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# MECHANICS

*Domenico Bertoloni Meli*

This chapter is devoted to mechanics in the sixteenth and seventeenth centuries. Following a distinction traceable at least to Hero of Alexandria (first century) and Pappus of Alexandria (third century), mechanics can be divided into rational and practical (or applied). The former is a mathematical science normally proceeding by demonstration, the latter a manual art with practical aims. Here I privilege rational over practical mechanics, which is discussed elsewhere in this volume (see Bennett, Chapter 27).[1]

A major problem with writing a history of mechanics during this period concerns the changing disciplinary boundaries and meaning of the term "mechanics." Traditionally, mechanics had dealt with the mathematical science of simple machines and the equilibrium of bodies. In the second half of the seventeenth century, however, mechanics became increasingly associated with the science of motion. Therefore, in dealing with an earlier period, it is useful to chart not simply the transformations of mechanics as it was understood before the second half of the seventeenth century but also the relevant transformations in the science of motion that belong more properly to natural philosophy.

Mechanics and natural philosophy differed widely intellectually, institutionally, and socially in the period covered by this chapter. Even rational mechanics retained a practical and engineering component but it was also progressively gaining a higher intellectual status with the editions of major works from antiquity and with a renewed emphasis on its utility; initially its role in the universities was at best marginal, however. By contrast, natural philosophy had been a major academic discipline for centuries and had closer links to theology than to the practical arts. It is therefore necessary to chart the changing contours and domains of mechanics by paying attention to how scholars at the time understood it, lest one write a history of a fictitious discipline by projecting a modern vision of that discipline onto the past.[2]

This chapter starts by examining the impact of the recovery of ancient and medieval learning both in what was understood to belong to mechanics proper and in those portions of natural philosophy dealing with motion. The critical editions and the assimilation of those sources in the sixteenth century led to a transformation of mechanics on many levels.[3] I then move to a brief characterization of some of the leading scholars on motion and mechanics in the sixteenth century that culminates with the work of Galileo Galilei (1564–1642). The interplay among mechanics, the philosophical study of motion, and quantitative experiment is at the center of my reading of Galileo's groping toward a new mathematical science of local motion.

The work of Galileo marks a turning point that organizes the chapter. Although disciplinary contours did not change overnight, a shift occurred between the first and second parts of the seventeenth century. I shall use the debates and controversies triggered by Galileo's major works as a guide to later developments. Moreover, more recent studies have emphasized that in his effort to formulate a new science of motion, Galileo remained enmeshed in portions of the old worldview. By relying on his work, the generation after him could more easily free itself from the past. This is an additional reason that justifies my partition.[4]

Philosophers who studied motion were almost invariably professors at a university or Jesuit college. Practitioners of mechanics, however, had a more varied professional profile. Niccolò Tartaglia (1506–1557) was a teacher of mathematics at the University of Venice who never attained high social standing. The Urbino mathematician Federico Commandino (1509–1575) was a refined humanist scholar, held a medical degree, and moved around papal and princely courts, especially the Farnese. His pupil Guidobaldo dal Monte (1545–1607) was the brother of a cardinal and a marquis himself, with close ties to the della Rovere in Urbino and the Medici in Florence. In his youth, dal Monte had been a military man and in the late 1580s became superintendent

[1] Pappus, *Mathematicae colletiones*, translated by Paul ver Eecke as *La collection mathématique* (Paris: Desclée de Brouwer, 1933), p. 810. See also Isaac Newton, *Principia*, new translation by I. Bernard Cohen and Anne Whitman (Berkeley: University of California Press, 1999), pp. 381–2. On this distinction, see G. A. Ferrari, "La meccanica 'allargata'," in *La scienza ellenistica*, ed. Gabriele Giannantoni and Mario Vegetti ([Naples]: Bibliopolis, 1984), pp. 225–96.

I wish to thank Karin Eckholm and Allen Shotwell for their helpful comments on an earlier draft of this work.

[2] For an example of such an anachronistic projection, see Marshall Clagett, *The Science of Mechanics in the Middle Ages* (Madison: University of Wisconsin Press, 1959). An excellent account that is sensitive to these concerns is John E. Murdoch and Edith D. Sylla, "The Science of Motion," in *Science in the Middle Ages*, ed. David C. Lindberg (Chicago: University of Chicago Press, 1978), pp. 206–64.

[3] A useful source is *Mechanics in Sixteenth-Century Italy*, translated and annotated by Stillman Drake and I. E. Drabkin (Madison: University of Wisconsin Press, 1969) on which the following two sections rely. See also the essay review by Charles B. Schmitt, "A Fresh Look at Mechanics in 16th-Century Italy," *Studies in the History and Philosophy of Science*, 1 (1970), 161–75.

[4] For details, see Peter Damerow, Gideon Freudenthal, Peter McLaughlin, and Jürgen Renn, *Exploring the Limits of Preclassical Mechanics* (New York: Springer, 2004).

of Tuscan fortifications. The Venetian Giovanni Battista Benedetti (1530–1590), a student of Tartaglia, became court mathematician first at Parma under the Farnese and then at Turin under the Savoy. The Dutch mathematician Simon Stevin (1548–1620) was a military man and an engineer who was made quartermaster of the army of the Low Countries in 1604. Many of these men had an interest in practical as well as theoretical matters in mechanics.[5]

## MECHANICAL TRADITIONS

The main works on mechanics can be associated with a number of texts and traditions beginning shortly after Aristotle's death. I identify four main traditions – those of pseudo-Aristotle, Archimedes, Alexandria (especially Pappus) – and the science of weights.

The first of these traditions is associated with *Quaestiones mechanicae* (Mechanical Problems), traditionally attributed to Aristotle (384–322 B.C.E.) but now considered to be an early product of his school. The work deals mostly with applications of the doctrines of the lever, which depends upon the balance, and of the balance, whose properties are analyzed by imagining that it rotates around its fulcrum so as to describe a circle; hence the use of motion in the study of equilibrium and the strange idea that the properties of the balance depend on the circle. The author claimed that nearly all mechanical problems depend on the lever and in some cases he provided some form of explanation of how some machines, such as the windlass, for example, operate and relate to the circle. One cannot say that there is a systematic and rigorous attempt in this direction, however, but only a number of appropriate remarks. A few passages deal with seafaring, others with the resistance of beams or the force exerted by a moving body on a wedge (later known as the force of percussion). In the age of the printing press, the text went through many editions and translations, often with valuable commentaries, starting in 1497.[6]

Archimedes (287–212 B.C.E.) wrote two major works on mechanics, *De centris gravium* (On the Equilibrium of Planes) and *De insidentibus aquae* (On Floating Bodies). Both survived in several manuscript copies and were first published in 1543 by Tartaglia.[7] Superior editions were later produced by

[5] Mario Biagioli, "The Social Status of Italian Mathematicians, 1450–1600," *History of Science*, 27 (1989), 41–95; Paul L. Rose, *The Italian Renaissance of Mathematics* (Geneva: Droz, 1975); and Simon Stevin, *The Principal Works of Simon Stevin*, 5 vol. (Amsterdam: Swets and Zeitlinger, 1955), 1: 1–24.

[6] Paul L. Rose and Stillman Drake, "The pseudo-Aristotelian *Questions of Mechanics* in Renaissance Culture," *Studies in the Renaissance*, 18 (1971), 65–104.

[7] Niccolò Tartaglia, ed., *Opera Archimedis* (Venice: Per Venturinum Ruffinellum, 1543). Tartaglia's edition contains only Book I of *Floating Bodies*. The chief source on the fortune of the Archimedean corpus through to the Renaissance is Marshall Clagett, *Archimedes in the Middle Ages*, 5 vols. (vol. 1, Madison: University of Wisconsin Press, 1964; vols. 2–5, Philadelphia: American Philosophical Society, 1976–84).



Commandino and his student dal Monte.[8] In *On the Equilibrium of Planes* Archimedes produced an axiomatic theory of the balance, thus introducing into mechanics a style based on pure mathematics that constituted a model for many later works. He also determined the centers of gravity of several plane figures.[9] In the sixteenth century, Commandino and Galileo, among others, extended those investigations. *On Floating Bodies* deals with hydrostatics and contains the celebrated statement that a body in a fluid receives an upward thrust equal to the weight of the volume of the displaced fluid, known as Archimedes' principle. The treatise provides equilibrium conditions for bodies with different shapes in a fluid. Archimedean mechanics, whether dealing with the balance or with bodies in a fluid, was based on equilibrium rather than motion.

Hero wrote *Spiritalia* (Pneumatics)[10] and a treatise on mechanics that was known only in part through references in Pappus's *Collectiones mathematicae* (Mathematical Collections), whose eighth book is devoted to mechanics. Following Hero, Pappus argued that all machines could be reduced to the five simple machines (balance or lever, pulley, wheel and axle, wedge, and screw), and the last four in turn could be reduced to the balance. In some instances, Pappus tried to show by means of a geometrical construction how a machine could be reduced to the balance. He sought to determine in this way the equilibrium conditions for a weight on an inclined plane, for example. Although his solution was problematic, it was an attempt to establish mechanics on a solid basis and clear first principles. In 1588, dal Monte supervised a printing of a Latin translation by Commandino from an imperfect manuscript of Pappus's text.[11]

Lastly, during the thirteenth century, several Latin authors contributed treatises in the tradition known as *Scientia de ponderibus* (Science of Weights) dealing with the equilibrium of bodies. The names of many of those authors have not survived, the main exception being Jordanus of Nemore (fl. early thirteenth century), who wrote a *Liber de ponderibus* (Book on Weights). After the Nuremberg *editio princeps* by the German cosmographer Petrus Apianus (Apian, 1495–1552), a new edition was published in Venice in 1565 from the papers of Tartaglia, who had already included some results in his own previous publications. Although works of this tradition generally lack the rigor and elegance found in Archimedean treatises, they contain original notions and valuable results. For example, the treatise *De ratione ponderis* (On the Theory of Weight) did not attempt to rely systematically on the lever

[8] Frederico Commandino, *Archimedis de iis quae vehuntur in aqua libri duo* (Bologna: Ex officina A. Benacii, 1565); and Guidobaldo dal Monte, *In duos Archimedis aequiponderantium libros paraphrasis* (Pesaro: Apud Hieronymum Concordiam, 1588).

[9] Archimedes did not define the expression "center of gravity." Later scholars defined it as that point such that a body suspended from it remains in equilibrium.

[10] Marie Boas, "Hero's *Pneumatica*: A Study of Its Transmission and Influence," *Isis*, 40 (1949), 38–48.

[11] Pappus, *Collectiones mathematicae* (Pesaro: Concordia, 1588); and L. Passalacqua, "Le Collezioni di Pappo," *Bollettino di Storia delle Scienze Matematiche*, 14 (1994), 91–156.

but provided a more satisfactory solution to the problem of equilibrium of a weight on an inclined plane than that of Pappus.[12]

## STUDIES ON MOTION

Texts dealing with the problem of motion go back to Aristotle and the host of commentators from antiquity to the Middle Ages. My chief concern here is not with all the topics pertinent to the study of motion in Aristotle and his commentators but only with those aspects relevant to the sixteenth-century development of the science of motion. Even within this limitation, my account remains highly selective.

The analysis of motion occupies a central position in Aristotle's study of nature, especially in *Physica* and *De caelo* (On the Heavens). By "motion" Aristotle understood virtually all change occurring in nature, whereas by "local motion" he meant something closer to our understanding of the term. Here and throughout I shall simply refer to "motion," meaning "local motion." Aristotelians drew a basic distinction between natural and violent motions. The former is the downward motion of heavy bodies, endowed with gravity, or the upward motion of light bodies, endowed with levity.[13] The latter is exemplified by the motion of projectiles. With regard to natural motion, Aristotle argued that the speed of a falling body is proportional to its weight and is the inverse of the resistance of the medium.[14] As a consequence, when the resistance tends to zero, as in a void, the velocity becomes infinite, a paradoxical result used by Aristotle to refute the existence of a void. With regard to violent motion, Aristotle argued that after the moving body has left the projector, the body is moved by the surrounding medium. This view derived from his principles that everything that moves is moved by something else and that the mover must be in contact with the moved body. These principles imply that a body set in motion requires an external cause to continue its motion, an opinion that was debated until the seventeenth century.

Starting in late antiquity, several commentators examined Aristotle's views on motion with a critical eye. Themistius (317–387), for example, argued that all bodies would fall in a void with the same speed and that this speed would be finite, not infinite, as Aristotle had claimed. Simplicius (d. after 533) often defended Aristotle's conclusions, such as the denial of the existence of a void,

[12] Ernest A. Moody and Marshall Clagett, eds., *The Medieval Science of Weights* (Madison: University of Wisconsin Press, 1960); J. E. Brown, "The Science of Weights," in Lindberg, ed., *Science in the Middle Ages*, pp. 179–205; Jordanus Nemorarius, *Liber de ponderibus*, ed. Petrus Apianus (Nuremberg: Iohannes Petreius, 1533); and Nemorarius, *Opusculum de ponderositate* (Venice: Curtius Troianus, 1565). On Tartaglia's publications, see the section on "Motion and Mechanics" in this chapter.

[13] Levity was understood by Aristotle as an independent quality, not as being caused by extrusion of a specifically lighter body in a specifically heavier medium, on an Archimedean model.

[14] Murdoch and Sylla, "The Science of Motion," p. 224.



but he was critical of Aristotle's proofs. Philoponus (d. ca. 570) was the most thorough ancient critic of Aristotle's physics and especially of his views on motion. He admitted that motion in a void could occur and argued that a medium would increase the time of fall of a body over the time of fall in a void. Philoponus also argued against Aristotle that according to experience the time of fall of two bodies differing greatly in weight is very small. Greek and Latin editions of the works of Themistius, Simplicius, and Philoponus appeared in print in the first half of the sixteenth century and contributed to an erosion of confidence in Aristotle's doctrines coming from a learned and ancient tradition of scholarship. Galileo was familiar with all three of them.[15]

In the Islamic world, one author in particular needs to be singled out here, the Spanish philosopher Avempace, or Ibn Bājja (d. 1138), who was sympathetic to Philoponus. His views became known in the West through the citations and criticisms of Averroes (Ibn Rushd, 1126–1198), whose works were well known in the Latin West.[16]

In medieval Europe, Aristotle became the cornerstone of university education, and the number of his commentators grew considerably. Here I wish to mention the Parisian John Buridan (ca. 1295–ca. 1358) and a group of authors known as *calculatores*. Buridan addressed the problem of projectile motion in a fashion different from Aristotle's. Instead of arguing that the medium had a role in propelling the projectile, Buridan claimed that the projectile moved because of a quality called *impetus* transmitted by the projector. His notion remained in vogue until the seventeenth century. The fourteenth-century study of motion witnessed a remarkable increase in the use of logic and mathematics, especially the theory of proportions. The protagonists of this tradition included Thomas Bradwardine (d. 1349) and Richard Swineshead (fl. 1340–1355) at Oxford and Nicole Oresme (ca. 1325–1382) at Paris. In addition to showing that motion could be treated mathematically, they developed sophisticated geometrical treatments, coined a refined terminology, and attained important results, such as the mean-speed theorem.[17]

However, there are important qualifications. The methods of inquiry developed by the *calculatores* were applied not only to local motion but also to a wide range of topics spanning medicine, theology, and natural philosophy. Secondly, with only one known exception, those methods of inquiry were applied to ideal imaginary entities, not to nature, in the style of a logical exercise.[18] Despite the heavy use of mathematics, the arrangement

[15] On the three Greek commentators, see Paolo Galluzzi, *Momento: Studi galileiani* (Rome: Edizioni dell'Ateneo, 1979), pp. 98–106.

[16] The classic work here is Ernest Moody, "Galileo and Avempace: The Dynamics of the Leaning Tower Experiment," *Journal of the History of Ideas*, 12 (1951), 163–93, 375–422.

[17] The theorem states that the space traversed with a uniformly accelerated or decelerated motion is the same as that traversed with a uniform speed equal to the mean degree of speed, namely the degree of speed in the mean instant of time.

[18] The exception was the Spanish Dominican Domingo de Soto (1495–1560).

of the surviving codices suggests that these works were considered part of natural philosophy rather than the mathematical disciplines. Texts from this tradition were published in about 1500.[19]

## MOTION AND MECHANICS IN THE SIXTEENTH CENTURY

Despite their brevity, the previous sections show that in the sixteenth century a number of key works on mechanics and motion became available in print. Never before had scholars been able to access such a wealth of intellectual resources on these topics with such ease. This section presents the main works by some of the leading figures in the sixteenth century. Whereas some, such as Tartaglia and Benedetti, sought to build bridges between mechanics and motion, others, such as dal Monte and Stevin, conceived the two domains as separate and saw little hope for a rapprochement.[20]

Tartaglia was a major figure in more ways than one. Besides his editorial work mentioned earlier, he published *La nova scientia* (The New Science, 1537) and *Quesiti et inventioni diverse* (Various Questions and Inventions, 1546). These are composite works dealing largely with the mathematical disciplines, such as gunnery, the science of weights, and the pseudo-Aristotelian *Quaestiones*, but they also include issues dealing with gunpowder and other military matters. Tartaglia sought to determine the trajectory of a projectile shot at different angles and claimed from dubious assumptions that the longest range was shot at 45° above the horizon. Several treatises and manuals on ballistics followed in the sixteenth and seventeenth centuries.[21]

In yet another work, *La travagliata inventione* (The Troubled Invention, 1551), Tartaglia hinted at a proportion applicable to bodies falling in water, suggesting that bodies sink faster in proportion to how much they are specifically heavier than water.[22] The same idea of extending Archimedean hydrostatics to account for motion was developed by Benedetti in a number of

[19] Christopher Lewis, *The Merton Tradition and Kinematics in Late Sixteenth and Early Seventeenth Century Italy* (Padua: Editrice Antenore, 1980).

[20] Other relevant figures include the Padua professor of mathematics Giuseppe Moletti (1531–1588), on whom see Walter R. Laird, *The Unfinished Mechanics of Giuseppe Moletti* (Toronto: University of Toronto Press, 2000), and the physician Girolamo Cardano (1551–1576), whose work in mechanics still awaits systematic investigation. Important works on sixteenth-century mechanics include Walter R. Laird, "The Scope of Renaissance Mechanics," *Osiris*, 2nd ser., 2 (1986), 43–68; and Laird, "Patronage of Mechanics and Theories of Impact in Sixteenth-Century Italy," in *Patronage and Institutions: Science, Technology, and Medicine at the European Court, 1500–1750*, ed. Bruce Moran (Rochester, N.Y.: Boydell Press, 1991), pp. 51–66.

[21] A. Rupert Hall, *Ballistics in the Seventeenth Century* (New York: Harper, 1969); and Serafina Cuomo, "Shooting by the Book: Notes on Niccolò Tartaglia's *Nova Scientia*," *History of Science*, 35 (1997), 155–88. Extensive translations from Tartaglia's works can be found in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, pp. 63–143.

[22] Clagett, *Archimedes in the Middle Ages*, 3: 3, 574.



works published in the 1550s dealing with falling bodies. One of them bears the none too subtle title *Demonstratio proportionum motuum localium contra Aristotilem et omnes philosophos* (Demonstration of the Proportions of Local Motions, against Aristotle and All Philosophers, 1553).[23] Benedetti expanded his reflection in his magnum opus, *Diversarum speculationum mathematicarum et physicarum liber* (Book of Different Speculations on Mathematical and Physical Matters, 1585), a composite work dealing with mechanics and the pseudo-Aristotelian *Quaestiones*, criticisms of Aristotle's views about motion, and including excerpts from his (Benedetti's) correspondence.[24]

Archimedes was a major source for sixteenth-century scholars of mechanics, but dal Monte had in addition a predilection for Pappus, whose work he knew before 1588 through Commandino's manuscripts. The marquis published the main work of its time on mechanics, *Mechanicorum liber* (Book of Mechanics, 1577), where he examined all the simple machines and, following Pappus, tried to show that they work in accordance with the principle of the balance, as if there were balances in disguise that had to be unmasked. He was concerned not just with results but also with foundations and proofs; being able to go back to the balance meant that he could rely on Archimedes' work on the equilibrium of planes and therefore solve the problem of foundations. The marquis loathed the medieval science of weights and those who worked in that tradition, including Tartaglia, because their proofs lacked the rigor found in the texts from antiquity. In some respects, dal Monte saw an unbridgeable gap between equilibrium and motion. The science of equilibrium could be formulated in mathematical fashion because of its regularity, whereas motion was subject to so many vagaries that mathematics generally remained out of the picture. In dealing with the wedge, however, dal Monte hinted at a proportion that involved motion: He argued that a body hitting a wedge produces a greater effect the greater the height from which it falls. In this way, height and speed of fall were linked to the effect they produce. This problem of determining the force of percussion was part of the classical repertoire from the time of the pseudo-Aristotelian *Quaestiones* and was later discussed by Galileo and other seventeenth-century mathematicians.[25]

Despite his geographical distance from Italian mechanicians, Stevin, too, relied on the editions of Commandino, much as did dal Monte and a host of other contemporary mathematicians. Stevin's main work in mechanics was a collection of treatises with separate title pages that was published in Leiden by Christoffel Plantijn in 1586. They include *De Beghinseln der Weeghconst* (The

[23] Carlo Maccagni, *Le speculazioni giovanili "de motu" di Giovanni Battista Benedetti* (Pisa: Domus Galilaeana, 1967).

[24] Extensive translations can be found in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, pp. 166–237.

[25] Also in this case extended translations can be found in Drake and Drabkin, *Mechanics in Sixteenth-Century Italy*, pp. 241–328; and Domenico Bertoloni Meli, "Guidobaldo dal Monte and the Archimedean Revival," *Nuncius*, 7, no. 1 (1992), 3–34.

Elements of the Art of Weighing), *De Weeghdaet* (The Practice of Weighing), and *De Behinselen de Waterwichts* (The Elements of Hydrostatics).[26] At the end of the last work, there are two short additions, *Preamble of the Practice of Hydrostatics* and *Appendix to the Art of Weighing*.[27] Stevin also performed experiments by dropping heavy bodies from high places, but his work focused primarily on equilibrium, and his fame rests on his extension of Archimedean hydrostatics and his brilliant solution to the problem of the inclined plane.

## GALILEO

Galileo's contributions to the mathematical disciplines and philosophy range from his telescopic findings to his onslaught on the Peripatetic school. Yet, it was the science of motion that he considered to be his most treasured investigation and that represents his most sustained and remarkable intellectual effort. Galileo's main work on motion and mechanics falls into three periods: at Pisa, Padua, and Florence. During his three years as professor of mathematics at the University of Pisa (1589–92), he probably drafted a dialogue, an essay, and a few fragments on motion, all collectively known as *De motu antiquiora* (The Older [Manuscripts] on Motion). All of this material remained unpublished at the time. As professor of mathematics at the University of Padua (1592–1610), Galileo worked intensively on the science of motion and the science of the resistance of materials, and he composed a short tract for his university lectures called *Le mecaniche* (On Mechanics). In this second period also, his works remained unpublished at the time. Lastly, after his return to Florence in 1610, Galileo started publishing on mechanical subjects, first with a treatise on hydrostatics, *Discorso intorno alle cose, che stanno in sù l'acqua, ò che in quella si muouono* (Discourse on Bodies on Water, or that Move in It, 1612), then with his masterpieces, the *Dialogo sopra i due massimi sistemi del mondo, Tolemaico e Copernicano* (Dialogue Concerning the Two Chief World Systems, Ptolemaic and Copernican, 1632) and the *Discorsi e dimostrazioni matematiche intorno a due nuove scienze* (Discourses and Mathematical Demonstrations about Two New Sciences, 1638).[28]

In *De motu antiquiora*, Galileo sought to formulate a science of motion by extending Archimedean hydrostatics, arguing that the speed of a body falling in a medium is proportional to the difference in specific density between the body and the medium. This means that, apart from an initial

[26] Stevin, *Works*, vol. 1, *De Beghinselen der Weeghconst*, pp. 35–285; *De Weeghdaet*, pp. 287–373; *De Behinselen des Waterwichts*, pp. 375–483.

[27] Stevin, *Works*, vol. 1, *Anvang der Waterwichtdaet*, pp. 484–501; *Anhang van de Weeghconst*, pp. 503–521.

[28] Several scholars have investigated Galileo's reflections on motion, from Alexandre Koyré and Winifred Wisan to Paolo Galluzzi and Enrico Giusti. For an excellent bibliography, see Damerow et al., *Exploring the Limits*.



period, the speed of falling bodies is constant. Although there are some similarities to Benedetti's work, it is unclear whether Galileo knew it at that time. In addition, following Pappus's and dal Monte's approach, Galileo attempted to use the balance and, more successfully, the inclined plane to account for hydrostatics. Even in this early work, Galileo showed familiarity with a number of authors we have already encountered, from Themistius and Philoponus to Avempace and Avicenna (Ibn Sina, 980–1037). But other more recent and geographically closer sources require attention, too. The University of Pisa professors of philosophy Girolamo Borro (1512–1592) and Francesco Buonamici (1533–1603) engaged in a dispute about motion that lasted for several years and covered the entire period beginning in 1580 from Galileo's education at Pisa to his teaching at the university. In *De motu gravium et levium* (On the Motion of Heavy and Light Bodies, 1576), Borro sided with Averroes and referred to experiments where heavy bodies were dropped from a high window. In his huge *De motu* (On Motion, completed in 1587 but first published in 1591), Buonamici defended Simplicius. Moreover, Buonamici attacked the views of more recent mathematicians, who defended an Archimedean approach. Their dispute was probably not a private affair but rather spilled over into university life, including lectures and the annual series of public disputations known as the *circuli*. A few references point to that dispute as the immediate context for *De motu antiquiora*.[29] At about this time, Galileo embarked on an extended study of philosophy on the basis of lecture notes originating from the chief Jesuit school, the Collegio Romano, probably to strengthen his knowledge of philosophy as well as his dialectical skills.[30]

In *De motu antiquiora* there are several references to experiments, including one of dropping weights from a high tower, probably the leaning tower at Pisa. Overall, however, the experiments performed by Galileo did not conform to his expectations. For example, Galileo investigated the inclined plane and believed he had determined the relationship between its inclination and the (constant!) speed of a body falling along it. By combining this result with his buoyancy theory of fall, Galileo sought to find a plane with the

[29] Michele Camerota and Mario Helbing, "Galileo and Pisan Aristotelianism: Galileo's *De motu antiquiora* and the *Quaestiones de motu elementorum* of the Pisan Professors," *Early Science and Medicine*, 5 (2000), 319–65; Mario O. Helbing, *La filosofia de Francesco Buonamici* (Pisa: Nistri-Lischi, 1989), chap. 6; and Charles B. Schmitt, "The Faculty of Arts at Pisa at the Time of Galileo," *Physis*, 14 (1972), 243–72.

[30] William Wallace, *Galileo and His Sources* (Princeton, N.J.: Princeton University Press, 1984). At pp. 91–2, Wallace suggests that the lecture notes were sent by Christophorus Clavius (1537–1612), professor of mathematics at the Collegio Romano, to Galileo in connection with a dispute over the center of gravity, but evidence for his claim is lacking. Lecture notes often circulated at the time, and Galileo may have obtained them from a student at the Collegio. See Corrado Dollo, "Galilei e la fisica del Collegio Romano," *Giornale Critico della Filosofia Italiana*, 71 (1992), 161–201. For a comparison between the Padua philosopher Jacopo Zabarella (1533–1589) and Galileo, see the classic by Charles B. Schmitt, "Experience and Experiment: A comparison of Zabarella's views with Galileo's in *De motu*," *Studies in the Renaissance*, 16 (1969), 80–138.

appropriate inclination whereby a body would fall along it in the same time as another body of different material falls along the vertical. His attempt failed but may have been at the root of his later findings both that falling bodies accelerate and that the acceleration is the same for all bodies.[31] By exploring motion along inclined planes, Galileo came to appreciate that a body needs no force to be set in motion along a plane with zero inclination, by which he meant the horizon. This thought remained one of the characteristic features of his reflections on motion.[32] About 1592 or slightly earlier, Galileo performed some important experiments with his mentor dal Monte. They threw inked balls across an inclined plane and found that their trajectories were symmetrical, resembling a hyperbola or parabola. A similar curve was described by a chain hanging from two nails fixed in a wall. A reference to that experiment can be found in the *Discorsi,* but it is unclear what the two mathematicians would have made of the result in 1592.[33]

Two differences spring to mind when comparing Galileo's early speculations with the works discussed in the previous section: the deep interplay with philosophy and the role of experiments. His experiment of dropping heavy bodies from high places was associated with the philosophical dispute at Pisa, but it also went hand-in-hand with the practice of performing trials that was typical of the mathematical disciplines, from weighing and surveying to music. Those trials would have been familiar to Galileo, who had started his career in mechanics with a short essay on accuracy in weighing and whose father was a musician (see Mancosu, Chapter 25, this volume).[34] Other experiments, however, even if they were problematic and unsuccessful, such as the one with spheres of different materials going down inclined planes with different inclinations, show Galileo seeking regularities in nature by means of contrived experiments, which revealed a sophistication that went beyond contemporary standards.

Between 1592 and 1620, Galileo taught mathematics at the University of Padua, a position, like the previous one at Pisa, he owed to dal Monte's support. At the university, he taught a number of subjects, including fortification and mechanics: His lecture notes show that his course was modeled on dal Monte's *Mechanicorum liber.* Galileo worked on mechanical issues having to do with oars and the size of galleys in collaboration with scholars

[31] Galileo Galilei, *On Motion and on Mechanics,* translated with introductions by Israel E. Drabkin and Stillman Drake (Madison: The University of Wisconsin Press, 1969). At p. 69, Galileo states that "the ratios that we have set down are not observed."

[32] At this stage, in all likelihood Galileo believed that a body set in motion would come naturally to rest. See Drake and Drabkin, *Mechanics in Sixteenth-Century Italy,* p. 379; and Galileo, *On Motion and Mechanics,* pp. 66–7.

[33] Damerow et al., *Exploring the Limits,* pp. 158–64.

[34] Galileo's father was a musician. See Stillman Drake, *Galileo at Work* (Chicago: University of Chicago Press, 1978), pp. 15–17; and Claude V. Palisca, *Humanism in Italian Renaissance Musical Theory* (New Haven, Conn.: Yale University Press, 1985), pp. 265–79. For *La Bilancetta,* see Galileo Galilei, *Opere,* ed. A. Favaro, 20 vols. (Florence: Giunti Barbera, 1890–1909), 1: 215–20; Drake, *Galileo at Work,* pp. 6–7; and Jim A. Bennett, "Practical Geometry and Operative Knowledge," *Configurations,* 6 (1998), 195–222.



and technicians at the Venice Arsenal, the city's chief military and industrial facility. It is partly from his work at the Arsenal that his science of the resistance of materials and of scaling originates.[35] At Padua, Galileo resumed his experimental and mathematical investigations on motion and realized a number of important features about falling, oscillating, and projected bodies. He seized on objects such as the inclined plane and the pendulum to investigate motion and realized that constant speeds are unsuitable for describing free fall because bodies accelerate. This finding led Galileo to some results from the *calculatores* tradition, which enabled him to treat acceleration in an elementary fashion. Both the terminology and the visual tools of representations used by Galileo testify to his reliance on this tradition, despite the fact that his itinerary started elsewhere and included a variety of sources.[36]

Galileo argued that a falling body goes through all the infinitely many degrees of speed, a belief that put considerable strain on the limited mathematical resources of his time. He further believed that, apart from small perturbations caused by air resistance, all bodies accelerate in the same way regardless of their weight or specific gravity. Moreover, Galileo realized that the acceleration is uniform and the spaces traversed are proportional to the squares of the times. He believed further that the oscillations of a pendulum are very nearly isochronous, a claim that is quite accurate for small oscillations but whose inaccuracy increases with the amplitude of the oscillations. Galileo also believed, quite erroneously, that the circle arc described by the bob was the curve of fastest descent. Several decades later, he still thought he could produce a proof of this.[37] Galileo further came to appreciate that a body set in motion on a horizontal plane does not stop as long as all accidental perturbations are removed. He finally realized that horizontal projection and vertical fall are independent and that each can be composed as if the other did not exist. This composition gives rise to parabolic trajectories, as Galileo's experiment with dal Monte (ca. 1592) had suggested. Although the fragmentary manuscript record from this period does not allow a detailed reconstruction of Galileo's intellectual itinerary in all circumstances, in some cases specific results have been achieved.[38]

Galileo's findings, remarkable as they were, did not constitute a science based on evident principles and rigorous proofs. In other words, Galileo had found a series of propositions and relations that needed to be given order and structure. When he realized that the balance could not be used

[35] Jürgen Renn and Matteo Valleriani, "Galileo and the Challenge of the Arsenal," *Nuncius,* 16, no.2 (2001), 481–504.

[36] Edith D. Sylla, "Galileo and the Oxford *Calculatores*: Analytical Languages and the Mean-Speed Theorem for Accelerated Motion," in *Reinterpreting Galileo,* ed. William A. Wallace (Washington, D.C.: The Catholic University of America Press, 1986), pp. 53–108.

[37] Galileo Galilei, *Two New Sciences,* trans. Stillman Drake (Madison: University of Wisconsin Press, 1974), pp. 212–3.

[38] A detailed analysis of this period can be found in Damerow et al., *Exploring the Limits,* chap. 3.

to found a science of motion, he started to look for a suitable axiom or principle to replace, in an Archimedean fashion, Archimedes' axioms in *On the Equilibrium of Planes*. Galileo was not seeking to establish his science on contrived and elaborate experiments but rather on axioms of the same nature as those of Archimedes, who had postulated at the outset of *On the Equilibrium of Planes* that in a balance equal weights at equal distances are in equilibrium, whereas equal weights at unequal distances are not in equilibrium and incline toward the weight at the greater distance. Letters from this period testify to Galileo's long search for new self-evident principles.[39]

Following his spectacular astronomical discoveries concerning Jupiter's moons and other celestial objects Galileo was called to Florence in 1610 as philosopher and mathematician to the Grand Duke of Tuscany, a highly paid position created especially for him.[40] The first area of mechanics on which Galileo worked after his return to Florence was hydrostatics. As part of a dispute with Aristotelian philosophers, Galileo published two editions in 1612 of the *Discorso intorno alle cose, che stanno in sù l'acqua, ò che in quella si muouono*. The philosophers argued that shape was a decisive factor in buoyancy, whereas Galileo followed Archimedes in identifying specific gravity as the decisive factor. In the controversy, Galileo was aided by his former student Benedetto Castelli (ca. 1577–1643), a Benedictine monk who was then professor of mathematics at the University of Pisa. It was Castelli who went to work on water flow and water management with a pioneering mathematical treatise, *Della misura dell'acque correnti* (On the Measurement of Running Waters, 1628). The problems associated with the motion of waters were Castelli's domain, but Galileo also worked on them. He had already expressed views on river flow while at Padua and continued to do so until the 1630s. There are obvious connections between the science of motion and the cluster of issues linked to river flow and water management, commonly referred to as the science of waters. Galileo considered water in a river to be like a body moving down an inclined plane and tried to apply the corresponding rules, with little success.[41]

After a long period of gestation caused by intellectual as well as political and religious matters, in 1632 Galileo published his scientific and literary masterpiece, the *Dialogo*, wherein three interlocutors discuss over four days the merits of the two chief world systems. Galileo's clumsy attempts at covering his Copernican views allowed the book to make it past the censors but

[39] The main letters are to Paolo Sarpi (1552–1623) in 1604 and Luca Valerio (1552–1618) in 1609. They are discussed in Galluzzi, *Momento*, pp. 269–76, 303–7.

[40] The social and intellectual implications of this move are discussed in Mario Biagioli, *Galileo Courtier* (Chicago: University of Chicago Press, 1993).

[41] Richard S. Westfall, "Floods along the Bisenzio: Science and Technology in the Age of Galileo," *Technology and Culture*, 30 (1989), 879–907; and Cesare S. Maffioli, *Out of Galileo: The Science of Waters, 1628–1718* (Rotterdam: Erasmus, 1994).



did not prevent it from being banned by the Inquisition in 1633. Galileo was put under arrest, which was later commuted to house arrest, for vehement suspicion of heresy until the end of his days.[42] The *Dialogo* deals with cosmological matters, but Galileo's defense of Copernicanism relied on the science of motion for the study of the behavior of objects on a moving earth. An important portion of Galileo's strategy was to argue that the motion of the earth would not produce any visible effects on falling or projected bodies on the earth; therefore it would be impossible to determine whether the earth moves in that fashion. His chief argument in favor of Copernicanism was that of tides, which he thought were an effect of the double rotation of the earth on its axis and around the sun. The *Dialogo* contains several passages relevant to the science of motion, but it is primarily in the second day where Galileo discusses relative motion and the effects of the earth's motion. It is in the *Dialogo* that Galileo stated for the first time a number of propositions about falling bodies, such as the odd-number rule or the proportionality between speed and time. Galileo discussed many problems related to the earth's rotation, such as why projectiles, birds, and clouds are not left behind by the earth's rotation, or why bodies on the earth's surface are not projected into the air by its rotation. Moreover, the *Dialogo* contains the first references to the isochronism of the pendulum's oscillations, though not the relation between period and length.[43]

Besides putting Galileo under arrest, the Inquisition prevented him from publishing on any subject. Therefore, the manuscript of his next and final work had to be smuggled out of Italy and published in the Protestant Low Countries by the Elzeviers. When the *Discorsi* appeared in 1638, Galileo was seventy-four years of age and had been working on the problem of motion for about half a century. The book is in the form of a dialogue among the same three personages as the *Dialogo*, but whereas the *Dialogo* was written in a style that imitated the open-ended discussions among the protagonists, the *Discorsi* contained portions structured in a more formal way. The first two days contain many digressions, notably on the nature of the continuum and the physical cause of cohesion, which Galileo believed was caused by infinitely many interstitial vacua. The first new mathematical science is the resistance of materials, whose principles are discussed mainly in day two. Distant roots of these problems can be found in some of the pseudo-Aristotelian *Quaestiones*,[44] but the more immediate context was Galileo's work at the Venice Arsenal. The problem consists in determining the resistance to rupture of a loaded beam of certain dimensions, knowing the resistance to rupture of a similar beam

[42] On the relationships between Galileo and the Church, including the 1616 ban of Copernicanism, see Annibale Fantoli, *Galileo: For Copernicanism and for the Church*, trans. George V. Coyne, S.J. (Vatican City: Vatican Observatory Publications, 1994).

[43] Peter Dear, *Mersenne and the Learning of the Schools* (Ithaca, N.Y.: Cornell University Press, 1988), p. 165.

[44] *Quaestiones*, numbers 14 and 16. I wish to thank Antonio Becchi for having pointed this out to me.

with different dimensions. Obviously, if length is unchanged, a thicker beam resists more than a thinner one, whereas if thickness is unchanged, a longer beam resists less than a shorter one. The exact proportions depend on the width, height, and length of the beam and on whether one considers it to be heavy or whether its weight is so much smaller than the loads that it can be neglected. Galileo believed that the foundations of this science lie in the doctrine of the lever; therefore the resistance of materials was seen as part of mechanics.

Days three and four of the *Discorsi* are devoted to the science of motion and are arranged in an unusual fashion: A formal treatise in Latin is interspersed with elucidations and comments in Italian by the three interlocutors. The Latin treatise consists of three parts: on uniform motion, uniformly accelerated motion, and projectile motion. Uniform motion was well understood, but Galileo needed to establish some basic proportions about it as a basis for the following parts. This example highlights a major problem in Galileo's treatment of accelerated motion, namely his lack of suitable mathematical tools, apart from some propositions derived by the *calculatores* tradition. Galileo would often use the mean-speed theorem to move from uniformly accelerated motion to an equivalent uniform one, and then apply the theorems about uniform motion from part one. He remained always doubtful of the theory of indivisibles, a remarkable mathematical achievement by his follower Bonaventura Cavalieri (1598–1647) that would have helped him in some respects.[45] It is in days two and three that Galileo presented some of the results attained at Padua and expanded on them. Although Galileo struggled to find out that in free fall the speed is proportional to time, in the published form he put this statement as a definition, seeking to present it as natural and simple. Galileo thought that he could found his new science on only one postulate, namely that the degrees of speed acquired by a body in falling along planes with different inclinations are equal whenever the heights of those planes are equal.[46] He sought to offer additional underpinning for this statement by claiming that a body falling either along an inclined plane or attached to the string of a pendulum acquires enough impetus to rise back to its original height. Galileo discussed an experiment where a sphere rolls down an inclined plane about twelve *braccia* (1 braccio = 550–655 millimeters) long and raised at one end by one or two *braccia*. Time was measured with a water clock, letting water out of a large container through a tap and then weighing it. The experiment showed that the distance traveled is as the square of the time. Galileo attributed no role to this experiment in the formal establishment of his science. Rather, he formulated the science of motion as a mathematical

[45] A useful introduction is Kirsti Andersen, "The Method of Indivisibles: Changing Understanding," *Studia Leibnitiana*, Sonderheft 14 (1986), 14–25.

[46] Galileo, *Two New Sciences*, p. 162.



construction and then used the experiment only at a later stage to show that the science he had formulated corresponded to nature's behavior. In this rather contrived construction, his science would retain a role as a purely mathematical exercise even if bodies fell according to a different rule. In the fourth day, Galileo presented his theory of projectile motion, arguing that trajectories are parabolic. In this case, too, Galileo claimed that his science would remain valid as a purely mathematical exercise even if nature behaved differently.

With regard to the role of experiment in his career, it seems helpful to draw a distinction between private research and public presentation. Private experiments, such as some of those found in *De motu antiquiora* and especially the Padua manuscripts, appear to have had a major heuristic role for Galileo. He seems to have performed them in order to gather quantitative information, especially in the form of proportions among variables, such as time and distance. In some cases, Galileo probably had some ideas as to the outcome and saw them confirmed, but in others the result probably came as a surprise to him, such as the experiment with dal Monte on projectile trajectories on an inclined plane. At times the manuscripts show Galileo calculating and comparing data from experiments to predicted values, his main aim being to determine proportions between variables rather than numerical values for their own sake. Galileo often sought to separate the fundamental features of a phenomenon from what he called accidental perturbations. It was this strategy that often enabled him to provide mathematical formulations of complex phenomena.[47] In print, Galileo's experimental reports vary enormously in style and scope. We have already seen how contrived his report of the inclined-plane experiment in the *Discorsi* was. In the *Discorso* on bodies in water, his reports sometimes have a legalistic tone associated with the nature of the dispute. In that case, Galileo knew from the start the principles of hydrostatics and was seeking a powerful rhetorical presentation. The *Dialogo* includes a large number of informal presentations of experiments, with a dazzling range of rhetorical styles. It is extremely difficult to pinpoint a general pattern, except to remark that Galileo was writing a masterpiece in scientific rhetoric wherein he was referring to experimental trials as if in informal conversation. At times he would claim great accuracy for an experiment, and in other instances he would say that the outcome was so certain that there was no need to perform it, even when we know that Galileo had in fact performed it. For example, Galileo discussed the experiment of dropping a weight from the mast of a moving ship as part of his discussions of motion on a moving earth, arguing that the weight falls at the foot of the mast regardless of whether the ship is in motion. Although in the *Dialogo* he claimed that the outcome could be determined even without an experiment,

[47] Damerow et al., *Exploring the Limits*, pp. 208–36; and Noretta Koertge, "Galileo and the Problem of Accidents," *Journal of the History of Ideas*, 38 (1977), 389–408.

we know from a previous letter that in fact he had performed this experiment a few years before.[48] This case highlights some of the problems in reading and interpreting Galileo.

Although Galileo proudly proclaimed at the outset of the third day of the *Discorsi* that he was putting forward a wholly new science about a most ancient topic, scholarly opinions about his actual achievements differ. Some see him as a key figure in careful experimentation, others in the application of mathematics to the study of nature. For some he resolutely broke with the past, whereas others detect long threads from classical and medieval times still entangling his thought and preventing him from offering a full formulation of a new science.[49] Either way, Galileo represents a nodal point in the history of mechanics and science. He had a major role in redefining questions and research topics and in setting the agenda for the following decades.

Several related processes took place in the science of mechanics during the period between the publication of the *Discorsi* in 1638 and the early eighteenth century. Initially, mechanics was situated among the mixed mathematical disciplines and had close ties with engineering, but from the middle of the century, it became progressively more integrated with natural philosophy. During the first part of the century, its practitioners interacted through correspondence networks, such as that centered on the French Minim Marin Mersenne (1588–1648). The second half of the century brought major changes to this landscape. Galileo died in 1642, followed in 1643 by his former student and professor of mathematics at Rome, Benedetto Castelli, and soon after by Galileo's successor at the Tuscan court, Evangelista Torricelli (1607–1647). Mersenne and René Descartes (1596–1650) died within a couple of years of each other. In addition to individual scholars, communication networks disappeared and had to be rebuilt by the new generation. Informal correspondence networks were replaced in the second half of the century by more formal scientific academies, such as the Royal Society in London and the Académie Royale des Sciences in Paris, which became major venues of research and debate on mechanics.[50] Lastly, the audience for works on mechanics changed from mathematicians and engineers to a broader public with philosophical and cosmological interests. The remainder of this chapter addresses in particular how the key works in the discipline were read by other practitioners and the broader intellectual public.

[48] Drake, *Galileo at Work*, pp. 84, 294.

[49] Classic interpretations include Drake, *Galileo at Work*; Galluzzi, *Momento*; and Damerow et al., *Exploring the Limits*, chap. 3.

[50] In the large literature on this theme, see Lorraine Daston, "Baconian Facts, Academic Civility, and the Prehistory of Objectivity," *Annals of Scholarship*, 8 (1991), 337–63; and Mario Biagioli, "Etiquette, Interdependence, and Sociability in Seventeenth-Century Science," *Critical Inquiry*, 22 (1996), 193–238.



## READING GALILEO: FROM TORRICELLI TO MERSENNE

A scholar of mechanics and the science of motion in about 1640 would have found the field to be in one of its most creative and exciting periods. After having discussed aspects of the science of motion in the *Dialogo*, in 1638 at age seventy-four, Galileo finally produced his masterpiece, the *Discorsi*. The first two days contained, among many digressions, the principles of the new science of the resistance of materials, which dealt especially with the problem of scaling applied to the transition from models of machines to real ones. Days three and four dealt with the science of motion, including falling and projected bodies. The *Dialogo* had received a first Latin translation in 1635 that was often reprinted, and in 1639, Mersenne put forward a free French translation of Galileo's *Discorsi*. In 1639, Castelli had published the second edition of his own work on water flow, *Della misura dell'acque correnti*. Outside Italy, Mersenne had published in 1636 the gigantic and labyrinthine *Harmonie universelle* (Universal Harmony), a work devoted to musical matters. Because sound is produced by motion, he included an extensive discussion of motion that was largely based on Galileo's *Dialogo*. All these works announced the emergence of new relationships between mathematics and the physical world: The science of motion was becoming an integral part of the mathematical disciplines and was tied to mechanics in multiple ways.[51]

The three new mathematical disciplines had technical and engineering roots: The origins of the science of waters were tightly bound to the problem of river flow in central Italy, especially the areas between Bologna and Ferrara, and of the Venetian lagoon; the science of resistance of materials and the problem of scaling were common concerns among engineers, so much so that Galileo introduced them in the *Discorsi* with a discussion inspired by a visit to the Venice Arsenal; and the science of motion had roots in gunnery. Despite such links, the university mathematicians and the philosophers promoting and discussing those disciplines had a more philosophical and learned audience in mind than that of technicians and engineers. On the one hand, their works emphasized not just utility but the importance of the reform of knowledge, especially natural philosophy; on the other hand,

[51] Although it is problematic to include the science of motion within mechanics throughout the seventeenth century, it is more problematic to exclude it. During the century, scholars increasingly took equilibrium, or statics, and the science of motion as two sides of the same coin. Alan Gabbey has produced several thoughtful articles on this issue: See Gabbey, "Newton's *Mathematical Principles of Natural Philosophy*: A Treatise on 'Mechanics'?" in *The Investigation of Difficult Things*, ed. Peter M. Harman and Alan E. Shapiro. (Cambridge: Cambridge University Press, 1992), pp. 305–22; Gabbey, "Descartes's Physics and Descartes's Mechanics: Chicken and Egg?" in *Essays on the Philosophy and Science of René Descartes*, ed. Stephen Voss (Oxford: Oxford University Press, 1993), pp. 311–23; and Gabbey "Between *ars* and *philosophia naturalis*: Reflections on the Historiography of Early Modern Mechanics," in *Renaissance and Revolution*, ed. Judith V. Field and Frank A. J. L. James (Cambridge: Cambridge University Press, 1993), pp. 133–45.

during the seventeenth century, the world, or at least significant portions of it, was seen more and more in mechanical terms, and therefore discussions of machines became colored with cosmological implications and natural philosophy. Occasionally Galileo's views were already being discussed and criticized in university textbooks of natural philosophy around the middle of the century.[52]

For several reasons, the science of motion attracted the lion's share of the interest. Galileo thought that he had established the science of the resistance of materials on the principle of the lever. Because it was thought to rely on mathematical and mechanical foundations, which were less problematic than the science of motion,[53] the science of resistance of materials generated fewer controversies and generally did not inspire broader philosophical debates. The mathematical treatment of the resistance of materials attracted the attention of the engineer and mathematician François Blondel (1618–1686), of Galilean mathematicians Alessandro Marchetti (1633–1714) and Vincenzo Viviani (1622–1703), of the Jesuit Honoré Fabri (1607–1688), of the experimental philosopher at the Paris Académie Edme Mariotte (ca. 1620–1684), and of mathematicians such as Gottfried Wilhelm Leibniz (1646–1716) and Jakob Bernoulli (1654–1705).[54] Although the science of waters was exceedingly complex, Castelli's fundamental proposition on water flow was quite straightforward. Descartes and Isaac Newton (1643–1727) implicitly used it with cosmological implications, but the science of waters remained largely a technical matter rooted in Italy.[55]

Let us now consider the science of motion. Galileo had provided different presentations: piecemeal, so to speak, in the *Dialogo*, where the motion of bodies on the earth was tied to Copernicanism, and structured in axiomatic form with definitions and theorems in the *Discorsi*. Even taking this major difference into account, it is striking to notice how differently his works were read by scholars in the late 1630s and 1640s. For some, Galileo had provided a series of propositions to be tested experimentally or examined in their mathematical or mechanical deductions on a one-by-one basis. Part of this process consisted in finding numerical values in which Galileo seemed

[52] See, for example, Niccolò Cabeo, *In quatuor libros Meteorologicorum Aristotelis commentaria* (Rome: Typis heredum Francisci Corbelletti, 1646). On this topic, the classic study is Charles B. Schmitt, "Galileo and the Seventeenth-Century Text-book Tradition," in *Novità celesti e crisi del sapere*, ed. Paolo Galluzzi (Florence: Giunti Barbera, 1984), pp. 217–28. For France, see Laurence W. B. Brockliss, *French Higher Education in the Seventeenth and Eighteenth Centuries: A Cultural History* (Oxford: Oxford University Press, 1987).

[53] Aspects of those problematic foundations are discussed later in this chapter.

[54] Edoardo Benvenuto, *An Introduction to the History of Structural Mechanics*, 2 vols. (Berlin: Springer, 1991), vol. 1.

[55] Castelli's proposition was first published in the 1628 *editio princeps* of *Della misura*. It states that, in a river in stationary flow, the areas of the cross sections are inversely as the speeds of the water flowing through them. Descartes, *Principia philosophiae*, pt.3, paras. 51, 98; pt. 4, para. 49. Newton, *Principia*, new translation, pp. 789–90. The main work on the Italian hydraulic tradition is Maffioli, *Out of Galileo*.



to have no particular interest, such as the length of the seconds pendulum or the distance traversed by a falling body in one second. Others were interested in the axiomatic structure of the new science as a whole, in the choice of definitions and axioms, and in the ensuing proofs. Still others became concerned with mathematico-philosophical aspects, such as the nature of the continuum, with special regard to time and speed. Lastly, some scholars objected to the very nature of Galileo's science, arguing that he had neglected physical causes and built an abstract science with no bearings on the real world. Those different readings provide valuable insights on the many perspectives from which mechanics and motion were studied in the mid-seventeenth century.

Galileo and his disciples Torricelli and Viviani were mainly concerned with the formulation of a science in imitation of Archimedes' work on the equilibrium of the balance. Generally, they were already convinced of the truth of individual propositions, but they worried about the overall structure and especially the choice of axioms. Ideally, in their opinion an axiom did not have to be established by experiments but rather had to be chosen as a principle of reason to which the mind naturally agrees. Soon after the publication of the *Discorsi*, Viviani pressed Galileo over the choice of his axiom, and Galileo conceived a way to prove it on the basis of received mechanical principles. The new proof appeared in the second edition of *Discorsi* in 1656, published together with other works by Galileo, excluding the *Dialogo*.[56] Torricelli also moved along similar lines, and in his reformulation and extension of Galileo's science in *De motu* (On Motion, 1644) he introduced a new principle, namely that two joined bodies do not move unless their common center of gravity descends. Although Torricelli instantiated it by mentioning bodies attached to a balance or pulley, it is clear that this principle was not based on experiments and had general validity beyond the specific cases mentioned.[57]

It would be erroneous to generalize these concerns to other quarters. Several readers questioned specific empirical claims and performed experiments that challenged Galileo's statements and results. For example, the Genoa patrician Gianbattista Baliani (1582–1666) and Mersenne in Paris expressed surprise and incredulity at Galileo's claim that in five seconds a body falls only one hundred *braccia*, less than seventy yards. Quite rightly, both believed the real distance to be far greater. We now know that Galileo had extrapolated results from fall along inclined planes to bodies in free fall. Such extrapolations were problematic for reasons unknown at the time, and when Mersenne tested Galileo's claims he found systematic errors for virtually all inclinations. A body falling along the inclined plane covered a distance noticeably shorter

[56] The axiom stated that the degrees of speed acquired by the same body over planes of different inclinations are equal whenever the heights of those planes are equal. Galileo, *Two New Sciences*, pp. 206, 214–18, includes the new proof in a footnote.

[57] *De motu* was part of Torricelli's *Opera geometrica* (Florence: Typis Amatoris Masse and Laurentii de Landis, 1644).

than that predicted by Galileo.[58] Galileo had also claimed that as a body shot upward falls back, it goes through the same degrees of speed as when it was going up. In particular, the speed with which it reaches the ground would be the same as that with which it was shot. Yet experiments based on the force of percussion reported by Mersenne and the Paris mathematician Gilles Personne de Roberval (1602–1675) showed the speed of the body after the fall to be much smaller than that with which it was shot. Experiments performed initially by gunners at Genoa and then at various locations in Europe tested Galileo's claims about parabolic trajectories. Were the trajectories parabolic? Was it true that the longest shot occurred with an inclination of 45°? Could one measure and predict the effect of air resistance? Lastly, probably the most extensive and accurate set of experiments inspired by Galileo was performed from many of the Bologna bell towers as well as civic towers under the direction of Jesuit mathematician Gianbattista Riccioli (1598–1671). Over approximately a decade, Riccioli dropped spheres of lead, clay (empty and full inside), wax, and different types of wood, finding that they followed Galileo's odd-number rule – namely that the distances traversed in successive time intervals are as 1, 3, 5, and so on – though it was not quite true that all bodies fell with the same acceleration. Riccioli also experimented on the length of the seconds pendulum, yet another common theme for Galileo's readers. At least some of these experiments were performed keeping in mind the issue of Copernicanism and the behavior of bodies on a moving earth, which was a major concern especially among Jesuit authors.[59]

Additionally, several readers were concerned that embedded in Galileo's views about motion were both specific and general philosophical propositions. Prominent among the former was the problem of continuity. Galileo had claimed on several occasions that a falling body goes through all the degrees of speed. Because the degrees of speed are infinitely many and the body falls in a finite time, it seems to follow that the body must go through each of them in an instant and that a finite time is composed of infinitely many instants. The composition of the continuum touched on many other themes as well, notably condensation, rarefaction, the cohesion of bodies, and their resistance to rupture – Galileo's main theme in the first day of the *Discorsi*. But even within the science of motion, Galileo's claim was

[58] Marin Mersenne, *Harmonie universelle contenant la théorie et la pratique de la musique* (Paris: Editions du CNRS, 1986), bk. 2, prop. 7, esp. pp. 111–12. The discrepancy was caused by the fact that a sphere rolling down an inclined plane behaves like a rigid body, not a point mass, and follows more complex laws because it rotates. A sphere rolling along an inclined plane covers only five-sevenths of the distance Galileo and Mersenne had in mind.

[59] Giovanni B. Riccioli, *Almagestum novum* (Bologna: Ex Typographia Haeredis Victorij Benatij, 1651), pt. I, pp. 84–91 and pt. II, pp. 381–97. Classic studies by Alexandre Koyré are: "An experiment in measurement," *Proceedings of the American Philosophical Society*, 97(1953), 222–37; "A Documentary History of the Problem of Fall from Kepler to Newton," *Transactions of the American Philosophical Society*, 45 pt. 4 (1955). More recently, Peter Dear has studied the role of experiments in the mathematical disciplines and the science of motion in *Discipline and Experience: The Mathematical Way in the Scientific Revolution* (Chicago: University of Chicago Press, 1995).



controversial and was challenged by several scholars on several grounds. The Society of Jesus was extremely cautious when it came to matters about the composition of the continuum, potentially impinging on such crucial issues as the dogma of transubstantiation of the Eucharist. They prohibited several propositions, such as "An infinity in number and magnitude can be contained between two unities or points," and "The continuum is composed of a finite number of indivisibles." Two Jesuit scholars, Honoré Fabri and Pierre le Cazre (1589–1664), were among those who became entangled in metaphysical disputes on continuity and defended propositions against both Galileo and surprisingly some of the views of their order. Fabri in particular argued that time was not continuous but consisted of tiny but finite instants. At a later stage, he also claimed that the instants were of variable size. Baliani also argued in a similar vein, possibly inspired by Fabri. In their opinion, a time interval would consist of a finite number of finite instants, and thus speed would change not continuously but discretely at each new instant. Fabri agreed that Galileo's rule was empirically adequate but denied that Galileo had provided a foundation for his science and that experience could serve this purpose. The advantage of his view would be to provide a different and more solid philosophico-metaphysical justification for the science of falling bodies. Additionally, if the instants are very small, the difference between Galileo's odd-number rule and Fabri's can become so small as to be empirically undetectable. Therefore, one would save the experimental side of Galileo's work while providing it with a more solid foundation as to the composition of the continuum. Not surprisingly, the reviver of ancient atomism, Pierre Gassendi (1592–1655), was also involved in these debates.[60]

Other scholars had other fundamental objections concerning the very nature of Galilean science. Descartes was the most prominent among those readers concerned not simply with physical causes but with the architecture of Galileo's science. Whereas Galileo had modeled his sciences on Archimedes and often emphasized his reliance on sensory experiences and mathematics as the new sources of knowledge, Descartes aimed more broadly at a reform of knowledge from its metaphysical foundations.

## DESCARTES' MECHANICAL PHILOSOPHY AND MECHANICS

One way to discuss some of the differences between Galileo's and Descartes' perspectives is to focus on their reading of Aristotle and their relation to the

[60] C. R. Palmerino, "Two Jesuit Responses to Galileo's Science of Motion: Honoré Fabri and Pierre Le Cazre," in *The New Science and Jesuit Science: Seventeenth Century Perspectives*, ed. Mordechai Feingold (Dordrecht: Kluwer, 2003, *Archimedes*, vol. 6), pp. 187–227, at p. 187. See also Paolo Galluzzi, "Gassendi and *l'Affaire Galilée* on the Laws of Motion," in *Galileo in Context*, ed. Jürgen Renn (Cambridge: Cambridge University Press, 2001), pp. 239–75.

Peripatetic tradition. Both rejected Aristotle and especially the Peripatetic learning of the universities, but whereas Galileo offered a powerful but narrower alternative that focued on motion, matter theory, and cosmology, Descartes offered a broader alternative that included the nature and foundations of our knowledge and aimed at replacing the Aristotelian worldview among scholars as well as in university teaching. Galileo's views on the relationship between mathematics and physical causes varied considerably even within the same work. In the *Discorsi*, for example, he tried to account for the resistance of bodies to rupture in terms of infinitely many interstitial vacua, joining matter theory with the analysis of the continuum. With regard to the science of motion, however, Galileo claimed through his spokesperson Filippo Salviati (1582–1614) that he did not wish to investigate causes and presented a new science in the Archimedean tradition of the science of equilibrium or statics.[61]

Descartes, by contrast, after a crucial collaboration with the Dutch scholar Isaac Beeckman (1588–1637), aimed at creating a new worldview where a mathematical description of nature, and especially motion, was joined to a physical causal explanation. The key to this link was the microworld of corpuscles and subtle fluids responsible for physical phenomena. Through a combination of his study of impacts among particles and of the behavior of fluids, Descartes was able to produce a new physico-mathematics by which he and Beeckman meant a science joining a mathematical description with a physical causal account. The mixed mathematics dealt with the properties of the lever, for example, without exploring the cause of gravity, whereas Descartes made that exploration a major feature of his work.[62] Descartes did not think much of Galileo's *Discorsi* and in a letter to Mersenne he disagreed with the physical account of the resistance of materials and the analysis of the continuum because he thought that Galileo had built a science of motion without foundations by failing to provide a deeper philosophical and causal analysis. Descartes argued that Galileo had solved easy mathematical exercises devoid of physical significance.[63] Descartes' view of gravity as caused by a stream of particles made Galileo's abstraction of the motion of falling bodies in a void meaningless because he was removing the very cause of motion.

Both Galileo and Descartes were familiar with *Quaestiones mechanicae*, a text then believed to be by Aristotle but now attributed to his school. *Quaestiones* relies on the principle of the lever and presents a series of cases and examples dealing with it. Whereas Galileo read it within the tradition of

[61] Galileo, *Two New Sciences*, pp. 109, 158–9.

[62] Stephen Gaukroger, *Descartes* (Oxford: Oxford University Press, 1995), chap. 3. Descartes and Beeckman used the term "physico-mathematics" in this sense. The emergence of the same term among Jesuit writers with a somewhat different meaning has been studied in Dear, *Discipline and Experience*. See also Stephen Gaukroger and John Schuster, "The Hydrostatic Paradox and the Origins of Cartesian Dynamics," *Studies in History and Philosophy of Science*, 33 (2002), 535–72.

[63] The relevant portion of the letter is translated in Drake, *Galileo at Work*, pp. 386–93.



mechanics, Descartes found in it and its interpretative tradition references to slings, whirlpools of water, and pebbles rounded on the seashore, three key elements of his worldview that we will encounter again in this section.[64]

Descartes' work, together with some of Galileo's passages on the constitution of matter and Gassendi's Christianized atomism, constitute the pillars of the so-called mechanical philosophy. In *Il Saggiatore* (The Assayer, 1623), Galileo had drawn a distinction between properties of matter such as shape, which is independent of human perceptions, and color, which resides only in the human mind. They were later called primary and secondary qualities, respectively. In a number of works, culminating with the imposing *Syntagma philosophicum* (Philosophical Work, 1653), Gassendi attempted to revive the ancient philosophy whereby elementary constituents of matter or atoms move through empty space. The mechanical philosophy was a heterogeneous collection of views with a common core based on the belief in the fundamental role of the size and shape of particles in motion. Descartes believed neither in the existence of empty space nor in the indivisibility of matter or atoms, unlike Gassendi.

Descartes first outlined his system in *Le monde* (The World), but this work appeared posthumously in 1664. The main text known to his contemporaries was *Principia philosophiae* (Principles of Philosophy), first published in Latin in 1644 and then in 1647 in a French translation by the abbé Claude Picot (d. 1668) partly under Descartes' supervision. This work consists of four parts, on the principles of human knowledge, the principles of material things, the visible world, and the earth. Descartes dedicated it to his friend and correspondent Princess Elizabeth of Pfalz (1596–1662). Descartes claimed that she had a detailed knowledge of all the arts and sciences and therefore was the only person to have understood all the books he had published. In the letter to Picot prefaced to the French edition, Descartes suggested that his book be read first as a novel to figure out what it was about. Only on a second reading should one examine it in greater detail and grasp the order of the whole. The letters to Elizabeth and Picot suggest that Descartes' work was extraordinarily ambitious in the range of matters it treated and in the broad audience it intended to address.[65]

I focus here on Parts 2–4, outlining a world system based on relatively simple laws but with a dazzlingly complex series of interactions among particles. In Part 2, Descartes argued that space could not be distinguished from corporeal substance and that a vacuum and atoms did not exist. Descartes also tried to define the motion of a body with respect to the bodies that touch it, so that if the earth is carried by a vortex, one could say that truly it is at

[64] H. Hattab, "From Mechanics to Mechanism: The *Quaestiones mechanicae* and Descartes' Physics," to appear in *Australasian Studies in the History and Philosophy of Science*. I am grateful to the author for having provided me with a draft of her work and for permission to refer to it.

[65] Gaukroger, *Descartes*, chaps. 7 and 9.

rest with respect to its surrounding bodies. Scholars disagree as to whether his definition was genuinely part of his views or was intended to protect his system from the charge of Copernicanism by the Catholic Church.[66]

In the study of motion, Descartes formulated three laws, a significant departure from the term "axiom" used in the mathematical disciplines. He justified them partly on physical and partly on theological grounds. He considered motion and rest as modes of a body and, at least to some extent, equivalent ones, whereby every body perseveres in its state of motion or rest indefinitely. This is his first law of motion. The second specifies that motion in itself is rectilinear and that everything moving circularly tends to escape from the center along the tangent. Descartes often talked of "determination," a notion close to that of direction. Later in the century, the ideas embodied in the first two laws would become known as the law of inertia, despite the fact that the term inertia had been initially employed by Johannes Kepler (1571–1630) with quite a different meaning, namely of a body's innate tendency to come to rest. Descartes tried to provide examples for his first two laws from objects of common experience, the motion of projectiles in the first case and that of a sling in the second.[67]

Traditionally, Descartes' reconceptualization of motion and geometrization of space have been considered as the major event in the history of seventeenth-century mechanics and natural philosophy.[68] According to Kepler and to traditional Aristotelian doctrines, a body in motion would naturally come to rest. In the work of Galileo, horizontal motion often is the limit of a motion along an inclined plane with zero inclination and is truly horizontal in the sense that it coincides with the horizon. Thus, over a short distance it may appear straight, but over a longer one it is circular because of the earth's curvature. Galileo seemed to extend similar views to orbital motions, implying that circular motion is natural. Descartes, by contrast, extended rectilinear uniform motion indefinitely in a Euclidean space. Despite the great significance of his and Gassendi's views on this matter, the transformations occurring in mechanics in mid-century were more broadly based. Mechanics and the science of motion involved a richer set of notions, practices, and problems than was suggested by Descartes' first two laws, such as falling bodies, the motions of strings and pendulums, the resistance of materials, and water flow.[69]

[66] Daniel Garber, *Descartes' Metaphysical Physics* (Chicago: University of Chicago Press, 1992), especially chap. 6.

[67] Garber, *Descartes' Metaphysical Physics*, pp. 188–93; Damerow et al., *Exploring the Limits*, pp. 103–23; Descartes, *Principia*, pt. 2, paras. 37–9; and Alan Gabbey, "Force and Inertia in Seventeenth-Century Dynamics," *Studies in History and Philosophy of Science*, 2 (1971), 1–67. See the entry on inertia by Domenico Bertoloni Meli in *Encyclopedia of the Scientific Revolution from Copernicus to Newton*, ed. W. Applebaum (New York: Garland, 2000), pp. 326–8.

[68] The most influential proponent of this view was Alexandre Koyré. See, for example, his *From the Closed World to the Infinite Universe* (Baltimore: Johns Hopkins University Press, 1957).

[69] We lack a comprehensive study of mechanics in the seventeenth century. On the science of waters, see Maffioli, *Out of Galileo*. On the resistance of materials, see Benvenuto, *Structural Mechanics*, vol. 1.



Descartes' third law of nature concerns collision and states that a body hitting a stronger one loses no part of its motion, whereas a body hitting a less strong one loses the same amount that it transmits to the other. Put another way, the third law states the conservation of motion, by which Descartes meant the product of the size of a body and its speed, taken with no regard to direction. Clearly this law required justification and qualification. Descartes claimed that quantity of motion is conserved not in individual bodies but in the universe as a whole because of God's plan and action. In the ensuing explanation, Descartes distinguished between hard and soft bodies, so one gets the impression that the strength of a body is associated with its hardness. Later on, however, he defined a body's strength in terms of its size, surface, and speed, and the nature of the impact. These conditions are quite complex, and when it comes to providing specific rules for actual impacts, Descartes introduced simplifications, such as that the bodies are perfectly hard and are taken in isolation from all surrounding bodies.[70]

These simplifications are quite radical because they describe abstract situations that cannot occur in the Cartesian world, which is a plenum. Even with them, Descartes' seven rules appear problematic, despite his claim that they were self-evident and required no proof. Rules 1–3 deal with bodies moving in opposite directions, and rules 4–6 deal with collisions between a body in motion and one at rest. The last rule examines the various cases of collisions between two bodies moving in the same direction. Descartes' rules do not lack surprising and unconvincing features. For example, in rule 4 he argued that a body at rest could not be set in motion by a smaller one, regardless of its speed. Another example of the problems associated with Descartes' rules emerges from those numbered 2 and 5. According to rule 2, if two bodies, one slightly larger than the other, collide with equal and opposite speeds, the smaller one rebounds and both move in the same direction with the same speed. According to rule 5, if a body in motion collides with a smaller body at rest, after the impact both will move in the direction of the impinging body with the same speed, a speed smaller than the impinging body's original speed. In both cases, their common final speed can be determined from the conservation of a Cartesian or scalar quantity of motion, which is larger in rule 2 than in rule 5 for equal bulks. Thus it appears that a body in motion is affected more by colliding with one at rest than with the same body moving with an opposite speed, an unconvincing result or at least one far from self-evident.[71]

Part 3 of Descartes' *Principia* deals with the sensible world and outlines a cosmogony based on the size and shape of particles in motion. He provided an account of the formation of the universe and of the motions of celestial bodies such as planets and comets. The heavens are fluid, and there are three types of

[70] Descartes, *Principia*, pt. 2, paras. 40–55.

[71] There is a vast body of literature on the rules of impact. See Damerow et al., *Exploring the Limits*, pp. 91–102; and Garber, *Descartes' Metaphysical Physics*, chap. 8.

matter or elements depending on shape and size. Over time, the particles of matter become rounded like pebbles on a beach, and their minute fragments form different types of matter. The first element forms the sun and fixed stars, and is fine-textured and moves very fast; the second element is coarser and forms the fluid filling the heavens; and finally, the third element is rather gross and forms planets and comets. Light is the pressure resulting from the endeavor of small particles to escape from a rotating vortex. From his second law of motion to his analysis of light, Descartes relied on motion being rectilinear. Curvilinear motion is caused by an external agent and generates a tendency to escape along the tangent. Descartes did not provide a quantitative measure of this outward tendency but laid the conceptual foundations for explaining it that remained in place for several decades. A body tending to escape along the tangent also tends to escape from the center. Thus a rotating stone pulls the sling retaining it and is counterbalanced by the hand holding the sling. Similarly, in the universe, orbiting bodies have a tendency to escape along the tangent, and those with a stronger tendency push the others toward the center. Thus, bodies appearing to have a tendency toward the center are in reality only the losing ones in the competition to move outward. In a universe with no empty space, for every particle moving outward there must be a corresponding one moving the opposite way. Descartes illustrated this with the example of straws floating in a whirlpool of water and being pushed by the rotation toward the center. In dealing with the real world, Descartes did not rely on his impact rules but considered factors such as the structure of matter, the nature of its pores, and the size and speed of the fluid particles flowing through them. In Descartes' eyes, the virtue of this type of explanation is that it accounts for all phenomena with philosophically acceptable notions, avoiding inexplicable attractions and repulsions and all actions not based on direct contact.[72]

Part 4 is devoted to the earth and examines a range of phenomena about its formation and features, extending among others to gravity, tides, chemical phenomena and reactions, the origin of flames, magnetism, and the elasticity of air and other substances. Concerning gravity, for example, Descartes argued that because of the different sizes and speeds of the particles generating it and flowing through terrestrial bodies, the weight of a body is not proportional to its quantity of solid matter or the grosser matter of Descartes' third element.[73]

Descartes' formulation of the first two laws, his insistence on conservation in the third, and his posing the problem of curvilinear motion and impact changed the landscape of the science of motion. By identifying matter with extension, Descartes set the scene in principle for a radical geometrization of the universe. In practice, however, the complexity of the interactions

[72] Descartes, *Principia*, pt. 3, paras. 48–63.

[73] Descartes, *Principia*, pt. 4, para. 25. Later in the century, Newton was to address precisely this point.



among streams of particles moving in all directions meant that the actual formulation of a mathematical description of nature could be accomplished only in limited areas.[74]

## READING DESCARTES AND GALILEO: HUYGENS AND THE AGE OF ACADEMIES

Although probably only a few devoted followers accepted Descartes' views in their entirety, the intellectual world of the second half of the seventeenth century was dominated by criticism, responses, reformulations, and refinements of his doctrines. No one who read his works went away unchanged. Already during Descartes' lifetime, and even more so in the second half of the century, university textbooks based on his philosophy began to appear, marking a major change in higher education. Henri Regis (1598–1679) and Jacques Rohault (1620–1675), just to mention two of the most prominent authors, wrote influential textbooks that went through many editions stretching, in the latter case, to the eighteenth century.[75]

Let us consider how Descartes' *Principia* was read, especially his laws of nature and impact rules. Among the community of mathematicians, the first two laws of motion fared much better than the third one and its instantiation in the impact rules. Scholars accepted that undisturbed motion is uniform and rectilinear, at least in principle, even if at times they forgot to apply it in practice, as did Giovanni Alfonso Borelli (1608–1679), holder of Galileo's former chair of mathematics at Pisa, in his study of falling bodies on a rotating earth.[76] The conservation of quantity of motion and the rules of impact were more problematic and were subjected to criticism and refutation. Both peaked in the late 1660s.

It may be useful to begin with a brief historiographic consideration. Traditionally, the study of impact has been considered simply as a search for the correct rules.[77] The rules, however, depend on the different types of bodies involved, and therefore the problem is twofold: to determine the rules and to classify bodies appropriately. In order to have meaningful rules, one must know the meaning of terms such as soft, hard, and elastic for impact phenomena. Thus, the study of impact involves the properties of material

[74] Optics was an area where Descartes was especially successful in this regard. See Gaukroger, *Descartes*, pp. 256–69.

[75] See, for example, Henricus Regis, *Fundamenta physices* (Amsterdam: Apud Ludovicum Elzevirium, 1646); Jacques Rohault, *Traité de physique* (Paris: Chez la Veuve de Charles Savreux, 1671); and Brockliss, *French Higher Education in the Seventeenth and Eighteenth Centuries*.

[76] On subtle differences between Descartes' laws and later formulations, see Gabbey, "Force and Inertia in Seventeenth-Century Dynamics"; Govanni A. Borelli, *De vi percussionis* (Bologna: Ex typographia Iacobi Montii, 1667), pp. 107–8; and Koyré, "Documentary History," pp. 358–60.

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[77] See, for example, Richard S. Westfall, *Force in Newton's Physics* (London: Macdonald, and New York: American Elsevier, 1971), ad indicem.

bodies. The most significant and wide-ranging of those properties was elasticity, a property amenable to mathematical description and linked to a large number of instruments, phenomena, and experiments such as the wind gun, pneumatic fountains, the spring, the Torricellian tube, and the air-pump.

In the late 1650s, the Dutch scholar Christiaan Huygens (1629–1695) had come to mistrust Descartes' rules and proceeded to formulate new ones, but his work was known in part only to a few correspondents, and the treatise he composed remained unpublished at the time. Huygens used pendulums to study impact, an effective technique whereby the speeds before and after the impact can be ascertained by the bobs' heights. His final formulation was axiomatic, in the style of Galileo's science of motion, and was based not on experiments but on the skillful use of general principles. Among the most famous were relativity of motion to disprove some of Descartes' rules, and a generalization of Torricelli's principle whereby the center of gravity of two colliding bodies cannot rise.[78] If one thinks of colliding pendulums, it is quite natural to consider the height the bodies can reach and to introduce an expression based on the square of their speed times their weight, a notion later known, following Leibniz, as *vis viva*, or living force.[79] In 1660 and 1661, Huygens demonstrated his prowess at solving impact problems in Paris and London in front of several members of the Royal Society.

In 1666 and 1667, Borelli published two works, on the motion of the Medicean planets and on the force of percussion, two exquisitely Galilean topics. In his works, he criticized some of Descartes' rules and proposed some of his own. For example, Borelli argued, *pace* Descartes, that a smaller body could set a larger one in motion, and he studied impact for what he called perfectly hard bodies that do not rebound. The key notion in impact for Borelli was *vis motiva*, or speed taken with its direction times the body, which he argued is conserved. Borelli had difficulties in dealing with the bodies' rebound, a common phenomenon in impact that was difficult to explain if the colliding bodies were conceived to be inflexible. He also dealt with elasticity, but did not formulate rules for elastic impacts, possibly because he did not believe they could be given in mathematical form.[80]

In the late 1660s, the Royal Society investigated the problem of motion and addressed the issue of impact at several of its meetings, where experiments and discussions took place. In 1668, the Society invited contributions on the problem, to which the Oxford Savilian professor John Wallis (1616–1703), the architect Christopher Wren (1632–1723), and Huygens provided solutions.

[78] Christiaan Huygens, *Oeuvres Complètes*, 22 vols. (The Hague: Martinus Nijhoff, 1888–1950), 16: 21–5 and 95 n. 10. This is a generalization of Torricelli's principle because the two colliding bodies are no longer joined.

[79] *Vis viva* first appeared in print in Leibniz's *Tentamen de motuum coelestium causis*, published in the *Acta eruditorum* for 1689. See Domenico Bertoloni Meli, *Equivalence and Priority* (Oxford: Oxford University Press, 1993), pp. 86–7; the relevant passage from the *Tentamen* is translated at p. 133.

[80] Westfall, *Force*, pp. 213–18.



Wallis's essay was very brief and did not address the main issue. He later expanded his discussion considerably in his *Mechanica, sive de motu tractatus geometricus* (Mechanics, or Geometrical Treatise on Motion, 1670–1), where he classified bodies as hard, soft, and elastic. These are abstractions because real bodies are not perfectly hard or elastic. Clay, wax, and lead are examples of soft substances, and steel and wood are examples of elastic ones. By hard bodies Wallis probably meant the ultimate constituents of matter, but he provided no example, despite the fact that he provided impact rules for them. Wallis excluded soft bodies from his rules, arguing that a portion of their quantity of motion is lost in impact. Thus he did not present a universal conservation rule that was valid for all bodies.

Wren provided a brief and rather cryptic essay quite similar to that of Huygens, who later complained that Wren had gotten his idea in 1661 when they had discussed the matter at the Society. Huygens was irritated that the contributions by Wren and Wallis appeared before his own, and he published a version of his paper first in the *Journal des sçavants* and then in the *Philosophical Transactions of the Royal Society*. He pointed out that Descartes' conservation law was not valid because the Cartesian quantity of motion can increase, remain constant, and decrease. What remains constant in impact for all types of bodies is the quantity of motion in one direction. Huygens distinguished between hard and soft bodies and argued that for the former impact is instantaneous, whereas for the latter it occurs over time. He also claimed that hard non-elastic bodies rebound like elastic ones. Examples of hard bodies are atoms, whose existence Huygens tentatively accepted, and Descartes' subtle matter. Here it would be impossible to understand the terms of the debate without realizing its Cartesian roots.[81]

In the study of curvilinear motion once again, Descartes provided an important conceptual framework with the example of the sling, and, once again, it was Huygens who had the vision and mathematical skill to offer a solution to the problem. As in the case of the impact rules, Huygens reached his important results in the late 1650s, but it took several years before they were published in curtailed form. The initial stimulus to work on this topic came somewhat indirectly from Mersenne. As we have seen, the French Minim's reading of Galileo was often aimed at determining numerical values. The distance fallen by a body in free fall in one second was one of the values sought. Tackling the question directly proved problematic because bodies fall very fast, and thus the problem was best rephrased. Huygens was led to consider the action of gravity in a conical pendulum, whose bob rotates on a plane parallel to the horizon, to be counterbalanced by centrifugal force. Relying on Galileo's science of motion and Descartes' notions, Huygens produced

[81] A. Rupert Hall, "Mechanics and the Royal Society, 1668–1670," *British Journal for the History of Science*, 3 (1966), 24–38, is still a useful essay. See also Westfall, *Force*, pp. 231–43.

a remarkable series of theorems, which he then published without proof in his masterpiece, the *Horologium oscillatorium* (The Pendulum Clock, 1673), one of the most original works in the history of mechanics as a whole and a model for Newton's *Principia mathematica philosophiae naturalis* (Mathematical Principles of Natural Philosophy, 1687).[82]

Mersenne had come to doubt on purely empirical grounds Galileo's claim that the oscillations of a pendulum are isochronous regardless of their amplitude. He surmised that this may be the case in a vacuum but was not true in air. While seeking the perfect or isochronous clock, Huygens moved the research from the world of experiments to a more theoretical level, producing a work addressing technical issues of horology and joining the new science of motion with higher mathematics. He proved that the problem was not simply air resistance because pendular oscillations depend on their amplitude, with greater ones being slower. The issue was not one of pure theory but was linked to the problem of finding the longitude at sea, a major concern for the burgeoning colonialism of European states.[83]

*Horologium oscillatorium* is closer to a Galilean tradition than a Cartesian one. Of course, it was Galileo who had started to use the pendulum as a time-measuring device and who had thought of building a clock regulated by the pendulum's oscillations. Much as Galileo had done with regard to the science of motion in the *Discorsi*, Huygens avoided issues such as the cause of gravity.[84] Rather, he sought to promote the virtues of his clocks, explain their principles of operation and construction, and produce new theories both in mechanics and mathematics. Much as Galileo had done at the end of the first day of the *Discorsi*, when he had tried to show technicians how to draw parabolas by throwing balls along inclined planes or hanging the extremes of a chain from two nails, Huygens showed practical ways for drawing a cycloid.[85] The cycloid was a new curve in the seventeenth century and the double mathematical protagonist of his treatise because in order to have isochronism the curve described by the bob had to be a cycloid, and in

82 Joella G. Yoder, *Unrolling Time* (Cambridge: Cambridge University Press, 1988), provides a detailed account of Huygens's research. Henk J. M. Bos, M. J. S. Rudwick, H. A. M. Snelders, and R. P. W. Visser, *Studies on Christiaan Huygens* (Lisse: Swets and Zeitlinger, 1980), contains many valuable contributions. See also Michael S. Mahoney, "Huygens and the Pendulum: From Device to Mathematical Relation," in *The Growth of Mathematical Knowledge*, ed. Herbert Breger and Emily Grosholz (Dordrecht: Kluwer, 2000), pp. 17–39.

83 Christiaan Huygens, *The Pendulum Clock*, trans. R. J. Blackwell (Ames: University of Iowa Press, 1986), p. 19; and William J. H. Andrews, ed., *The Quest for Longitude: The Proceedings of the Longitude Symposium, Harvard University, Cambridge, Massachusetts, November 4–6, 1993* (Cambridge, Mass.: Harvard University Press, 1996).

84 It should be remembered, however, that although he expressly ruled out discussions on the cause of gravity in day three of the *Discorsi*, Galileo embarked on extensive discussions on the cause of cohesion in day one. Thus, it would be inaccurate to take his attitude toward the cause of gravity as representative of his views on physical causes in general.

85 Galileo, *Two New Sciences*, pp. 142–3. The curve traced by a sphere rolling on an inclined plane is a parabola, whereas that described by a hanging chain resembles the parabola but is more complex. Huygens, *Pendulum Clock*, pp. 21–4.



order to make the bob move along a cycloid, it is necessary to constrain it between cheeks that are also cycloid arcs. Also in Part II of the *Horologium oscillatorium*, on falling bodies and their motion in a cycloid, Huygens often referred to Galileo's proofs in the *Discorsi* and rephrased them.[86] Of course, Huygens expanded on Galileo's results by including a mathematical treatment of motion along a cycloid, something beyond Galileo's capabilities.

Part III of the *Horologium oscillatorium* is a mathematical investigation of curves generated by unrolling a thread on another curve and their mutual relationships. With Part IV, we return to mechanics proper and to a debate involving Mersenne, Descartes, and Roberval dating from the 1640s, namely to find the center of oscillation for a compound pendulum. A simple pendulum has all its mass concentrated in one point, whereas in a real physical pendulum the mass is distributed over a finite area. Whereas the period of a simple pendulum can be determined by its length, in a real pendulum there is no obvious point whereby the length associated with its period can be determined. Finding the center of oscillation means determining that point in a real pendulum. Huygens's success in finding a procedure to determine that point counts as one of the finest achievements of seventeenth-century mechanics.

In Huygens's work in mechanics there is an interesting dichotomy between Galilean and Cartesian approaches. Whereas the *Horologium oscillatorium* clearly looked to Galileo's science of motion as a model for content and structure, other works looked more to Descartes' *Principia philosophiae*. At a debate in 1669 at the Paris Académie Royale des Sciences on the cause of gravity, Huygens proposed a mechanism emphasizing the account of physical causes over mathematical accuracy. He took a bucket of water with a small sphere floating in it that was constrained by two strings stretched between opposite sides of the rim. By setting the water in circular motion and then stopping it, the sphere moved toward the center, thus showing an effect analogous to gravity. Unlike Descartes, Huygens did not believe that the universe was a plenum but accepted empty space and argued that the matter of the vortex rotates in all directions. Heavy bodies do not follow the motion of the particles of the fluid but are pushed toward the center because of their lack of centrifugal force. Huygens also attempted a quantitative estimation of the speed of the particles of the fluid based on his theorems on centrifugal force. He found that the speed of a particle of fluid required to produce gravity was seventeen times the speed of a point on the earth's equator.[87]

With mechanical explanations extended to all types of phenomena, from falling bodies to magnetism, fluids and vortices became common explanatory models, but they were not the only ones. At times elasticity, for example, was explained in terms of subtle fluids, but it was also considered an autonomous property of matter accountable mathematically in mechanical terms and

86 Huygens, *Pendulum Clock*, esp. pp. 40–5.

87 Westfall, *Force*, chap. 4.

capable of explaining a number of other phenomena. Robert Hooke (1635–1703), curator of experiments at the Royal Society and professor of geometry at Gresham College, was one of the most prominent scholars of elasticity, and the author of *Lectures de potentia resitutiva, or of springs* (1674). Elasticity was tied not only to mathematics and physical explanations but also to more practical concerns such as horology. Huygens and Hooke realized that the oscillations of a spring were isochronous and tried to construct clocks based on that principle.

Huygens was one among many scholars of his age who sought to combine explanations of physical phenomena in Cartesian terms with mathematical descriptions. For several years, Isaac Newton, since 1669 the Cambridge Lucasian Professor of Mathematics, also followed a similar approach. On the one hand, Newton speculated on the specific mechanisms causing gravity, and on the other he calculated that the terrestrial vortex was compressed by the solar vortex by approximately 1/43 of its width.[88]

## NEWTON AND A NEW WORLD SYSTEM

The dichotomy between mathematical and physical explanations of gravity mentioned earlier was not unique to Huygens. Hooke also studied certain problems with a similar dual approach whereby mathematical and physical concerns were not always present at the same time. In his study of the motion of celestial bodies, Hooke talked of attractions and provided several inspiring comments. His analysis of the role of force in curvilinear motion differed from that of most Continental scholars. Whereas on the Continent mathematicians favored the idea of an imbalance between opposing centrifugal and center-seeking tendencies, Hooke explored the combination of a rectilinear uniform motion with a center-seeking tendency. It seems plausible that Hooke developed this approach in the mid-1660s while studying the bending of light rays. Curiously, it was the same comet of 1664 that first aroused Newton's interest in astronomy. Hooke saw an imperfect but revealing analogy between the motion of celestial bodies and that of the bob of a conical pendulum. In both cases, a central attraction deflects a body from its rectilinear path, but whereas in the conical pendulum the force increases with distance, in celestial bodies the central force was likely to decrease.[89]

Starting from a celebrated correspondence with Hooke in 1679 on falling bodies on a moving earth, Newton began working at the problem of curvilinear, and especially planetary, motion following Hooke's approach. Shortly

[88] Eric J. Aiton, *The Vortex Theory of Planetary Motion* (New York: American Elsevier, and London: Macdonald, 1972); and Derek T. Whiteside, ed., *The Preliminary Manuscripts for Isaac Newton's 'Principia', 1684–1686* (Cambridge: Cambridge University Press, 1989), p. x.

[89] Jim A. Bennett, "Hooke and Wren," *British Journal for the History of Science*, 8 (1975), 32–61; Ofer Gal, *Meanest Foundations and Nobler Superstructure* (Dordrecht: Kluwer, 2002).



thereafter, Hooke began to discuss mathematical problems of planetary orbits with London mathematicians Edmond Halley (1656–1742) and Christopher Wren. Thus, in the first half of the 1680s, several English mathematicians debated problems of celestial motion, such as the elliptical orbits of planets, from a mathematical standpoint, without paying immediate attention to physical causes. Moreover, they were using analogous conceptual tools, without having recourse to a Huygensian centrifugal force. Only Newton succeeded in finding an answer to the problem of the attractive force required to produce Keplerian elliptical orbits.[90]

In 1681, Newton engaged in a correspondence on the huge comet of 1680–1 with John Flamsteed (1646–1719), the Astronomer Royal at Greenwich. Initially, like most of his contemporaries, Newton believed in the existence of two comets, one approaching the sun and another regressing from it. Flamsteed made some clumsy attempt to convince him of the contrary by arguing that the comet had turned in front of the sun and that it was attracted by its magnetic virtue while approaching it and repelled when moving away, but to no avail. Newton pointed out that the comet could not possibly have turned in front of the sun but had to move behind it, and objected that the sun could not be magnetic because magnets are known to lose their power when heated. Despite Newton's rejection of Flamsteed's views, it is easy to see how crucial those views were to become in just a few years, when comets became assimilated with other celestial bodies such as planets and satellites moving under the action of universal gravity.

It is not clear when Newton attained his first result, namely that for elliptical orbits the force is inversely proportional to the square of the distance. Most likely this occurred at the time of his correspondence with Hooke, but thereafter Newton let the matter sleep. By the fall of 1684, following a visit to Cambridge by Halley, Newton produced his first tract on the subject, *De motu corporum in gyrum* (On the Motion of Bodies in a Circle), which was registered at the Royal Society. Newton was able to account also for the two other Keplerian laws of planetary motion besides the first, which states that the orbits are ellipses, where the sun occupies one of the foci. He proved that trajectories described under a central force describe areas proportional to the times, Kepler's second law, and that the squares of the revolution periods of both planets and satellites are as the third power of the major semiaxis of the ellipse – Kepler's third law.

In the following months, Newton went through an extraordinarily creative period during which he accounted for a huge number of phenomena on the basis of his inverse-square law of gravitational attraction and underwent a radical transformation in his views about nature and its creator. The

[90] Derek T. Whiteside, "The Prehistory of the *Principia* from 1664 to 1686," *Notes and Records of the Royal Society of London*, 45 (1991), 11–61, provides an excellent account. See also D. Bertoloni Meli, "Inherent and Centrifugal Forces in Newton," forthcoming in *Archive of History of Exact Sciences*.

mathematical and physical results of his research appeared in 1687 as *Principia mathematica*, a large 500-page book published under the auspices of the Royal Society and seen through the press by Halley. What Newton achieved in a couple of years would have more commonly been the result of as many decades and not surprisingly was to prove exceedingly challenging to most of his contemporaries and even his immediate successors.

The work starts with a set of definitions and laws. Especially prominent among Newton's definitions are those of mass, separating it conceptually from weight, and centripetal force. Newton later established with a famous experiment reported in Book 3 that weight and mass are proportional, most likely in response to Descartes, who had denied as much in Part IV of *Principia philosophiae*.[91] Centripetal force was a neologism that became a symbol of Newtonianism. Among the laws of motion, the first, known as the law of inertia, states that a body preserves its state of rest or rectilinear uniform motion unless it is acted upon, and expressed a notion that was generally accepted by 1687. The second law was valid both for attractions and for impulses and stated that the change in quantity of motion is proportional to the motive force impressed and is directed in the same line. The third law, stating that action equals reaction, was the only law Newton attempted to prove experimentally both for collisions, using pendulum bobs, and attractions, using magnetic bodies. It is equivalent to the conservation of quantity of motion in one direction, or as we would say, vectorially.[92]

Books 1 and 2 deal with the motion of bodies in spaces void of resistance and in resisting media, respectively. Book 1 is almost exclusively mathematical, whereas Book 2 provides a mathematical account of motion in resisting media and a refutation of the existence of an aethereal fluid medium filling the spaces and penetrating bodies on the earth. In Book 3, Newton moved to the system of the world and stated the law of universal gravity, according to which all parts of matter attract each other with a force inversely proportional to the square of their distance.

We have seen in the previous section that up to the late 1670s, and probably until the beginning of the 1680s, Newton subscribed to a view of nature that was dominated by subtle fluids and vortices largely inspired by Descartes' *Principia philosophiae*. Probably late in 1684 or in 1685, Newton's views changed dramatically, and he rejected those physical explanations he had followed for decades. It is likely that Newton chose the title of his work to mark his rejection of Cartesianism. His emphasis on mathematical principles highlights a key difference from Descartes. The latter developed philosophical principles, as his title suggests, starting from the principles of human knowledge. Although Descartes stated that the principles of his *physica* were

[91] Newton, *Principia*, new translation, pp. 403–4, 806–7. I. Bernard Cohen has provided a detailed and reliable account of the contents of the *Principia* in the guide accompanying the new translation.
[92] Newton, *Principia*, new translation, pp. 416–7.



the same as those of mathematics,[93] in practice most of the mathematics amounted to accounts of the shape and size of particles. In the work of Newton, by contrast, mathematics occupied a leading position in a very different sense. The mathematical principles prominently advertised in the title examined theoretically a series of situations. Experiments and observations then often served to select from those possible mathematical constructions those applying to the real world. At times, Newton tried to argue that this was a more secure method of inquiry in natural philosophy,[94] but elsewhere he implied that his method required due caution. In Book 2, for example, he examined the properties of hypothetical fluids composed of particles repelling each other with forces varying as a power of their distances. One of those repulsion laws led, with significant mathematical simplifications, to Boyle's law for gases, whereby the density is proportional to the compression. Newton showed, assuming that fluids consist of particles repelling each other, that the converse was also true. Yet he added a significant qualification: "Whether elastic fluids consist of particles that repel one another is, however, a question for physics. We have mathematically demonstrated a property of fluids consisting of particles of this sort so as to provide natural philosophers with the means with which to treat the question."[95] A similar reasoning could be easily applied to the universal attractive force at the center of his treatise.

By investigating more and more areas, Newton realized that the phenomena of the heavens, as well as tides and the shape of the earth, came under the compass of his inverse-square law. Celestial motions were especially significant because they had been observed for millennia: The motions of planets, for example, were known to be exceedingly regular, a sign for the astute mathematician that force decreases exactly as the inverse square of the distance. The regularity of the motions of planets and satellites, and the motion of comets in all directions, led Newton to suspect that celestial motions were not due to a fluid vortex, which would have hindered them. The initial suspicion became more and more ingrained with a pincer movement, on the one hand explaining more and more phenomena from the same assumptions and on the other showing the contradiction arising from the hypothesis of vortices.

The structure of *Principia mathematica* reflects Newton's methodological predicament. Book 1 can be seen as a carefully contrived *pars construens*, whereas Book 2 was intended as a lengthy *pars destruens*, clearing the way for his system of the world in Book 3. Book 2 ends with a refutation of Cartesian vortices, arguing that they were incompatible with Kepler's laws of planetary motion, but the entire book was geared toward an attack on vortices

[93] Descartes, *Principia philosophiae*, p. 2, para. 64.
[94] George Smith, "The Methodology of the *Principia*," in *The Cambridge Companion to Newton*, ed. I. Bernard Cohen and G. Smith (Cambridge: Cambridge University Press, 2002), pp. 138–73.
[95] Newton, *Principia*, new translation, pp. 588–9, Scholium to sec. 11, and pp. 696–9, quotation at p. 699. Here, by "physics" Newton meant experiment as well. See Smith, "The Methodology of the *Principia*."

even in innocent-looking parts. For example, Newton tried to determine the speed of sound to show, contrary to Robert Boyle (1627–1691), that air is the only medium through which sounds propagate and that no other medium is required.[96] By removing from the heavens the material fluid commonly believed to carry the planets, Newton left open the problem of the cause of gravity. His opinion oscillated somewhat in later years, but at the time of composition of the first edition of *Principia mathematica* it appears that Newton believed God was responsible for gravity through his presence in space. Gravity would thus be caused by an immaterial divine agent immediately present and acting on all the bodies in the universe. Although Newton was not so explicit in *Principia mathematica*, he believed he had said enough for those wishing to understand to realize that the cause of gravity he had envisaged was not material.

These preliminary observations and the contrast between Descartes and Newton highlight both the high methodological profile Newton gave to mathematics and also the range of his investigations. Fundamental as the link between ellipses and an inverse-square attraction was, it proved to be only one of the wealth of results attained by Newton in *Principia mathematica*. In Book 3, Newton put forward his demonstration about universal gravity and was able to account for the motion of planets and satellites, especially the moon, and also for the precession of the equinoxes, tides, and the motion of comets. While writing Book 3, Newton collaborated extensively with Flamsteed, who willingly provided a wealth of astronomical data on the moon, the satellites of Jupiter and Saturn, the shape of Jupiter, and the trajectory of comets.

Although Newton was acutely aware of the importance of styles and methods in mathematics, in *Principia mathematica* the main emphasis was on attaining results. Newton used a heterogeneous set of tools, including the method of first and last ratios – a form of infinitesimal geometry – series expansions, and occasionally the calculus of fluxions. One of the most remarkable features of Newton's work was its use of a wide range of mathematical tools and techniques to produce quantitative predictions and assess orders of magnitude. He did so even for such famously intractable cases as the three-body problem, namely the determination of the motions of three reciprocally attracting bodies.

## READING NEWTON AND DESCARTES: LEIBNIZ AND HIS SCHOOL

Unlike Descartes, Newton made sure his *Principia* could not be read as a novel. Descartes could address in print Princess Elizabeth as the ideal reader, whereas probably the first female reader who could truly understand – and

[96] Newton, *Principia*, new translation, pp. 776–8. The relevant passage from the first edition is translated in a footnote. See also Boyle, *New Experiments Physico-Mechanical Touching the Spring of the Air*, experiment 27.



indeed translate into French – Newton's work was the Marquise du Châtelet (1706–1749), over a half-century after the book was first published. For a number of reasons, male readers did not fare much better. Even though Newton had kept the calculus of fluxions at the margin of the *Principia*, there was still plenty of cutting-edge mathematics in his work to make it exceedingly challenging for anyone who was not an expert mathematician. A leading philosopher such as John Locke (1632–1704) had to ask Huygens whether he could trust Newton's theorems because he was unable to assess them on his own.[97] Even Huygens and Leibniz, the two leading mathematicians on the Continent, found the work daunting. Because they were not prepared to accept universal gravity on philosophical and, in the case of Leibniz, also theological grounds, they were reluctant to follow page after page of challenging mathematics: Why go through them all if Newton's system was based on the absurd principle of attraction? Wren, too, expressed doubts about Newton's apparent rejection of a physical cause for gravity. Until the turn of the eighteenth century, the problem of a physical cause for gravity was the major concern of the few readers who could follow Newton.[98]

In many respects, reading *Principia mathematica* was colored by contemporary readings of *Principia philosophiae* and developments of Cartesianism, broadly conceived. Physical causes were not the only problem, conservation being another prominent issue. The third law of nature in Descartes' *Principia philosophiae* stated the conservation of quantity of motion in the universe and specifically in impact. Others, too, had relied on different notions of conservation in a range of contexts. Galileo, for example, had claimed that a pendulum displaced from the equilibrium position could rise back to its original height. In the second half of the seventeenth century, several scholars worked with the notion of conservation, but Newton was not among them. Whereas Descartes had seen in conservation a sign of divine order, Newton saw with equal if not greater commitment the lack of conservation in the form of a constant decay and the appearance of periodic phenomena such as comets as a sign of God's intervention and action in the world. These radically different views were prominently debated in 1716–7 by Samuel Clarke (1675–1729), a theologian allied with Newton, and the German polymath Leibniz, councilor and librarian to the Duke of Hanover. Their exchange went through Caroline, Princess of Wales (1683–1737), a woman with deep theological concerns. From our perspective here, it is worth highlighting

[97] Niccolò Guicciardini, *Reading the 'Principia': The Debates on Newton's Mathematical Methods for Natural Philosophy from 1687 to 1736* (Cambridge: Cambridge University Press, 1999), explores the range of mathematical methods used by Newton and the way his work was read by mathematicians. On reading Newton's *Principia* largely in an English context, see Rob Iliffe, "Butter for Parsnips: Authorship, Audience, and the Incomprehensibility of the *Principia*," in *Scientific Authorship: Credit and Intellectual Property in Science*, ed. Mario Biagioli and Peter Galison (London: Routledge, 2003), pp. 33–65.

[98] Isaac Newton, *The Correspondence of Isaac Newton*, ed. Herbert W. Turnbull, J. F. Scott, A. R. Hall, and L. Tilling, 7 vols. (Cambridge: Cambridge University Press, 1959–77), 4: 266–7. See also Bertoloni Meli, *Equivalence and Priority*.

a curious feature of Newton's views, namely his belief that motion in the universe would decay were it not for the presence of active replenishing principles such as gravity and fermentation.[99] Newton's emphasis on the decay of motion caused by the lack of elasticity of bodies means that although he disagreed with Descartes on the conservation of quantity of motion – in a Cartesian sense, not including direction – he still considered it a meaningful notion.

Leibniz was probably the most ardent advocate of conservation principles and at the same time paradoxically the most prominent critic of Descartes, because he disagreed with Descartes on what was conserved. Leibniz did not consider quantity of motion as a very significant notion, either in the Cartesian sense without direction or in the Huygensian sense with direction. Rather, he believed that he had identified the conservation of a different notion as a key law of nature. The new conservation law concerned "force," which Leibniz claimed was proportional either to the square of the speed or to the height to which a body can rise. In the case of impact, Leibnizian force was called *vis viva*, or living force, and was proportional to the body's mass and square of the velocity, or $mv^2$. The problem is that when the colliding bodies are not elastic, *vis viva* is not conserved. In those cases, Leibniz argued that the portion of *vis viva* that appeared to be lost was in fact absorbed by the small components of the colliding bodies. Leibniz provided no direct empirical justification for his claim, but rather he seems to have established the conservation principle in general terms as a law of nature and then found ways to apply it to all cases, including problematic ones. In 1686, Leibniz published in the *Acta eruditorum* a brief essay designed to enrage the Cartesians, *Brevis demonstratio erroris memorabilis Cartesii* (A Brief Demonstration of Descartes' Celebrated Error). His plan succeeded probably beyond his own expectations, and the controversy over the conservation of force became a major feature of mechanics in the first half of the eighteenth century.[100]

With the new century, interest shifted to Newton's mathematics for two main reasons. With the explosion of the priority dispute over the invention of calculus between Newton and Leibniz, Continental mathematicians such as Johann (1667–1748) and Niklaus (1687–1759) Bernoulli started combing through *Principia mathematica* in search of errors showing Newton's inadequate knowledge of calculus. Secondly, in 1700, French mathematician Pierre

[99] Samuel Clarke, *A Collection of Papers, which Passed between the Late Learned Mr. Leibnitz and Dr. Clarke in the Years 1715 and 1716* (London: Printed for J. Knapton, 1717); E. Vailati, *Leibniz & Clarke: A Study of Their Correspondence* (Oxford: Oxford University Press, 1997); D. Bertoloni Meli, "Caroline, Leibniz, and Clarke," *Journal of the History of Ideas*, 60 (1999), 469–86; and Isaac Newton, *Opticks* (New York: Dover, 1952), Query 31, pp. 397–401, at p. 398.

[100] An excellent account is Daniel Garber, "Leibniz: Physics and Philosophy," in *The Cambridge Companion to Leibniz*, ed. Nicholas Jolley (Cambridge: Cambridge University Press, 1995), pp. 270–352.



Varignon (1654–1722), a member of the Paris Académie Royale des Sciences, started publishing a series of memoirs on motion under central forces and in resisting media where he translated several propositions by Newton into the language of the differential calculus. Leibniz had published the key rules of calculus in a series of essays in the German journal *Acta eruditorum*, starting with the celebrated *Nova methodus pro maximis et minimis* (New Method for Finding Maxima and Minima, 1684). Although Varignon's work did not contain new results and was largely dependent on Newton, he made some of Newton's results more accessible by systematizing mathematical procedures and notation. Thus his originality cannot be assessed so much with new theorems as with a new style to deal with the science of motion using the differential calculus. In particular, Varignon wrote equations of motion where time appears prominently rather than being swallowed by other symbols.[101] Varignon was careful to treat Newton's work in a purely mathematical fashion, maintaining a noncommittal attitude toward the issue of physical causes. He managed to remain on good terms with both the Newtonian and Leibnizian camps. Despite the talent of Continental mathematicians, the effectiveness of the differential calculus, and their tireless efforts, it is probably fair to say that the results they achieved in competition with *Principia mathematica* were negligible, amounting to the correction of a few inaccuracies and the tightening of some theorems.

Continental mathematicians, however, also worked on other themes relevant to mechanics, such as the study of new curves described by bodies under given conditions and elasticity. Prominent among the new curves were the catenary, described by a chain hanging perpendicular to the horizon from two nails fixed in a wall, and the curve of fastest descent between two points, which remarkably turned out to be the same cycloid that Huygens found to make pendulum oscillations isochronous. Jakob Bernoulli's works on the elastic beam were especially noteworthy.[102]

In 1713, Newton published a second edition of his *Principia*, and the third edition followed in 1726. The second was seen through the press by the Cambridge Plumian Professor of Astronomy Roger Cotes (1682–1716), a very talented mathematician, who transformed large portions of the work and corrected several mistakes. Cotes was the sort of editor whose letters authors open with a shaky hand. He combed the text with unparalleled acumen and patience and never let the matter rest even when Newton made it clear that he was unwilling to embark on major revisions. Newton reshaped large sections of Book 2 especially and performed many new experiments on the motion of bodies in resisting media. From the standpoint of the role of experiments, the second edition – and the third, too – appears like a different book.

[101] See Michael Blay, *La naissance de la mécanique analytique* (Paris: Presses Universitaires de France, 1992), for an account of Varignon's achievements and bibliography.

[102] Benvenuto, *Structural Mechanics*, ad indicem.