural structures, Villalpando's statics were more orientated to equilibrium conditions in the Archimedian tradition than to simple machines. As Ugo Baldini had recently remarked, the equilibrium conditions established by Villalpando became *loci classici*.[48] According to Pierre Duhem, Villalpando was inspired, directly or indirectly, to establish those propositions by Leonardo's studies of the ambulation of living beings and the flight of birds.[49] In fact, the Jesuit architect does not quote any author, except in the first part of the chapter, which he devotes to definitions of the centre of gravity and the line of direction and refers to Aristotle, Pappus, and Commandino. Apparently Villalpando was primarily interested in conditions of static equilibrium as they applied to architecture.

DIEGO DE ALAVA: BALLISTICS AND MATHEMATICS

As we have seen, one of the principal matters proposed by Herrera and studied in the Academy, at least during the period 1595–1600, when Julian Ferrufino held the chair of mathematics, was artillery.[50] Firrufino's book on the subject was dedicated principally to artillery pieces and to gunpowder. I have mentioned before the interest in ballistics of the successor of Firrufino, García de Céspedes, although apparently this author had not yet taught artillery in his classes, having established in 1605 a separate chair of mathematics, which was held by Julián Firrufino's son, Julio César Firrufino.[51] Nevertheless, Céspedes's successor in the chair of mathematics and cosmography, Juan Cedillo Díaz, maintained his interest in ballistics, as is proved by his Spanish translation of Tartaglia's *Nova Scientia*, which is preserved in a manuscript.[52]

One of the themes that emerges in the works on artillery written by engineers, military figures, and mathematicians is that of the relationship between the study of the motion of projectiles and the construction of

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48 Baldini, "Animal Motion Before Borelli".

49 Duhem, *Les origines de la statique*, vol. II, pp. 115ff.; *id.*, *Études sur Léonard de Vinci*, vol. I, pp. 53–85.

50 With the transfer of the Court to Valladolid, the activities of the Chair of Madrid apparently ceased. The then incumbent of the Chair, Julian Ferrufino, died in 1604. The Chair resumed its activities in the 1607–1608 course, being occupied by Andrés García of Céspedes. See Vicente and Esteban, *Aspectos*, pp. 143ff.

51 The chair of fortification and artillery that Julio Cesar Firrufino held from its foundation in 1605 was in fact a creation of the Council of War. See Vicenti and Esteban, *Aspectos*, pp. 173ff.

52 Preserved in the National Library of Madrid, Ms. 9092, with the title "Tratado primero de artillería".

the modern science of motion. Some authors, such as Rupert Hall, have fervently denied any relation or influence between the two, while others have defended the influence in various ways.[53] Likewise, historians have begun to analyze these works with respect to their functions, objectives, and contexts. The list of works on military art and artillery containing the study of the trajectory of projectiles includes texts by various Spanish authors, the most outstanding of whom, besides García de Céspedes, are Luis Collado, an engineer in the "Royal Army of Lombardy and Piedmont" and author of the *Platica manuale de artilleria* (1586), Diego de Alava, a lawyer and author of *El Perfecto Capitán, instruido en la disciplina Militar y nueva ciencia de la Artillería* (1590), and Diego Ufano, a military engineer based in the Low Countries and author of a *Tratado de la artillería y uso della* (1613). It is not my intention to analyse these works individually but instead to comment on certain aspects of the book and personality of Diego de Alava, whose work on artillery, which he dedicated the work to the memory of his father, has been recently examined by Gerhard Arend.[54] As Arend explains, Alava based his work mainly on Tartaglia. Nevertheless, Alava criticizes the Italian in several places, particularly in connection with the shape of the trajectory, which for Alava is never circular, but moves obliquely in a very gradual way from the very beginning of its movement (Alava fails to recognize that Tartaglia himself, in the *Nova Scientia* and in the *Quesiti*, had considered this fact). To evaluate the range of the shots, Alava proposes replacing the proportionality between the range and the angle of elevation (suggested by Tartaglia) by a relation deduced from the theory of the balance: for Alava the range would be proportional to the sine of the angles of elevation.

In the letter to his father, Alava relates that he was able to write his book thanks to the sciences that he learned at the University of Salamanca. The book contains various references to his Salamanca mathematics professor, Jerónimo Muñoz, who according to Alava had carried out a series of experiments to contrast Tartaglia's doctrines. Muñoz, professor of Hebrew and mathematics and one of the most distinguished Spanish scientists of the 16th century, published important works on astronomy, cosmology, geography, cartography, and mathematics. He taught in Salamanca arithmetic, geometry,

[53] Hall, *Ballistics in the 17th century*, p. 161: "The practice of artillery contributed nothing to seventeenth-century science". For a different approach, see the interesting suggestions of P. Thuillier, "Del arte a la ciencia", about the relations science-art in the discovery of the parabolic trajectory. See also Büttner et al., "The Challenging Images of Artillery"; Büttner et al., "Traces of an Invisible Giant"; Arend, *Die Mechanik des Niccolò Trataglia*.

[54] See Arend, *Die Mechanik des Niccolò Trataglia*, pp. 265–296, on Alava, Collado and Ufano's ballistics. For biographical data of Alava, Collado and Ufano, see the entries in *Diccionario histórico de la ciencia moderna en España*, pp. 31–32, 236–237, 381–382.

trigonometry, perspective, land surveying, astronomy, astrology, the use of instruments, geography, cartography, the art of navigation, and probably military arts (artillery and fortification). The last two subjects were included in the Statutes of 1594 of the University of Salamanca, which have a retrospective character, as I have explained elsewhere, and were elaborated by Muñoz's students and successors strictly in line with his teaching.[55]

For Alava, artillery was fundamentally based on geometry, to which it owed its origin: "Ella (la geometría) le produjo … haciéndonos llano su trato, y muy puestos en arte y razón sus efectos" (Geometry produces artillery, facilitating its study and grounding its effects in art and reason).[56] Thus he expressed his opinion in the opening chapter of the work, dedicated to "the admirable effects of geometry". He uses Plato to affirm that geometry is "the most excellent discipline because it loves and pursues the certain and true and repudiates the false and apparent". Alava contrasts the certainty of mathematics with the variety of opinions in the other disciplines (such as philosophy).[57] This assessment of mathematics and its superiority with respect to the other disciplines is in exact agreement with his master Jeronimo Muñoz. As a mathematician, Muñoz felt himself perfectly entitled to discuss questions of natural philosophy, especially in cosmology and astronomy. For example, in his discussion about the nature of comets, Muñoz said that Aristotle did not understand Democritus' opinion because he was not an astronomer, nor did he possess the theoretical basis (from geometry and arithmetic) to understand it.[58]

CONCLUSION

Mechanics, or the theory of machines, was cultivated in Spain by humanists, engineers, cosmographers, and mathematicians and had a fundamentally practical orientation, although its cultivators also extolled the nobility of the discipline by virtue of its close relation to and dependence on mathematics. In Spain, mechanics found an institutional setting exceptional in Europe at the time in the Academy of Mathematics, thanks to Juan de Herrera's initiative. Outside of the academy, as far we know, neither the pseudo-Aristotelian *Mechanica* nor any other work of mechanics was included in the curriculum of any Spanish university of the period, whether in the teaching of

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[55] On Muñoz, see Navarro-Brotons and Rodríguez, *Matemáticas, cosmología y humanismo en la España del siglo XVI*, pp. 16ff.; Muñoz, *Introducción a la astronomía y la geografía.*

[56] Alava, *El perfeto Capitán*, fol. XIIIv.

[57] Alava, *El perfeto Capitán*, fols XIIr–XIIIv.

[58] Muñoz, *Libro del nuevo cometa,* p. A3r.

natural philosophy or the mathematical disciplines.[59] The only exception was artillery, which was taught in connection with the mathematical disciplines in the University of Salamanca, and in several artillery schools.[60] This separation between mechanics and scholastic natural philosophy prevented the amalgamation of the science of machines with the study of motion, which scholastic natural philosophers tended to explain in their commentaries on Aristotle's theory of motion or in their investigations *de intensiones et remissione formarum*. And it was precisely this kind of amalgamation that would become the basis of Galileo's research program.[61]

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[59] See Navarro-Brotons, "El Renacimiento científico y la enseñanza", pp. 216ff. In Portugal, on the contrary, the *Mechanics* was used by Pedro Nuñes, educated in Salamanca, since the 1540s in his teaching in the University of Coimbra; see Leitâo, *O Comentario de Pedro Nunes à navegaçao a remos*, pp. 40ff.

[60] See García Tapia and Vicente Maroto, "Las escuelas de artillería".

[61] The disciplinary divide between mechanics as an *ars* or *scientia media* and *natural philosophy*, or "science of natural bodies, in so far as the are natural", was a distinctive European concept. Treatises on mechanics were all written by mathematicians, not by "scholastic" advocates of *physica peripatetica*. Galileo, as much a natural philosopher as author of *Le mechaniche*, succeeded in bridging the disciplinary gap. But even Galileo failed to construct a mechanical foundation for his theory of movement of falling bodies in a consistent form. Moreover, concerning the question of the mechanics and the Scientific Revolution, A. Gabbey, in "The case of mechanics", has suggested speaking not of a single revolution in mechanics, but perhaps of several revolutions or "mini-revolutions" in collision theory, in statics, and the theory of machines, in hydrodynamics, in vibration theory, and in the theories of central forces and of rigid-body motion, each with its own new principles and procedures.

GEERT VANPAEMEL[1]

MECHANICS AND MECHANICAL PHILOSOPHY IN SOME JESUIT MATHEMATICAL TEXTBOOKS OF THE EARLY 17TH CENTURY

The establishment of mechanical philosophy in the 17th century was a slow and complex process, profoundly upsetting the traditional boundaries of knowledge. It encompassed changing views on the scope and nature of natural philosophy, an appreciation of "vulgar" mechanical knowledge and skills, and the gradual replacement of a causal physics with mathematical explanations. The revolutionary steps taken by Galileo and Descartes in creating mechanical philosophy were foreshadowed by a long, ongoing debate on these issues, in which Jesuit mathematicians played an important part. In this paper I shall examine some mathematical treatises by Jesuit authors in the first half of the 17th century, in particular the *Disciplinae mathematicae* by the Flemish Jesuit Joannes Ciermans. These authors blurred the traditional distinctions between philosophy and mathematics, even giving mechanics (or statics) pride of place in dealing with natural phenomena. It will be become apparent that a prudent mechanical way of thinking and even a corpuscular notion of matter was not absent from contemporary Jesuit mathematics.

The Jesuit Order holds a special place in the history of modern science. Taking education as one of its main objectives, the Society of Jesus formed the first international network of scientific institutions, with schools and research centres dispersed all over Europe. These Jesuit institutions were particularly important in introducing mathematical disciplines within the regular philosophical program of education. Whereas in most universities, mathematics was a marginal discipline with only a very limited number of students, the Jesuit curriculum included a mathematics course during the second year of its three-year program. This arrangement stimulated debates on the status of mathematics and related disciplines in relation to philosophy. This arrangement was not without criticism, even within the Society itself. For this reason, particularly during the first half of the 17th century, Jesuit mathematicians had to defend their position, writing on the value of mathematics and its meaning for philosophy and theology.

The centre of Jesuit mathematics was at the Collegio Romano in Rome, where the German Jesuit Christopher Clavius (1538–1612) formed a core

[1] Katholieke Universiteit Leuven. I gratefully acknowledge the assistance of my colleague Jeanine De Landtsheer in helping me with the translation of the Latin quotations.

W.R. Laird and S. Roux (eds.), Mechanics and Natural Philosophy before the Scientific Revolution, 259–274.

group of young mathematicians, fully convinced of his lofty view of mathematics.[2] But mathematical debates were not restricted to Rome. In fact, it was primarily after Clavius's death, with the foundation of mathematical chairs in various countries, that there arose a more explicit discussion on the proper place of mathematics.[3] In this paper, I shall focus on the southern Netherlands, where the Jesuit Order was very successful and its membership growing at a quick pace. The Counter-Reformation, fully supported by the Spanish rulers, offered the ideal background for the Jesuits to infiltrate local elite circles. They were, however, banned from university teaching, as the University of Leuven boldly managed to defend its monopoly. The teaching of mathematics, which was only taught marginally in the Leuven curriculum and which was not perceived as part of the legal monopoly of the university, offered an opportunity for the Jesuits to organize public courses and to attract students. They did so, first in the commercial metropolis of Antwerp, but soon also in Leuven itself, directly challenging the university privilege. It was like dancing on a hot tin roof: at once trying to outdo the university by offering a "modern" practical course on all mathematical disciplines, and at the same time maintaining a "traditional" academic profile. In this context it is not surprising that the Flemish Jesuits very soon diverged from Clavius's views, presenting a very different view of mathematics from their Roman predecessor. One aspect of this shift was the greater importance attached to mechanics and to the place of mechanics within the general structure of the mathematical sciences.

THE ANTWERP COURSE OF MATHEMATICS

In the fall of 1617, after several years of negotiations and abortive attempts, the Jesuits of the Flandro-Belgian province finally inaugurated their course of mathematics in the city of Antwerp. For about half a century afterwards, a number of Jesuit professors taught mathematics in Antwerp and Leuven, holding public disputations and sometimes publishing scholarly theses. The history of the course is still quite obscure; we know that the course covered an unusually wide range of topics and that it was open to the public, but we know very little regarding the recruitment of students, the institutional aspects of the course, or even the precise content of the lectures.[4]

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[2] Lattis, *Between Copernicus and Galileo*.

[3] A particularly important example, setting the tone for later texts, is by Clavius's student, Giuseppe Biancani's *De mathematicarum natura dissertatio* (Bologna, 1615). An English translation is available in Mancosu, *Philosophy of Mathematics*, pp. 178–212.

[4] Van de Vyver, "L'école de mathématiques"; Vanpaemel, "Jesuit Science in the Spanish Netherlands". A finer analysis of the Jesuit mathematics course is currently being carried out by Angelo De Bruycker at Leuven.

Nevertheless, that the course was at a high level is beyond doubt. For under the guidance of the first professor, Gregorius a Sancto Vincento (1584–1667), a pupil of Christopher Clavius in Rome, the school formed an impressive number of able Jesuit mathematicians, many of whom later taught mathematics at other Jesuit colleges, such as Jean Charles de la Faille (1597–1652) in Madrid and Theodorus Moretus (1602–1667) in Prague. Another student at Antwerp, Andreas Tacquet (1612–1660), was later to become the author of the standard Jesuit textbook on geometry and arithmetic.

The foundation of the Antwerp school was part of a more general wave of creation of mathematical chairs in Jesuit colleges all over Europe. Steven Harris's count of mathematical chairs shows that between 1620 and 1640 the number of chairs doubled, reaching a total of about 50 around the middle of the century.[5] Most of these chairs were integrated into a Jesuit university, but not so in Antwerp. The Jesuits of the Flandro-Belgian province were unsuccessful in their attempts to gain access to the University of Leuven, which held the monopoly of higher education in the Spanish Netherlands.[6] Under these circumstances, the public lectures on mathematics constituted their only access to public, university-level teaching. The school was transferred several times to Leuven, possibly in an attempt to attract students from the university and to gain some influence in university circles, but the official relations with the university were never very cordial.

From the few surviving printed theses to be publicly defended by students, we can reconstruct a rough outline of what was taught. The subjects included all mathematical disciplines, including geometry, arithmetic, optics, dioptrics, music, astronomy, architecture, mechanics, hydrostatics, and the military arts. The most comprehensive exposition of the course content, running over 200 pages, was published in 1640 by Joannes Ciermans (1602–1648) under the title *Disciplinae mathematicae*.[7] It consisted of twelve chapters, one for each month of the year, subdivided into weekly programs. The title page suggests that these chapters represent the actual lectures taught during the year. This is rather unlikely, since it would require a schedule of daily lectures of one or two hours. As parts of the text had already been published as separate theses in previous years, the *Disciplinae* could be seen as a compilation of mathematical topics, taught over the course of several years, either treated in a pedagogical order, or adapted to

[5] Harris, "Les chaires de mathématiques".
[6] On the scientific education at Leuven university, see Vanpaemel, *Echo's van een wetenschappelijke revolutie*.
[7] O. Van de Vyver, "Jan Ciermans (Pascasio Cosmander) 1602–1648. Wiskundige en vestingbouwer".

the needs of the students willing to take the course. Although the *Disciplinae* is neither a textbook nor a fully elaborated set of arguments, it is a rare and very revealing source for what was actually taught.

The Jesuit school of mathematics was from its origin very much immersed in the tradition of Clavius. Gregorius a Sancto Vincento appears to have followed Clavius in his arrangement of the mathematics course, though with a rather more limited number of topics and with less sophistication to suit the local students. Clavius considered mathematics an essential and fundamental part of all true philosophy, both for its intellectual content as well as for its pedagogical approach to the problem of certainty.[8] Gregorius's research interests did not exceed pure mathematics. For much of his life, he struggled with finding a solution to the squaring of the circle, developing novel methods for calculating the area under various curves.[9] Among his extant manuscripts, only a handful of pages discuss non-mathematical topics, although some of the theses published under his supervision dealt in fact with astronomy and mechanical problems.[10]

DE LA FAILLE AND THE ARCHIMEDEAN TRADITION

The mechanics of these early years of the Antwerp school is well represented by Gregorius's first successor, Jean Charles de la Faille. After his studies in Antwerp, he was sent to the College of Dole to teach mathematics. He briefly returned to Antwerp before being sent to the Imperial College of Madrid. In 1632 he published – with the Antwerp printer Joannes Meursius – a booklet of mathematical demonstrations, *De centro gravitatis partium circuli et ellipsis*.[11] Here de la Faille took his inspiration from Archimedes, who had, by making use of the centre of gravity of a parabolic segment, solved the quadrature of the segment. If the centre of a segment of the circle could be found, de la Faille hoped to solve the quadrature of the circle. This was a long-standing problem with the Flemish Jesuits, on which Gregorius and several of his students would work for many years. De la Faille had apparently discussed these topics with his students in Dole and had stated

[8] On the mathematical views and teaching of Clavius, see Baldini, "The Academy of Mathematics of the Collegio Romano"; Romano, *La Contre-Réforme mathématique*, in particular chapters I–III.
[9] On the mathematical work of Gregory, see Hofmann, *Das* Opus geometricum *des G. a Sancto Vincentio.*
[10] Van Looy, "Chronologie et analyse des manuscripts mathématiques".
[11] Bosmans, "Le traité *De centro gravitatis*".

some of his results in some *Theses mechanicae*, probably published in or around 1624 but which have not been found.

De centro gravitatis is a rigorous mathematical work in the Archimedean tradition. De la Faille scorned those authors on the quadrature of the circle who tried to solve this problem with the aid of instruments. He wrote: "Mathematici multi sciunt, Mathesim pauci" (Many are the mathematicians who know [something], but few have real understanding).

There was a big difference between, on the one hand, knowing some elementary propositions and using them in a rather loose way, and on the other, truly understanding the nature of the science, penetrating into its deeper secrets, and deducing and demonstrating a great number of theorems and problems from universal principles. De la Faille likened the vulgar mathematician to a painter who may have acquired a great skill but not a true understanding of his art, which only the science of optics can deliver.

For de la Faille, as for Gregorius and other contemporary Flemish Jesuit mathematicians of his school, mathematics was a theoretical science, based on the books of the classical Greek authors such as Euclid, Pappus, Appolonius, and Archimedes. Their interest in mechanical problems was limited to its mathematical aspects. Among the many extant papers of Gregorius, only a few pages deal with a non-mathematical topic, the theory of the tides. In 1624 Gregorius published a set of mechanical theses, which were publicly defended by Joannes Ciermans. But also in these theses, the emphasis was on the mathematical theorems, not on their applications.

SEMPILIUS BETWEEN PURE MATHEMATICS AND MECHANICAL APPLICATIONS

This predilection for theoretical mathematics probably reflects the difficult position of the Jesuit mathematicians in the Collegio Romano under Clavius. The debate on the position and status of mathematics and its relationship to natural philosophy was a very delicate subject. Clavius had argued successfully for the inclusion of mathematics in the *Ratio studiorum*, but at a certain price. If mathematics was included as a separate course in the Jesuit curriculum, it was not to interfere with natural philosophy. Mathematics was distrusted because it seemed to impinge on natural philosophy by studying natural things. Moreover, mathematical demonstrations were deemed inferior to physical demonstrations because they lacked a real middle term. The properties invoked in the mathematical demonstrations were merely accidental, not essential to the objects considered. The leading Jesuit philosopher Benito Pereyra argued forcefully in 1562 that the "mathematical disciplines are not proper sciences". The Jesuits of Coimbra agreed in their widely disseminated *Commentaries*.

In the 17th century many of Clavius's students would write elaborate treatises to defend the status of mathematics, while at the same time delineating its boundaries and recognizing its special status alongside but not a part of natural philosophy. This does not exclude an interest in practical mathematics, but it puts more emphasis on the epistemological values of the mathematical approach than on the practical results obtained.

A good example of this position is given by yet another volume published a few years later in Antwerp. In 1635 a large volume *De mathematicis disciplinis* was produced by the prestigious Plantin Press and dedicated to the Spanish king Philips IV. Its author, Hugo Sempilius, was a Scottish Jesuit, born in Craifever in 1596. Not much is known about him. He joined the Jesuit Society in Toledo in 1615 and later became rector of the Scottish Seminary in Madrid. He also became professor of mathematics in the Reales Estudios of the Imperial College, and hence a colleague of de la Faille.[12]

De mathematicis disciplinis is a rather peculiar book. It is neither a textbook, nor an original account of discoveries made by the author. It contains in fact very little mathematics in the ordinary sense; there are no theorems or demonstrations, no worked out examples or mathematical problems, not a single illustration or mathematical diagram. Its purpose was rather to offer the reader a first glimpse of mathematics, "a rough sketch", to inform him which books to read, which authors to consult, and how the study of mathematics could be useful in very different circumstances. For those wishing to learn mathematics without a teacher, it presented a method and an order by which to tackle the different disciplines. Sempilius avoided difficult phrases and a fancy rhetorical style, because "true science pleases by its innate beauty". He quoted Aristotle in pointing out that "a wrestler should go naked into the arena".[13]

In spite of his prestigious position in Madrid, Sempilius was not an original author. The first chapter "on the dignity of mathematics", for example, is largely based on *De mathematicarum natura* of Giuseppe Biancani (1566–1624), a Jesuit professor of mathematics at the University of Parma. Sempilius follows Biancani in regarding mathematics as a true science, with its own subject matter, the "limited quantity". Demonstrations in pure

[12] Almost everything known about Sempilius can be found in Navarro, "Tradition and Scientific Change in Early Modern Spain".

[13] Sempilius, *De mathematicis disciplinis*, "Lectori S.".

mathematics proceed from formal and material cause, thus making it a true science in the Aristotelian sense.

Sempilius was not, however, a mathematician like de la Faille and hence his view of mathematics was not so strictly confined to "pure" mathematics. His knowledge of mathematics was self-acquired and it is difficult to assess from his book just how good his mathematical skills were. At the end of his book, he gives an impressive list of mathematical authors in very different disciplines, but these are meant as general reading lists for students, not as actual bibliographical references to the material discussed in the book. The scope of mathematics he proposes is much broader than de la Faille's. Sempilius distinguished eleven different disciplines: apart from geometry and arithmetic, he discusses optics, mechanics, music, cosmography, geography, hydrography and meteorology, astronomy, astrology, and chronology. Sempilius gives no reason why these particular disciplines belong to mathematics or should be treated in this particular order. When explaining the nature of mixed mathematics, he also mentions architecture and tactics, topics not included in his original list.[14]

One can easily recognize the apologetic character of Sempilius's argument. The book is not intended for his fellow mathematicians, but rather is directed towards those philosophers who look down on mathematics as unscientific. Sempilius makes an effort to counter their arguments, which are very varied. The dignity of mathematics is, according to Sempilius, supported by many different authorities, and he mentions first of all the Bible. In the book of Wisdom, King Salomon is grateful to the Lord for sending to him the spirit of Wisdom, which is more important to him than his sceptre and throne, and added a detailed description of what this knowledge was:

> For he gave me sound knowledge of existing things, that I might know the organization of the universe and the force of its elements, The beginning and the end and the midpoint of times, the changes in the Sun's course and the variations of the seasons.
>
> Cycles of years, positions of the stars, natures of animals, tempers of beasts, powers of the winds and thoughts of men, uses of plants and virtues of roots.

Sempilius uses all his ingenuity to recognize in these verses the various disciplines of mathematics (such as astronomy, astrology, chronology, mechanics and hydrology) along with topics belonging to natural philosophy

[14] *Ibid.*, p. 11.

or medicine. Mathematics is therefore a true science, because it is nothing less than a divine present of wisdom, for which Solomon was very grateful.[15]

Yet, as Sempilius emphasizes throughout the book, mathematics is neither to be denied its useful applications nor to be disdained for having them. In particular mechanics, which Sempilius usually calls *statica*, is to be praised for its many practical uses. Nowhere is art more similar to nature and nowhere more capable of overcoming nature's power than in the science of statics. It comprises all human industry to contribute to the splendour of the State, to augment the power of the arts and to be of assistance in times of war or peace, on land or at sea. It is the product of both astronomy and geometry and in its turn nurtures physics and develops arithmetic. It is of use to smiths, architects, bearers of loads, farmers, seamen, painters and many others. Sempilius makes a long digression to enumerate all the different uses of mechanics in natural phenomena (it is the reason why the Sun keeps its place in the world) and technical achievements.[16]

Sempilius divides mechanics into a number of separate disciplines. The first division is the science of equilibrium, which predicts how much force will be needed to lift a certain body to a certain height by a perpendicular or oblique path. It also explains why bodies of different gravities fall with different velocity towards the centre of the earth. The second division is the study of the centres of gravity; Sempilius explicitly mentions the centre of gravity of a segment of a circle as calculated by his colleague de la Faille. Although these two approaches represent the basic principles of mechanics, Sempilius makes no attempt to state or to explain these principles, or to show how they are used in the other mechanical disciplines, which are more concerned with the study of mechanical devices or tools. These other disciplines have colourful names: *zygostatica*, *mochlostatica*, *trochleostatica*, *axis in peritrochio*, *sphenostatica*, *cochlostatica*, *spartostatica*, *hydrostatica*, *aërostatica*, *pyrotechnica*, *automata*, *polymechanostatica* and *poliorcetica*. In discussing these different disciplines, Sempilius only gives a catalogue of results to be found in the books of famous authors such as Archimedes, Vitruvius, Guidobaldo dal Monte, and Simon Stevin. The order is increasingly directed towards the more spectacular applications, which appear to be for Sempilius the main point of attraction in the whole science of mechanics. When talking about automata, for instance, he describes the use of mechanical devices in staging a play in the theatre. Such a play with

15 *Ibid.*, pp. 2–5.
16 *Ibid.*, Liber quintus, "De statica", pp. 87–101.

moving statues was produced with great success on stage at the beginning of 1629 in the Imperial College in Madrid in the presence of the King and many of his courtiers.[17] The final chapters are devoted to military applications, among which Sempilius also included a short digression on ballistics and modern artillery.

The basic concepts of mechanics as considered by de la Faille and Sempilius have not much in common. Both indeed refer to Archimedes as their main source, but it is clearly a very different Archimedes. Sempilius was more impressed by the medieval picture of Archimedes, the ingenious inventor of clever devices, the artificer and military engineer.[18] De la Faille admired the mathematical purity of Archimedes's mechanics, the possibility to restructure all mathematical disciplines and even some technical arts in a geometrical form.

Although Della Faille's Archimedes was quite akin to the Archimedes of Galileo and other Italian authors on mechanics, suggesting a similar path towards the renewal of mechanics, the more superficial enthusiasm for the technical aspects of mechanics as exemplified by Sempilius became more and more important within Jesuit circles. In later decades it would become the hallmark of Jesuit mechanics, as testified particularly by Athanasius Kircher and Gaspar Schott. An example of this development is to be found in yet another pupil of the Leuven Jesuit school of mathematics, Ferdinand Verbiest (1623–1688), who as a missionary to the Chinese emperor became very influential by his shrewd use of European science to impress the Chinese scholars. In his *Astronomia europaea* (1687), he describes not only his success as an astronomer but also the importance of the other mathematical disciplines that had been of use to him.[19] His book was aiming to stimulate future missionaries to study mathematics with extra care, since it would be very helpful to open the doors of the imperial Court. In talking of mechanics, he relates how he explained some fundamental principles of mechanics to impress Chinese scientists, who otherwise would consider mechanics purely a technical art. But he then goes on to describe the many wonders he has produced for the Emperor. The production of spectacular performances was to remain an important part of Jesuit mechanics.

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[17] *Ibid*., p. 95.

[18] The different renaissance perceptions of Archimedes are discussed in Laird, "Archimedes among the Humanists".

[19] Golvers and Libbrecht, *Astronoom van de Keizer*.

CIERMANS AND CORPUSCULAR MECHANICS

Now let us return to Ciermans. Ciermans was himself a former student of the mathematics course. In 1621, at the age of nineteen, he was one of the students of Gregory a Sancto Vincento to take part in a public disputation on the mechanics of falling bodies along inclined planes. Little is known about his further mathematical education or activities, but his talents were mentioned in 1635 by de la Faille in a letter to the royal cosmographer Michel Florentius Van Langren.[20] In 1637 he was called back to Leuven to teach the mathematics course, which he continued to do until 1641. He then left for Lisbon, where he was planning to embark for India. But instead of leaving Europe, he was charged with teaching mathematics to the Portuguese prince Theodosius. Because of his military talents and his knowledge of fortification, he was enlisted in the Portuguese army, acting as chief engineer and taking part in military operations, which led to his official dismissal from the Society of Jesus.[21] When he was captured by the Spanish army, he worked for them in turn until he was finally killed in action in 1648.

Although Ciermans had been a student of Gregorius a Sancto Vincento, his own lectures bear little relation to the mathematical preoccupations of his former master. One year after Ciermans finished his mathematical studies, Gregorius went to Rome and then to Prague. After the sack of Prague in 1631 he returned to the Spanish Netherlands but set up residence in the College in Gent. On rare occasions he came to Leuven or Antwerp to attend the public defence of theses. In his absence, his successors could easily follow a different direction, focusing on practical mathematics rather than pure geometry. One of these successors, Guilielmus Hesius, is reported to have held corpuscular views, while Ciermans read Descartes's *Discours de la méthode* and commented briefly on the law of optical refraction and the nature of light. Ciermans's letter does not reveal him to be a mechanical philosopher, but it shows his profound interest in the mechanical analysis offered by Descartes. The general tone of the letter is admiring. Ciermans praises Descartes for having "discovered new lands". He sums up Descartes's method as "explaining all the most hidden things in nature only by things that can be seen or touched".[22] The *Disciplinae*, written only a few years after these events, shows no explicit signs of Cartesian physics, but it

[20] O. Van de Vyver, "Lettres de J. Ch. de la Faille".

[21] In the mean time, he had changed his name to Pascasio Cosmander, a Greek translation of the Dutch Ciermans. Pascasio may refer to the fact that he was born on Easter day.

[22] Letter from Ciermans to Descartes, [March 1638], in *Oeuvres de Descartes*, vol. II, pp. 55–62.

indicates how Ciermans in his own way was heading towards a mechanistic approach to natural philosophy.

The *Disciplinae mathematicae* of Ciermans was very much directed towards practical mathematics. One quarter of the book is devoted to military arts, a subject well chosen in a country that was still at war with its neighbours. An equal part concerned mechanics. Three chapters bore the heading "Statica", the first dealing with mechanical instruments and moving bodies, the second with hydrostatics, and the third with hydraulics, nautical arts, and navigation using a magnet. Mechanics was for Ciermans the foundation of every art. It created equilibrium and rest among all the bodies that by their nature strive up or downwards. In several other chapters, Ciermans referred to mechanics as the place where an explanation could be found.[23]

Ciermans's exposition of the basic mechanical machines holds no particular interest. He follows the traditional mechanical treatises inspired by pseudo-Aristotle and Archimedes. His treatment of problems is superficial, aimed at practical applications and spectacular paradoxes. Further, the third part concerning the nautical arts, has little bearing on the scientific principles of mechanics. It is interesting to note that Ciermans refers to Gilbert's *De magnete*, an experimental approach to natural philosophy that he apparently values very highly. So far, Ciermans's lectures can be viewed in accordance with Sempilius and the predilection of Jesuit mathematicians for practical applications.

But there is more. With regard to hydrostatics, Ciermans makes some very crucial statements that reveal a subtle theoretical shift towards a new kind of mechanics. The main part of hydrostatics is as usual devoted to machines (how to make a good water level? How do aqueducts transport water? etc.) or simple experiments based on the equilibrium between fluids or the immersion of solid bodies in fluids. Yet, while discussing the causes of hydrostatical equilibrium (in itself a quite unusual – even forbidden – topic for a mathematical course), Ciermans makes an interesting comparison between the mechanics of machines and the mechanics of fluids. Fluids behave exactly like machines, but they do so without the presence of any instruments such as levers or pulleys. Everything here is performed by nature. But does this mean that nature must also be mechanical? Ciermans is not entirely clear on this point. He appears not to be merely making an analogy between nature and art, but rather seems

[23] For example, Ciermans, *Disciplinae mathematicae*, discusses the immobility of the earth under Geography (first week of July), and magnetism under Nautics (third week of March). (There are no page numbers; rather, Ciermans orders his theses according to the months of the year, each month having three weeks.

to imply that the laws of statics are in fact extended to nature and that a true understanding of nature can best be attained through the study of the proper laws of statics. The repeated emphasis on the statical laws of liquids is clearly intended to highlight the relevance of statics to the study of water:[24]

> Thus far, while we have been teaching from the laws of statics how to lift weights with the aid of instruments, we have dismissed any consideration of the fluids in which they were moving. We did this because first it is easier for our understanding and for our handling the phenomena to have the things separated instead of combined, secondly because statics paves the way for hydrostatics. For what is effected by instruments in the former, is in hydrostatics performed by nature, and only by her. Hence the most exact balances and levers are produced by her without the use of any beams or yokes.[25]

Is this a metaphor for the sake of rhetoric, or is it a genuine philosophical statement on the nature of fluids? It is difficult to judge. The literary style of the *Disciplinae mathematicae*, bringing together short unexplained statements to be discussed in public, does not allow for a thorough philosophical discussion. The argument is repeated in another thesis, regarding the weight of a fluid in its natural place:

> Those who presume that water has no weight when it is in its natural place, as they say, because they cannot feel any at all when they move [through] the water, are erring; they do not understand that nature establishes equilibrium between liquids; do not look for levers or beams, by which we have said that the weights are thus connected to each other, so that when one of them descends, the other necessarily must ascend: even without these [instruments] all liquids are in equilibrium

[24] That Ciermans was merely hinting at the more traditional Aristotelian view that nature imitates art cannot be completely excluded. His position would then be quite similar to Moletti's, as studied by Roy Laird in his contribution to this volume.

[25] "Hactenus dum pondera ex Staticae legibus per instrumenta elevare docuimus, tantisper humidi, in quo movebantur, considerationem seposuimus; tum, quod ingenio perinde, ac manu facilius res separatas, quam coniunctas capiamus: tum quod illa ad haec struant viam; quam enim ibi varijs instrumentis fiunt, hîc natura, se solâ, peragit, ideoque sine ullis, aut radijs, aut iugis, exactissimas profert libras, & vectes" (Ciermans, *Disciplinae mathematicae*, "Argumentum").

> by such a firm nexus that no part [of the liquid] can descend without the ascent of another part.[26]

Ciermans continues his philosophical remarks with an attack on the *horror vacui*, a philosophical concept that was still commonly used to explain the ascent of liquids. Although he concedes that the mathematician should not attempt to give a complete philosophical treatment of the problem, Ciermans points to quite conclusive observations against the arguments of the philosophers:

> With ropes and nails solid bodies are connected to each other, [but] fluids are connected through tubes or siphons with no smaller nexus: for as long as you exclude all ambient air (or any other fluid) you cannot separate one part of the water with a force however great. The philosophers said that this is so because of the *horror vacui*, but they did not sufficiently explain how this works. Say that nature abhors as much as possible the void; so what? Will it lift the water to avoid the vacuum? Does it come to the assistance of the one who attempts to lift the water? Through experiments we learn that water is always lifted with the same force in siphons as in any other recipient and that everywhere forces equal to the gravity of water are required: so what is left to be done by the *horror vacui* or by nature who tries to remove it? Unless there is such a strong connection between [the parts of the fluid], that they cannot be separated. The solids achieve this through the union of their parts; the same applies to fluids; what this union is, is of no importance to the Mathematician, to whom it is enough that they cannot be separated by any force, and that their weight can only be estimated by the gravity of the fluid.[27]

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[26] "Qui aquae, ut loquuntur, in loco naturali existenti pondus nullum inesse autumant: quod illud neutiquam sentiant dum aquam commovent: nae illi non intelligunt, quod natura inter liquida constituit aequilibrium; librarum tamen in his ne quaere iuga, aut radios, quibus ita Pondera inter sese copulari diximus, ut hoc dum descendit, alterum ascendere necesse sit: stant enim sine his, omnia liquida tam firmo inter se copulata nexu, ut pars nulla, sine alterius ascensu, descensum invenire queat" (Ciermans, *Disciplinae mathematicae*, Februarii Hebdomas Prima, "Humidi Aequilibrium").

[27] "Funibus, clavis iunguntur res solidae, tubis, siphonibus fluidae, neque nexu minori: quandiu enim his circumstantem excludis aërem (idem de omni humido intellige) vi nulla quantumvis magna partem unam aquae ab altera, divelles. Atque hoc periculo vacuo fieri dixerunt Philosophi, modum tamen, quo fiat, non satis explicarunt: ut enim natura quam maxime a vacuo abhorreat; quid deinde? num aquam vacuum prohibuturum sublevat? Num

Notwithstanding this concession of the limited scope of mathematical arguments, Ciermans does not stop there. In a further statement on bodies immersed in water he wonders how nature can establish equilibrium according to the laws of statics without any instruments. Ciermans does not explain fully his views, but he gives at least a hint, which points to a small scale force acting at a short distance between the parts (or corpuscles?) of the liquid:

> Liquids do not balance each other in such way that they do not allow solid bodies as well in their company, with this law however: that those bodies that are heavier fall to the bottom, whereas those that are less heavy tend to rise to the surface of the water. Even lead does not break this law: whenever it is immersed in mercury, it does not surface, as experience shows. Yet you should not say that the body that you see ascending through the liquid is expelled by that part of the water of which it occupies the place, but by the liquid that is around it; that [this liquid] has less weight and mass than the expelled body does not go against the law of equilibrium, because even all the most widely diffused parts of the water are armed [with the power] to propel the slightest weight upwards; similarly, both a small and a large quantity of water allow a body to be immersed.[28] Hence there is no reason why a diver should be more afraid of the weight of the ocean above him, than of the weight of [that part] that barely touches the top of his head: in fact, it can happen sometimes that he experiences a more difficult ascent through a small [quantity of] water, than through a large one.[29]

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subsidio venit illi, qui aquam attollere nititur? At experientâ magistrâ habemus eadem vi per siphones, ac per vas aliud quodcunque; aquam in altum efferri, & ubique vires gravitati aquae pares exigi: quid ergo periculo vacui, aut naturae illud amolienti agendum relinquitur? Nisi ut haec tamen firmo inter sese copulet [*sic*] nexu, ut divelli nequeant. Hoc solida à partium suarum habent unione; habeant & hoc à suâ fluida; quae quid sit, curae non est Mathematico, cui satis est, Nulla vi haec à se mutuo distrahi, nec eorum pondus aliter, quam penes humidi gravitatem aestimari" (Ciermans, *Disciplinae mathematicae*, Februarii Hebdomas Secunda, "Siphonum vis").

[28] Ciermans probably refers to the total mass of the liquid in which the body is immersed, for example a large lump of lead in a small recipient full of mercury. Obviously, according to our laws of hydrostatics, the specific gravity of the liquid should be larger than that of the body immersed.

[29] "Non ita inter sese trutinantur liquida, quin solida quoque corpora in societatem admittant, ea tamen lege, ut quae graviora sunt, fundum petant; quae leviora, ut sursum ex aquis

Interestingly Ciermans suggests how the study of fluids forms the transition between the science of machines, and the work of nature. Fluids act like machines (instruments) but they should not be equated with machines. Ciermans refrains from giving any mechanical explanation of how the union between the parts of a fluid is maintained, but he still defends the full applicability of mechanical laws to a truly natural phenomenon. As for Descartes, for Ciermans these mechanical laws have a much wider meaning than only an explanation of a well-circumscribed set of phenomena and instruments. Furthermore, he tries to locate the philosophical foundation of this mechanicism not in any abstract principle such as the *horror vacui*, but in the actual constitution of water, and in particular in some physical union of its parts.

It is difficult to give a precise meaning to Ciermans's sometimes-enigmatic statements. At least two things can be concluded with some degree of certainty. First of all, Ciermans was not to be trapped in any mathematician's cage. Notwithstanding his explicit refusal to speculate on the causal explanation of the nexus of fluids, Ciermans does feel free as a mathematician to inquire into causal explanations. Not surprisingly, this would be most apparent in such disciplines as mechanics and optics, where mathematical explanations replaced the more vague notions of natural philosophy, where speculative explanations could be more easily proposed and maintained. Mechanics became therefore (together with optics, and to some extent also astronomy) the primary paradigm for mathematical explanations of natural phenomena.

It cannot be inferred, however, that Ciermans defended a truly mechanistic worldview. He merely notes the astonishing similarity between mechanics and nature; he does not conclude from this that some mechanical model lies at the heart of natural phenomena, or that natural bodies should be conceived as composed of corpuscles. In other words, Ciermans may still be prepared to accept a hidden cause to explain the physical properties of natural bodies.

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assurgant: nec hanc infringit plumbum, quod in argentum vivum quandoque immersum, teste experientia, non emergit. Corpus vero, quod sursum per liquidum ascendere videris, ne expelli dixeris ab illa aqua, cuius locum occupat, sed a liquido, quod illud circumstat; quod tamen corpore expulso minus esse in pondere & mole, nihil vetat aequipondij ratio, ut & aquae latissime diffusae partes omnes armari, ut pondus levissimum sursum propellant, idem enim parva magnaque aquae copia corpus immersum patitur: ideoque non est, quod magis timeat urinator oceani incumbentis molem, quam illam, quae vix capitis tegit verticem: imo fieri quandoque potest, ut difficiliorem ascensum inveniat paucas inter aquas, quam multas" (Ciermans, *Disciplinae mathematicae*, Februarii Hebdomas Secunda, "In humidum descendentia").

The position of Ciermans suggests just how important mechanics and corpuscular theories had become in the scientific doctrines of the Flemish Jesuit mathematicians. This situation was not to last. Ciermans's successor, Andreas Tacquet, withdrew to a more traditional mathematical discourse, leaving natural philosophy to the philosophers.

BIBLIOGRAPHY

MANUSCRIPTS

Firrufino, J., *Descripción y tratado muy breve y lo más provechoso de la artillería*, Biblioteca Nacional of Madrid, Ms. 9027.

Galilei, G., *Discorsi e dimostrazioni matematiche intorno a due nuove scienze* (Galileo's own copy with annotations), Firenze, Biblioteca Nazionale Centrale, Ms. Gal. 79.

Galilei, G., *Manoscritto, frammenti attenenti ai Discorsi e dimostrazioni intorno a due nuove scienze,* Firenze, Biblioteca Nazionale Centrale, Ms. Gal. 74.

Galilei, G., *Notes on motion*, Firenze, Biblioteca Nazionale Centrale, Ms. Gal. 72.

Hurtado de Mendoza, *Paraphrasis de phisico auditu en 14 quadernos*, Escorial, Ms. F-II-6, ff. 63–242.

Moletti, G., *Discorso*, Milano, Biblioteca Ambrosiana, Ms. S 103 sup., ff. 122–175.

Moletti, G., *In librum mechanicorum Aristotelis expositio*, Milano, Biblioteca Ambrosiana, Ms. S 100 sup., ff. 156–196.

PUBLISHED SOURCES

Achillini, A., *De proportionibus motuum*, Bologna: Hyeronimus de Benedictis Bononiensis, 1515; repr. in *Opera omnia*, Venice: Hieronymus Scotus, 1545.

Adelard of Bath, *Euclid's* Elements. See Busard (ed.), *The First Latin Translation of Euclid's* Elements *Commonly Ascribed to Adelard of Bath.*

Alava, D. de, *El perfeto Capitán, instruido en la disciplina Militar y nueva ciencia de la Artillería,* Madrid: Pedro Madrigal, 1590.

Albert of Saxony, *Expositio et quæstiones in Aristotelis physicam ad Albertum de Saxonia attributæ*, ed. B. Patar, 3 vols., Louvain and Paris: Éditions de l'Institut Supérieur de Philosophie & Éditions Peeters, 1999.

Albert of Saxony, *Tractatus proportionum*, ed. H.L.L. Busard, *Der Tractatus proportionum von Albert von Sachsen*, Wien: Österreichische Akademie der Wissenschaften & Springer Verlag Wien, 1971.

Aristotle, *De motu animalium*, ed. and Engl. trans. M. Nussbaum, Princeton: Princeton University Press, 1978.

Aristotle, *Mechanica*, see Pseudo-Aristotle.

Aristotle, *Physics*, trans. R.P. Hardie and R.K. Gaye in *The Basic Works of Aristotle*, ed. R. McKeon, New York: Random House, 1941.

Arnald of Villanova, *Aphorismi de gradibus*, in *Arnaldi de Villanova opera medica omnia*, McVaugh (ed.), vol. II, Granada and Barcelona: Seminarium Historiae Medicae Granatensis, 1975.

Averroes, *Aristotelis opera cum Averrois commentariis*, Venice: Iunctas, 1572.

Avicenna, *Sufficientia*, in *Opera*, ed. C. Fabriano, Venice: Octavianus Scotus, Bonetus Locatellus, 1508; here repr. Frankfurt a.M.: Minerva 1961.

Baldi, B., *In mechanica Aristotelis problemata exercitationes*, Moguntiae: Joannis Albini, 1621.

Baliani, G.B., *De motu naturali gravium solidorum Joannis Baptistae Baliani,* Genova: Farroni, 1638.

Beeckman, I., *Journal tenu par Isaac Beeckman de 1604 à 1634*, ed. C. de Waard, 4 vols., La Haye: Martinus Nijhoff, 1939–1953.

Benedetti, G.B., *Io. Baptistae Benedicti … diversarum speculationem mathematicarum et physicarum liber,* Taurini, 1585.

Blasius of Parma, *Questiones circa tractatum proportionum Magistri Thome Braduardini*, ed. and French trans. J. Biard and S. Rommevaux, Paris: Vrin, 2005.

Boethius, A.M.C., *Fundamentals of Music*, ed. and Engl. trans. C. Bower and C. Palisca, New Haven and London: Yale University Press, 1989.

Borro, G., *De motu gravium et levium*, Firenze: Marescotti, 1573.

Bradwardine, T., *Geometria speculativa*, ed. and Engl. trans. G. Molland, Wiesbaden: Steiner, 1989.

Bradwardine, T., *Tractatus de proportionibus*, ed. and Engl. trans. H.L. Crosby, Jr, *Thomas Bradwardine, His* Tractatus de proportionibus: *Its Significance for the Development of Mathematical Physics*, Madison: The University of Wisconsin Press, 1976.

Bruno, G., *The Ash Wednesday Supper (La cena de le ceneri)*, trans. S.L. Jaki, The Hague and Paris: Mouton, 1975.

Buonamici, F., *De motu libri X*, Firenze: Sermartelli, 1591.

Buridan, J., *Expositio et quaestiones in Aristotelis* De caelo, ed. B. Patar, Louvain and Paris: Éditions de l'Institut Supérieur de Philosophie & Éditions Peeters, 1996.

Buridan, J., *Questiones super octo phisicorum libros Aristotelis*, ed. J.D. de Gandavo, Paris: Denis Roce, 1509; here repr. Frankfurt a.M.: Minerva, 1964.

Buridan, J., *Super octo physicorum libros Aristotelis*, Paris: Dyonisius Roce, 1509.

Cabeo, N., *In quatuor libros meteorologicorum Aristotelis commentaria et quaestiones,* Roma: Corbelleti, 1646.

Cardano, G., *Les livres de Hierôme Cardan […] intitulés de la Subtilité et subtiles inventions, ensemble les causes occultes et raisons d'icelles, traduits de latin en français par Richard Le Blanc,* Paris: Le Noir, 1566.

Cardano, G., *Opus novum de proportionibus numerorum, motuum, ponderum, sonorum, aliarumque mensurandam, non solum geometrico more stabilitum, sed etiam variis experimentis et obervationibus rerum in natura, solerti demonstratione illustratum, ad multiplices usus accommodatum, et in V libros digestum,* Basileæ: Henricpetrina, 1570.

Caus, S. de, *Les raisons des forces mouvantes avec diverses machines tant utiles que plaisantes (…),* Francfort: Jean Norton, 1615.

Cesalpino, A., *Quaestiones peripateticae*, Venice: Giunti, 1571.

Ciermans, J., *Disciplinae mathematicae*, Leuven: Everard de Witte, 1640.

Clavius, C., *Corrispondenza*, eds. U. Baldini and P.D. Napolitani, 7 vols., Pisa: Quaderno del Dipartimento di Matematica, 1992.

Clavius, C., *Euclidis elementorum libri XVI*, Mainz, A. Hierat and R. Eltz, 1611; reprint under the title *Commentaria in Euclidis elementa geometrica*, Hildesheim: Olms, 1999.

Descartes, R., *Oeuvres de Descartes*, eds. C. Adam and P. Tannery (13 vols., Paris, 1897–1913), nouv. présentation, B. Rochot and P. Costabel, 11 vols., Paris: Vrin, 1964–1974.

Euclid, *The Elements of Euclid, viz. the First Six Books, together with the Eleventh and Twelfth*, ed. and Engl. trans. R. Simpson, Philadelphia: Thomas Desilver & Robert Desilver, 1829.

Euclid, *The Thirteen Books of the Elements*, ed. and Engl. trans. T. Heath, 2nd ed., 3 vols., New York: Dover Publications, 1956.

Fernel, J., *Ioannis Fernelii ambianatis de proportionibus libri duo*, Paris: Simonis Colinaeus, 1528.

Galilei, G., *Le mecaniche*, ed. and introd. R. Gatto, Firenze: Olschki, 2002.

Galilei, G., *Le opere di Galileo Galilei*, eds. A. Favaro and I. Del Lungo (Edizione Nazionale), 20 vols., Firenze: Barbèra, 1890–1909 (repr. 1929–1939 and 1964–1968).

Galilei, G., *Les mécaniques*, ed., French trans. and introd. E. Festa and S. Roux, Paris: Les Belles–Lettres, forthcoming.

Galilei, G., *On Motion and On Mechanics*, introd. and Engl. trans. S. Drake and I. Drabkin, Madison: University of Wisconsin Press, 1960.

Galilei, G., *Opere di Galileo Galilei, prima edizione completa condotta sugli autentici manoscritti palatini e dedicata a S.A.I. Leopoldo II Granduca di Toscana,* ed. E. Alberi, Firenze: Società Editrice Fiorentina, 1842–1856.

Galilei, G., *Two New Sciences*, Engl. trans. S. Drake (Madison: University of Wisconsin Press, 1974), Toronto: Wall & Thompson, 1989.

García de Céspedes, A., *Libro de instrumentos de geometria*, Madrid: Juan de la Cuesta, 1606.

Gerard of Cremona, *Euclid's* Elements. See Busard (ed.), *The Latin translation of the Arabic version of Euclid's* Elements *commonly ascribed to Gerard of Cremona.*

Gesner, C., *Biblioteca universalis*, Tiguri: Froschoverus, 1545.

Guidobaldo dal Monte, *Mechanicorum liber*, Pesaro: Hyeronimus Concordia, 1577.

Heron of Alexandria, *Heronis Alexandrini opera quae supersunt omnia*, 5 vols., Leipzig: B.G. Teubner, 1899–1914; Vol. 2, *Mechanica et catoptrica*, ed. and Germ. trans. L. Nix and W. Schmidt, 1900.

Heron of Alexandria, *Les mécaniques ou l'élévateur des corps lourds*, ed. and French trans. B. Carra de Vaux (*Journal asiatique*, série IX, vol. 1 (1893), 386–472, and vol. 2 (1894), 152–269 and 420–514), introd. D.R. Hill and notes A.G. Drachmann, Paris: Les Belles–Lettres, 1988.

Herrera, J. de., *Discurso del Sr. Juan de Herrera, aposentador mayor de S.M., sobre la figura cúbica*, eds. E. Simons and R. Godoy, Madrid: Editora Nacional, 1976.

Herrera, J. de., *Institucion de la Academia Matemática*; repr. of the 1584 ed., introd. by J. Simón Díaz and L. Cervera Vera, Madrid: Instituto de Estudios Madrileños, 1995.

Hippocrates, *On Fractures*, in E. Littré, *Oeuvres complètes d'Hippocrate*, Paris: Ballière, 1839–1861, vol. III, 412–563.

Hobbes, T., *De corpore. Elementorum philosophiae sectio prima*, ed. K. Schuhmann, Paris: Vrin, 1999.

Hurtado de Mendoza, D., *Mechanica de Aristóteles*, ed. R. Foulché-Delbosc, *Revue Hispanique*, 5, 1898, 365–405.

Jordanus de Nemore, *De elementis arithmetice artis*, ed. H.L.L. Busard, *Jordanus de Nemore.* De elementis arithmetice artis. *A Medieval Treatise on Number Theory*, 2 vols., Stuttgart: Steiner, 1991.

Jordanus de Nemore, *Liber Jordani de ratione ponderis*, ed. and Engl. trans. in E. A. Moody and M. Clagett (eds.), *The Medieval Science of Weight*, Madison and Milwaukee and London: The University of Wisconsin Press, 1960, 167–222.

Kronland, J.M.M. von, *De proportione motus figurarum rectilinearum et circuli quadratura ex motu,* Typographia academica, 1648.

Lastanosa, P.J., *Los veintiùn libros de los ingenios y màquinas de Juanelo Turriano,* see Turriano.

Leonardo da Vinci, *Il codice atlantico di Leonardo da Vinci nella biblioteca ambrosiana di Milano*, ed. G. Piumati, Milan: Ulrico Hoelpi, 1902–1903.

Leonardo da Vinci, *Les manuscrits de Léonard de Vinci*, transcr., ed. and French trans. C. Ravaisson–Mollien, 6 vols., Paris: Quantin, 1881–1891.

Leonardo da Vinci, *I codici de Madrid I: Tratado de estatica y mechanica en italiano*, ed. G. Piumati, Firenze: Giunti & Barbèra, 1974.

Leonico, N.T., *Nicolai Leonici Thomaei opuscula nuper in lucem edita*, Paris: Simonis Colinaeus, 1530.

Lokert G., *De proportione proportionum et proportionalitate,* in *Questiones et decisiones physicales insignium virorum: Alberti de Saxonia … Thimonis … Buridani … recognitae rursus et emendatae summa accuratione et iudicio Magistri Georgii Lokert Scoti: a quo sunt tractatus proportionum additi,* Paris: Iodicus Badius Ascensius & Conradus Resch, 1518.

Mandey, V., and Moxon, J., *Mechanick Powers: or, the Mistery of Nature and Art unvail'd […]*, London: Venterus Mandey, 1696.

Marchetto of Padua, *Lucidarium*, in J. Herlinger, *The Lucidarium of Marchetto of Padua. A Critical Edition, Translation, and Commentary*, Chicago and London: University of Chicago Press, 1985.

Marliani, G., *Questio de proportione motuum in velocitate*, Pavia: Damianus de Confaloneriis de Vinasco, 1482.

Mersenne, M., *Correspondance du P. Marin Mersenne, religieux minime*, eds. P. Tannery, C. de Waard, and A. Beaulieu, 17 vols., Paris: Éditions du CNRS, 1932–1988.

Moletti, G., *Dialogue on Mechanics*, ed. and Engl. trans. in W.R. Laird, *The Unfinished Mechanics of Guiseppe Moletti*, Toronto: University of Toronto Press, 2000.

Moxon, J., *Mechanick Exercises of the Doctrine of Handy-Works*, ed. C.F. Montgomery, New York: Praeger Publishers, 1970.

Muñoz, J., *Libro del nuevo cometa* (Valencia: Pedro de Huete, 1573). *Littera ad Bartholomaeum Reisacherum* (1574). *Summa del prognostico del cometa* (Valencia, Juan Navarro, 1578), facsimile ed. with an introduction, appendices and anthology by V. Navarro Brotóns, Valencia: Hispaniae Scientia, 1981.

Muñoz, J., *Introducción a la astronomía y la geografía*, ed. V. Navarro, introd. V. Navarro and V.L. Salavert, ed. and Span. trans. V. Navarro, A. Pastor, E. Pastor, V. Salavert, Valencia: Consell Valenciá de Cultura, 2003.

Nicolaus Cusanus, *Philosophisch-theologische Schriften*, ed. L. Gabriel, vol. 3, Vienna: Herder, 1967.

Oresme, N., *De proportionibus proportionum and Ad pauca respicientes*, ed. and Engl. trans. E. Grant, Madison: The University of Wisconsin Press, 1966.

Oresme, N., *Le livre du ciel et du monde*, ed. A.D. Menut and A.J. Denomy, Madison: University of Wisconsin Press, 1968.

Páez de Castro, J., *Memorial sobre los libros y utilidad de la librería y orden y traza que en ella se ha de tener,* ed. B.A. Nasarre, *Revista de Archivos, Bibliotecas y Museos,* 9 (1883), 165–178.

Pappus of Alexandria, *Pappi Alexandrini collectionis quae supersunt*, ed. and Latin trans. F. Hultsch, 3 vols., Berlin: Weidmann, 1876–1878.

Philoponus, J., *Commentaria in primos quatuor libros Aristotelis de naturali ausculatione. Nunc primum e graeco in latinum fideliter translata, Guilelmo Dorotheo Veneto interprete,* Venice: Octavianus Scotus, 1539.

Philoponus, J., *Joannis Philoponi de opificio mundi libri vii*, ed. W. Reichardt, Leipzig: Teubner, 1897.

Philoponus, J., *On Aristotle's Physics 5–8,* in *Philoponus, On Aristotle's Physics 5–8, with Simplicius, On Aristotle on the Void*, ed. and Engl. trans. Paul Lettinck and J. O. Urmson, London: Duckworth, and Ithaca: Cornell University Press, 1994.

Piccolomini, A., *Alexandri Piccolominei in mechanicas quaestiones Aristotelis, paraphrasis paulo quidem plenior… ad Ferdinandum de Mendoza.* Roma: Bladus Asulanus, 1542.

Piccolomini, A., *Della institutione di tutta la vita del homo nobile*, Venice: Scitus, 1542.

Pigafetta, F., *Le mechaniche dell'Illustriss. Sign. Guido Ubaldo de' Marchesi del Monte tradotte in volgare dal Sig. Filippo Pigafetta,* Venice: Francesco di Franceschi Senese, 1581.

Plato, *Theaetetus*, trans. M.J. Levett, rev. M.F. Burnyeat, in *Plato, Complete Works*, ed. J.M. Cooper, Indianapolis/Cambridge: Hackett, 1997, pp. 157–234.

Plutarch, *Plutarch's Lives*, ed. and Engl. trans. B. Perrin, 11 vols., Cambridge, Mass., and London: Harvard University Press and Heinemann, 1955.

Politus, B., *Tractatus proportionum introductorius ad calculationes suisset*, published together with his *Questio de modalibus*, Venice: Bonetus Locatellus for Octavianus Scotus, 1505.

Prado, J. and J.B. Villalpando, *In Ezechielem explanationes et apparatus urbis, ac templi hierosolymitami. Commentariis et imaginibus illustratus,* 3 vols., Roma, A. Zanetti, J. Ciaconii, C. Vulietus, 1596–1604.

Pseudo–Aristotle, *Aristotelis quaestiones mechanicae*, ed. and Latin trans. J.P. van Capelle, Amsterdam: Petrum den Henghst et Filium, 1812.

Pseudo–Aristotle, *Mechanical Problems*, ed. and Engl. trans. W.S. Hett, in *Aristotle: Minor Works*, Cambridge, Mass., and London: Harvard University Press and Heinemann, 1936.

Richard of Wallingford, *An Edition of his writings with introduction, English translation and commentary*, ed. J. North, 3 vols., Oxford: Oxford University Press, 1976.

Ridolfi, V., *De proportione proportionum disputatio*, Roma: Iacobus Mazochius, 1516.

Robert of Chester, *Euclid's* Elements. See Busard and Folkerts (eds.), *Robert of Chester's (?) Redaction of Euclid's Elements, the so-called Adelard II Version.*

Rocamora, G., *Sphera del universo*, Madrid: Juan de Herrera, 1599.

Rojas, C. de., *Teórica y práctica de fortificación*, Madrid: Luis Sánchez, 1598.

Sempilius, H., *De mathematicis disciplinis*, Antwerp: Balthasar Moretus, 1635.

Simplicius, *On Aristotle on the Void*, in *Philoponus, On Aristotle's Physics 5–8, with Simplicius, On Aristotle on the Void*, ed. and Engl. trans. Paul Lettinck and J. O. Urmson, London: Duckworth, and Ithaca: Cornell University Press, 1994.

Stevin, S., *La statique ou art pondéraire*, in *Oeuvres mathématiques*, French trans. A. Girard, vol. 4, Leyde: Bonaventure & Abraham Elzevier, 1634.

Stigliola, C., *De gli elementi mechanici* (1597); repr. in R. Gatto, *La Meccanica a Napoli ai tempi di Galileo*, Napoli, Istituto Italiano per gli Studi Filosofici: La Città del Sole, 1996.

Swineshead, R., *Liber calculationum*, ed. Giovanni Tolentini, Pavia: Franciscus Girardengus, 1498.

Tartaglia, N., *Quesiti et inventioni diverse* (1554); repr. A. Masotti, Brescia: Ateneo di Brescia, 1959.

Tartaglia, N., *Quesiti et inventioni diversi*, Venice: N. de Bascarini, 1546.

Thomas Aquinas, *In Aristotelis libros de caelo et mundo, de generatione et corruptione, meteorologicorum expositio,* ed. R.M. Spiazzi, Torino and Roma: Marietti, 1952.

Thomas Aquinas, *In octo libros de physico auditu sive physicorum Aristotelis commentaria*, ed. A.M. Pirotta, O.P., Napoli: M. d'Auria, 1953.

Turriano, J., *Los veintiún libros de los ingenios y máquinas de Juanelo Turriano*, Madrid: Fundación Juanelo Turriano et al., 1996.

Varchi, B., *Lezioni sulla pittura e la scultura II* (1546), in *Opere*, 2 vols., Trieste: Lloyd, 1858–1859, vol. II, 611–647.

Varro, M., *De motu tractatus* (1584), ed. and Ital. trans. in M. Camerota and M. Helbing, *All' alba della scienza galileiana. Michel Varro e il suo* De motu tractatus. *Un importante capitolo della meccanica di fine cinquecento,* Cagliari: CUEC, 2000.

Veranzio, F., *Machinae novae*, Milano: Ferro Edizioni, 1968.

Vitruvius, *De architectura*, ed. and Engl. trans. F. Granger, 2 vols., Cambridge, Mass. and London: Harvard University Press and Heinemann, 1985.

Viviani, V., *Quinto libro degli elementi di Euclide, ovvero scienza universale delle proporzioni […] Aggiuntevi cose varie e del Galileo e del Torricelli; i ragguagli dell'ultime opere loro dall'indice si manifesta,* Firenze: alla Condotta, 1674.

Viviani, V., *Racconto istorico di Vincenzo Viviani,* in Favaro (ed.), *Le Opere di Galileo Galilei,* vol. XIX, 597–632.

Wallis, J., *A Treatise of Algebra, both Historical and Practical*, London: John Playford for Richard Davis, 1685.

Wolff, C., *Cosmologia generalis. Methodo scientifica pertractata, qua ad solidem, inprimis Dei atque naturae cognitionem via sternitur, in Opera,* II 4, ed. J. École, Leipzig and Frankfurt a.M.: Renger, 1737; here repr. Hildesheim and New York: Olms, 1964.

SECONDARY LITERATURE

Actes de la VII Trobada d'Història de la Ciencia i de la Técnica, Barcelona: Societat Catalana d'Història de la Ciencia i de la Técnica, 2003.

Allard, G.H., "Les Arts mécaniques aux yeux de l'idéologie médiévale", in Allard and Lusignan (eds.), *Les arts mécaniques au Moyen Âge*, 13–21.

Allard, G.H., and S. Lusignan (eds.), *Les arts mécaniques au Moyen Âge*, Montréal and Paris: Bellarmin and Vrin, 1982.

Aramburu-Zabala, M.A., and J. Gómez Martínez (eds.), *Juan de Herrera y su influencia*, Santander: Universidad de Cantabria, 1993.

Arcaute, A.R. de, *Juan de Herrera, arquitecto de Felipe II*, Madrid: Espasa Calpe, 1936.

Arend, G., *Die Mechanik des Niccoló Trataglia im Kontext der zeitgenössischen Erkenntnis– und Wissenschaftstheorie,* München: Institut für Geschichte der Naturwissenschaften, 1998.

Ariotti, P.E., "Aspects of the Conception and Development of the Pendulum in the 17th Century", *Archive for History of Exact Sciences*, 8 (1971–1972), 329–410.

Arnaldi, G., and M.P. Stocchi (eds.), *Storia della cultura veneta*, 4 vols., Venice: Neri Pozza Editore, 1976–1986.

Ashworth, E.J., "The '*Libelli sophistarum*' and the Use of Medieval Logic Texts at Oxford and Cambridge in the Early Sixteenth Century", *Vivarium* 17 (1979), 134–158.

Ayoub, R., "The Lemniscate and Fagnano's Contributions to Elliptic Integrals", *Archive for History of Exact Sciences*, 29 (1984), 131–149.

Badaloni, N., *Il tempo, periodo pisano nella formazione di Galileo,* Firenze, 1965.

Baldini, U., "Animal Motion Before Borelli", in Bertoloni Meli (ed.), *Marcello Malpighi*, 193–246.

Baldini, U., "Archimede nel seicento Italiano", in Dolo (ed.), *Archimede Mito Tradizione Scienza*, 237–289.

Baldini, U., "The Academy of Mathematics of the Collegio Romano from 1553 to 1612", in Feingold (ed.), *Jesuit Science and the Republic of Letters*, 47–98.

Barnes, J., J. Brunschwig, M. Burnyeat, and M. Schofield (eds.), *Science and Speculation: Studies in Hellenistic Theory and Practice*, Cambridge: Cambridge University Press, 1982.

Bedini, S.A., *The Pulse of Time: Galileo Galilei, the Determination of Longitude, and the Pendulum Clock,* Firenze: Olschki, 1991.

Beretta, M. (ed.), *From Private to Public. Natural Collections and Museums*, Sagamore Beach, Mass.: Science History Pub., 2005.

Bertoloni Meli, D. (ed.), *Marcello Malpighi Anatomist and Physician*, Firenze: Olschki, 1997.

Biard, J., "Mathématiques et philosophie dans les *Questions* de Blaise de Parme sur le *Traité des rapports* de Thomas Bradwardine", *Revue d'histoire des sciences*, 56/2 (2003), 383–400.

Biard, J., and S. Rommevaux (eds.), *Mathématiques et théorie du mouvement (XIV^e–XVI^e siècles)*, Villeneuve d'Ascq: Presses Universitaires du Septentrion, forthcoming.

Blumenberg, H., "Die Vorbereitung der Neuzeit", *Philosophische Rundschau*, 9 (1961), 81–133.

Blumenberg, H., *Die Genesis der kopernikanischen Welt* (1975), Frankfurt a.M.: Suhrkamp Taschenbuch Wissenschaft, 1981.

Boas Hall, M., *The Scientific Renaissance, 1450–1630*. New York: Dover Publications, 1962.

Bosmans, H., "Le traité *De centro gravitatis* de Jean-Charles de la Faille S.J.", *Annales de la société scientifique de Bruxelles*, 38 (1913), 255–317.

Brugmans, A., "Specimen mechanicae veterum per mechanicam recentiorem plenius expositum", *Commentationes societatis regiae scientiarum Gottingensis, Mathematicae Classis*, 7 (1784–1785), 75–88.

Bucciantini, M., and M. Torrini (eds.), *La diffusione del copernicanesimo in Italia, 1543–1610*, Firenze: Olschki, 1997.

Bucciantini, M., *Galileo e Keplero: Filosofia, cosmologia e teologia nell'éta Controriforma,* Torino: Einaudi, 2003.

Busard, H.L.L. (ed.), *The First Latin Translation of Euclid's* Elements *Commonly Ascribed to Adelard of Bath*, Toronto: Pontifical Institute of Mediaeval Studies, 1983.

Busard, H.L.L. (ed.), *The Latin Translation of the Arabic Version of Euclid's* Elements *Commonly Ascribed to Gerard of Cremona,* Leiden: E.J. Brill, 1984.

Busard, H.L.L., “Die Traktate *De proportionibus* von Jordanus Nemorarius und Campanus”, *Centaurus* 15 (1971), 193–227.

Busard, H.L.L., and M. Folkerts (eds.), *Robert of Chester’s (?) Redaction of Euclid’s* Elements*, the so-called Adelard II Version*, 2 vols., Basel, Boston and Berlin: Birkhäuser, 1992.

Büttner, J., P. Damerow and J. Renn, “Galileo’s Unpublished Treatises”, in Palmerino and Thijssen (eds.), *The Reception of the Galilean Science of Motion*, 99–117.

Büttner, J., P. Damerow and J. Renn, “Traces of an Invisible Giant: Shared Knowledge in Galileo’s Unpublished treatises”, in Montesinos and Solís (eds.), *Largo campo di filosofare,* 183–203.

Büttner, J., P. Damerow, J. Renn and M. Schemmel., “The Challenging Images of Artillery: Practical Knowledge at the Roots of the Scientific Revolution”, in Lefèvre, Renn and Schoepflin (eds.), *The Power of Images in Early Modern Science*, 3–28.

Camerota, M., and M.O. Helbing, “Galileo and Pisan Aristotelianism: Galileo’s *De motu antiquiora* and the *Quaestiones de motu elementorum* of the Pisan Professors”, *Early Science and Medicine*, vol. V-4 (2000), 319–366.

Camerota, M., and M.O. Helbing, *All’ alba della scienza galileana. Michel Varro e il suo* De motu tractatus. *Un importante capitolo della meccanica di fine cinquecento,* Cagliari: CUEC, 2000.

Camerota, M., *Gli scritti* De motu antiquiora *di Galileo Galilei: il Ms. Gal 71. Un’analisi storico–critica,* Cagliari: CUEC, 1992.

Caroti, S., and P. Souffrin (eds.), *La nouvelle physique du XIV*e *siècle*, Florence: Olschki, 1997.

Carugo, A., “L’Insegnamento della matematica all’ Università di Padova prima e dopo Galileo”, in Arnaldi and Stocchi (eds.), *Storia della Cultura Veneta*, 4/II (1984), 151–199.

Castiglioni, A., “La vita e l’opera di Santorio Santorio Capodistriano” (1920), Eng. trans. E. Recht, “Life and Work of Sanctorius”, *Medical Life*, 38 (1931), 729–785.

Caverni, R., *Storia del metodo sperimentale in Italia*, 6 vols., Firenze: Civerni, 1891–1900.

Celeyrette, J., “La mécanique, une science médiévale?”, available online on the website: www.univ–lille3.fr/set.

Celeyrette, J., and E. Mazet, “Le mouvement du point de vue de la cause et le mouvement du point de vue de l’effet dans le *Traité des rapports* d’Albert de Saxe”, *Revue d’histoire des sciences*, 56/2 (2003), 419–437.

Cervera Vera, L., *El manuscrito de Juan de Hererra indebidamente titulado Arquitectura y Máquinas,* Valencia: Patrimonio, 1996.

Cervera Vera, L., *Inventario de los bienes de Juan de Herrera*, Valencia: Albatros, 1977.

Clagett, M., *Archimedes in the Middle Ages. Vol. II. The Translations from the Greek by William of Moerbeke,* Philadelphia: The American Philosophical Society, 1976.

Clagett, M., *Giovanni Marliani and Late Medieval Physics* (New York: Columbia University Press, 1941), New York: AMS Press, 1967.

Clagett, M., *The Science of Mechanics in the Middle Ages*, Madison: The University of Wisconsin Press, 1959.

Clavelin, M., “Le copernicianisme padouan de Galilée”, in *Tribute to Galileo in Padua*, 149–166.

Clavelin, M., *La philosophie naturelle de Galilée* (Paris: Armand Colin, 1968), Paris: Albin Michel, 1996.

Clement, J., “A Conceptual Model Discussed by Galileo and Used Intuitively by Physics Students”, in Gentner and Stevens (eds.), *Mental Models*, 325–340.

Clericuzio, A. (ed.), *Science Teaching in Early Modern Europe*. Special Issue of *Science and Education*, vol. XV (2006).

Cohen, M.R., and I.E. Drabkin (eds.), *A Source Book in Greek Science*, New York: McGraw Hill Book Co, 1948.

Cooke, R., “Elliptic Integrals and Functions”, in Grattan–Guinness (ed.), *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences*, 529–539.

Cranz, E.F., *A Bibliography of Aristotle Editions, 1501–1600*, Baden–Baden: Valentin Koerner, 1971.

Cuomo, S., *Pappus of Alexandria and the Mathematics of Late Antiquity*, Cambridge: Cambridge University Press, 2000.

Damerow, P., and W. Lefèvre, *Wissenssysteme im geschichtlichen Wandel* (Preprint 5), Berlin: Max Planck Institut für Wissenschaftsgeschichte, 1994.

Damerow, P., G. Freudenthal, P. McLaughlin and J. Renn, *Exploring the Limits of Preclassical Mechanics*, New York, Berlin and Heidelberg: Springer Verlag, 1992.

Damerow, P., J. Renn and S. Riege, “Hunting the White Elephant: When and How did Galileo Discover the Law of Fall?”, in Renn (ed.), *Galileo in Context*, 29–150.

De Gandt, F., “Force et science des machines”, in Barnes, Brunschwig, Burnyeat and Schofield (eds.), *Science and Speculation: Studies in Hellenistic Theory and Practice*, 96–127.

Dewender, T., “Lawrence of Lindores and his Commentaries on Aristotle”, in Marenbon (ed.), *Aristotle in Britain During the Middle Ages*, 323–335.

Dijksterhuis, E.J., *The Mechanization of the World Picture* (1950), trans. C. Dikshoorn, Oxford: Clarendon Press, 1961.

Dollo, C. (ed.), *Archimede Mito Tradizione Scienza*, Firenze: Olschki, 1992.

Dollo, C., “Galileo e la fisica del Collegio Romano”, *Giornale critico della Filosofia italiana*, sesta serie, vol. XII-2 (1992), 161–201.

Dormer, D.J., and J.F.A. de Uztarroz, *Progresos de la historia en el reyno de Aragón*, Zaragoza: Herederos de Diego Dormer, 1680; here repr. Zaragoza, Biblioteca de Escritores Aragoneses, 1878.

Drabkin, I.E., and S. Drake (eds.), *Mechanics in Sixteenth–Century Italy. Selections from Tartaglia, Benedetti, Guido Ubaldo and Galileo*, Madison, Milwaukee and London: The University of Wisconsin Press, 1969.

Drachmann, A.G., “Heron’s Screwcutter”, *Journal of Hellenic Studies*, 56 (1936), 72–77.

Drachmann, A.G., “A Note on Ancient Cranes”, in Singer (ed.), *A History of Technology*, vol. II, 658–662.

Drachmann, A.G., “Fragments from Archimedes in Heron’s Mechanics”, *Centaurus*, 8 (1963), 91–146.

Drachmann, A.G., *Ancient Oil Mills and Presses (Kgl. Danske Videnskabernes Selskab, Archaeologisk–kunsthistoriske Meddelelser,* vol. I-1), Copenhagen: Levin & Munksgaard, 1932.

Drachmann, A.G., *The Mechanical Technology of Greek and Roman Antiquity*, Copenhagen: Munksgaard, 1963.

Drake, S., “A Further Reappraisal of Impetus Theory: Buridan, Benedetti, and Galileo”, *Studies in History and Philosophy of Science*, 7 (1976), 319–336.

Drake, S., “Free Fall from Albert of Saxony to Honoré Fabri”, *Studies in History and Philosophy of Science*, 5 (1974–1975), 347–366.

Drake, S., “Renaissance Music and Experimental Science”, *Journal of the History of Ideas*, 31 (1970), 483–500.

Drake, S., *Essays on Galileo and the History and Philosophy of Science*, 3 vols., Toronto: University of Toronto Press, 3 vols., 1999.

Dugas, R., *A History of Mechanics*, Neuchatel: Ed. Du Griffon, 1955.

Duhem, P., *Études sur Léonard de Vinci. Ceux qu’il a lus et ceux qui l’ont lu* (1ère, 2ème et 3ème séries), Paris, Hermann, 1906–1913.

Duhem, P., *Le système du monde. Histoire des doctrines cosmologiques de Platon à Copernic,* 10 vols., Paris: Hermann, 1913–1959.

Duhem, P., *Les origines de la statique*, 2 vols., Paris: Hermann, 1905–1906.

Esteban Piñeiro, M., and M. Jalón, “Juan de Herrera y la Real Academia Matemáticas”, in *Los instrumentos científicos del siglo XVI*, 55–67.

Favaro, A., “Alcuni scritti inediti di Galileo Galilei tratti dai manoscritti della Biblioteca Nazionale di Firenze”, *Bullettino di bibliografia e di storia delle scienze matematiche e fisiche*, XVI (1883), 1–97 and 135–157.

Favaro, A., “Ancora sulla cosidetta ‘Lampada di Galileo’ nel Duomo di Pisa”, in *Scampoli Galileiani*, a cura di L. Rossetti e M.L. Soppelsa, Trieste: Lint, 1992, 668–670.

Favaro, A., *Galileo Galilei e lo studio di Padova*, 2 vols. (Firenze: Le Monnier, 1883), Padua: Editrice Antenore, 1966.

Feingold, M. (ed.), *Jesuit Science and the Republic of Letters*, Cambridge, Mass.: MIT Press, 2003.

Feldhaus, F.M., “Beiträge zur Geschichte der Uhr”, *Deutsche Uhrmacher–Zeitung*, XXXII-11 (1908), 159–160.

Feldhaus, F.M., “Das Pendel bei Leonardo da Vinci”, *Deutsche Uhrmacher–Zeitung*, XXXIV–2 (1910), 23–24.

Festa, E., and S. Roux, "La moindre petite force suffit à mouvoir un corps sur un plan horizontal. L'émergence d'un principe mécanique et son devenir cosmologique chez Galilée", *Galilaeana*, 3 (2006), 123–147.

Festa, E., and S. Roux, "Le '$\pi\alpha\rho\grave{\alpha}$ $\varphi\acute{\upsilon}\sigma\iota\nu$' et l'imitation de la nature dans quelques commentaires du prologue des *Questions mécaniques*", in Montesinos and Solís (eds.), *Largo campo di filosofare*, 237–253.

Field, J.V., and F.A.J.L. James (eds.), *Renaissance and Revolution: Humanists, Scholars, Craftsmen and Natural Philosophers in Early Modern Europe,* Cambridge: Cambridge University Press, 1993.

Foley, V., S. Darlene, C. Widule and D. Ellis, "Besson, da Vinci, and the Evolution of the Pendulum: some Findings and Observations", *History and Technology*, 6 (1988), 1–43.

Foulché–Delbosc, R., "Mechanica de Aristóteles", *Revue Hispanique*, 5 (1898), 365–405.

Fowler, D.H., *The Mathematics of Plato's Academy: a New Reconstruction*, 2nd ed., Oxford: Clarendon Press, 1999.

Fraser, D., H. Hibbard and M.L. Levine (eds.), *Essays in the History of Architecture Presented to Rudolph Wittkower*, London: Phaidon, 1967.

Fredette, R., "Galileo's *De motu antiquiora*: Notes for a Reappraisal", in Montesinos and Solís (eds.), *Largo campo di filosofare*, 165–182.

Fredette, R., "Galileo's *De motu antiquiora*", *Physis*, XIV-4 (1972), 321–348.

Funkenstein, A., "Some Remarks on the Concept of Impetus and the Determination of Simple Motion", *Viator. Medieval and Renaissance Studies*, 2 (1971), 329–348.

Funkenstein, A., *Theology and the Scientific Imagination from the Middle Ages to the Seventeenth Century*, Princeton: Princeton University Press, 1986.

Gabbey, A., "Between *Ars* and *Philosophia naturalis*: Reflections on the Historiography of Early Modern Mechanics", in Field and James (eds.), *Renaissance and Revolution*, 133–146.

Gabbey, A., "Newton's *Mathematical Principles of Natural Philosophy*: a Treatise on 'Mechanics'?", in Harman and Shapiro (eds.), *An Investigation of Difficult Things*, 305–322.

Gabbey, A., "The Case of Mechanics: One Revolution or Many?", in Lindberg and Westman, *Reappraisals of the Scientific Revolution*, 493–528.

Galluzzi, P., *Momento, Studi Galileiani*, Roma: Ateneo e Bizzari, 1979.

Gamba, E., and Montebelli, V., *Le Scienze a Urbino nel tardo Rinascimento*, Pesaro: Quattroventi, 1988.

Garber, D., "On the Frontlines of the Scientific Revolution: How Mersenne Learned to Love Galileo", *Perspectives on Science*, 12 (2004), 135–164.

García Tapia, N., "The Manuscript '*Architectura y Machinas*' of c.1564 of Juan de Herrera", ICON. *Journal of the International Committee for the History of Technology*, 2 (1996), 176–197.

García Tapia, N., and M.I. Vicente Maroto, "Las escuelas de artillería y otras instituciones técnicas", in López Piñero (ed.), *Historia de la ciencia y de la técnica en la Corona de Castilla*, vol. 3, 73–83.

Garcia Tapia, N., *Ingeniería y arquitectura en el Renacimiento español*, Valladolid: Universidad de Valladolid, 1990.

Garin, E., "Galileo e la cultura del suo tempo", in *Scienza e vita civile nel Rinascimento italiano*, Bari: Laterza, 1965, 109–146.

Gentner, D., and A. Stevens (eds.), *Mental Models*, Hillsdale, N.J.: Erlbaum, 1983.

Giannetto, E., G.D. Maccarone, S. Pappalardo and A. Tinè, "*Impulsus* and *Impetus* in the *Liber Jordani de ratione ponderis*", *Mediaeval Studies*, 54 (1992), 162–185.

Giard, L. (ed.), *Les Jésuites à la Renaissance. Système éducatif et production du savoir*, Paris: PUF, 1995.

Gillispie, C.C. (ed.), *Dictionary of Scientific Biography*, 18 vols., New York: Charles Scribner's Sons, 1970–1990.

Giusti, E., *Elements for the Relative Chronology of Galilei's* "De motu antiquiora", Firenze: Olschki, 1998.

Giusti, E., *Euclides Reformatus. La teoria delle proporzioni nella scuola galileiana,* Torino: Bollati Boringhieri, 1993.

Glick, T.F., J.M. López Piñero, V. Navarro Brotóns and V.E. Portela Marco (eds.), *Diccionario histórico de la ciencia moderna en España*, 2 vols., Barcelona: Península, 1983.

Goddu, A., "Avicenna, Avempace, and Averroes – Arabic Sources of 'Mutual Attraction' and Their Influence on Mediaeval and Modern Conceptions of Attraction and Gravitation", in Zimmermann (ed.), *Orientalische Kultur und Europäisches Mittelalter*, 218–239.

Goldstein, J., "A Matter of Great Magnitude: The Conflict over Arithmetization in 16th-, 17th-, and 18th-Century English Editions of Euclid's *Elements* Books I Through VI (1561–1795)", *Historia Mathematica*, 27 (2000), 36–53.

Golino, C.L. (ed.), *Galileo Reappraised* (Contributions of the UCLA Center for Medieval and Renaissance Studies, vol. 2.), Berkeley: University of California Press, 1966.

Golvers, N., and U. Libbrecht, *Astronoom van de Keizer. Ferdinand Verbiest and zijn Europese Sterrenkunde,* Leuven: Davidsfonds, 1988.

Gómez López, S., "Natural Collections in the Spanish Renaissance", in Beretta (ed.), *From Private to Public*, 13–41.

González Palencia, A., and E. Mele, *Vida y obras de Don Diego Hurtado de Mendoza*, 3 vols., Madrid: Instituto de Valencia de don Juan, 1941–1943.

Grattan-Guinness, I. (ed.), *Companion Encyclopedia of the History and Philosophy of the Mathematical Sciences,* London: Routledge, 1994.

Graux, C., *Essai sur les origines du fonds grec de l'Escurial*, Paris: Vieweg & C. Durand, 1880.

Grün, K.J., *Vom unbewegten Beweger zur bewegenden Kraft: der pantheistische Charakter der Impetustheorie im Mittelalter,* Paderborn: Mentis, 1999.

Hall, A.R., *Ballistics in the 17th Century*, Cambridge: Cambridge University Press, 1952.

Hall, B.S., and D.C. West (eds.), *On Pre–modern Technology and Science: a Volume of Studies in Honor of Lynn White,* Jr., Malibu: Undena Publ., 1976.

Harig, G., "Walter Hermann Ryff und Nicolo Tartaglia. Ein Beitrag zur Entwicklung der Dynamik im 16. Jahrhundert" (*Forschungen und Fortschritte*, 32, 2 (1958), 40–47) in *id.*, *Physik und Renaissance*, Leipzig: Akademische Verlags-Gesellschaft, 1981, 13–36.

Harman, P.M., and A.E. Shapiro (eds.), *An Investigation of Difficult Things: Essays on Newton and the History of the Exact Sciences*, Cambridge: Cambridge University Press, 1992.

Harris, S.J., "Les chaires de mathématiques", in Giard (ed.), *Les Jésuites à la Renaissance*, 239–261.

Harrison, A., "Blasius of Parma's Critique of Bradwardine's *Tractatus de proportionibus*", in Poppi (ed.), *Scienza et filosofia all'Università de Padova*, 19–69.

Helbing, M.O., "Mobilità della terra e riferimenti a Copernico nelle opere dei professori dello Studio di Pisa", in Bucciantini and Torrini (eds.), *La Diffusione del copernicanesimo in Italia*, 57–66.

Helbing, M.O., *La Filosofia naturale di Francesco Buonamici, professore di Galileo a Pisa*, Pisa: Nistri–Lischi, 1989.

Hill, D.K., "Pendulums and Planes: what Galileo didn't Publish", *Nuncius. Annali di Storia della Scienza*, 9 (1994), 499–515.

Hofmann, J.E., *Das* Opus geometricum *des G. a Sancto Vincentio und seine Einwirkung auf Leibniz*, Berlin: Verlag der Akademie der Wissenschaften, 1941.

Hoskin, M.A., and A.G. Molland, "Swineshead on Falling Bodies: An Example of Fourteenth–Century Physics", *British Journal for the History of Science*, 10 (1966), 150–182.

Hugonnard-Roche, H., "L'hypothétique et la nature dans la physique parisienne du XIV^e siècle", in Caroti and Souffrin (eds.), *La nouvelle physique du XIV^e siècle*, 161–177.

Jackson, D.E.P., *The Arabic Version of the Mathematical Collection of Pappus Alexandrinus, book VIII,* PhD thesis, University of Cambridge, 1970.

Jung–Palczewska, E., "Works by Richard Kilvington", *Archives d'histoire doctrinale et littéraire du Moyen Âge*, 67 (2000), 181–233.

Kaluza, Z., *Thomas de Cracovie*, Wroclaw: Ossolineum, 1978.

Keller, A.G., "A Manuscript Version of Jacques Besson's Book of Machines, With His Unpublished Principles of Mechanics", in Hall and West (eds.), *On Pre-modern Technology and Science*, vol. 1, 75–106.

Kirschner, S., *Nicolaus Oresmes Kommentar zur Physik des Aristoteles*, Stuttgart: Steiner, 1997.

Klamroth, M., "Über den arabischen Euklid", *Zeitschrift der Deutschen morganländischen Gesellschaft*, 35 (1881), 270–326 and 788.

Knorr, W., "On Heiberg's Euclid", *Science in Context*, 14 (2001), 133–143.

Knorr, W., "The Wrong Text of Euclid: On Heiberg's Text and its Alternatives", *Centaurus*, 38 (1996), 208–276.

Knorr, W., *The Evolution of the Euclidean Elements*, Dordrecht: Reidel, 1975.

Koyré, A., "Galileo and Plato", *Journal for the History of Ideas*, IV-4 (1943), 400–428.

Koyré, A., "Jean-Baptiste Benedetti critique d'Aristote" (1959), in Koyré, *Études d'histoire de la pensée scientifique*, 140–165.

Koyré, A., *Études d'histoire de la pensée scientifique*, Paris: Gallimard, 1973.

Koyré, A., *Études galiléennes*, Paris: Hermann, 1966.

Koyré, A., *Metaphysics and Measurement*, London: Chapman & Hall, 1968.

Krafft, F., *Die Anfänge einer theoretischen Mechanik und die Wandlung ihrer Stellung zur Wissenschaft von der Natur,* Wiesbaden: Steiner, 1967.

Krafft, F., *Dynamische und statische Betrachtungsweise in der antiken Mechanik,* Wiesbaden: Steiner, 1970.

Kuhn, T.S., *The Copernican Revolution. Planetary Astronomy in the Development of Western Thought,* Cambridge, Mass.: Harvard University Press, 1957.

Kuhn, T.S., *The Structure of Scientific Revolutions*, 2nd ed., Chicago: University of Chicago Press, 1970.

Laird, W.R., "Archimedes among the Humanists", *Isis*, 82 (1991), 629–638.

Laird, W.R., "Renaissance Mechanics and the New Science of Motion", in Montesinos and Solís (eds.), *Largo campo di filosofare*, 255–267.

Laird, W.R., "The Scope of Renaissance Mechanics", *Osiris*, 2nd series, 2 (1986), 43–68.

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

Lefèvre, W. (ed.), *Picturing Machines: 1400–1700*, Cambridge, Mass.: MIT Press, 2004.

Lefèvre, W., "Galileo Engineer: Art and Modern Science", in Renn (ed.), *Galileo in Context*, 11–29.

Lefèvre, W., J. Renn, and U. Schoepflin (eds.), *The Power of Images in Early Modern Science*, Basel: Birkhäuser, 2003.

Leitão, H.S. de, *O Comentário de Pedro Nunes à navegação a remos*, Lisboa: Edições Culturais da Marinha, 2002.

Lettinck, Paul, and J.O. Urmson, *Philoponus, On Aristotle's Physics 5–8, with Simplicius, On Aristotle on the Void*, London: Duckworth, and Ithaca: Cornell University Press, 1994.

Lewis, M.J.T., *Surveying Instruments of Greece and Rome*, Cambridge: Cambridge University Press, 2001.

Liddell, H.J., and R. Scott, *A Greek–English Lexicon.* Revised and augmented throughout by Sir Henry Stuart Jones with the assistance of Roderick McKenzie, Oxford: Clarendon Press, 1940.

Lindberg, D.C. (ed.), *Science in the Middle Ages*, Chicago and London: The University of Chicago Press, 1978.

Lindberg, D.C., and R.S. Westman (eds.), *Reappraisals of the Scientific Revolution*, Cambridge: Cambridge University Press, 1990.

Llaguno, E., and A. Ceán Bermúdez, *Noticias de los arquitectos y arquitectura de España desde su Restauración,* Madrid: Imp. Real, 1929.

Lohr, C.H., *Latin Aristotle Commentaries: II. Renaissance Authors,* Firenze: Olschki, 1988.

López Piñero, J.M., (ed.), *Historia de la ciencia y de la Técnica en la Corona de Castilla*, 4 vols., Valladolid: Junta de Castilla y León, 2002.

López Piñero, J.M., *Ciencia y técnica en la sociedad española de los siglos XVI y XVII,* Barcelona: Labor, 1979.

Los instrumentos científicos del siglo XVI. La corte española y la escuela de Lovaina, Madrid: Fundación Carlos de Amberes, 1998.

Maier, A., *Ausgehendes Mittelalter*, 2 vols., Roma: Edizioni di Storia e Letteratura, 1964 and 1967.

Maier, A., *Die Vorläufer Galileis im 14. Jahrhundert*, 2nd ed., Roma: Edizioni di Storia e Letteratura, 1966.

Maier, A., *Zwei Grundprobleme der scholastischen Naturphilosophie. Das Problem der intensiven Größe. Die Impetustheorie,* 3rd ed., Roma: Edizioni di Storia e Letteratura, 1968.

Maier, A., *Zwischen Philosophie und Mechanik*, Roma: Edizioni di Storia e Letteratura, 1958.

Mancosu, P., *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century,* Oxford: Oxford University Press, 1996.

Marenbon, J. (ed.), *Aristotle in Britain during the Middle Ages*, Turnhout: Brepols, 1996.

Markowski, M., *Burydanizm w Polsce w okresie Przedkopernikaniskim*, Warszawa, e. a.: Ossolineum & Polska Akademia Nauk, 1971.

McVaugh, M., "Arnald of Villanova and Bradwardine's Law", *Isis*, 59 (1967), 56–64.

Mendelsohn, E. (ed.), *Transformation and Tradition in the Sciences: Essays Presented to I. Bernard Cohen,* Cambridge: Cambridge University Press, 1984.

Menut, A.D., and A.J. Denomy (eds.). *Nicole Oresme: Le livre du ciel et du monde*, Madison: University of Wisconsin Press, 1968.

Michael, B., *Johannes Buridan. Studien zu seinem Leben, seinen Werken und zur Rezeption seiner Theorien im Europa des späten Mittelalters* (PhD dissertation, Freie Universität Berlin), Berlin: Private print, 1985.

Micheli, G., *Le origini del concetto di macchina*, Firenze: Olschki, 1995.

Montesinos, J., and C. Solís (eds.), *Largo campo di filosofare. Eurosymposium Galileo 2001 (Tenerife, 19–23 febrero 2001)*, La Orotava: Fundación Canaria Orotava de Historia de la Ciencia, 2001.

Moody, E.A., and M. Clagett (eds.), *The Medieval Science of Weights (Scientia de ponderibus): Treatises Ascribed to Euclid, Archimedes, Thabit ibn Qurra, Jordanus de Nemore and Blasius of Parma,* 2nd ed., Madison, Milwaukee and London: The University of Wisconsin Press, 1960.

Moonan, L., "The Scientific Writings of Lawrence of Lindores (d. 1437)", *Classica et Mediaevalia*, 38 (1987), 217–266, and 39 (1988), 273–317.

Moscheo, R., "L'Archimede del Maurolico", in Dollo (ed.), *Archimede: Mito Tradizione Scienza*, 111–164.

Mota, T. de, "O Cartógrafo João Baptista Lavanha e a sua Obra", in *Portugaliae Monumenta Cartographica*, Lisboa: Imprensa Nacional–Casa da Moeda, 1987, vol. IV, 63–69.

Murdoch, J., "Euclid: Transmission of the *Elements*", in Gillispie (ed.), *Dictionary of Scientific Biography*, vol. IV, 437–459.

Murdoch, J., "From Social into Intellectual Factors: An Aspect of the Unitary Character of Late Medieval Learning", in Murdoch and Sylla (eds.), *The Cultural Context of Medieval Learning*, 280–287.

Murdoch, J., and E. Sylla (eds.), *The Cultural Context of Medieval Learning*, Dordrecht and Boston: Reidel, 1975.

Murdoch, J., and E. Sylla, "The Science of Motion", in Lindberg (ed.), *Science in the Middle Ages*, 223–231.

Napolitani, P.D., and P. Souffrin (eds.), *Medieval and Classical Traditions and the Renaissance of Physico-Mathematical Sciences in the 16th Century* (Proceedings of the XXth International Congress of History of Science, vol. VIII), Turnhout: Brepols, 2001.

Napolitani, P.D., *Sull'Opuscolo de proportione composita di Guidobaldo dal Monte,* Università degli Studi di Pisa: Departimento di Matematica, 1982.

Navarro Brotons, V., "Astronomía y cosmografía entre 1561 y 1625. Aspectos de la actividad de los matemáticos y cosmógrafos españoles y portugueses", *Cronos*, 3 (2000), 349–381. English trans. in Saraiva and Leitão (eds.), *The Practice of Mathematics in Portugal*, 225–275.

Navarro Brotons, V., "De la filosofía natural a la física moderna (siglos XVI–XVII)", in López Piñero (ed.), *Historia de la ciencia*, 383–437.

Navarro Brotons, V., "El Renacimiento científico y la enseñanza de las disciplinas matemáticas en las Universidades de Valencia y Salamanca en el siglo XVI", in Peset and Correa (eds.), *Doctores y Escolares*, 141–159.

Navarro Brotons, V., "Espanya i la Revolució Científica: Aspectes historiogràfics, reflexions i perspectives", in *Actes de la VII Trobada d'Història de la Ciencia i de la Técnica*, 15–32.

Navarro Brotons, V., "La astronomía (siglos XVI–XVII)", in López Piñero (ed.), *Historia de la ciencia*, 259–319.

Navarro Brotons, V., "La filosofía natural", in Vernet and Parés (eds.), *La Ciencia en la Història dels Països Catalans*, vol. II, in press.

Navarro Brotons, V., "The Teaching of the Mathematical Disciplines in Sixteenth–Century Spain", in Clericuzio (ed.), *Science Teaching in Early Modern Europe*, 209–233.

Navarro Brotons, V., "Tradition and Scientific Change in Early Modern Spain: The Role of the Jesuits", in Feingold (ed.), *Jesuit Science and the Republic of Letters*, 331–387.

Navarro Brotons, V., and E. Rodríguez, *Matemáticas, cosmología y humanismo en la España del siglo XVI. Los* Comentarios al Segundo Libro de la Historia Natural de Plinio *de*

Jerónimo Muñoz, Valencia: Instituto de Estudios Documentales e Históricos sobre la Ciencia, 1998.

Navarro Brotons, V., and W. Eamon (eds.), *Beyond the Black Legend. Spain and the Scientific Revolution*, Valencia: Instituto de Historia de la Ciencia y Documentación "López Piñero", 2006.

Neal, K., *From Discrete to Continuous. The Broadening of Number Concepts in Early Modern England,* Dordrecht, Boston and London: Kluwer Academic Publishers, 2002.

Ovitt, G., "The Status of the Mechanical Arts in Medieval Classifications of Learning", *Viator*, 14 (1983), 89–105.

Palmerino, C.R., and J.M.M.H. Thijssen (eds.), *The Reception of the Galilean Science of Motion in Seventeenth-Century Europe*, Dordrecht, Boston and London: Kluwer Academic Publishers, 2004.

Passalacqua, A., "Le collezioni di Pappo: polemiche editoriali e circolazione di manoscritti nelle corrispondenze di Francesco Barozzi con il Duca di Urbino", *Bolletino di storia delle scienze matematiche*, 14 (1994), 91–156.

Peset, M., and J. Correa (eds.), *Doctores y Escolares. II Congreso Internacional sobre las Universidades Hispánicas*, Valencia: Universidad de Valencia, 1998.

Picatoste Rodríguez, F., *Apuntes para una biblioteca científia española del siglo XVI,* Madrid: Tello, 1891.

Pines, S., "Études sur Awhad al-Zaman Abu'l Barakât al-Baghdâd", *Revue des études juives*, 3 (1938), 3–64 and 4 (1938), 1–33.

Pines, S., "Les précurseurs musulmans de la théorie de l'*impetus*", *Archeion*, 21 (1938), 298–306.

Pines, S., "*Omne quod movetur necesse est ab aliquo moveri*: A Refutation of Galen by Alexander of Aphrodisias and the Theory of Motion", *Isis*, 52 (1961), 21–54.

Pines, S., "Un précurseur bagdadien de la théorie de l'*impetus*", *Isis*, 44 (1953), 247–251.

Pluta, O. (ed.), *Die Philosophie im 14. und 15. Jahrhundert. In memoriam Konstanty Michalski (1879–1947),* Amsterdam: Grüner, 1988.

Poppi, A. (ed.), *Scienza e filosofia all'università de Padova*, Padova: Lint, 1983.

Ramírez, J.A., et al., *El Templo de Salomón según Prado y Villalpando*, 3 vols., Madrid: Siruela, 1991.

Recht, E., "Life and Work of Sanctorius" (Engl. trans. of A. Castiglioni, "La vita e l'opera di Santorio Santorio Capodistriano", Bologna and Trieste, 1920), *Medical Life*, 38 (1931), 729–785.

Renn, J. (ed.), *Galileo in Context*, Cambridge: Cambridge University Press, 2001.

Renn, J., "Editor's Introduction: An Engineer–Scientist, Artist, and Courtier at the Origins of Classical Science", in Renn (ed.), *Galileo in Context*, 1–11.

Renn, J., P. Damerow and P. McLaughlin, "Aristotle, Archimedes, Euclid, and the Origin of Mechanics: The Perspective of Historical Epistemology", in Sirera (ed.), *Symposium Arquímedes*, 43–59.

Ribémont, B. (ed.), *Le temps, sa mesure et sa perception au Moyen Âge*, Caen: Paradigme, 1992.

Riera Blanco, J.J., *Juan Bautista de Toledo y Felipe II: la implantación del clasicismo en España*, Valladolid: Universidad De Valladolid, 1984.

Romano, A., *La Contre-Réforme mathématique. Constitution et diffusion d'une culture mathématique jésuite à la Renaissance,* Rome: École française de Rome, 1999.

Rommevaux, S., "Les règles du mouvement de Blaise de Parme dans les deux versions de ses *Questions sur le traité des rapports de Thomas Bradwardine*", in Biard and Rommevaux (eds.), *Mathématiques et théorie du mouvement*, forthcoming.

Rose, P.L., and S. Drake, "The Pseudo–Aristotelian *Questions of Mechanics* in Renaissance Culture", *Studies in the Renaissance*, 18 (1970), 65–104; repr. in Drake, *Essays on Galileo and the History and Philosophy of Science*, vol. 3, 131–170.

Rose, P.L., *The Italian Renaissance of Mathematics. Studies on Humanists and Mathematicians from Petrarch to Galileo,* Genève: Droz, 1975.

Rusnock, P., and P. Thagard, "Strategies for Conceptual Change: Ratio and Proportion in Classical Greek Mathematics", *Studies in the History and Philosophy of Science*, 26 (1995), 107–131.

Russo, L., *The Forgotten Revolution: How Science was born in 300 BC and why it had to be reborn,* Berlin: Springer-Verlag, 2004.

Saito, K., "Compounded Ratio in Euclid and Apollonius", *Historia Scientiarum* 31 (1986), 25–59.

Sánchez Pérez, J. A., "Monografía sobre Juan Bautista Labaña", in *Discurso leído en el Acto de su recepción …*, Madrid: Academia de Ciencias Exactas, Físicas y Naturales, 1934, 7–30.

Saraiva, L., and H. Leitão (eds.), *The Practice of Mathematics in Portugal*, Coimbra: Por Ordem da Universidad, 2004.

Sarnowsky, J., "Ein Albert von Sachsen zugeschriebener Physikkommentar aus der Mitte des 14. Jahrhunderts", *Medioevo. Rivista di Storia della Filosofia Medievale*, 27 (2002), 449–474.

Sarnowsky, J., *Die aristotelisch–scholastische Theorie der Bewegung. Studien zum Kommentar Alberts von Sachsen zur Physik des Aristoteles,* Münster: Aschendorff, 1989.

Schmitt, C.B., "The Faculty of Arts in Pisa at the Time of Galileo", *Physis*, 14 (1972), 243–272.

Sezgin, F. (ed.), *Hero of Alexandria in the Arabic Tradition: Texts and Studies*, Frankfurt a.M.: Institute for the History of Arabic–Islamic Science at the Johann Wolfgang Goethe Universität, 2001.

Singer, C. (ed.), *A History of Technolog*y, 8 vols., Oxford: Clarendon Press, 1954–1984.

Sirera, J.L.M. (ed.), *Symposium Arquímedes* (Preprint 239), Berlin: Max Planck Institut für Wissenschaftsgeschichte, 2003.

Solana, M., *Historia de la Filosofia Española*, 3 vols., Madrid: Asociación Española para el Progreso de las Ciencias, 1940–1941.

Sorabji, R. (ed.), *Philoponus and the Rejection of Aristotelian Science*, London: Duckworth, 1987.

Souffrin, P., "Motion on Inclined Planes and Liquids in Galileo's Earlier *De motu*", in Napolitani and Souffrin (eds.), *Medieval and Classical Traditions*, 107–114.

Souffrin, P., "Sur l'histoire du concept de vitesse. Galilée et la tradition scolastique", in Ribémont (ed.), *Le temps, sa mesure et sa perception au Moyen Âge*, 243–268.

Souffrin, P., "*Velocitas totalis*. Enquête sur une pseudo-dénomination médiévale", in Caroti and Souffrin (eds.), *La nouvelle physique du XIVe siècle*, 251–275.

Spivakovsky, E., *Son of the Alambra. Don Diego Hurtado de Mendoza, 1504–1575*, Austin: University of Texas Press, 1970.

Sylla, E., "Compounding Ratios: Bradwardine, Oresme, and the first edition of Newton's *Principia*", in Mendelsohn (ed.), *Transformation and Tradition*, 11–43.

Sylla, E., "Medieval Concepts of the Latitude of Forms: The Oxford Calculators", *Archives d'histoire doctrinale et littéraire du Moyen Âge*, 40 (1973), 223–283.

Sylla, E., *The Oxford Calculators and the Mathematics of Motion (1320–1350). Physics and Measurement by Latitudes*, Harvard University dissertation (1970), published in Harvard University Dissertations in History of Science, New York: Garland, 1991.

Taylor, R., "Architecture and Magic", in Fraser, Hibbard and Levine (eds.), *Essays in the History of Architecture*, 81–110.

Taylor, R., *Arquitectura y Magia. Consideracione sobre la idea de El Escorial,* Madrid: Siruela, 1992.

Thijssen, J.M.M.H., "The Buridan School Reassessed. John Buridan and Albert of Saxony", *Vivarium* 42 (2004), 18–42.

Thuillier, P., "Del arte a la ciencia: El descubrimiento de la trayectoria parabólica", in *De Arquímedes a Einstein. Las caras ocultas de la investigación científica*, Madrid: Alianza, 1990, vol. I, 255–282.

Thurot, F.C.E., "Recherches historiques sur le principe d'Archimède de 1565 à 1663", *Revue archéologique*, 1869, 284–299.

Trapp, D., "Peter Ceffons of Clairvaux", *Recherches de théologie ancienne et médiévale*, 24 (1957), 101–154.

Tribute to Galileo in Padua. Atti delle celebrazioni galileiane (1592–1992), Trieste: Lint, 1995.

Turner, H., "Recasting Establishment History. 'The Roots of Science' " (2001), available online at http://www.spc.org.nz/8.pdf (16.01.2004).

Vailati, G., "Il principio delle velocità virtuali da Aristotele a Erone d'Alessandria", *Atti della R. Accademia delle Scienze di Torino*, vol. XXXIII, 1897, here in Vailati, *Scritti*, 91–106.

Vailati, G., *Scritti*, ed. M. Quaranta, Sala Bolognese: Forni, 1987.

Van de Vyver, O., "Jan Ciermans (Pascasio Cosmander) 1602–1648. Wiskundige en vestingbouwer", *Communications from the Seminar in the History of Science at the University of Louvain*, 7 (1975), 8 pp.

Van de Vyver, O., "L'école de mathématiques des Jésuites de la province flandro-belge au XVII[e] siècle", *Archivum Historicum Societatis Iesu*, 49 (1980), 265–278.

Van de Vyver, O., "Lettres de J. Ch. de la Faille, S.J., Cosmographe du roi à Madrid, à M.F. van Langren, cosmographe du roi à Bruxelles, 1634–1645", *Archivum Historicum Societatis Iesu*, 46 (1977), 72–183.

Van Looy, H., "Chronologie et analyse des manuscrits mathématiques de Grégoire de Saint-Vincent (1584–1667)", *Archivum Historicum Societatis Iesu*, 49 (1980), 279–303.

Vanpaemel, G., "Jesuit Science in the Spanish Netherlands", in Feingold (ed.), *Jesuit Science and the Republic of Letters*, 389–432.

Vanpaemel, G., *Echo's van een wetenschappelijke revolutie. De mechanistische natuurwetenschap aan de Leuvense Artesfaculteit (1650–1797)*, Brussels: Koninklijke academie voor wetenschappen, letteren en schone kunsten van België, 1986.

Vernet, J., and R. Parés (eds.), *La ciencia en la història dels Països Catalans*, 3 vols., Valencia: Institut d'Estudis Catalans–Universitat de Valencia, 2004 (vols. 2 and 3, in press).

Vescovini, G.F., *"Arti" e filosofia nel secolo XIV. Studi sulla tradizione aristotelica et i "moderni",* Firenze: Nuovedizioni Enrico Vallecchi, 1983.

Vicente Maroto, M.I., and M. Esteban Piñeiro, M., *Aspectos de la ciencia aplicada en la España del Siglo de Oro*, Valladolid: Junta de Castilla y León, 1991.

Vilain, C., "Mouvement droit–mouvement courbe (I) et (II)", *Archives internationales d'histoire des sciences*, 139 (1997), 271–294, and 141 (1998), 255–268.

Wallace, W.A., *Galileo and his Sources. The Heritage of the Collegio Romano in Galileo's Science,* Princeton: Princeton University Press, 1984.

Weisheipl, J.A., "Curriculum of the Faculty of Arts at Oxford in the Early Fourteenth Century", *Mediaeval Studies*, 26 (1964), 143–185.

Weisheipl, J.A., "Ockham and Some Mertonians", *Mediaeval Studies*, 30 (1968), 163–213.

Weisheipl, J.A., "The Principle *Omne quod movetur ab alio movetur* in Medieval Physics", *Isis*, 56 (1965), 26–45.

Westfall, R.S., *The Construction of Modern Science. Mechanisms and Mechanics*, Cambridge: Cambridge University Press, 1971.

White, L., "Pumps and Pendula: Galileo and Technology", in Golino (ed.), *Galileo Reappraised*, 96–110.

Whitney, E., "Paradise Restored: The Mechanical Arts from Antiquity through the Thirteenth Century", *Transactions of the American Philosophical Society*, Part 1, 80 (1990).

Wielgus, S., "Ausgewählte Probleme der '*Quaestiones in libros physicorum*' des Benedikt Hesse von Krakau", in Pluta (ed.), *Die Philosophie im 14. und 15. Jahrhundert*, 85–101.

Wilkinson–Zerner, C., *Architect to Philip II of Spain*, New Haven: Yale University Press, 1993.

Wilson, W., "On the Origins of Horrocks's Lunar Theory", *Journal for the History of Astronomy*, 18 (1987), 77–94.

Wolff, M., *Fallgesetz und Massebegriff. Zwei wissenschaftshistorische Untersuchungen zur Kosmologie des Johannes Philoponus,* Berlin: de Gruyter, 1971.

Wolff, M., *Geschichte der Impetustheorie. Untersuchungen zum Ursprung der klassischen Mechanik,* Frankfurt a.M.: Suhrkamp, 1978.

Yoder, J.G., *Unrolling Time: Christiaan Huygens and the Mathematization of Nature*, Cambridge: Cambridge University Press, 1988.

Zarco Cuevas, J., *Catálogo de los manuscritos castellanos de la Real Biblioteca de El Escorial*, 3 vols., Madrid: Imp. Helénica & Real Monasterio, 1924.

Zimmermann, A. (ed.), *Orientalische Kultur und Europäisches Mittelalter*, Berlin and New York: de Gruyter, 1985.

Zimmermann, F., "Philoponus's Impetus Theory in the Arabic tradition", in Sorabji (ed.) *Philoponus and the Rejection of Aristotelian Science*, 121–129.

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