SEEGER

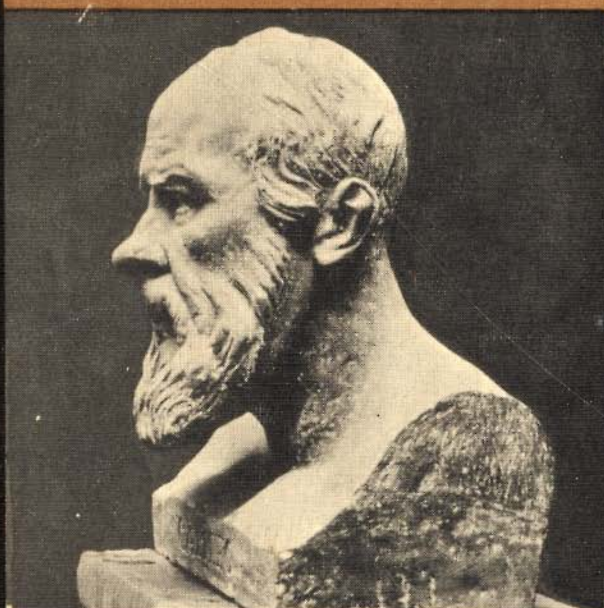
MEN OF PHYSICS: GALILEO GALILEI,
HIS LIFE AND HIS WORKS

Selected Readings in Physics

MEN OF PHYSICS:

Galileo Galilei,
his life and his works

RAYMOND J. SEEGER
National Science Foundation



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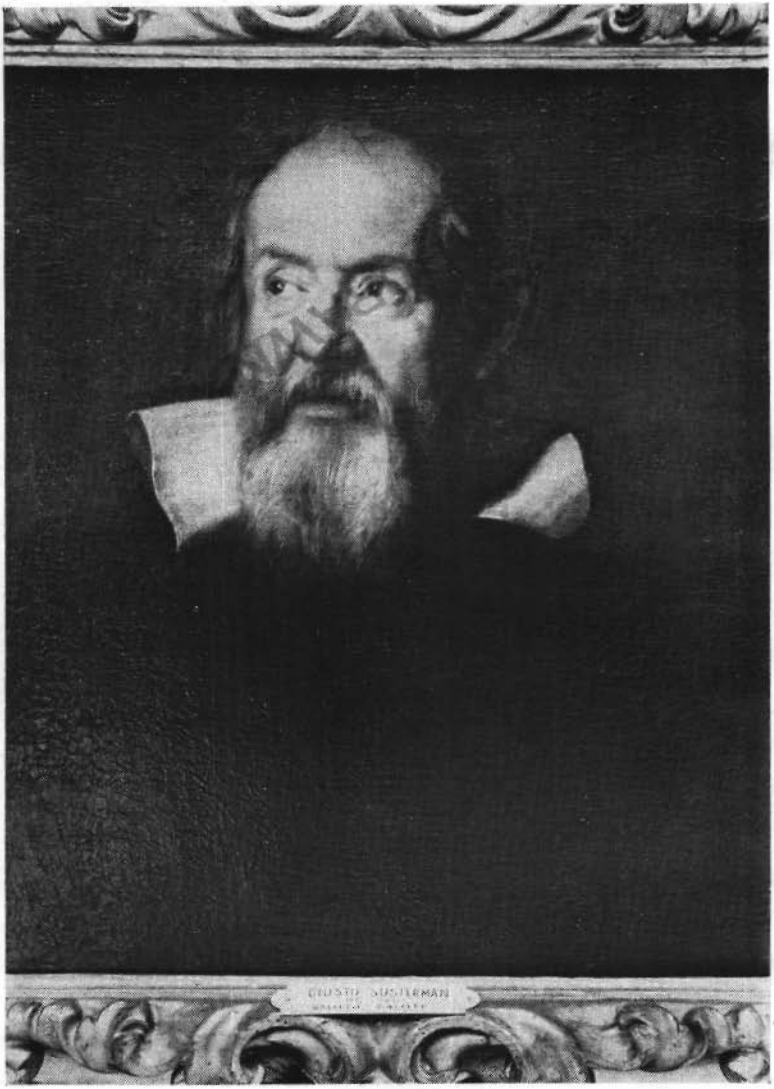

Portrait of Galileo by Süsterman, Uffizi Gallery, Florence

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Men of Physics

Galileo Galilei, his life and his works

BY

RAYMOND J. SEEGER

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PREFACE

THE distinctive feature of modern Western culture has been the growing social importance of science and technology. No one, however, can hope to comprehend the historical development of science without taking into account the role of Galileo Galilei. He lived during the critical transition period between the medieval and modern eras. He pioneered the path to our own understanding of physical phenomena. He grappled with complex problems that still face us; his methodology and ideas are embedded in the very foundations of physics today. As we examine directly the thinking inherent in his works, from the vantage point of the present, we encounter new light on old experiences. The tacit assumptions of crystallized science today stand out vividly in the fumbling procedures of its amorphous beginnings. They need to be re-evaluated to give us a sense of direction for their future use.

Galileo began his career in academic institutions and concluded it in the service of a powerful city-state. His prominent position afforded many contacts with intellectuals in the universities and in the Church. His radical approach and dominating personality led inevitably to friction with both the philosophers and the theologians of his day. The Church trial of Galileo is one of the celebrated cases of history. Despite his social problems, Galileo was sincerely a religious man of science.

Galileo, with his broad interests in art and music and with his skill as a popular writer, was a typical renaissance man—with an interest also in science. His integrated life, therefore, is well worth studying in view of our current concern about the fragmentation of the culture of our own time.

Thanksgiving Day 1965.
Washington, D.C.

Part 1. HIS LIFE

CHAPTER 1

CURIOUS STUDENT

GALILEO GALILEI,[1,2,4] the founder of modern physics, was born near the Arno River in Pisa (a Roman colony of the second century B.C., conquered by Florence in 1405) on 15 February 1564 when Cosimo de' Medici (1519-74) the Great (not Cosimo I, the Elder, (1389-1464) who founded the Medici dynasty), was Duke of Florence (first Grand Duke of Tuscany as of 1569). Galileo's family, which was of the lower nobility, had originally been named Bonajuti. Two of his ancestors had held high offices in the Republic of Florence. The burial-place of one of these, a physician also named Galileo Galilei, is marked in the floor of the nave in Florence's Franciscan Church of Santa Croce. Galileo's own father, Vincenzio (1520-91), was the great-grandson of the brother of this physician. He was traditionally a musician; he played the lute. He had broad intellectual interests, including mathematics and the classics; he studied music in a spirit of free inquiry. In the Introduction of his *Dialogue on Ancient and Modern Music* (1571) he claimed: "They who in proof of any assertion rely simply on the weight of authority, without adducing any argument in support of it, act very absurdly." Vincenzio's own outlook was thus indicative of the independenec of mind and the spirit of combat that later characterized his son. In 1562 he married Giulia Ammannati (1538-1620) of nearby Pescia; they had seven children (three sons and four daughters), the eldest of whom was Galileo.

Galileo's early education was at the Pisan school of Jacopo Borghini, supplemented by his father's help in the classics. As a boy, Galileo showed a certain mechanical inventiveness. Some-time after his family moved to the ancient Etruscan town of Florence in 1574, he himself attended the monastery school (founded in 1060) of Santa Maria di Vallombrosa (25 miles east of Florence), but he was withdrawn by his father in 1579 — possibly owing to a seemingly undue religious influence. In these beautiful wooded heights, he received the typical literary education of that period, viz. primarily the classics, including his favorite authors like Ovid, Seneca, and Virgil. He enjoyed even then and throughout his whole life the popular Italian poetry of his day for its own value (in contrast to learning *per se*); for example, that of Dante Alighieri (1265-1321), Francesco Petrarca (1304-74), Ludovico Ariosto (1474-1533), and Torquato Tasso (1544-95). In his youth he preferred Ariosto and is said to have known by heart the "Orlando Furioso" (popular in every century except the twentieth); in the "Two New Sciences" he referred to Tasso as a "divine poet". In addition, he was interested and skillful in music; he played the lute best, but was also adept in other instruments such as the organ. He showed such skill in drawing and painting that, as he himself confessed later, he would probably have chosen painting as a career if circumstances had permitted. (Ludovico Cardi da Cigoli (1559-1613) admitted his own indebtedness for Galileo's views on painting; in 1612 he painted the Virgin, with Galileo's sketch of the moon at her feet, for Santa Maria Maggiore in Rome.) Galileo, in short, was a typical humanist of the Italian Renaissance, who had also a genuine curiosity about physical phenomena.

On 5 September 1581 Galileo matriculated as a student in the Faculty of Arts of the University of Pisa, presumably to study medicine. His interest, however, gradually became focused upon natural philosophy, toward which he adopted so critical an attitude that he was dubbed "the wrangler". Throughout his life, indeed, he was opposed to authority as expressed in mere dogma, unsupported by experiential evidence. He undoubtedly became acquainted in 1583 with mathematics through the extra-curricular interest of a Tuscan court tutor, Ostilio Ricci, a pupil of Nicolo of Brescia, Tartaglia (1500-57), who, in 1543, had translated the Greek writings of Archimedes (287-212 B.C.) into Latin.

There is a well-known legend about the discovery of the isochronism of a pendulum by Galileo, whose curiosity was aroused while watching a swinging lamp during a service in the Cathedral of Pisa in 1583 (not the 1587 Possenti bronze one now hanging there). Although the amplitude of the vibration was observed to decrease steadily, the period of a complete swing to and fro (timed by his pulse) remained the same. He later designed an instrument, known as the pulsilogium, which enabled one to adjust the length of a pendulum for a fast or slow oscillation, corresponding to the frequency of a patient's pulse — typical of his innate desire to translate his discoveries into use. His physical insight is exhibited in his replacement of the complex chandelier by a simple pendulum (i.e. a thin cord with a bob at the end) as an approximate model. He was not satisfied with merely describing the phenomenon qualitatively, but subsequently investigated the quantitative factors that determine the period (cf. Section 15a). His whole scientific approach was motivated by a desire to know "how" and "how much" in order to understand "why". Pendulum motion remained a stimulant to Galileo's thinking throughout his entire life.

Owing to a lack of funds and his failure to obtain a needed scholarship, in 1585 he had to leave the University and return to Florence, an academic dropout; he never did receive a university diploma. He intermittently tutored students in mathematics both at Florence and the neighbouring Tuscan town of Siena. While at home he attempted also to write plays and poems. Having studied Euclid (*c.* 365 to *c.* 275 B.C.), Galileo turned his attention to the works of another Greek who had been adept in geometry, namely, the Syracusan Archimedes. In 1586, accordingly, he constructed a hydrostatic balance (cf. Section 12) to determine accurately the relative amounts of two metals in an alloy mixture, which he described, in Italian, in a paper entitled, "The Little Balance",† not published until 1644. About the same time he became interested in another Archimedian concept, the center

† Ref. (2), Appendix, p. 137.

of gravity, and wrote, in Latin, a scholarly paper on "Theorems about the Center of Gravity of Solids" (not published until 1638, and then as an appendix to the *Two New Sciences*). In 1587 he made his first trip to Rome, presumably to make himself known to scholars there and to enlist their support for his studies; he met the German astronomer, Father Christopher Clavius (1537-1612), professor at the Jesuit Romano Collegio (founded in 1552, succeeded by the modern pontifical Gregorian University after governmental occupation of the buildings), who had been responsible for the Gregorian revision of the calendar in 1582. In 1588 he gave two lectures on the site and dimensions of Dante's Inferno to the Florentine Accademia della Crusca (1582, word means chaff). Upon the recommendation of Guidobaldo, Marquis del Monte (1545-1607), a nobleman with scholarly interests, in July 1589 he was appointed by Ferdinand I de'Medici (1549-1609) to the Chair of Mathematics at the University of Pisa, although he was only 25 years old — not at all surprising in view of the general lack of Italian interest in mathematics at this time and the exceptional skill of Galileo in its use.

CHAPTER 2

RESEARCH PROFESSOR

As a professor, Galileo combined his teaching with research. He was sincerely and enthusiastically interested in communicating his ideas to serious minds. He was never a neutral teacher; convinced of the correctness of his own conclusions, he was eager to have others share his views. Continuing his geometrical studies, in 1590 he discovered the cycloid, i.e. the curve traced by a point on the circumference of a wheel rolling on a horizontal plane. Although he surmised (possibly from the relative weights of cut-out pasteboard figures) that the area under this curve was probably three times that of a circle, he himself was never able to demonstrate this fact mathematically. Shortly thereafter, the Ponti de Mezzo bridge across the Arno at Pisa was constructed from such a cycloidal design.

He turned his attention now to Aristotle's natural philosophy, which had been critically examined by many before him, including the Pisan Professor of Philosophy, Francesco Buonamico (d. 1603), and the Venetian mathematician Giovanni Battista Benedetti (1530-90). Galileo, however, impressed critically upon all previous speculations a new criterion, namely, the checking of all conclusions directly with nature to ascertain if those obtained by pure reasoning agree with the actual observations. Nature was to be both source and resource of physical theory. One must not only look and listen for clues in nature's universal broadcast, but also formulate leading questions and ferret out nature's answers. Metaphysics might be appropriate, if at all, *after* physics — certainly not before physics. Anthropocentric speculations had to give way to nature-centered thinking. Inability to deduce physical phenomena from *a priori* first principles of general philosophy necessitated a search for experiential first principles of a restricted physics that make theoretical deductions practicable.

Tradition relates that Galileo performed his celebrated experiment of dropping two different weights from the Campanile, the 179-foot high Leaning Tower of Pisa, in 1590-1. Although this event is truly legendary, there being no specific record by Galileo or by anyone else as to its actual occurrence, it seems to be quite in keeping with Galileo's own interests, opportunity, and character. The Greek philosopher Aristotle (384-322 B.C.) had reasoned that the speed of a freely falling body should be in proportion to its weight, the cause of its natural motion. Experience, however, shows that the speed of fall is independent of the weight of a body, if the frictional resistance of the medium is negligible.

During this same period (*c.* 1590), Galileo compiled some scholarly notes (in Latin), "On Motion"(3) (not published completely until 1883). It was a peculiar mixture of old conceptions, current misgivings, and new speculations; unquestionably rooted in medieval soil, but growing toward modern light. For example, he definitely retained the Aristotelian idea of natural places for different earthly materials (cf. Section 9): water ever-present, the earth below, the air above, and fire above all. Aristotle's idea that there must always be a cause for motion was combined with the medieval notion that some vague property, impetus, is retained by a moving body, even when the external cause has been removed. Galileo reported some of his early investigations about bodies falling in different media. Here one finds also the first record of his pendulum observations. To understand motion, indeed, may be said to have been a primary lifetime interest of Galileo. It is preferable, accordingly, to judge his dynamics ideas on the basis of the *Two New Sciences* (1638), which expressed his cumulatively mature judgments. Galileo's inquiring mind was certainly not unique, but rather typical of the critical atmosphere prevailing in Europe, both in the southern Renaissance countries and in the northern Reformation ones.

The death of Galileo's father in 1591 brought with it the ever-growing financial needs of a family of which he was now the responsible head — exemplified in the dowry requisite for the marriage of his oldest sister Virginia to Benedetto di Luca Landucci. Occasional tutoring, indeed, had already been undertaken to supplement the meager salary he had been receiving at Pisa. The University, therefore, which had not been particularly hospitable, became less and less attractive. Matters came to a head when he could not honestly recommend the design of a dredging machine for the Leghorn harbor by Giovanni de' Medici (d. 1621), a natural son of Cosimo. In the summer of 1592, consequently, he resigned from the University of Pisa and returned to Florence.

Fortunately, the mathematical chair at the University of Padua (founded traditionally by the Trojan Antenor, but part of Venice as of 1405) had been vacant since 1588 — indicative of the little interest in mathematics there. He secured appointment to it in September 1592. There he found some kindred spirits in free thinking. He soon became friends with the famous Averroes (1126-98) philosopher Cesare Cremonini and the distinguished anatomicist and surgeon Geronimo Fabrizio d'Acquapendente (*c.* 1533-1619), for whom the famous anatomical theater was built in 1594; one of his students was Gustavus Adolphus (1594-1632), later king of Sweden. William Harvey (1578-1657), the English physician who later discovered the circulation of the blood, was also a student there about that time. In many respects the eighteen years that Galileo spent at Padua were the most productive in the development of his own understanding of physical phenomena. Early (1593, 1594, 1600) he wrote, in Italian, some notes, "On Mechanics",[3] which represented the best systematic summary of the statics of simple machines then known (not published until 1634, and then in French by Père Marin Mersenne (1588-1648); published in Italian in 1649). Nowadays it is particularly valuable as a source of the knowledge of this interesting subject during that period.

Machines all operate on the same physical principle so that a complete understanding of any one of them is adequate for the deduction of the mechanical properties of all others. Galileo, for example, chose the lever as fundamental, and used it to derive the law for an inclined plane (cf. Section 13). In this connection, he recognized the experiential characteristic of all machines, namely, that whatever is gained in force is lost in speed (sometimes loosely said to be the principle of virtual velocities, which was not enunciated clearly until 1717 by Jean Bernoulli (1667-1748)). Even in this instance Galileo made some significant contributions, namely, the importance of the direction of the motion, and the relation between a machine's input and its output — more precisely stated later by Evangelista Torricelli (1608-47), viz. that any spontaneous motion of an isolated system's center of gravity can only be downward. About the same time, Galileo obtained a patent from the Venetian Republic for a machine to raise water — apparently successful, but not much used.

In 1593, in company with some friends, one of whom died therefrom, he received a severe chill while asleep in a room "air-conditioned" from a nearby conduit leading to a cave. He suffered an arthritic condition which bothered him the rest of his life.

In 1597 Galileo designed and constructed a popular "Geometric and Military Compass", which served as a compass, a divider, and a quadrant. In addition, various lines were marked off numerically for different calculated results: for example, an arithmetical line containing the "rule of three" among others, a geometrical line for mean proportion, etc., a stereometrical line for cube roots, etc.; on the other side, a polygraphic line as an aid for drawing regular polygons, etc., and a pentagonal geometrical line for "squaring" figures. Galileo not only designed this instrument but, with the aid of a full-time mechanic, actually manufactured it as well as magnetic compasses, drawing instruments, and, later, other devices — all for sale. Galileo's curiosity about natural phenomena was undoubtedly closely linked with his own skill in handling materials. At Venice, indeed, he was a frequent visitor at the celebrated Arsenal with its galleys and shipyards; he was a consultant on military engineering. In 1597 he obtained a Venetian patent for a machine to hoist water.

About 1602, having obtained a copy of the first English scientific book, *On Magnetism* (*De Megnete*) by William Gilbert of Colchester (1544-1603), physician to Queen Elizabeth I, he at once set about investigating magnetic phenomena, in particular, the production of powerful armatures (described later in the *Two Chief World Systems*, cf. Section 7).

An interesting letter was written by Galileo in 1604 to his friend Fra Paolo Sarpi (1552-1623) of the Order of Servites, a Venetian councillor. In it he gave the correct law for the increase in speed of freely falling bodies, but he deduced it quite incorrectly. Scholars today still debate whether Galileo arrived at this law primarily inductively, or deductively, or by mixture of these two — certainly not the second in view of the wrong proof. At any rate, later he derived the law satisfactorily, as is evident in the *Two New Sciences.*

In 1606 he made a thermoscope for measuring relative changes of temperature. Despite the fact that not all quantities can or should be measured, it is amazing that no one had previously produced a more objective means of determining temperature than the human body. Specifically, he took a glass bulb with a long slender stem, heated it to drive out much of the air, and then inserted the inverted stem into a container of water. The amount of the rise of the water in the stem is dependent upon the temperature of the residual air. Unfortunately, barometric pressure is also a factor — not corrected until 1653 when the practice of hermetically sealing a tube was introduced by Leopold de' Medici (later Cardinal, d. 1675), cofounder with his brother, Ferdinand II (1610-70), of the Florentine Accademia del Cimento (1657-67) with its cautious motto, "Probando e Reprobando" (testing and re-testing).

About the same time he wrote a manual of instructions for his Geometric and Military Compass and dedicated it to young

Cosimo, whom he had been tutoring during the previous summer vacation at Florence. This book was plagiarized by a student from Milan, Baldassare Capra, so that Galileo felt compelled in 1607 to write a defense of his own priority; he received a favorable judgment from the University. In the same book he mentioned his own views about the 1604 Nova (cf. the 1572 Nova is Cassiopeia) in the Constellation Ophiuchus, which had also been attacked by Capra. (Actually both these so-called novae were supernovae.)

In passing, we note some facts about Galileo's personal life during this period. At first, he lived in a house on Pratto delle Valle (now the Piazza Vittorio Emmanuele) near the Benedictine Church of Santa Giustina. In 1599, however, he established separate quarters for his Venetian mistress, Marina Gamba. His first daughter, Virginia, was born in August 1600; his second, Livia, in August 1601; and a son Vincenzio in August 1606 (d. 1649). At the same time he moved to a larger house (No. 9) in the Via Vignali (now Via Galileo), where he had as many as twenty students rooming with him. Here he was able to garden and to enjoy vines, fruits, and flowers. His family problems, however, were accentuated by the need to find a dowry for the marriage of his sister Livia to Taddeo Galletti — not to mention the continual drain upon his resources by his musical brother Michelangelo, who was quite selfish and remained financially dependent upon Galileo during most of his life.

Although Galileo achieved outstanding success in physics research during his Paduan sojourn, he is usually remembered more for his dramatic astronomical discoveries there. To be sure, he had always been interested in astronomical phenomena. As early as 1597, in acknowledging receipt of a copy of the *Mysterium Cosmographicum* by Johannes Kepler (1571-1630), the German astronomer, he confessed, "Many years ago, I became a convert to the opinions of Copernicus". Galileo began correspondence with the Danish astronomer Tycho Brahe (1546-1601) in May of 1600 — terminated soon thereafter by the latter's death. The 1604 appearance of the (super) Nova for 18 months afforded him an opportunity in January 1605 to give three extraordinary lectures on this phenomenon in the Aula Magna of the University. Such popular lectures would fill this room, which seated more than 1000. He himself believed the Nova to be truly a new star. From the determination of its parallax, indeed, he concluded that it was beyond the atmosphere, farther away even than the moon.

Galileo became involved also in court horoscopes; for instance, at the request of the dowager Grand Duchess Christina, on 16 January 1609, Galileo predicted a long and active life for Ferdinand I, who unfortunately died only 22 days later. How much Galileo and other scientists of his time sincerely believed in such astrology is difficult for us to ascertain now. Pope Paul III, Alessandro Farnese (1468-1549), kept a private astrologer at the Vatican. In view of Galileo's general scepticism, however, one might suspect that he had his tongue in his cheek whenever he made such horoscopes.

Galileo's lasting astronomical fame, however, developed out of his instinctive curiosity. He happened to hear of a spy-glass which had been made by a Fleming lens-grinder Johannes Lippershey (d. 1619) with two spectacle lenses. Galileo himself reasoned that the combination of a concave eyepiece for magnification and a plane convex object-glass for distinctness might be more successful. His second telescope (so-named later) consisted of two such lenses at the end of a lead tube; it had a magnification of 9 times ($9\times$). He used it for his first public appearance before the Doge, Leonardo Donati, and the Venetian Senate in August 1609; from the San Marco Campanile one could see Santa Giustina in Padua, 21 miles away. In January 1610, by use of such a telescope, he discovered mountains on the moon (contrary to the belief in its perfect sphericity). With his fourth telescope ($20\times$) the planets appeared to be moonlike discs rather than starlike points. With his fifth telescope ($33\times$) he unveiled the starry structure of the Milky Way and the existence of moons (named the Medicean planets by Galileo) revolving about the planet Jupiter. On 4 March 1610 he promptly published all these discoveries in the *Starry Messenger*,[(4)] which he dedicated to the new Grand Duke, Cosimo II (1590-1621), with great expectations of his patronage.

Here again his personal curiosity led him to careful observations with startling results (cf. Section 18). On 7 January he happened to notice three bright "stars" lying on a straight line in the vicinity of Jupiter, one on the west side and two on the east. On the next night, turning to Jupiter again, he noticed that all three were now on the west. On 10 January, however, there were only two, both on the east. On 11 January these two were still on the east but the outer one was twice as large. He continued to observe these celestial objects until 2 March, when he had completed sixty-six observations. He had then concluded that these bodies were moons revolving about Jupiter. The popular excitement caused by this announcement is comparable only to that of Sputnik in our own times. It was, however, more important philosophically inasmuch as Aristotle had maintained that the heavens are unchangeable. It was even more significant in that it lent credence to the notion of the Pole Nicolaus Copernicus (1473-1543) that the earth itself might not be the unique center for all celestial revolutions; that it, too, might be a planet shining on the moon. The prejudices of some of Galileo's contemporaries were as deep-rooted as those of many people today. Giulio Libri (1550-1610), the leading philosopher of Pisa, for example, refused even to look through the telescope — not to mention Cremonini of Padua. On 30 July 1610 Galileo noted a peculiar broadening of the planet Saturn. It looked to him as if three planets were touching (this triple nature was later shown to be merley a confused appearance of Saturn's rings; Galileo himself noted a disappearance of the projections during a certain period). In order to safeguard his priority claim he announced this discovery enigmatically in a group of scrambled letters, which read when transposed: "Altissimum planetam tergeminum observavi" (I have observed the most distant planet is triple).

With these demonstrations Galileo became an internationally known figure. No longer was he an individual scientist simply carrying on his personal research and casually communicating the results to friendly scholars and students; he had now become a spokesman for a wholly new point of view, which stressed the importance of phenomena themselves. Not everyone, indeed, would have — nor could have — transmuted a hearsay story about a curious toy into a powerful research tool. Not unconnected with his success was the fact that throughout most of his life Galileo ground his own lenses — a task not too easy, particularly in the case of the object — glass. By March 1610 he had made 100 telescopes, only ten of which showed Jupiter's moons. It was more than 20 years before anyone else in Europe could make even one telescope as satisfactory as those of Galileo. Kepler, for example, who had a deeper understanding of the optical functioning of a telescope and who designed a terrestrial one of his own, never actually made one himself.

Meanwhile, Galileo's nostalgia for his native Tuscany had come to the fore; he found himself still an alien amid the Venetian culture — so different from the Florentine. Once more he had found the teaching "load" burdensome, particularly that done on the side to supplement his income — necessary despite continual increases in salary. Taking advantage of his world-wide renown, he approached Belisario Vinta, secretary of state for the Grand Duke of Tuscany, about the possibility of a position there. In July 1610 Cosimo II appointed Galileo chief mathematician and philosopher to himself as Grand Duke, as well as head mathematics professor at the University of Pisa, without any teaching duties — quite modern. In September Galileo left his mistress but took his children and returned to Florence. A new period of Galileo's life had begun.

CHAPTER 3

CLEVER COURTIER

TRADITIONALLY Guelfic Florence was much more closely identified with the established Roman Catholic Church than Venice, which had long been respected for its encouragement of freedom of expression. For example, in 1606, when Paul V, Camillo Borghese (1552-1621) of Siena, had put Venice under interdict, the Venetian Republic successfully defied the interdict and retaliated by expelling all the Jesuits, Capuchins, and Theatines from its entire domain. This change of atmosphere turned out unfortunately to be critical for Galileo in view of his unorthodox (but Christian) opinions. In Florence he continued his astronomical investigations; in particular, in December 1610, he announced in a letter to Kepler the crescent phase of the planet Venus in the form of an anagram: "Cynthiae figuras aemulatur mater amorum" (the mother of love emulates the shapes of Cynthia).

In the spring of the following year, Galileo decided to make a good-will trip to Rome to inform people there about his celestial discoveries. He did so at the expense of the Grand Duke, who permitted him also to lodge at the Florence embassy (No. 27 Piazza Firenze). He had a letter of introduction from Michelangelo Buonarrati, Junior, to the Florentine, Jesuit-educated Cardinal Maffeo Barberini (1568-1644). He was received with honor by all, particularly after confirmation of his astronomical findings (but not their interpretations) by a Commission, including Clavius and his successor Father Christopher Grienberger (1561-1636) of the Romano Collegio, which had been set up at the request of the Jesuit Cardinal Robert Bellarmine (1547-1621, later canonized) of Montepulciano. He had a long audience with Pope Paul V.

He was elected to the Accademia dei Lincei (lynx-eyed), which had been founded in 1603 by the 18-year old Prince Federigo Cesi, Marquis di Monticelli and son of the Duke of Aquasparta. The analogous Royal Society of London was chartered in 1662 by Charles II, and the Académie des Sciences in 1666 by Louis XIV.

In Rome he announced his discovery of sunspots (apparently first noted by him in November 1610). In January 1612 the German Jesuit Father Christopher Scheiner (1575-1650), a professor of mathematics at Ingolstadt, published his own observations on sunspots together with an interpretation of them as stars revolving about the sun. Galileo's opinion (cf. Section 17) about his views was requested by Mark Welser, a banker of Augsburg. Galileo wrote three letters in reply in May, August, and December of that year, all from the villa delle Selve (at Signa, about 9 miles west of Florence) of a Florentine nobleman and Lincean, Filippo Salviati (1542-1614), where he was often a guest. The dispute with Father Scheiner was unfortunate; it ignited a spreading fire of misunderstandings with the politically powerful Jesuits (it turned out that credit for the sunspot discovery belongs strictly to still another person, namely, Johannes Fabricius of Wittenberg, who was the first to publish his findings in June 1611). Although Scheiner was quite contentious and the Jesuits increasingly irritated, there is little evidence as to the part that they may have played in the final development of the celebrated Galileo case. In 1613 Galileo's letters were published in a book,[(4)] entitled *History and Demonstrations Concerning Sunspots and their Phenomena*; it was dedicated to Salviati under the auspices of the Accademia dei Lincei.

Being in a public position Galileo was naturally called upon for pronouncements on various subjects; his publications invariably encountered opposition. A major controversy was critically reviewed at a dinner party of the Grand Duke in the fall of 1611. A discussion had arisen as to the factors physically significant in the case of a floating body. From the current Aristotelian point of view, shape was all important; it was supposed to account for the floating of a piece of ice, which being frozen water, should presumably be denser and sink. Noting that ice is not more dense, Galileo pointed out that ice of any shape will float in water. Cardinal Maffeo Barberini approved his argument. A more critical question later concerned the floating of a thin piece of ebony (cf. Section 11). Here Galileo noted shrewdly that a depression in the surface is always associated with such floating materials that are not wet by the liquid. He showed considerable experimental ingenuity in investigating this phenomenon. Remarking that his own complete ignorance compelled him to seek information directly from nature itself, he employed wax models (size of an orange) for observing free fall in specific cases; he impregnated them with lead or sand, and varied the temperature of the water (cf. Ref. (9), pp. 68-70). At the request of the Grand Duke he compiled his findings in a book entitled *Discourse on Bodies in Water*,[5] dedicated to the Duke himself — his first publication (1612) on experimental physics. Modern humanists who seek in Galileo primarily a Platonic philosopher find it convenient to ignore this experiential work. They fail to recognize that Galileo's greatest contribution was probably not so much his specific findings, but rather his general methodology. Galileo, indeed, preferred Aristotle's emphasis upon experience to Plato's glorification of pure reasoning; he insisted, however, upon supplementing it with logic as an arbiter for sense errors and with mathematical reasoning for physical quantities in lieu of qualitative speculations. With a few significant exceptions (cf. Sections 5b, 6) he had little interest in mathematics *per se* except in conjunction with observations; he regarded it practically as a handmaiden of science. Under any circumstances he was indifferent to popular metaphysical verbiage such as caused the downfall of Giordano Bruno (1548-1600), who was burned at the stake in 1600, a heretic both to Catholics and to Protestants.

The peripatetic philosophers, now placed on the defensive, decided to take the offensive; they were led chiefly by Ludovico delle Colombe (b. 1565), who thus became the arch-villain of the Galileo case. The uncompromising boldness of Galileo and the secret envy of some contemporaries, coupled with deep-rooted prejudices of many, gradually led all his enemies to unite informally against him: disappointed Jesuits and expedient Dominicans, Aristotelian professors, and conservative Churchmen. Failing to defeat Galileo with logic they found a common bond in theology — in this instance, truly more of a political issue than an intellectual one. This so-called League was nicknamed the Pigeon League by Galileo inasmuch as Colombe derives from the word for dove.

Here again, in 1613, it was table talk at a Grand Ducal dinner party at Pisa that sparked an inflammatory controversy. This dinner was attended by a former pupil of Galileo, the Benedictine Father Benedetto Castelli ((1578-1643), then Professor at Pisa, later Papal Mathematician), but not by Galileo himself. Upon praising Galileo's discoveries Castelli found Cosimo Boscalgia, philosophy professor at the University of Pisa, insidiously remarking that any double motion of the earth would be contrary to the Scriptures. The Grand Duchess Christina immediately reacted unfavorably — sensitive to possible theological repercussions. After dinner she gathered together a small group to discuss the matter in more detail. Castelli reported this conversation to Galileo, who replied at once in a letter in which he emphasized that the object of the Bible is not to teach astronomy, and that the understanding of natural phenomena must begin preferably with experience itself. On the fourth Sunday in Advent (20 December 1614) Fra Tommaso Caccini (1574-1648), a friar of the Dominican Convent of Santa Maria Novella in Florence, preached against the Copernican hypothesis as being unbiblical, and particularly against the reprehensible failure of laymen to abide by the orthodox interpretations handed down from the Church Fathers. The Dominicans, of course, were loyal to Thomas Aquinas (*c.* 1225-74), who had effectually encompassed Christian faith in a modified Aristotelian framework. Caccini was immediately answered in a sermon at the cathedral of Santa Maria del Fiore by a preacher, who, in turn, defended the Copernican hypothesis and praised Galileo as a good Catholic. (In general, after 1616 the Jesuits preferred the compromise system of Tycho Brahe.)

A formal apology was immediately made to Galileo by Fra Luigi Maraffe, Florentine patrician and a Preacher-General of the Dominican Order in Rome.

Meanwhile, Castelli lent Galileo's apology to Father Niccolò Lorini, a professor at the Dominican Convent of San Marco, and thus unknowingly played into the hands of his opponents, who objected generally to the raising of any question as to the universal authority of the Bible (a critical issue for both Protestants and Catholics during the post-Reformatian period, and specifically to Galileo's *ad hoc* interpretation of the reported Joshua incident about the sun apparently standing still).

Early in 1615 Lorini actually denounced Galileo to the Sacred Congregation of the Holy Office (the Inquisition) in Rome. Galileo, meanwhile, decided to clarify, somewhat arrogantly, his own views in a formal letter (4) (16 February 1615) to the Grand Duchess Christina — not published until 1636 at Strasbourg, after the Galileo trial in 1633. The central tenet (cf. Section 20) of his whole philosophy was the ultimate unity of truth; it allowed for no permanently bad consequences of any temporary, practical separation of the world of everyday phenomena and the eternal world beyond phenomena, which, he believed, must be rationally similar. At any rate, theological and metaphysical implications of physics were never his major concern or main forte; rather, he considered only those relevant to his investigations of physical phenomena themselves. Nevertheless, Galileo's handling of any apparent conflicts between biblical statements and scientific findings would be generally acceptable now to both Catholics and Protestants; his opinions are well worth reviewing from our present standpoint. It was not until 1893 that Pope Leo XIII, Gioacchino Pecchi (1810-1903), announced in the Encyclical Providentissimus Deus the official position of the Roman Church on the relation of science and religion; it is not much different from Galileo's own posture (cf. also the 1943 encyclical on modern biblical scholarship by Pope Pius XIII, Eugenio Pacelli (1876-1958), on "Divino Afflante Spiritu").

Galileo became increasingly uneasy about the whole state of natural philosophy and, naively optimistic, decided to make another good-will visit to Rome in order to win friends for the Copernican theory, which he so ardently admired and aggressively promoted after his first public endorsement of it in his third sunspot letter. He was genuinely surprised to find deep-rooted opposition, both to this teaching and to his own arguments. While there, he was shocked to learn that Qualifiers (official experts) of the Holy Office had just taken (ill advisedly) action on two points: namely, (1) that "the sun is the center of the world, and, therefore, immovable from its place", which was "unanimously declared to be false and absurd philosophically, and formally heretical"; and (2) that "the earth is not the center of the world and is not immovable, but moves and also with a diurnal motion", which was "declared unanimously to deserve the like censure (as the first) in philosophy, and, as regards its theological aspect, to be at least erroneous in faith". This report, given to the Holy Office on 24 February, resulted in Cardinal Bellarmine being instructed "to summon before him the said Galileo, and admonish him to abandon the said opinion; and in case of refusal the Commissary is to intimate to him, before a notary and witnesses, a command to abstain altogether from teaching or defending the said opinion and even from discussing it". The recorded, but strangely unsigned, minute of 26 February reported more broadly that Galileo was "to relinquish altogether the said opinion, that the sun is the center of the world, and immovable, and that the earth moves; nor henceforth to hold, teach, or defend it in any way whatsoever, verbally or in writing". This document, cited and apparently accepted, at the trial in 1633, became the critical evidence in that phase of the Galileo case. Opinions have differed not only as to its actual content, but even as to its genuineness: was the minute maliciously fabricated in 1616 or deliberately forged later in 1632, or was it actually bona fide? (Ultraviolet testing has shown it to be similar to the accredited 1616 materials.) Some would interpret it as merely a clerical overemphasis inasmuch as Copernicus's book itself was not prohibited at that time, provided only that certain minor details were corrected. As to what actually did transpire in Galileo's interview with Bellarmine on 25 February, who knows? In May of that year, he himself obtained a paper from the Cardinal which indicated that there had been no official censure. The stage, however, was undoubtedly being set for the next act in this developing tension between old traditional ideas and new experiential findings, between the concensus of a group and the innovation of an individual. Upon the recommendation of the anxious Tuscan ambassador, Piero Guicciardini, Galileo's tenacious and contentious, but fruitless, efforts were finally temporarily curbed by the Grand Duke's order for his return to Florence.

Galileo, meanwhile, was discussing with the Spanish government a proposed use of the Jupiter satellites for the determination of longitude at sea, a perennially troublesome problem. Specifically, he suggested a comparison of the precise local time of the frequent (more than 1000 a year) eclipses of these moons with their predicted occurrence, say, at Florence. The time difference (in hours) multiplied by 15° would then give the angular distance (from Florence). In order to insure good observations from the deck of a moving ship, he proposed to "float" the telescope. A major handicap, however, was the lack of a good chronometer (not available until 1736). Galileo's negotiations were without success.

After his return to Florence in June 1616, Galileo suffered his chronic malady, which was aggravated by a long seizure of hypochondria, owing undoubtedly to his failure to persuade the Roman dignitaries and scholars to adopt the Copernican view. During much of the years 1617-18 he was ill so that he missed the exciting appearance of three comets in August 1618.

In the following year one of his Florentine disciples, Mario Guiducci, wrote a *Discourse on Comets*, in which he presented Galileo's ideas about their nature. (It so happened that Galileo was wrong in his own Aristotelian view of a comet as a terrestrial exhalation — an atmospheric phenomenon like a rainbow.) Immediately (1619) a Jesuit, Father Orazio Grassi of Savona, Professor of Mathematics at the Collegio Romano and builder of Sant' Ignazio, made an anonymous reply under the pseudonym Lothario Sarsi. In *The Astronomical and Philosophical Balance* he attacked directly and vigorously many of Galileo's ideas. The Jesuits had already adopted Tycho Brahe's view of comets being in the highest heaven based upon the parallax determination of the comet of 1577. Galileo was urged by friends to prepare his own answer.

About this time Galileo lost a good friend and loyal supporter in 1621 upon the death of Cosimo II (his successor, Ferdinand II, became of age only in 1627). He felt greatly encouraged, however, when Maffeo Barberini, who had written a poem in his honor in 1620, was made Pope Urban VIII in 1623.

In that year, under sponsorship of the Lincei and with a dedication to Pope Urban VIII, himself, Galileo published *The A sayer*,[4,6] who deliberately used not just any balance, but a precise one like that for gold. This supposed letter to a Lincean Academician, Virginio Cesarini, Lord Chamberlain to the Pope, turned out to be a sharp, clever polemic and, what is more, an outstanding philosophical defense (virtually a manifesto) of science, emphasizing chiefly the need for first-hand experience rather than second-hand authority, for intellectual freedom and an experimental approach, for identification of physical quantities and the use of mathematics. Here (cf. Section 5b) was formulated his practical distinction between primary (objective) qualities (like size) and secondary (subjective) ones (like color), which became a source of controversy among philosophers (cf. John Locke (1632-1704) and George Berkeley (1685-1753)) during the following centuries. Here Galileo exhibited his unusual ability to present an opponent's arguments in a most plausible manner — and then to annihilate them with devastating logic. The opposition he thus often aroused resulted more from the manner of his presentation than from the doctrines themselves. Never has such excellent scientific methodology been advocated — in defense of such poor scientific conclusions.

In the spring of 1624, Galileo made a fourth visit to Rome to pay his personal respects to the new Pope. Despite six long interviews, in which he was able to present his own Copernican arguments, he failed to change the Pope's fixed opinions. Nevertheless, the latter wrote a commendatory letter to the Grand Duke in which he praised Galileo's "virtue and piety". It was during this visit that Galileo designed and constructed a compound microscope.

A few words about Galileo's personal living about this time. From 1617 to 1631 he rented the villa (later called Villa Albrizzi and then Villa L'Ombrellino) of Lorenzo Giovanni Battista Segni at Bellosguardo, a western hill across the Arno. This dwelling was not too far from the poor Carmelite Convent (1611) of San Matteo in Arcetri where, owing in large measure to his selfishness and own unwillingness to assume the parental burden of responsibility, Galileo had conveniently committed his two daughters at an early age — and they later had become nuns: Virginia as Sister Maria Celeste in 1616, and in 1617 Livia as Sister Arcangela, who became an invalid under the conditions of poverty there. Throughout her entire life the former showed continually a genuine love for her father and a great anxiety for his problems. She exhibited simple piety; her filial correspondence,(1) extant from May 1623, is quite revealing (Galileo's replies are not available; they were probably destroyed because of a fear for his safety). His son Vincenzio, legitimized in 1619 by the Grand Duke, is often confused with Vincenzio, the son of his brother, Michelangelo, who was idle and careless with money, particularly that of other people (in 1628, for example, he moved with his wife and seven children into Galileo's house). In 1628 Galileo's son received his law degree from the University of Pisa and in 1629 married Sestilia Bocchineri. They had three sons: Galileo (b. 1629), Carlo (b. 1632), and Cosimo (b. 1638) — the end of Galileo's direct family line. Some time earlier (1621) his brother-in-law Landucci had left the country, and for a long period abandoned his family to Galileo's responsibility. Galileo's personal problems were compounded by his own continual illness (serious in 1628, aggravated by hernia in 1633), as well as by his family's perpetual and burdensome financial requests. (He was much more solicitous about his sisters and brother than about his own children.) At the same time, however, he had to maintain his social position as a courtier and to respond to the demands made daily upon a prominent personage such as himself, particularly requests to explain his scientific views.

CHAPTER 4

POPULAR AUTHOR

Galileo had ended the *Starry Messenger* with a promise to continue his report on the new astronomy at a later date. In his 1610 application to the Grand Duke he had declared, "I wish to gain my bread by my writings". His difficulties in 1616, however, had made him cautious. Hope was born anew in the 1624 discussion with the new Pope, who himself suggested a title more strictly in line with the actual contents of the book, which obviously compared the old Ptolemaic and the new Copernican theories, rather than the proposed "Dialogues on the Flux and Reflux of the Tides". The Pope urged also that the subject be treated hypothetically, with special consideration of his own supposedly unanswerable argument, namely, that inasmuch as an all-powerful God can do anything, to assert only the possibility of a double motion for the earth would appear to limit His omnipotence. Galileo did not actually begin writing until 1626, when his attention became focused again on magnetism and resulted in further investigations along this line. The *Two Chief World Systems*, [7,8] indeed, was not completed until early in 1630.

Galileo then made a fifth visit to Rome, primarily to solicit personally a license for printing the book with the aid of the Tuscan Ambassador, Marquis Francesco Niccolini (his wife, Caterina, was Riccardi's cousin), and Monsignor Giovanni Ciampoli, private secretary to the Pope. Unfortunately, he no longer had the support of his deceased friend and sponsor Prince Cesi, and many delays were encountered. The Imprimatur was finally authorized by the Florentine Dominican Master of the Sacred Palace, Father Niccolò Riccardi, a friend of Galileo — not definitive, however, owing to the need for certain minor revisions such as a Preface to emphasize the hypothetical character of the Copernican thesis and a conclusion to embody the Pope's "unanswerable" argument. The plague now contributed to poor communications, aggravated by the aggressive delaying tactics of his enemies, so that little progress was made in printing the book at Rome. Galileo, accordingly, sought and finally obtained permission in late 1631 to have the work done by the press of Landini in Florence. The book was published at last on 21 February 1632; it was dedicated to Ferdinand II.

Meanwhile, in the summer of 1631, he sought to lease a country house closer to the convent of his daughters, and in November of that year he moved to Il Gioiello (the Jewel, later called the Villa Galileo, No. 29), which belonged to Esaù Martellini and bordered on the convent (not far from the home of Giusto Susterman (1597–1681), who painted in 1635 the celebrated portrait (cf. Frontispiece) of Galileo that still hangs in the Galleria degli Uffizi).

The *Two Chief World Systems* was a literary masterpiece (in Italian); it presented a dialogue taking place in Venice among three people: Sagredo (cf. Galileo's friend diplomat and mathematician, Giovanni Francesco Sagredo (1571-1620)), an urbane Venetian patrician, supposedly neutral, listening to the arguments of a genial Aristotelian philosopher Simplicio (presumably named after a distinguished commentator of the sixth century) and of an astute Copernican enthusiast, the nobleman Salviati of Florence, representing Galileo himself. (Note that the adjective "two" deliberately ruled out the Tychonic system.) It was a clever creation, written in popular style for the cultured layman. It had a light tone (Galileo had a good sense of humor, cf. his Pisan burlesque poem, "In Abuse of Gowns") with interesting digressions — typical of the Italian Renaissance. In view of its avowed purpose to destroy old notions and introduce new ideas it was decidedly more philosophical and pedagogical than scientific (cf. continual repetitions and multiple illustrations). It did not succeed in winning the Copernican war; but it was a critical battle in the history of science.

The First Day started with a comparison of Aristotle's notion of celestial perfection and the actual imperfections like sunspots revealed through a telescope. Consideration was then given to many resemblances among the earth, the moon, and the planets; for example, earthshine and moonshine, the phases both of the moon and Venus, the universal spherical form, etc. Salviati suggested that similar movements of all planets about the sun might imply a simple common explanation. He cited the moons of Jupiter as celestial evidence of a non-geocentric system. (Galileo was so fascinated by the Copernican theory that he failed to consider seriously the equally satisfactory explanation of Jupiter's moons and of Venus' phases offered by Tycho Brahe's compromise model of a sun-centered planetary system revolving about the earth.)

The Second Day was devoted to a consideration of traditional reasons, pro and con, for the motion of the earth, particularly its daily rotation. The major objection was not astronomical, but physical, viz. the expected sternward lag of a stone falling from the mast of such a moving ship. Salviati emphatically made the radical declaration that such a body would fall just as if the ship were not moving at all — an example of what has been later called Galileian relativity (cf. Section 16g). He went further and predicted that a ball would actually fall slightly eastward, owing to its greater speed at a greater distance from the center of the earth.

On the Third Day the group considered the annual revolution and stellar parallax, the apparent displacement of stars owing to the annual motion of the earth (cf. Section 19). They ascribed the failure to detect any such shift as due to the relatively great distance of the stars. In 1838 the German scientist Friedrich Wilhelm Bessel (1784-1846) actually measured the parallax of 61 Cygni by this very method.

The Fourth Day was devoted to Galileo's primary scientific reason for his belief in the Copernican theory, namely, the explanation of tidal phenomena — a false hypothesis, based upon his incomplete understanding of gravitational attraction and of dynamics itself (he supposed the effect due merely to the combining of the earth's steady annual revolution with its daily periodic rotation). Nevertheless, the local terrain is an important factor in determining the actual behaviour of disturbed water in terrestrial basins, as Galileo indicated.

All in all, the book reads well. Despite our own greater knowledge today, there is still a novelty of expression for the modern reader. One is impressed particularly with Galileo's skillful ferreting out of errors, as well as his instinctive ability to uncover the truth. It is ironic that the best evidence for the Copernican theory was ignored by Galileo although he had it in his own hands, namely, Kepler's laws (cf. his *New Astronomy*, 1609) for planetary orbits, which were quantitative — not just qualitative — announced in the very same year as Galileo's own telescopic discoveries. Strangely enough, too, Galileo would have fared better, both scientifically and socially, if he been less dogmatic, more tolerant, and had followed the prudent advice of churchmen like the Catholic Bellarmine, i.e. in the cautious spirit of the Lutheran Andreas Osiander (1498-1552) in his unsigned Preface to Copernicus' epoch-making book, *On the Revolutions of Celestial Orbs* (1543), to accept the Copernican scheme as a true mathematical description of planetary motions, but as a yet unverified physical hypothesis.

Sales were officially stopped six months after the book's publication, owing to the appointment of a Papal Commission to examine it (no scientifically knowledgeable person was on the Commission). It reported that the presentation of the two theories was hardly hypothetical; on the contrary, the tides were seriously being proposed as physical evidence of the earth's motion; and, what was seemingly worse, in the very seeking of the Imprimatur Galileo had evidently disobeyed the injunction given him in 1616 — both charges were cited in the final judgment. As we have seen, there is some reasonable doubt as to whether the latter conclusion was legally justified; Galileo, it would seem, was to receive an injunction only if he persisted in refusing not to hold the Copernican view. There is little doubt, however, that Galileo had shrewdly used all his influential contacts to gain the license. And yet, the Church, too, was not blameless inasmuch as it had given official approval when it issued the Imprimatur.

Despite numerous presentations of the Galileo case, Catholic and Protestant, national and scientific, the true story remains hidden amid the debris of history. Certainly the Roman Church had a right to discipline the breach of a formal promise by one of its members or of, at best, a roguish compliance. Much of the procedure of the Church must be ascribed to the personal involvement and apparent animosity of the Pope himself, who had presumably been persuaded that his own "unanswerable argument" had been put in the mouth of a simpleton (translation of the name Simplicio). In a letter to the Grand Duke the Tuscan ambassador Niccolini reported that the Pope had referred to Galileo as one "who did not fear to make game of me". Urban VIII, who was certainly not one of the better popes, was undoubtedly the guiding mind behind the entire course of action adopted (otherwise the judgment would probably have been more moderate). An extenuating circumstance was the 1632 political pressure being brought by Spain against his seeming assistance to heretics, e.g. Gustavus Adolphus, and his unquestionable nepotism (cf. Cardinal Antonio Barberini, the Pope's brother, and the Lincean Cardinal Francesco Barberini, his nephew (friendly to Galileo)). Galileo, moreover, was too prominent a person to be allowed to defy openly the established church; his pupils held many influential academic positions. The case was formally handed over by the Pope to the Sacred Congregation of the Holy Office, and Galileo was so notified on 1 October.

Galileo's immediate reaction was one of naive surprise. It was long before he himself realized the seriousness of the charges. Meanwhile, by various delaying tactics, he tried to evade the order to appear before the Inquisition. Only under threat of transportation under chains did the feeble 69-year-old man obediently set out on his last visit to Rome on 20 January 1633. Although he could have found refuge at Venice, he remained loyal to the Church — probably convinced of the truth of his own reasoning and the skill of his own persuasiveness. Galileo arrived in Rome on 13 February.

In all fairness, it should be emphasized that throughout his confinement and the whole hearing the Inquisition treated Galileo quite well, and gave him all the consideration that might be due to a distinguished scholar who was a sincere churchman.

In the first examination, on 12 April, under the Dominican Commissary General of the Holy Office, Fra Vincenzio Maculano da Firenzuola, Galileo claimed that he had not been given any specific injunction with respect to the Copernican doctrine being held or defended, and he presented Cardinal Bellarmine's note as evidence that he had not been prohibited from discussing it. Furthermore, had he not spoken about the book with the Pope himself who had not objected, although he was certainly familiar with the action of 1616? Galileo, I believe, was sincere in his denial of any remembrance of such a specific command having been given him earlier by the Dominican Father Commissary. Nevertheless, Galileo's absurd pretense of not favoring Copernicanism in the book weakened his whole position. The Inquisition, moreover, was particularly concerned over Galileo's failure even to mention the 1616 inquiry at the time of his request for the Imprimatur. A preliminary secret report, submitted on 17 April, concluded that Galileo was guilty.

By the time of the second interrogation on 30 April, friends had apparently persuaded Galileo to acquiesce to the requests of the Inquisition. He must have undergone an agonizing spiritual struggle between his old religious ties and his new scientific aspirations before yielding as a loyal and sincere Catholic. At any rate, Galileo now admitted that he had truly defended the Copernican doctrine—contrary to his previous denial: "My error then had been — and I confess it — one of vainglorious ambition and of pure ignorance and of inadvertence." After dismissal he returned voluntarily and offered to re-do some of the book in order to make certain that there could be no possible misunderstanding of his present point of view — and hopefully also to add one or two more desirable Days to the Dialogue. He was returned this time to the care of Niccolini.

At his third appearance, 10 May, he was formally asked if he had any defense at that time; he repeated his admitted failure to obey the command "not to hold, not to defend and not to teach". He again insisted, however, that he did not recall any precept other than that in Bellarmine's note. The old man appeared pitiful as he now pleaded for leniency in view of his old age and poor health. The Inquisition, with the Pope in the Chair, made its final decision on 16 June, and informed Galileo (confined now at the Inquisition) on 21 June, when they asked him three specific questions in order to confirm his present intent. On Wednesday morning, 22 June, in the hall of the Dominican Convent of Santa Maria sopra Minerva, garbed as a penitential criminal and kneeling, Galileo was sentenced by the Inquisition and compelled to renounce his beliefs under oath. Galileo confessed publicly, "I abjure, curse, and detest the said errors and heresies". As for the controversial Copernican theory, Blaise Pascal (1623-62) foresaw the final verdict of history, when he wrote in the Provincial Letters: "It is in vain that you have procured against Galileo a decree from Rome condemning his opinion of the earth's motion." The real intellectual crisis in Galileo's own life had probably occurred earlier when he realized that his clever arguments had failed to win converts and when he had accordingly pleaded guilty. The legend that he arose muttering "Eppur si muove" (it moves, nevertheless) sounds like a natural afterthought which he may well have expressed in reminiscing later about the incident. It truly expressed his lifelong feeling. He certainly had the courage of his convictions, but it was tempered by shrewd expediency, including possibly lying and even perjury. Galileo was in no sense a martyr — either for science or for religion. His human limitations, though understandable, are evident; his personal and social conscience, I believe, was sadly wanting. Every student of Galileo, however, has to form his own personal judgment on the basis of the incomplete evidence submitted by history.

Galileo's books, of course, were placed on the *Index Expurga-*

torius; they remained there until its 1835 edition when they were removed pursuant to an 1822 action based upon the Holy Office's 1820 decision not to oppose Copernican views any more.

Galileo, a heresy suspect, was given life imprisonment by the Inquisition — an unexpected measure; however, he was allowed to return immediately to the Villa Medici (the Académie Nationale de France since 1863) on Pincian hill. Although permitted by the Pope to leave Rome on 30 June he could go only to Siena, where he had to remain under house arrest with the kind Archbishop Ascania Piccolomini (one of his former students). Here, with the respect and stimulation of local visitors, he began to write his scientifically most significant work, the *Two New Sciences*, which he had listed as another project in his letter to the Grand Duke's secretary in 1610 and which Sagredo had mentioned at the end of the *Two Chief World Systems.* In December he was finally given permission to return to his home in Arcetri under house confinement and without visitors (opposed by his enemies after the Siena experience). His comfort, however, was short-lived in view of the death of his favorite daughter, Sister Maria Celeste, on 2 April 1634.

The *Two New Sciences*, begun when the ever-industrious Galileo was almost 70, was completed in 1636, but had to wait until 1638 for transmission by Fra Fulgenzio Micanzio of Venice for publication at non-Catholic Amsterdam by Louis Elzevier. It was dedicated to the French ambassador Comte de Noaillis, who had been a pupil of Galileo at Padua. Like the *Two Chief World Systems* it consisted of four dialogues, with the same group of persons, viz. Sagredo, Simplicio, and Salviati; it lacked, however, the literary value and crusader-spirit of the older book. The two new sciences were the strength of materials and dynamics.

The book began with an everyday question as to the relative strength of scaled structures (cf. Section 14), notably the failure of geometry *per se* to solve this problem. Galileo, it will be recalled, had always been consulted on practical matters owing to his prominent position as a science adviser (for example, he had been Superintendent of Tuscan waters). Strictly speaking, this analysis was the beginning of engineering science, and was certainly treated *de novo* by Galileo.

The first two days were presented as a real dialogue, involving many incidental experiments, whereas the last two were concerned primarily with formal, mathematical deduced theorems about motion, read supposedly from a manuscript of an academician friend (Galileo).

Throughout the book, one is impressed with Galileo's keen physical insights into everyday phenomena (cf. Sections 8, 10). He compared physical atomism with a mathematical continuum (cf. Section 6), the speed of light with that of sound, acoustical vibrations with musical intervals (cf. Section 15), falling bodies (cf. Section 16) with downhill rollings, natural motion with violent ones (projectiles). He was at once experiential in his outlook and theoretical in his understanding; he used mathematics wherever practicable (probably not his mode of discovery), but subject to natural observations.

With respect to the second new science, as Joseph-Louis, Comte de Lagrange (1736-1813), noted: Galileo undoubtedly laid the foundations of dynamics as a science — with due credit to the many particular results achieved by various precursors. Newton acknowledged his own indebtedness to Galileo for the laws of motion. Galileo certainly understood the principle of inertia, viz. the motion of a force-free body with constant velocity (though not in the generalized form of Newton's first (axiomatic) law for an isolated body). He also had some inkling of the relationship between force and acceleration, but here his understanding was definitely much less. Although neither a devastating polemic like *The Assayer*, nor a great literary creation like the *Two Chief World Systems*, at least it communicated more accurately the fundamental scientific ideas of the author.

In 1636 Galileo continued his discussions about longitude, only this time with the Dutch, who presented him with a golden chain, which he felt obliged to decline in view of the watchful eye of the Inquisition. In June 1637 he lost the sight of his diseased right eye (glaucoma) and in December that of his left. Meanwhile, in

November, in a letter to Fra Fulgenzio Micanzio he announced his last observation, namely, his discovery of lunar librations. In March 1638 he finally received papal permission to get medical attention while staying at his son's house (No. 11 Via della Costa near the Porta San Giorgio in Florence), where he was visited by the Grand Duke himself in September (cf. the plaque there commemorative of this visit). In December he was at last permitted to have his long-time friend Castelli as a visitor. The next month he returned to Arcetri, probably not voluntarily in view of his initial eagerness to get to the city. At this time, however, there was a definite relaxation of supervision on the part of the Inquisition. In March of that year he was visited by the English poet John Milton (1608-74), who returned to Florence after having stopped there in September 1638, when he probably called upon Galileo at his son's house. It is significant that the French philosopher and mathematician René Descartes did not go to see Galileo when in Florence, owing presumably to Descarte's indifference to his somewhat empirical approach to nature; the Frenchman Pierre Gassendi (1592-1655), however, did visit him.

In the summer of 1639, 18-year old Vincenzio Viviani (1622-1703, F.R.S. 1696) came to live with him as his "last disciple". In 1641 blind Galileo was still thinking purposefully. He was developing a scheme to use a pendulum to regulate the movement of an ordinary clock. In September of that year, he was again visited by Castelli; and in October Torricelli, a former student of Castelli, came to stay with him at his invitation (and later succeeded Galileo at the Court of the Grand Duke of Tuscany). Bonaventura Cavalieri (1598-1647), a former pupil of Galileo, who had become professor of mathematics at Bologna, and Micanzio sent regrets that they could not join Galileo during this period. Galileo was always idolized by his friends.

On 5 November Galileo went to bed with an illness from which he never recovered. On 20 December, he wrote his last letter — to the thrice-married Alessandra Bocchineri, sister of Sestilia, with whom he had corresponded continually after her return to Italy in 1630. Almost 78, Galileo died on the evening of 8 January

1642, blessed with the Last Rites of the Church. Rome, however, refused to allow the public funeral and honor proposed by Florence. Accordingly, he was buried in the small Cappella del Campanile del Noviziato in Santa Croce, the Florentine Westminster Abbey. On 12 March 1737 his remains (together with those of Viviani, who had been buried next to him) were finally transferred to the main part of Santa Croce, across from Michelangelo's tomb; the memorial monument has a bust of Galileo flanked by a figure of astronomy and by a figure of geometry (physical). At the exhumation various souvenirs of Galileo were removed. The left index finger, taken by Professor Antonio Francesco Gorri of Florence, may still be seen in the Instituto e Museo di Storia delle Scienze di Firenze, which replaced the Museo di Fisica e Storia Naturale, established in 1841 by Leopold II (1797-1870), the last Grand Duke of Tuscany, when he opened the Tribuna di Galileo in Florence (this museum still contains a statue of Galileo by Costoli and some remarkable frescoes painted from his life — at present (1965) not open to the public, pending reorganization). The thumb and right forefinger, taken by Canon Giovanni Vincenzio Capponi, President of the Florentine Academy, are said to be in Rome. At the University of Padua one will find Galileo's fifth lumbar vertebra, taken by Professor Antonio Cocchi.

Some Galileo enthusiasts at the Vatican Council II in 1964 proposed that the Church review the Galileo case. In 1965 Pope Paul VI, Giovanni Battista Montini, visited Santa Croce and mentioned Galileo graciously — at last.

Despite the fact that the Galileo story has become historically celebrated in the relations between scientists and churchmen and that most of Galileo's work is extant, much of his personal thinking remains an unsolved puzzle. Recently there has developed a controversy even as to the public significance of his private scientific investigations. It has become fashionable for some professional historians of science to relegate Galileo to a minor position in a social movement of science beginning in the thirteenth century and culminating with Newton. Professional physicists, on the other hand, still recognize in Galileo the founder of physics as an experimental *and* theoretical discipline; he stressed both the necessity of experiential data and the role of quantitative theory. For us all his works still live in the modern era of science — symbolic of his inquiring life.

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Part 2. HIS WORKS

INTRODUCTORY COMMENTS

THE following sixteen sections contain excerpts from Galileo's important writings. They have been selected for their significant contributions to the methodology and concepts of cumulative science — not for their illustrative value of intellectual and social history. In each case introductory comments are provided to indicate the modern viewpoint and to make up for any shortcomings of translation or brevity.

CHAPTER 5

INTERPRETING SENSE IMPRESSIONS

(a) Primary Qualities

Our experiences of physical phenomena are transmitted through our senses. Some of our sense data we can share with others, namely, those geometrical and external properties which seem to belong to a particular object, e.g. its size, its shape, and its position. Others vary more with the viewer's own physiological and psychological perception, e.g. hotness, sound, color, etc. The former are more objective — or *primary*, as Locke later called them — the latter are seemingly *secondary*, i.e. more subjective. Galileo discussed this distinction in connection with his idea of heat.

Primary qualities have inherently a common denominator; therefore, they can be used as the basis of a common description. Hence physical scientists have used them to seek acceptable standards for communication, and by allowing certain tolerances compatible with inherent errors of measurement have been remarkably successful in establishing intersubjective universal agreements.

Galileo initially included sensory experiences like hotness and sound among secondary qualities. He himself, however, discovered some measurable characteristics of hot bodies and of musical harmony so that the vague line of demarcation gradually shifted. Philosophers, who delight in logical distinctions, debated the essential meaning of this nebulous dichotomy for centuries. Scientists, on the other hand, have recognized more and more that each experience has both objective and subjective aspects that cannot be wholly dissociated.

The Assayer†

It now remains, in accordance with the promise made above to your Excellency, for me to tell you some of my thoughts about the proposition, "Motion is the cause of heat," and to show in what sense this may be true in my opinion. But first I must give some consideration to what we call "heat," for I much suspect

† Ref. (6), pp. 308–13.

that in general people have a conception of this which is very remote from the truth, believing heat to be a real attribute, property, and quality which actually resides in the material by which we feel ourselves warmed.

Therefore I say that upon conceiving of a material or corporeal substance, I immediately feel the need to conceive simultaneously that it is bounded and has this or that shape; that it is in this place or that at any given time; that it moves or stays still; that it does or does not touch another body; and that it is one, few, or many. I cannot separate it from these conditions by any stretch of my imagination. But that it must be white or red, bitter or sweet, noisy or silent, of sweet or foul odor, my mind feels no compulsion to understand as necessary accompaniments. Indeed, without the senses to guide us, reason or imagination alone would perhaps never arrive at such qualities. For that reason I think that tastes, odors, colors, and so forth are no more than mere names so far as pertains to the subject wherein they reside, and that they have their habitation only in the sensorium. Thus, if the living creature (*l'animale*) were removed, all these qualities would be removed and annihilated. Yet since we have imposed upon them particular names which differ from the names of those other previous real attributes, we wish to believe that they should also be truly and really different from the latter.

I believe I can explain my idea better by means of some examples. I move my hand first over a marble statue and then over a living man. Now as to the action derived from my hand, this is the same with respect to both subjects so far as the hand is concerned; it consists of the primary phenomena of motion and touch which we have not designated by any other names. But the animate body which receives these operations feels diverse sensations according to the various parts which are touched. Being touched on the soles of the feet, for example, or upon the knee or under the armpit, it feels in addition to the general sense of touch another sensation upon which we have conferred a special name, calling it *tickling*; this sensation belongs entirely to us and not to the hand in any way. It seems to me that anyone would seriously err who might wish to say that the hand had within itself, in addition to the properties of moving and touching, another faculty different from these; that of tickling — as if the tickling were an attribute which resided in the hand. A piece of paper or a feather drawn lightly over any part of our bodies performs what are inherently quite the same operations of moving and touching; by touching the eye, the nose, or the upper lip it excites in us an almost intolerable titillation while in other regions it is scarcely felt. Now this titillation belongs entirely to us and not to the feather; if the animate and sensitive body were removed, it would remain no more than a mere name. And I believe that many qualities which we come to attribute to natural bodies, such as tastes, odors, colors, and other things, may be of similar and no more solid existence.

A body which is solid and, so to speak, very material, when moved and applied to any part of my person produces in me that sensation which we call *touch*; this, though it pervades the entire body, seems yet to reside principally in the palms of the hands and especially in the fingertips, by means of which we sense the most minute differences of roughness, smoothness, and hardness, these being not so clearly distinguished by other parts of the body. Of these sensations, some are more pleasant to us and some less so, and they are smooth or rough, acute or obtuse, hard or yielding, according to the differences of shape which exist among tangible bodies. This sense being more material than the others and having its rise in the solidity of matter, it seems to be related to the earthy element. And since some bodies continually dissolve into minute particles of which some are heavier than air and descend, while others are lighter and rise on high, perhaps herein lies the origin of two other senses, accordingly as these particles strike us upon two parts of our bodies which are very much more sensitive than the skin and which feel the invasion of such subtle, tenuous, and yielding matter. The tiny descending particles are received upon the upper surface of the tongue, penetrating and mixing with its moisture; and their substance gives rise to tastes, sweet or unsavory accordingly as the shapes of these particles differ, as they are few or many, and as they are fast or slow. The other particles, ascending, enter by our nostrils and strike upon some small protuberances which are the instrument of smell; and here likewise their touch and passage are received to our liking or our dislike accordingly as they have this or that shape, move quickly or slowly, and are few or many. The tongue and the nasal passages are indeed seen to be providently arranged for the above as to location; the former extends from below to receive the incursions of descending particles, while the latter are accommodated for those which ascend. Perhaps the excitation of tastes may be likened with a certain analogy to fluids, which descend through the air, and odors to fires, which ascend.

Next the element of air remains available for sounds; these come to us indifferently from below, from above, and from all sides, since we are situated in the air and its movements within its own domain displace it equally in all directions. And the situation of the ear is most fittingly accommodated to all positions in space. Sounds are created and are heard by us when — without any special "sonorous" or "transonorous" property — a rapid tremor of the air, ruffled into very minute waves, moves certain cartilages of a tympanum within our ear. External means capable of producing this ruffling of the air are very numerous, but for the most part they reduce to the trembling of some body which strikes upon the air and disturbs it; waves are thereby very rapidly propagated, and from their frequency originates a high pitch, or from their rarity a deep sound.

I do not believe that for exciting in us tastes, odors, and sounds there are required in external bodies anything but sizes, shapes, numbers, and slow or fast movements; and I think that if ears, tongues, and noses were taken away, shapes and numbers and motions would remain but not odors or tastes or sounds. These, I believe, are nothing but names, apart from the living animal — just as tickling and titillation are nothing but names when armpits and the skin around the nose are absent. And as the four senses considered here are related to the four elements, I believe that vision, the sense which is eminent above all others, is related to light, but in that ratio of excellence which exists between the finite and the infinite, the temporal and the instantaneous, the quantity and the indivisible; between darkness and light. Of this sense and the matters pertaining to it, I pretend to understand but a trifle, and since a long time would still not suffice for me to explain that trifle — or even to hint at its explanation in writing — I pass this over in silence.

Returning now to my original purpose, and having already seen that many sensations which are deemed to be qualities residing in external subjects have no real existence except in ourselves, and outside of us are nothing but names, I say that I am inclined to believe that heat is of this character. Those materials which produce heat in us and make us feel warmth, which we call by the general name *fire*, would be a multitude of minute particles having certain shapes and moving with certain velocities. Meeting with our bodies, they penetrate by means of their consummate subtlety, and their touch which we feel, made in their passage through our substance, is the sensation which we call *heat*. This is pleasant or obnoxious according to the number and the greater or lesser velocity of these particles which thus go pricking and penetrating; that penetration is pleasant which assists our necessary insensible transpiration, and that is obnoxious which makes too great a division and dissolution of our substance. To sum up, the operation of fire by means of its particles is merely that in moving it penetrates all bodies by reason of its great subtlety, dissolving them more quickly or more slowly in proportion to the number and velocity of the fire-corpuscles (*ignicoli*) and the density or rarity of the material of these bodies, of which many are such that in their decomposition the major part of them passes over into further tiny corpuscles (*ignei*), and the dissolution goes on so long as it meets with matter capable of being so resolved. But I do not believe at all that in addition to shape, number, motion, penetration, and touch there is any other quality in fire which is "heat"; I believe that this belongs to us, and so intimately that when the animate and sensitive body is removed, "heat" remains nothing but a simple vocable. And since this sensation is produced in us by the passage and touch of the tiny corpuscles through our substance, it is obvious that if they were to remain at rest their operation would remain null. Thus we see that a quantity of fire retained in the pores and narrow channels of a piece of quicklime does not warm us even when we hold it in our hands, because it rests motionless. But place the quicklime in water, where the fire has a greater propensity to motion than it has in air — because of the greater gravity of this medium, and because the fire opens the pores of water as it does not those of air — and the little corpuscles will escape; and, touching our hand, they will penetrate it and we shall feel heat.

Since, then, the presence of the fire-corpuscles does not suffice to excite heat, but we need also their movement, it seems to me that one may very reasonably say that motion is the cause of heat. This is that motion by which arrows and other sticks are burned and by which lead and other metals are liquefied when the little particles of fire penetrate the bodies, being either moved by themselves or, their own strength not sufficing, being driven by the impetuous draught of a bellows. Of these bodies, some resolve into other flying particles of fire and some into a most minute powder; some liquefy and become as fluid as water. But I hold it to be foolish to take that same proposition from the common point of view — that a stone or a piece of iron or a stick must heat up if moved.

The rubbing together and friction of two hard bodies, either by resolving their parts into very subtle flying particles or by opening an exit for the tiny fire-corpuscles within, finally set these in motion and, upon their encountering our bodies and penetrating and coursing through them, our conscious mind (*anima sensitiva*) feels that pleasant or obnoxious sensation which we have named heat, burning, or scalding. And perhaps when the thinning and attrition stop at or are confined within the tiniest particles (*i minimi quanti*), their motion is temporal and their action is calorific only, but, when their ultimate and highest resolution into truly indivisible atoms is reached, light is created which has instantaneous motion — or let us say instantaneous expansion and diffusion — and is capable of occupying immense spaces by its — I do not know whether to say by its subtlety, its rarity, its immateriality, or yet some other property different from all these, and nameless.

(b) Mathematical Language

Qualities that have quantitative (numerical) aspects can be compared relatively easily. What is more, the highly developed techniques of mathematics can then be applied in their analysis. Galileo, therefore, sought to replace vague (and speculative) qualities, wherever possible, with measurable (and repeatable) physical quantities. He was deeply impressed with the applicability of abstract (and symbolic) mathematics to concrete (and phenomenological nature).

Is the universe essentially mathematical, i.e. amenable to particular mathematical reasoning, or is it wholly mathematics, i.e. a physical realization of some universal mathematical ideas? Or, is its mathematical appearance solely man's impression? It is obviously quite different, on one hand, to admit that the universe has mathematical properties (natural or human), and, on the other, to claim that is it nothing but mathematics. The connotations about mathematics should not be read out of context. In this instance it is not even the chief topic of discussion. The real foe is authoritarianism. Mathematics is used merely as a contrasting illustration, i.e. rigorous reason (in the form of mathematical logic) in comparison with poetic (speculative or metaphysical) imagination, the book of nature interpretable in quantitative terms with the books of men expressing qualitative feelings. Mathematics is not being compared with phenomena, Platonic idealism with Aristotelian experience.

Most physicists today consider that mathematical proofs are validated only in terms of their experiential assumptions and conclusions — physical meaning *per se* cannot be found in the meaningless symbols of pure mathematics.

The Assayer†

But getting back to the point, you see how once more he will have it that I deemed it a great defect on the part of Father Grassi to have adhered to Tycho's doctrine, and he asks resentfully: "Whom then should he have followed? Ptolemy, whose doctrine has been revealed to be false by the recent observations of Mars?

† Ref. (6), pp. 183–7.

Or perhaps Copernicus? But he must rather be rejected by everyone, in view of the ultimate condemnation of his hypothesis." Here I note several things, and I reply first that it is quite false that I have ever criticized anyone for following Tycho, even though I might very reasonably have done so, as will indeed at last become clear to his adherents from the *Anti-Tycho* of the distinguished Chiaramonti. Hence, so far as this remark is concerned, Sarsi is very wide of the mark. Even more irrelevant is his introduction of Ptolemy and Copernicus, who are never to be found writing a word relative to the distances, magnitudes, movements, and theory of the comets, which (and which alone) are here under consideration. He might with as much reason have brought in Sophocles, Bartoli, or Livy.

It seems to me that I discern in Sarsi a firm belief that in philosophizing it is essential to support oneself upon the opinion of some celebrated author, as if when our minds are not wedded to the reasoning of some other person they ought to remain completely barren and sterile. Possibly he thinks that philosophy is a book of fiction created by some man, like the *Iliad* or *Orlando Furioso* — books in which the least important thing is whether what is written in them is true. Well, Sig. Sarsi, that is not the way matters stand. Philosophy is written in this grand book — I mean the universe — which stands continually open to our gaze, but it cannot be understood unless one first learns to comprehend the language and interpret the characters in which it is written. It is written in the language of mathematics, and its characters are triangles, circles, and other geometrical figures, without which it is humanly impossible to understand a single word of it; without these, one is wandering about in a dark labyrinth. Sarsi seems to think that our intellects should be enslaved to that of some other man (I shall disregard the fact that in thus making everyone, including himself, an imitator, he will praise in himself what he has blamed in Sig. Mario), and that in the contemplation of the celestial motions one should adhere to somebody else.

CHAPTER 6

CONTINUA—MATHEMATICAL AND PHYSICAL

IN CONSIDERING the cohesion of materials, which renders them resistant to fracture, Galileo discussed also a mathematical continuum as contrasted with physical atomism. By drawing radii from a common center through concentric circles he noted that the different circumferences always have corresponding points so that, in a certain sense, they are equivalent. He concluded that it is meaningless to speak of relative magnitudes of infinite classes of objects. An *infinite class*, indeed, is quite different from a finite class — a distinction that became significant only in connection with infinite sets in the nineteenth century. All numbers, too, as he noted, can be put in one-to-one correspondence with their squares.

All in all, however, Galileo himself admitted the incomprehensibility of the infinite and its association with divisibility. Although Galileo showed little interest in perusing such questions of abstract mathematics (preferring applications of mathematics to natural phenomena), he exhibited shrewd understanding of some of the difficulties of the continuum that have worried man from ancient times to today.

The inquisitive Florentine Salviati represents Galileo in the following discussion. He is instructing the urbane Venetian Sagredo and the conservative Aristotelian Simplicio.

Dialogues Concerning Two New Sciences†

SALV. Let us return to the consideration of the above mentioned polygons whose behavior we already understand. Now in the case of polygons with 100000 sides, the line traversed by the perimeter of the greater, i.e., the line laid down by its 100000 sides one after another, is equal to the line traced out by the 100000 sides of the smaller, provided we include the 100000

† Ref. (9), pp. 24–34.

vacant spaces interspersed. So in the case of the circles, polygons having an infinitude of sides, the line traversed by the continuously distributed [*continuamente disposti*] infinitude of sides is in the greater circle equal to the line laid down by the infinitude of sides in the smaller circle but with the exception that these latter alternate with empty spaces; and since the sides are not finite in number, but infinite, so also are the intervening empty spaces not finite but infinite. The line traversed by the larger circle consists then of an infinite number of points which completely fill it; while that which is traced by the smaller circle consists of an infinite number of points which leave empty spaces and only partly fill the line. And here I wish you to observe that after dividing and resolving a line into a finite number of parts, that is, into a number which can be counted, it is not possible to arrange them again into a greater length than that which they occupied when they formed a *continuum* [*continuate*] and were connected without the interposition of as many empty spaces. But if we consider the line resolved into an infinite number of infinitely small and indivisible parts, we shall be able to conceive the line extended indefinitely by the interposition, not of a finite, but of an infinite number of infinitely small indivisible empty spaces.

Now this which has been said concerning simple lines must be understood to hold also in the case of surfaces and solid bodies, it being assumed that they are made up of an infinite, not a finite, number of atoms. Such a body once divided into a finite number of parts it is impossible to reassemble them so as to occupy more space than before unless we interpose a finite number of empty spaces, that is to say, spaces free from the substance of which the solid is made. But if we imagine the body, by some extreme and final analysis, resolved into its primary elements, infinite in number, then we shall be able to think of them as indefinitely extended in space, not by the interposition of a finite, but of an infinite number of empty spaces. Thus one can easily imagine a small ball of gold expanded into a very large space without the introduction of a finite number of empty spaces, always provided the gold is made up of an infinite number of indivisible parts.

SIMP. It seems to me that you are travelling along toward those vacua advocated by a certain ancient philosopher.

SALV. But you have failed to add, "who denied Divine Providence," an inapt remark made on a similar occasion by a certain antagonist of our Academician.

SIMP. I noticed, and not without indignation, the rancor of this ill-natured opponent; further references to these affairs I omit, not only as a matter of good form, but also because I know how unpleasant they are to the good tempered and well ordered mind of one so religious and pious, so orthodox and God-fearing as you.

But to return to our subject, your previous discourse leaves with me many difficulties which I am unable to solve. First among these is that, if the circumferences of the two circles are equal to the two straight lines, CE and BF, the latter considered as a *continuum*, the former as interrupted with an infinity of empty points, I do not see how it is possible to say that the line AD described by the center, and made up of an infinity of points, is equal to this center which is a single point. Besides, this building up of lines out of points, divisibles out of indivisibles, and finites out of infinites, offers me an obstacle difficult to avoid; and the necessity of introducing a vacuum, so conclusively refuted by Aristotle, presents the same difficulty.

SALV. These difficulties are real; and they are not the only ones. But let us remember that we are dealing with infinities and indivisibles, both of which transcend our finite understanding, the former on account of their magnitude, the latter because of their smallness. In spite of this, men cannot refrain from discussing them, even though it must be done in a roundabout way.

Therefore I also should like to take the liberty to present some of my ideas which, though not necessarily convincing, would, on account of their novelty, at least, prove somewhat startling. But such a diversion might perhaps carry us too far away from the subject under discussion and might therefore appear to you inopportune and not very pleasing.

SAGR. Pray let us enjoy the advantages and privileges which come from conversation between friends, especially upon subjects freely chosen and not forced upon us, a matter vastly different from dealing with dead books which give rise to many doubts but remove none. Share with us, therefore, the thoughts which our discussion has suggested to you; for since we are free from urgent business there will be abundant time to pursue the topics already mentioned; and in particular the objections raised by Simplicio ought not in any wise to be neglected.

SALV. Granted, since you so desire. The first question was, How can a single point be equal to a line? Since I cannot do more at present I shall attempt to remove, or at least diminish, one improbability by introducing a similar or a greater one, just as sometimes a wonder is diminished by a miracle.

And this I shall do by showing you two equal surfaces, together with two equal solids located upon these same surfaces as bases, all four of which diminish continuously and uniformly in such a way that their remainders always preserve equality among themselves, and finally both the surfaces and the solids terminate their previous constant equality by degenerating, the one solid and the one surface into a very long line, the other solid and the other surface into a single point; that is, the latter to one point, the former to an infinite number of points.

SAGR. This proposition appears to me wonderful, indeed; but let us hear the explanation and demonstration.

SALV. Since the proof is purely geometrical we shall need a figure. Let AFB be a semicircle with center at C; about it describe the rectangle ADEB and from the center draw the straight lines CD and CE to the points D and E. Imagine the radius CF to be drawn perpendicular to either of the lines AB or DE, and the entire figure to rotate about this radius as an axis. It is clear that the rectangle ADEB will thus describe a cylinder, the semicircle AFB a hemisphere, and the triangle CDE, a cone. Next let us remove the hemisphere but leave the cone and the rest of the cylinder, which, on account of its shape, we will call a "bowl." First we shall prove that the bowl and the cone are equal; then we shall show that a plane drawn parallel to the circle which forms the base of the bowl and which has the line DE for diameter and F for a center — a plane whose trace is GN — cuts the bowl in the points G, I, O, N, and the cone in the points H, L, so that the part of the cone indicated by CHL is always equal to the part of the bowl whose profile is represented by the triangles GAI and BON. Besides this we shall prove that the base of the cone, i.e., the circle whose diameter is HL, is equal to the circular surface which forms the base of this portion of the bowl, or as one might say, equal to a ribbon whose width is GI. (Note by the way the

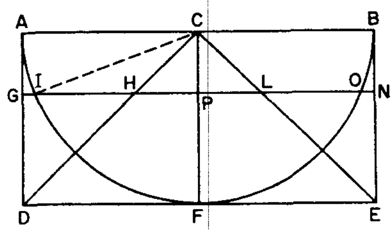

FIG. 1

nature of mathematical definitions which consist merely in the imposition of names or, if you prefer, abbreviations of speech established and introduced in order to avoid the tedious drudgery which you and I now experience simply because we have not agreed to call this surface a "circular band" and that sharp solid portion of the bowl a "round razor.") Now call them by what name you please, it suffices to understand that the plane, drawn at any height whatever, so long as it is parallel to the base, i.e., to the circle whose diameter is DE, always cuts the two solids so that the portion CHL of the cone is equal to the upper portion of the bowl; likewise the two areas which are the bases of these solids, namely the band and the circle HL, are also equal. Here we have the miracle mentioned above; as the cutting plane approaches the line AB the portions of the solids cut off are always equal, so also the areas of their bases. And as the cutting plane comes near the top, the two solids (always equal) as well as their bases (areas which are also equal) finally vanish, one pair of them degenerating into the circumference of a circle, the other into a single point, namely, the upper edge of the bowl and the apex of the cone. Now, since as these solids diminish equality is maintained between them up to the very last, we are justified in saying that, at the extreme and final end of this diminution, they are still equal and that one is not infinitely greater than the other. It appears therefore that we may equate the circumference of a large circle to a single point. And this which is true of the solids is true also of the surfaces which form their bases; for these also preserve equality between themselves throughout their diminution and in the end vanish, the one into the circumference of a circle, the other into a single point. Shall we not then call them equal seeing that they are the last traces and remnants of equal magnitudes? Note also that, even if these vessels were large enough to contain immense celestial hemispheres, both their upper edges and the apexes of the cones therein contained would always remain equal and would vanish, the former into circles having the dimensions of the largest celestial orbits, the latter into single points. Hence in conformity with the preceding we may say that all circumferences of circles, however different, are equal to each other, and are each equal to a single point.

SAGR. This presentation strikes me as so clever and novel that, even if I were able, I would not be willing to oppose it; for to deface so beautiful a structure by a blunt pedantic attack would be nothing short of sinful. But for our complete satisfaction pray give us this geometrical proof that there is always equality between these solids and between their bases; for it cannot, I think, fail to be very ingenious, seeing how subtle is the philosophical argument based upon this result.

SALV. The demonstration is both short and easy. Referring to the preceding figure, since IPC is a right angle the square of the radius IC is equal to the sum of the squares on the two sides IP, PC; but the radius IC is equal to AC and also to GP, while CP is equal to PH. Hence the square of the line GP is equal to the sum of the squares of IP and PH, or multiplying through by 4, we have the square of the diameter GN equal to the sum of the squares on IO and HL. And, since the areas of circles are to each other as the squares of their diameters, it follows that the area of the circle whose diameter is GN is equal to the sum of the areas of circles having diameters IO and HL, so that if we remove the common area of the circle having IO for diameter the remaining area of the circle GN will be equal to the area of the circle whose diameter is HL. So much for the first part. As for the other part, we leave its demonstration for the present, partly because those who wish to follow it will find it in the twelfth proposition of the second book of *De centro gravitatis solidorum* by the Archimedes of our age, Luca Valerio, who made use of it for a different object, and partly because, for our purpose, it suffices to have seen that the above-mentioned surfaces are always equal and that, as they keep on diminishing uniformly, they degenerate, the one into a single point, the other into the circumference of a circle larger than any assignable; in this fact lies our miracle.

SAGR. The demonstration is ingenious and the inferences drawn from it are remarkable. And now let us hear something concerning the other difficulty raised by Simplicio, if you have anything special to say, which, however, seems to me hardly possible, since the matter has already been so thoroughly discussed.

SALV. But I do have something special to say, and will first of all repeat what I said a little while ago, namely, that infinity and indivisibility are in their very nature incomprehensible to us; imagine then what they are when combined. Yet if we wish to build up a line out of indivisible points, we must take an infinite number of them, and are, therefore, bound to understand both the infinite and the indivisible at the same time. Many ideas have passed through my mind concerning this subject, some of which, possibly the more important, I may not be able to recall on the spur of the moment; but in the course of our discussion it may happen that I shall awaken in you, and especially in Simplicio, objections and difficulties which in turn will bring to memory that which, without such stimulus, would have lain dormant in my mind. Allow me therefore the customary liberty of introducing some of our human fancies, for indeed we may so call them in comparison with supernatural truth which furnishes the one true and safe recourse for decision in our discussions and which is an infallible guide in the dark and dubious paths of thought.

One of the main objections urged against this building up of continuous quantities out of indivisible quantities [*continuo d' indivisibili*] is that the addition of one indivisible to another cannot produce a divisible, for if this were so it would render the indivisible divisible. Thus if two indivisibles, say two points, can be united to form a quantity, say a divisible line, then an even more divisible line might be formed by the union of three, five, seven, or any other odd number of points. Since however these lines can be cut into two equal parts, it becomes possible to cut the indivisible which lies exactly in the middle of the line. In answer to this and other objections of the same type we reply that a divisible magnitude cannot be constructed out of two or ten or a hundred or a thousand indivisibles, but requires an infinite number of them.

SIMP. Here a difficulty presents itself which appears to me insoluble. Since it is clear that we may have one line greater than another, each containing an infinite number of points, we are forced to admit that, within one and the same class, we may have something greater than infinity, because the infinity of points in the long line is greater than the infinity of points in the short line. This assigning to an infinite quantity a value greater than infinity is quite beyond my comprehension.

SALV. This is one of the difficulties which arise when we attempt, with our finite minds, to discuss the infinite, assigning to it those properties which we give to the finite and limited; but this I think is wrong, for we cannot speak of infinite quantities as being the one greater or less than or equal to another. To prove this I have in mind an argument which, for the sake of clearness, I shall put in the form of questions to Simplicio who raised this difficulty.

I take it for granted that you know which of the numbers are squares and which are not.

SIMP. I am quite aware that a squared number is one which results from the multiplication of another number by itself; thus 4, 9, etc., are squared numbers which come from multiplying 2, 3, etc., by themselves.

SALV. Very well; and you also know that just as the products are called squares so the factors are called sides or roots; while on the other hand those numbers which do not consist of two equal factors are not squares. Therefore if I assert that all numbers, including both squares and non-squares, are more than the squares alone, I shall speak the truth, shall I not?

SIMP. Most certainly.

SALV. If I should ask further how many squares there are one might reply truly that there are as many as the corresponding numbers of roots, since every square has its own root and every root its own square, while no square has more than one root and no root more than one square.

SIMP. Precisely so.

SALV. But if I inquire how many roots there are, it cannot be denied that there are as many as there are numbers because every number is a root of some square. This being granted we must say that there are as many squares as there are numbers because they are just as numerous as their roots, and all the numbers are roots. Yet at the outset we said there are many more numbers than squares, since the larger portion of them are not squares. Not only so, but the proportionate number of squares diminishes as we pass to larger numbers. Thus up to 100 we have 10 squares, that is, the squares constitute 1/10 part of all the numbers; up to 10000, we find only 1/100 part to be squares; and up to a million only 1/1000 part; on the other hand in an infinite number, if one could conceive of such a thing, he would be forced to admit that there are as many squares as there are numbers all taken together.

SAGR. What then must one conclude under these circumstances?

SALV. So far as I see we can only infer that the totality of all numbers is infinite, that the number of squares is infinite, and that the number of their roots is infinite; neither is the number of squares less than the totality of all numbers, nor the latter greater than the former; and finally the attributes "equal," "greater," and "less," are not applicable to infinite, but only to finite, quantities. When therefore Simplicio introduces several lines of different lengths and asks me how it is possible that the longer ones do not contain more points than the shorter, I answer him that one line does not contain more or less or just as many points as another, but that each line contains an infinite number. Or if I had replied to him that the points in one line were equal in number to the squares; in another, greater than the totality of numbers; and in the little one, as many as the number of cubes, might I not, indeed, have satisfied him by thus placing more points in one line than in another and yet maintaining an infinite number in each? So much for the first difficulty.

SAGR. Pray stop a moment and let me add to what has already been said an idea which just occurs to me. If the preceding be true, it seems to me impossible to say either that one infinite number is greater than another or even that it is greater than a finite number, because if the infinite number were greater than, say, a million it would follow that on passing from the million to higher and higher numbers we would be approaching the infinite; but this is not so; on the contrary, the larger the number to which we pass, the more we recede from [this property of] infinity, because the greater the numbers the fewer [relatively] are the squares contained in them; but the squares in infinity cannot be less than the totality of all the numbers, as we have just agreed; hence the approach to greater and greater numbers means a departure from infinity.†

SALV. And thus from your ingenious argument we are led to conclude that the attributes "larger," "smaller," and "equal"

† A certain confusion of thought appears to be introduced here through a failure to distinguish between the *number n* and the *class* of the first *n* numbers; and likewise from a failure to distinguish infinity as a number from infinity as the class of all numbers. [*Trans.*]

have no place either in comparing infinite quantities with each other or in comparing infinite with finite quantities.

I pass now to another consideration. Since lines and all continuous quantities are divisible into parts which are themselves divisible without end, I do not see how it is possible to avoid the conclusion that these lines are built up of an infinite number of indivisible quantities because a division and subdivision which can be carried on indefinitely presupposes that the parts are infinite in number, otherwise the subdivision would reach an end; and if the parts are infinite in number, we must conclude that they are not finite in size, because an infinite number of finite quantities would give an infinite magnitude. And thus we have a continuous quantity built up of an infinite number of indivisibles.

CHAPTER 7

MAGNETISM

THE attraction of magnetite ore (an oxide of iron) was first mentioned by Thales of Miletus (b. 624 B.C.), one of the seven sages of ancient Greece, sometimes called also the father of physics. Comparatively few additional facts were noted until 1600 when William Gilbert published his systematic study, marking the beginning of magnetism as a science; Gilbert, accordingly, has been called "the father of magnetism and electricity".

Galileo apparently read this book shortly afterwards. He proceeded at once to make some investigations of his own, e.g. the behavior of magnetic poles. In particular, he experimented with *armatures*, i.e. a thin hemisphere of iron fitting a lodestone like a jacket. By this means the attractive force could be made considerably greater. Galileo boasted that he could thus make an armature sustain 26 times its own weight — in comparison with Gilbert's maximum of only 4 times.

Galileo was convinced by Gilbert's experiments with a lodestone model of the earth, a so-called terella, that the earth itself is a large magnet, but he did not exhibit much interest in Gilbert's speculations as to this force being applicable to planetary motions. He expressed regrets, moreover, that Gilbert failed to quantify his work.

Dialogue Covering Two Chief World Systems — Ptolemaic and Copernican†

SIMP. Then you are one of those people who adhere to the magnetic philosophy of William Gilbert?

SALV. Certainly I am, and I believe that I have for company every man who has attentively read his book and carried out his experiments. Nor am I without hope that what has happened to me in this regard may happen to you also, whenever a curiosity similar to mine, and a realization that numberless things in nature remain unknown to the human intellect, frees you from slavery

† Ref. (7), pp. 400–8.

to one particular writer or another on the subject of natural phenomena, thereby slackening the reins on your reasoning and softening your stubborn defiance of your senses, so that some day you will not deny them by giving ear to voices which are heard no more.

Now, the cowardice (if we may be permitted to use this term) of ordinary minds has gone to such lengths that not only do they blindly make a gift — nay, a tribute — of their own assent to everything they find written by those authors who were lauded by their teachers in the first infancy of their studies, but they refuse even to listen to, let alone examine, any new proposition or problem, even when it not only has not been refuted by their authorities, but not so much as examined or considered. One of these problems is the investigation of what is the true, proper, basic, internal, and general matter and substance of this terrestrial globe of ours. Even though neither Aristotle nor anybody else before Gilbert ever took it into his head to consider whether this substance might be lodestone (let alone Aristotle or anybody else having disproved such an opinion), I have met many who have started back at the first hint of this like a horse at his shadow, and avoided discussing such an idea, making it out to be a vain hallucination, or rather a mighty madness. And perhaps Gilbert's book would never have come into my hands if a famous Peripatetic philosopher had not made me a present of it, I think in order to protect his library from its contagion.

SIMP. I frankly confess myself to have been one of these ordinary minds, and it is only since I have been allowed during the past few days to take part in these conferences of yours that I am aware of having wandered somewhat from the trite and popular path. But I do not yet feel so much awakened that the roughness of this new and curious opinion does not make it seem to me very laborious and difficult to master.

SALV. If what Gilbert writes is true, it is not an opinion; it is a scientific subject; it is not a new thing, but as ancient as the earth itself; and if true, it cannot be rough or difficult, but must be smooth and very easy. If you like, I can make it evident to you that you are creating the darkness for yourself, and feeling a horror of things which are not in themselves dreadful — like a little boy who is afraid of bugaboos without knowing anything about them except their name, since nothing else exists beyond the name.

SIMP. I should enjoy being enlightened and removed from error.

SALV. Then answer the questions I am about to ask you. First, tell me whether you believe that this globe of ours, which we inhabit and call "earth," consists of a single and simple material, or an aggregate of different materials.

SIMP. I can see that it is composed of very diverse substances and bodies. In the first place, I see water and earth as its major components, which are quite different from each other.

SALV. For the present let us leave out the oceans and other waters, and consider just the solid parts. Tell me whether these seem to you to be all one thing, or various things.

SIMP. As to appearances, I see them various, finding great fields of sterile sand, and others of fertile and fruitful soil; innumerable barren and rugged mountains are to be seen, full of hard rocks and stones of the most various kinds, such as porphyry, alabaster, jasper, and countless sorts of marble; there are vast mines of many species of metal, and, in a word, such a diversity of materials that a whole day would not suffice to enumerate these alone.

SALV. Now of all these different materials, do you believe that in the composition of this great mass they occur in equal proportions? Or rather that among them all there is one part which far exceeds the others and is in effect the principal matter and substance of this huge bulk?

SIMP. I believe that the stones, the marbles, the metals, the gems, and other materials so diverse are exactly like jewels and ornaments, external and superficial to the original globe, which I think immeasurably exceeds in bulk all these other things.

SALV. Now this vast principal bulk, of which the things you have named resemble excrescences and ornaments: Of what do you believe this to be made?

SIMP. I think it is the simple, less impure, element of earth.

SALV. But what is it that you understand by "earth"? Is it perhaps that which is spread over fields, which is broken with spades and plows, in which grain and fruit are sown and great forests spring up spontaneously? Which, in a word, is the habitat of all animals and the womb of all vegetation?

SIMP. This, I should say, is the primary substance of our globe.

SALV. Well, that does not seem to me to be a very good thing to say. For this earth that is broken, sown, planted, and that bears fruit is one part of the surface of the globe, and quite a shallow part. It does not go very deep in relation to the distance to the center, and experience shows that by digging not far down materials are to be found very different from the external crust; harder, and not any good for producing vegetation. Besides, the more central parts may be supposed, from being compressed by the very heavy weights which rest upon them, to be compacted together and to be as hard as the most solid rock. Add to this that it would be vain to endow with fertility material never destined to produce crops, but merely to remain buried forever in the deep dark abysses of the earth.

SIMP. Who is to say that the interior parts, close to the center, are sterile? Perhaps they also have their produce of things unknown to us.

SALV. Why, you, of all people, since you understand so well that all the integral parts of the universe are produced for man's benefit alone — you ought to be most certain that this above all should be destined for the sole convenience of us inhabitants of it. And what good could we get out of materials so hidden from us and so remote that we can never make them available? The interior substance of this globe of ours, then, cannot be material which can be broken or dissipated, or is loose like this topsoil which we call "earth," but must be a very dense and solid body; in a word, very hard rock. And if it must be such, what reason have you for being more reluctant to believe that it is lodestone than that it is porphyry, jasper, or some other hard stone? If Gilbert had written that the inside of this globe is made of sandstone, or chalcedony, perhaps the paradox would seem less strange to you?

SIMP. I grant that the most central parts of this globe are much compressed, and therefore compacted together and solid, more and more so as they go deeper; Aristotle also concedes this. But I am not aware of any reasons which oblige me to believe that they degenerate and become other than earth of the same sort as this on the surface.

SALV. I did not interject this argument for the purpose of proving conclusively to you that the primary and real substance of this globe of ours is lodestone, but merely to show you that there is no reason for people to be more reluctant to grant that it is lodestone than any other material. And if you think it over, you will find that it is not improbable that merely a single and arbitrary name motivated men to believe that this substance is earth, from the name "earth" being commonly used to signify that material which we plow and sow, as well as to name this globe of ours. But if the name for the latter had been taken from stone (as it might just as well have been as from earth) then saying that its primary substance was stone would surely not have met resistance or contradiction from anybody. Indeed, this is much more probable; I think it certain that if one could husk this great globe, taking off only a bulk of one or two thousand yards, and then separate the stones from the earth, the pile of rocks would be much, much larger than that of fertile earth.

Now I have not adduced for you any of the reasons which conclusively prove *de facto* that our globe is made of lodestone, nor is this the time to go into those, the more so as you may look them up in Gilbert at your leisure. I am merely going to explain, with a certain likeness to my own, his method of procedure in philosophizing, in order that I may stimulate you to read it. I know that you understand quite well how much a knowledge of events contributes to an investigation of the substance and essence of things; therefore I wish you to take care to inform yourself thoroughly about many events and properties that are found uniquely in lodestone. Examples of this are its attraction of iron, and its conferring this same power upon iron merely by its presence; likewise its communicating to iron the property of pointing towards the poles, just as it retains this power in itself. Moreover, I want you to make a visual test of how there resides in it a power of conferring upon the compass needle not only the property of pointing toward the poles with a horizontal motion under the meridian — a property long since known — but also a newly observed faculty of vertical dip when it is balanced upon a small sphere of lodestone on which this meridian has been previously marked. I mean that the needle declines from a given mark, a greater or less amount according as the needle is taken closer to or farther from the pole, until at the pole itself it stands erect and perpendicular, while in the equatorial regions it remains parallel to the axis.

Next, make a test of the power of attraction being more active in every piece of lodestone, nearer the poles than at the middle, and noticeably stronger at one pole than at the other, the stronger pole being the one which points toward the south. Note that in a small lodestone this stronger south pole becomes weaker whenever it is required to support some iron in the presence of the north pole of a much larger lodestone. To make a long story short, you may ascertain by experiment these and many other properties described by Gilbert, all of which belong to lodestone and none to any other material.

Now, Simplicio, suppose that a thousand pieces of different materials were set before you, each one covered and enclosed in cloth under which it was hidden, and that you were asked to find out from external indications the material of each one without uncovering it. If, in attempting to do this, you should hit upon one which plainly showed itself to have all the properties which you had already recognized as residing only in lodestone and not in any other material, what would you judge to be the essence of that material? Would you say that it might be a piece of ebony, or alabaster, or tin?

SIMP. There is no question at all that I should say it was a piece of lodestone.

SALV. In that case, declare boldly that under this covering or wrapper of earth, stone, metal, water, etc. there is concealed a huge lodestone. For in regard to this there are recognized, by anyone who observes carefully, all the same events which are perceived to belong to a true and unconcealed sphere of lodestone. If nothing more were to be observed than the dipping of the needle, which, carried around the earth, tilts more upon its approach to the pole and less as it goes towards the equator, where it finally becomes balanced, this alone ought to persuade the most stubborn judgment. I say nothing of another remarkable effect which is plainly seen in all pieces of lodestone and causes the south pole of a lodestone to be stronger than the other† for us inhabitants of the Northern Hemisphere. This difference is found to be the greater, the more one departs from the equator; at the equator, both sides are of equal strength, though noticeably weaker. But in the southern regions, far from the equator, it changes its nature and the side which is the weaker for us acquires power over the other. All this conforms with what we see done by a little piece of lodestone in the presence of a big one whose force prevails over the smaller and makes it subservient, so that according as it is held near to or far from the equator of the large one, it makes just such variations as I have told you are made by every lodestone carried near to or far from the earth's equator.

SAGR. I was convinced at my first perusal of Gilbert's book, and, having found an excellent piece of lodestone, I made many observations over a long period, all of which merited the greatest wonder. But what seemed most astonishing of all to me was the great increase in its power of sustaining iron when provided with an armature† in the manner taught by this same author. By thus equipping my piece I multiplied its strength by eight, and where previously it would scarcely hold up nine ounces of iron, with the armature it would sustain more than six pounds. Perhaps you have seen this very piece, sustaining two little iron anchors, in the gallery of your Most Serene Grand Duke, on whose behalf I parted with it.†

SALV. I used to look at it frequently with great amazement, until a still greater admiration seized me because of a little specimen in the possession of our Academician. This, being not over six ounces in weight and sustaining no more than two ounces unarmatured, supports one hundred sixty ounces when so equipped. Thus it bears eighty times as much with an armature as without, and holds up twenty-six times its own weight. This is a greater marvel than Gilbert was able to behold, since he writes that he was never able to get a lodestone which succeeded in sustaining four times its own weight.

SAGR. It seems to me that this stone opens to the human mind a large field for philosophizing, and I have often speculated to myself on how it imparts to the iron which arms it a force so greatly superior to its own. But I was unable ever to find any satisfactory solution, nor did I find anything to much advantage in what Gilbert has to say on this particular. I wonder whether the same is true of you.

SALV. I have the highest praise, admiration, and envy for this author, who framed such a stupendous concept regarding an object which innumerable men of splendid intellect had handled without paying any attention to it. He seems to me worthy of great acclaim also for the many new and sound observations which he made, to the shame of the many foolish and mendacious authors who write not just what they know, but also all the vulgar foolishness they hear, without trying to verify it by experiment; perhaps they do this in order not to diminish the size of their books. What I might have wished for in Gilbert would be a little more of the mathematician, and especially a thorough grounding in geometry, a discipline which would have rendered him less rash about accepting as rigorous proofs those reasons which he puts forward as *verae causae* for the correct conclusions he himself had observed. His reasons, candidly speaking, are not rigorous, and lack that force which must unquestionably be present in those adduced as necessary and eternal scientific conclusions.

I do not doubt that in the course of time this new science will be improved with still further observations, and even more by true and conclusive demonstrations. But this need not diminish the glory of the first observer. I do not have a lesser regard for the original inventor of the harp because of the certainty that his instrument was very crudely constructed and more crudely played; rather, I admire him much more than a hundred artists who in ensuing centuries have brought this profession to the highest perfection. And it seems to me most reasonable for the ancients to have counted among the gods those first inventors of the fine arts, since we see that the ordinary human mind has so little curiosity and cares so little for rare and gentle things that no desire to learn is stirred within it by seeing and hearing these practiced exquisitely by experts. Now consider for yourself whether minds of that sort would ever have been applied to the construction of a lyre or to the invention of music, charmed by the mere whistling of dry tortoise tendons, or the striking of four hammers!† To apply oneself to great inventions, starting from the smallest beginnings, and to judge that wonderful arts lie hidden behind trivial and childish things is not for ordinary minds; these are concepts and ideas for superhuman souls.

Now, in answer to your question, I say that I also thought for a long time to find the cause for this tenacious and powerful connection that we see between the iron armature of a lodestone and the other iron which joins itself to it. In the first place, I am certain that the power and force of the stone is not increased at all by its having an armature. for it does not attract through a longer distance. Nor does it attract a piece of iron as strongly if a thin slip of paper is introduced between this and the armature; even if a piece of gold leaf is interposed, the bare lodestone will sustain more iron than the armature. Hence there is no change here in the force, but merely something new in its effect.

And since for a new effect there must be a new cause, we seek what is newly introduced by the act of supporting the iron via the armature, and no other change is to be found than a difference in contact. For where iron originally touched lodestone, now iron touches iron, and it is necessary to conclude that the difference in these contacts causes the difference in the results. Next, the difference between the contacts must come, so far as I can see, from the substance of the iron being finer, purer, and denser in its particles than is that of the lodestone, whose parts are coarser, less pure, and less dense. From this it follows that the surfaces of the two pieces of iron which are to touch, when perfectly smoothed, polished, and burnished, fit together so exactly that all the infinity of points on one touch the infinity of points on the other. Thus the threads which unite the pieces of iron are, so to speak, more numerous than those which join lodestone to iron, on account of the substance of lodestone being more porous and less integrated, so that not all the points and threads on the surface of the iron find counterparts to unite with on the surface of the lodestone.

Now we may see that the substance of iron (especially when much refined, as is the finest steel) is much more dense, fine, and pure in its particles than is the material of lodestone, from the possibility of bringing the former to an extremely thin edge, such as a razor edge, which can never be done to a piece of lodestone with any success. The impurity of the lodestone and its adulteration with other kinds of stone can next be sensibly observed; in the first place by the color of some little spots, gray for the most part, and secondly by bringing it near a needle suspended on a thread. The needle cannot come to rest at these little stony places; it is attracted by the surrounding portions, and appears to leap toward these and flee from the former spots. And since some of these heterogeneous spots are large enough to be easily visible, we may believe that others are scattered in great quantity throughout the mass but are not noticeable because of their small size.

What I am telling you (that is, that the great abundance of contacts made between iron and iron is the cause of so solid an attachment) is confirmed by an experiment. If we present the sharp point of a needle to the armature of a lodestone, it attaches itself no more strongly than it would to the bare lodestone; this can result only from the two contacts being equal, both being made at a single point. But now see what follows. A needle is placed upon the lodestone so that one of its ends sticks out somewhat beyond, and a nail is brought up to this. Instantly the needle will attach itself to it so firmly that upon the nail being drawn back, the needle can be suspended with one end attached to the lodestone and the other to the nail. Withdrawing the nail still farther, the needle will come loose from the lodestone if the needle's eye is attached to the nail and its point to the lodestone; but if the eye is toward the lodestone, the needle will remain attached to the lodestone upon withdrawing the nail. In my judgment, this is for no other reason than that the needle, being larger at the eye, makes contact in more places than it does at its very sharp point.

SAGR. The entire argument looks convincing to me, and I rank these experiments with the needle very little lower than mathematical proof. I frankly admit that in the entire magnetic science I have not heard or read anything which gives so cogently the reasons for any of its other remarkable phenomena. If their causes were to be explained to us this clearly, I can think of nothing pleasanter that our intellects could wish for.

SALV. In investigating the unknown causes of our conclusions, one must be lucky enough right from the start to direct one's reasoning along the road of truth.

CHAPTER 8

THE PUMP THAT FAILED

THE *lift pump*, often used to raise water in wells, has a piston that moves up and down; a flap valve opens on the down-stroke but closes on the up-stroke. The first few strokes force air out of the cylinder, thus creating a partial vacuum into which water flows up from the well. The lift pump had been used for wells of various depths. It was quite unexpected, therefore, to find a practical limit (about 27 ft) to the depth, regardless of the size of the lift pump. In order to account for this phenomenon Galileo pictured a column of water breaking under its own weight. The true explanation, however, had to await his successor Torricelli's explanation in terms of the external atmospheric pressure (due to the weight of air) on the water in the well with the invention of the *barometer* as a consequence.

Dialogues Concerning Two New Sciences†

SAGR. Thanks to this discussion, I have learned the cause of a certain effect which I have long wondered at and despaired of understanding. I once saw a cistern which had been provided with a pump under the mistaken impression that the water might thus be drawn with less effort or in greater quantity than by means of the ordinary bucket. The stock of the pump carried its sucker and valve in the upper part so that the water was lifted by attraction and not by a push as is the case with pumps in which the sucker is placed lower down. This pump worked perfectly so long as the water in the cistern stood above a certain level; but below this level the pump failed to work. When I first noticed this phenomenon I thought the machine was out of order; but the workman whom I called in to repair it told me the defect was not in the pump but in the water which had fallen too low to be

† Ref. (9), pp. 16–17.

raised through such a height; and he added that it was not possible, either by a pump or by any other machine working on the principle of attraction, to lift water a hair's breadth above eighteen cubits; whether the pump be large or small this is the extreme limit of the lift. Up to this time I had been so thoughtless that, although I knew a rope, or rod of wood, or of iron, if sufficiently long, would break by its own weight when held by the upper end, it never occurred to me that the same thing would happen, only much more easily, to a column of water. And really is not that thing which is attracted in the pump a column of water attached at the upper end and stretched more and more until finally a point is reached where it breaks, like a rope, on account of its excessive weight?

SALV. That is precisely the way it works; this fixed elevation of eighteen cubits is true for any quantity of water whatever, be the pump large or small or even as fine as a straw. We may therefore say that, on weighing the water contained in a tube eighteen cubits long, no matter what the diameter, we shall obtain the value of the resistance of the vacuum in a cylinder of any solid material having a bore of this same diameter.

CHAPTER 9

APPARENT LIGHTNESS

IN HIS early lectures, although Galileo accepted many traditional views about motion, he was not wholly Aristotelian. Later he became increasingly critical of Aristotle's static universe, particularly as interpreted by fanatic followers; he himself showed an inquiring spirit and sought to understand phenomena themselves.

Thus while accepting the Aristotelian doctrine of a place for everything and a natural tendency for everything to go to its proper place, he explained the actual motion of a body in a medium, i.e. its apparent *lightness* or *heaviness* (he early regarded these as relative terms inasmuch as all materials have weight and hence would naturally move down if free), as dependent basically upon the buoyant force of the medium itself. By Archimedes' principle the *unbalanced force*, i.e. the apparent weight W_A of a body A of volume V_A and density D_A in a medium of density d_M is

$$W_A = (D_A - d_M)\ V_A g\ ,$$

where g is the acceleration due to gravity. The actual cause of motion in this instance was regarded by Galileo as analogous to the law of a balance (e.g. a lever), which moves downward on the apparently heavier side. Intuitively, he assumed that the resulting speed v_A is proportional to the unbalance force, i.e.

$$W_A = (D_A\ \ V_A)\ v_A$$

(true according to *Newton's second law* of motion for a uniformly accelerated body starting from rest). Galileo, of course, recognized that "the points set forth . . . cannot very well be further elucidated mathematically; they require rather a physical explanation". Likewise for a second body B in a medium N,

$$W_B = (D_B V_B) v_B\ .$$

Hence, dividing and substituting for the apparent weights, we obtain

$$\frac{v_A}{v_B} = \frac{D_B}{D_A} \cdot \frac{D_A - d_M}{D_B - d_M}\ .$$

Let us apply this analysis to several different problems.

(1) A body having the same density as the medium, i.e. $D_A = d_M$. In this case $W_A =$; 0 hence $f_A = 0$. The body will move neither upward nor downward.

(2) A body less dense than the medium. Here $D_A < d_M$ so that W_A is negative, signifying upward motion.

(3) A body more dense that the medium. W_A is now positive, inasmuch as $D_A > d_M$, so that the body moves downward.

(4) Two different bodies of the same material A in the same medium M, hence

$$\frac{v_A}{v_B} = \frac{D_A - d_M}{D_A - d_M} = 1 \ .$$

In other words, regardless of their size, if dropped simultaneously from rest, these bodies of different weights will nevertheless fall at the same rate — contrary to Aristotle's opinion that a larger body moves more swiftly.

(5) For any body in a *vacuum*, i.e. $d_M = 0.$, there is a finite speed, namely,

$$v = g$$

owing to dependence upon the difference of densities — not any absolute value. Aristotle, on the contrary, believed that the speed of a body in a vacuum would be infinite inasmuch as it varied inversely only as the "resistance" of a medium (zero for a vacuum). Considering such instantaneous motion to be impossible, Aristotle concluded that a vacuum cannot exist.

On Motion†

CHAPTER 3

THAT NATURAL MOTION IS CAUSED BY HEAVINESS OR LIGHTNESS. In the previous chapter we asserted, and assumed it as well known, that nature has so arranged it that heavier bodies remain at rest under lighter. We must, therefore, now note that bodies which move downward move because of their heaviness, while those that move upward move because of their lightness. For, since heavy bodies have, by reason of their heaviness, the property of remaining at rest under lighter bodies — inasmuch as they are heavy, they have been placed by nature under the lighter — they will also have the property, imposed by nature, that, when they are situated above lighter bodies, they will move down below these lighter bodies, lest the lighter remain at rest under the heavier, contrary to the arrangement of nature. And, in the same way, light bodies will move upward by their lightness, whenever they are under heavier bodies. For if they have, by reason of their lightness, the property of remaining at rest above heavier bodies, they will also, by that same lightness, have the property of

† Ref. (3), pp. 16–47.

not remaining at rest below heavier bodies, unless they are constrained.

Now it is clear from this that, in the case of motion, we must consider not merely the lightness or heaviness of the moving body, but also the heaviness or lightness of the medium through which the motion takes place. For if water were not lighter than stone, a stone would not sink in water. But a difficulty might arise at this point, as to why a stone cast into the sea moves downwards naturally, despite the fact that the [sum total of] water of the sea is far heavier than the stone that was thrown. We must therefore recall what we pointed out in chapter [1], viz., that the stone is, in fact, heavier than the water of the sea, if we take a quantity of water equal in volume to that of the stone; and so the stone, being heavier than the water, will move downward in the water.

But again a difficulty will arise as to just why we must consider a quantity of water equal in volume to the volume of the stone, rather than of the whole sea. In order to remove this difficulty, I have decided to adduce some proofs, on which not only the solution of this difficulty, but the whole discussion will depend. Though, to be sure, there are many media through which motions take place, e.g., fire, air, water, etc., and in all of them the same principle applies, we shall assume that water is the medium in which the motion is to take place. And first we shall prove that bodies equally heavy with the water itself, if let down into the water, are completely submerged, but then move neither downward nor upward. Secondly, we shall show that bodies lighter than water not only do not sink in the water, but are not even completely submerged. Thirdly, we shall prove that bodies heavier than water necessarily move downward.

CHAPTER 4

FIRST DEMONSTRATION, IN WHICH IT IS PROVED THAT BODIES OF THE SAME HEAVINESS AS THE MEDIUM MOVE NEITHER UPWARD NOR DOWNWARD. Coming now to the proofs, let us first consider a body of the same heaviness as water, i.e., one whose weight is equal to the weight of a quantity of water equal in volume to the volume of the given body. Let the body be *ef*. It must then be proved that body *ef*, if let down into the water, is completely submerged, and then moves neither upward nor downward. Let *abcd* be the position of the water, before the body is let down into it. And suppose that body *ef*, after being let down into the water, is not completely submerged, if that is possible, but that some part of it, say *e*, protrudes, only part *f* being submerged. While part *f* of the body is being submerged, the level of the water is necessarily raised. Thus, suppose surface *ao* of the water is raised to surface *st*. Clearly, the volume of water *so* is exactly equal to the volume of *f*, the submerged part of the body. For it is necessary that the space into which the body enters be vacated by the water, and that a volume of water be removed equal to the volume of that portion of the body that is submerged. Therefore

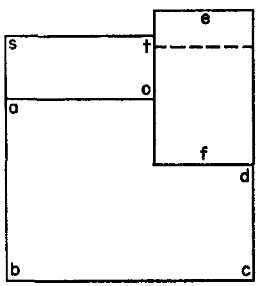

FIG. 2

the volume of water *so* is equal to the volume of the submerged part *f* of the body. Hence also the weight of *f* will equal the weight of water *so*.

Now the water *so* strives by its weight to return downwards to its original position, but cannot achieve this unless solid *ef* is first lifted out of the water, i.e., raised by the action of the water. And the solid resists such raising with all the weight that it has; moreover, both the solid body and the water are assumed to be at rest in this position. Therefore the weight of water *so*, by which the water strives to raise the solid upward, must necessarily be equal to the weight with which the solid resists and presses downward. (For if the weight of water *so* were greater than the weight of solid *ef*, *ef* would be raised and forced out of the water; if, on the other hand, the weight of solid *ef* were greater, the water level would be raised. But everything is assumed to be at rest in this position.) Hence the weight of water *so* is equal to the weight of the whole magnitude *ef*. But that is impossible, for the weight of *so* is equal to that of part *f*. It is clear, therefore, that no part of the solid magnitude *ef* will protrude [above the water level], but that the whole will be submerged.

This is the complete demonstration which I have set forth in fuller detail so that those who come upon it for the first time may be able to understand it more easily. But it might be better explained more briefly, so that the entire heart of the proof would be as follows. We must prove that magnitude *ef*, which is assumed to be of the same weight as water, is completely submerged. Suppose, then, if it is *not* completely submerged, that some part of it protrudes. Let *e* be the part protruding, and let the water level be raised to the surface *st;* and, if possible, let the water and the body both be at rest in this position. Then, since magnitude *ef* presses by its weight and tends to raise water *so*, and water *so* by its weight resists being further raised, it must follow that the weight of *ef* pressing down is exactly equal to the weight of water *so* which resists. For since they are assumed to be at rest in this position, neither will the pressure be greater than the resistance, nor the resistance greater than the pressure. Hence the weight of water *so* is equal to the weight of magnitude *ef*. But this is impossible. For, since the volume of the whole body *ef* is greater than the volume of the water *so*, the weight of body *ef* will also be greater than that of the water *so*. It is therefore clear that bodies of the same heaviness as water will be completely submerged in water.

And I say further that they will move neither upward nor downward, but will remain at rest wherever they are placed. For there is no reason why they should move downward or upward. Since they are assumed to be of the same heaviness as water, to say that they sink in water would be the same as saying that water, when placed in water, sinks underneath this water; and then that the water which rises above the first-mentioned water again moves downward, and that the water thus continues to move alternately downward and upward forever. This is impossible.

CHAPTER 5

SECOND DEMONSTRATION, IN WHICH IT IS PROVED THAT BODIES LIGHTER THAN WATER CANNOT BE COMPLETELY SUBMERGED. Now since the demonstration in the previous chapter had to do with a state of rest, we must now consider a case which involves motion upward. I say, then, that bodies lighter than water, when let down into water, are not completely submerged, but that some portion protrudes. Let the first level of the water, before the body is let down, be along surface *ef*; and suppose that body *a*, lighter than water, is let down into the water, and, if possible, is completely submerged, and the water level raised to surface *cd*. And suppose, if it is possible, that both the water and the body remain at rest in this position. Now the weight with which the body

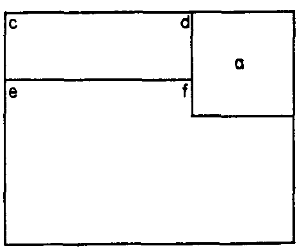

FIG. 3

exerts pressure and tends to raise water *cf* will be equal to the weight with which water *cf* exerts pressure to raise body *a*. But the volume of water *cf* is equal to that of body *a*. There are therefore two magnitudes, one the body *a*, the other the water *cf*; and the weight of *a* is equal to that of *cf*, and also the volume of *a* is equal to the volume of the water *cf*. Therefore body *a* is of the same heaviness as the water. But this is impossible: for the body was assumed to be lighter than water. Therefore body *a* will not remain completely submerged under the water. It will therefore of necessity move upward.

It is clear, then, why and how motion upward results from lightness. And, from what has been said in this and in the previous chapter, it can easily be understood that bodies heavier than water are completely submerged and must keep moving downward. That they are completely submerged is a necessary conclusion. For if they were not completely submerged, they would be lighter than water, and this would be contrary to our assumption. For it follows from the converse of the proposition just proved that bodies which are not completely submerged are lighter than water. Moreover, these bodies [i.e., those heavier than water] must continue to move downward. For if they did not move downward, they would either be at rest or move upward. But they could not be at rest: for it was proved in the preceding chapter that bodies having the same heaviness as water remain at rest and move neither upward nor downward. And it has just been shown that bodies lighter than water move upward. Therefore, from all these considerations, since bodies which move downwards must be heavier than the medium through which they move, it is clear that heavy bodies move downward by reason of their weight. And it is clear that, in the case of the stone thrown into the sea, we must reckon not with all the water of the sea, but only with that small part which is removed from the place into which the stone enters.

But the points set forth in these last two chapters cannot very well be further elucidated mathematically; they require rather a physical explanation. For this reason I propose, in the next chapter, to reduce the matter to a consideration of the balance, and to explain the analogy that holds between bodies that move naturally and the weights of the balance. My aim is a richer comprehension of the matters under discussion, and a more precise understanding on the part of my readers.

CHAPTER 6

IN WHICH IS EXPLAINED THE ANALOGY BETWEEN BODIES MOVING NATURALLY AND THE WEIGHTS OF A BALANCE. We shall first consider what happens in the case of the balance, so that we may then show that all these things also happen in the case of bodies moving naturally. Let line *ab*, then, represent a balance, whose center, over which motion may take place, is the point *c* bisecting line *ab*. And let two weights, *e* and *o*, be suspended from points *a* and *b*. Now in the case of weight *e* there are three possibilities:

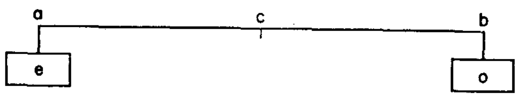

FIG. 4.

it may either be at rest, or move upward, or move downward. Thus if weight *e* is heavier than weight *o*, then *e* will move downward. But if *e* is less heavy, it will, of course, move upward, and not because it does not have weight, but because the weight of *o* is greater. From this it is clear that, in the case of the balance, motion upward as well as motion downward takes place because of weight, but in a different way. For motion upward will occur for *e* on account of the weight of *o*, but motion downward on account of its own weight. But if the weight of *e* is equal to that of *o*, then *e* will move neither upward nor downward. For *e* will not move downward unless weight *o*, which tends to raise it, is less heavy; nor will *e* move upward unless weight *o*, by which it must be raised, is heavier.

Having examined the case of the balance, we return to naturally moving bodies. We can assert this general proposition: that the heavier cannot be raised by the less heavy. On this assumption it is easy to understand why solids lighter than water are not completely submerged.

If, for example, we let a piece of wood down into water, then, if the wood is to be submerged, water must necessarily leave the place into which the wood enters, and this water must be raised, that is, must be moved in a direction away from the center of the universe. If, then, the water which has to be raised is heavier than the wood, it surely will not be able to be raised by the wood. But if the whole piece of wood is submerged, then from the place into which the wood enters a volume of water must be removed equal to the volume of the whole piece of wood. But a volume of water equal to the volume of the wood is heavier than the wood (for it is assumed that the wood is lighter than water). It will therefore not be possible for the wood to be completely submerged. And this is in agreement with what was said in the case of the balance, namely, that a smaller weight cannot raise a larger.

But if the wood were of the same heaviness as the water, that is, if the water which is raised by the complete submerging of the wood is not heavier than but only just as heavy as the wood, the wood will of course then be completely submerged, since it does not meet [sufficient] resistance from the lifting action of the water. But once it is entirely submerged it does not continue to move either upward or downward. And this corresponds analogically to what was said, in the case of the balance, about equal weights neither of which moves upward or downward.

But if, on the other hand, the wood is heavier than that part of the water which tends to be lifted by the wood, that is to say, if the wood is heavier than a volume of water equal to the volume of the wood (for, as has often been said, the volume of water that is lifted by the submerged wood is equal to the volume of the wood), then surely the wood will continue to move downward. And this corresponds analogically to what was said in the case of the balance — namely, that one weight moves down and sends the other one up when it is heavier than that other.

Besides, in the case of bodies moving naturally, as in the weights in a balance, the cause of all motions, up as well as down, can be referred to weight alone. For when something moves up it is at that time being raised by the weight of the medium. Thus, if a piece of wood lighter than water is forcibly held under water, then, since the submerged wood displaces a volume of water equal to its own volume, and since a volume of water equal to the volume of the wood is heavier than the piece of wood, doubtless the wood will be lifted by the weight of that water and will be impelled upward. Thus upward motion will occur because of the heaviness of the medium and the [relative] lightness of the moving body; and downward motion because of the heaviness of the moving body and the [relative] lightness of the medium.

And from this one can easily understand (contrary to Aristotle *De Caelo* 1.89) that what moves moves, as it were, by force and by the extruding action of the medium. For when the wood is forcibly submerged, the water violently thrusts it out when, with downward motion, it moves toward its own proper place and is unwilling to permit that which is lighter than itself to remain at rest under it. In the same way, the stone is thrust from its position and impelled downward because it is heavier than the medium. It is therefore clear that this kind of motion may be called "forced," although commonly the upward motion of wood in water and the downward motion of stone in water are called "natural." And Aristotle's argument is invalid when he says: "If the motion were forced, it would lose speed at the end and not gain it, as it does." For forced motion loses speed only when the moving body leaves the hand of the mover, not while it is still in contact with the mover.

It is therefore clear that the motion of bodies moving naturally can be suitably reduced to the motion of weights in a balance. That is, the body moving naturally plays the role of one weight in the balance, and a volume of the medium equal to the volume of the moving body represents the other weight in the balance. So that, if a volume of the medium equal to the volume of the moving body is heavier than the moving body, and the moving body lighter, then the latter, being the lighter weight, will move up. But if the moving body is heavier than the same volume of the medium, then, being the heavier weight, it will move down. And if, finally, the said volume of the medium has a weight equal to that of the moving body, the latter will move neither up nor down, just as the weights in the balance, when they are equal to each other, neither fall nor rise.

And since the comparison of bodies in natural motion and weights on a balance is a very appropriate one, we shall demonstrate this parallelism throughout the whole ensuing discussion of natural motion. Surely this will contribute not a little to the understanding of the matter.

CHAPTER 7

THE CAUSE OF SPEED AND SLOWNESS OF NATURAL MOTION. Since it has been quite fully explained above that natural motions are caused by heaviness and lightness, we must now consider how the greater or lesser speed of such motion comes about. In order to be able to accomplish this more easily, we must make the following distinction, viz., that inequalities in the slowness and speed of motion occur in two ways: either the same body moves in different media, or else there are different bodies moving in the same medium. We shall show presently that in both these cases of motion the slowness and speed depend on the same cause, namely, the greater or lesser weight of the media and of the moving bodies. But first we shall show that the cause given by Aristotle to account for this effect is insufficient.

Aristotle wrote (*Physics* 4.71) that the same body moves more swiftly in a rarer than in a denser medium, and that therefore the cause of slowness of motion is the density of the medium, and the cause of speed its rareness. And he asserted this on the basis of no other reason than experience, viz., that we see a moving body move more swiftly in air than in water.

But it will be easy to prove that this reason is not sufficient. For if the speed of motion depends on the rareness of the medium, the same body will always move more swiftly through rarer media. But this is erroneous. For there are many moving bodies that move more swiftly with natural motion in denser media than they do in rarer ones, e.g., more swiftly in water than in air. If, for example, we take a very thin inflated bladder, it will descend slowly with natural motion in air. But if we release it in deep water, it will fly up very fast, again with natural motion. At this point I know that someone may reply that the bladder moves in air and is swiftly carried down, but in water not only does it not fall faster, it does not fall at all. I would say in answer that the bladder moves up very swiftly in the water, but then does not continue moving in the air. But, not to prolong the argument, I say that in the rarer media not every motion, but only downward motion, is swifter; and upward motion is swifter in denser media. And this is reasonable. For in a place where a downward motion takes place with difficulty, an upward motion necessarily takes place with ease. Clearly, then, the statement of Aristotle that slowness of natural motion is due to the density of the medium is inadequate.

Therefore, dismissing his opinion, so that we may adduce the true cause of slowness and speed of motion, we must point out that speed cannot be separated from motion. For whoever asserts motion necessarily asserts speed; and slowness is nothing but lesser speed. Speed therefore proceeds from the same [cause] from which motion proceeds. And since motion proceeds from heaviness and lightness, speed or slowness must necessarily proceed from the same source. That is, from the greater heaviness of the moving body there results a greater speed of the motion, namely, downward motion, which comes about from the heaviness of that body; and from a lesser heaviness [of the body], a slowness of that same motion. On the other hand, from a greater lightness of the moving body will result a greater speed in that motion which comes about from the lightness of the body, namely, upward motion.

Thus it is clear that a difference in the speed and slowness of motion occurs in the case of different bodies moving in the same medium. For if the motion is downward, the heavier substance will move more swiftly than the lighter; and if the motion is upward, that which is lighter will move more swiftly. But whether two bodies moving in the same medium maintain the same ratio between the speed of their motions as there is between their weights, as Aristotle believed, will be considered below.

And in the case of the speed and slowness of the same body moving in different media, the situation is similar. The body moves downward more swiftly in that medium in which it is heavier, than in another in which it is less heavy; and it moves upward more swiftly in that medium in which it is lighter, than in another in which it is less light. Hence it is clear that if we find in what media a given body is heavier, we shall have found media in which it will fall more swiftly. And if, furthermore, we can show how much heavier that same body is in this medium than in that, we shall have shown how much more swiftly it will move downward in this medium than in that. Conversely, in considering lightness, when we find a medium in which a given body will be lighter, we shall have found a medium in which it will rise more swiftly; and if we find how much lighter the given body is in this medium than in that, we shall also have found how much more swiftly the body will rise in this medium than in that.

But in order that all this may be more precisely grasped in any particular case of motion, we shall first speak of the motions of different bodies in the same medium, and show what ratio there is between these motions, with respect to slowness and speed. We shall then consider motions of the same body moving in different media, and show, likewise, what ratio there is between the motions.

CHAPTER 8

IN WHICH IT IS SHOWN THAT DIFFERENT BODIES MOVING IN THE SAME MEDIUM MAINTAIN A RATIO [OF THEIR SPEEDS] DIFFERENT FROM THAT ATTRIBUTED TO THEM BY ARISTOTLE. In order to deal more easily with the matters under investigation, we must understand, in the first place, that a difference between two such bodies can arise in two ways. They may be of the same material, e.g., both lead, or both iron, and differ in size [i.e., volume]; or else they may be of different materials, e.g., one iron, the other wood, and differ either in size and weight, or in weight but not size, or in size but not weight.

Of those [naturally] moving bodies which are of the same material, Aristotle said that the larger moves more swiftly. This is found in *De Caelo* 4.26, where he wrote that any body of fire moves upward, and that body which is larger moves faster; also that any body of earth moves downward, and, similarly, that that body which is larger moves faster. Aristotle also wrote (*De Caelo* 3.26): "Suppose a heavy body *b* moves on line *ce*, which is divided at point *d*. If, then, body *b* is divided in the same ratio as line *ce* is divided by *d*, clearly a part of *b* will move over line *cd* in the same time as the whole of *b* moves over the whole line *ce*." From

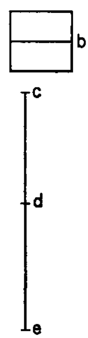

FIG. 5.

this it is obvious that Aristotle holds that, in the case of bodies of the same material, the ratio of the speeds of their [natural] motion is equal to the ratio of the sizes of the bodies. And he puts this most clearly when he says (*De Caelo* 4.16) that a large piece of gold moves more swiftly than a small piece.

But how ridiculous this view is, is clearer than daylight. For who will ever believe that if, for example, two lead balls, one a hundred times as large as the other, are let fall from the sphere of the moon, and if the larger comes down to the earth in one hour, the smaller will require one hundred hours for its motion? Or that, if two stones, one twice the size of the other, are thrown from the top of a high tower at the same moment, the larger reaches the ground when the smaller is only halfway down from the top of the tower? Or, again, if a very large piece of wood and a small piece of the same wood, the large piece being a hundred times the size of the small one, begin to rise from the bottom of the sea at the same time, who would ever say that the large piece would rise to the surface of the water a hundred times more swiftly?

But, to employ reasoning at all times rather than examples (for what we seek are the causes of effects, and these causes are not given to us by experience), we shall set forth our own view, and its confirmation will mean the collapse of Aristotle's view. We say, then, that bodies of the same kind (and let "bodies of the same kind" be defined as those that are made of the same material, e.g., lead, wood, etc.), though they may differ in size, still move with the same speed, and a larger stone does not fall more swiftly than a smaller. Those who are surprised by this conclusion will also be surprised by the fact that a very large piece of wood can float on water, no less than a small piece. For the reasoning is the same.

Thus, if we imagine that the water on which a large piece of wood and a small piece of the same wood are afloat, is gradually made successively lighter, so that finally the water becomes lighter than the wood, and both pieces slowly begin to sink, who could ever say that the large piece would sink first or more swiftly than the small piece? For, though the large piece of wood is heavier than the small one, we must nevertheless consider the large piece in connection with the large amount of water that tends to be raised by it, and the small piece of wood in connection with the correspondingly small amount of water. And since the volume of water to be raised by the large piece of wood is equal to that of the wood itself, and similarly with the small piece, those two quantities of water, which are raised by the respective pieces of wood, have the same ratio to each other in their weights as do their volumes (for portions of the same substance are to each other in weight as they are in volume; which would have to be proved) — i.e., the same ratio as that of the volumes of the large and the small piece of wood. Therefore the ratio of the weight of the large piece of wood to the weight of the water that it tends to raise is equal to the ratio of the weight of the small piece of wood to the weight of the water that *it* tends to raise. And the resistance of the large amount of water will be overcome by the large piece of wood with the same ease as the resistance of the small amount of water will be overcome by the small piece of wood.

Again, if we imagine, for example, a large piece of wax floating on water, and we mix this wax either with sand or with some other heavier substance, so that it ultimately becomes heavier than the water and just barely begins to sink very slowly, who could ever believe, if we take a small piece of that wax, say one-hundredth part, that it would either not sink at all or would sink a hundred times more slowly than the whole piece of wax? Surely no one. And one may make the same experiment with the balance. For if the weights on both sides are equal and very large, and then some weight, but a small one, is added to one side, the heavier side will fall, but not any faster than if the weights were small. Similarly in the case of the water: the large piece of wood represents one weight in the balance, and the other weight is represented by a volume of water equal to the volume of the wood. Now if this volume of water is of equal weight with the large piece of wood, the wood will not sink. But if the wood is made a little heavier so that it sinks, it will not sink any faster than will a small piece of the same wood, which at first weighed the same as an [equally] small volume of water, and then was made a little bit heavier.

But we may reach this same conclusion by another argument. Let us first make this assumption: if there are two bodies of which one moves [in natural motion] more swiftly than the other, a combination of the two bodies will move more slowly than that part which by itself moved more swiftly, but the combination will move more swiftly than that part which by itself moved more slowly. Thus, if we consider two bodies, e.g., a piece of wax and an inflated bladder, both moving upward from deep water, but the wax more slowly than the bladder, our assumption is that if both are combined, the combination will rise more slowly than the bladder alone, and more swiftly than the wax alone. Indeed it is quite obvious. For who can doubt that the slowness of the wax will be diminished by the speed of the bladder, and, on the other hand, that the speed of the bladder will be retarded by the slowness of the wax, and that some motion will result intermediate between the slowness of the wax and the speed of the bladder?

Similarly, if two bodies move downward [in natural motion], one more slowly than the other, for example, if one is wood and the other an [inflated] bladder, both falling in air, the wood more swiftly than the bladder, our assumption is as follows: if they are combined, the combination will fall more slowly than the wood alone, but more swiftly than the bladder alone. For it is clear that the speed of the wood will be retarded by the slowness of the bladder, and the slowness of the bladder will be accelerated by the speed of the wood; and, as before, some motion will result intermediate between the slowness of the bladder and the speed of the wood.

On the basis of this assumption, I argue as follows in proving that bodies of the same material but of unequal volume move [in natural motion] with the same speed. Suppose there are two bodies of the same material, the larger *a*, and the smaller *b*, and suppose, if it is possible, as asserted by our opponent, that *a* moves

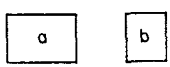

FIG. 6.

[in natural motion] more swiftly than *b*. We have, then, two bodies of which one moves more swiftly. Therefore, according to our assumption, the combination of the two bodies will move more slowly than that part which by itself moved more swiftly than the other. If, then, *a* and *b* are combined, the combination will move more slowly than *a* alone. But the combination of *a* and *b* is larger than *a* is alone. Therefore, contrary to the assertion of our opponents, the larger body will move more slowly than the smaller. But this would be self-contradictory.

What clearer proof do we need of the error of Aristotle's opinion? And who, I ask, will not recognize the truth at once, if he looks at the matter simply and naturally? For if we suppose that bodies *a* and *b* are equal and are very close to each other, all will agree that they will move with equal speed. And if we imagine that they are joined together while moving, why, I ask, will they double the speed of their motion, as Aristotle held, or increase their speed at all? Let us then consider it sufficiently corroborated that there is no reason *per se* why bodies of the same material should move [in natural motion] with unequal velocities, but every reason why they should move with equal velocity. Of course, if there were some accidental reason, e.g., the shape of the bodies, this will not be considered among causes *per se*. Moreover, as we shall show in the proper place, the shape of the body helps or hinders its motion only to a small extent.

Still we must not immediately go to extremes, as many do, and compare, say, a large piece of lead with a very thin plate or even leaf of the same substance, which would sometimes even float on water. For since there is a certain cohesiveness of the parts both of air and of water, and, so to speak, a tenacity and viscosity, this cannot be overcome by a very small weight. Our conclusion must therefore be understood to apply to [two] bodies when the weight and volume of the smaller of them are large enough not to be impeded by the small viscosity of the medium, e.g., a leaden sphere of one pound. Moreover, as for those scoffers who, perhaps, believe that they can defend Aristotle, what happens to them if they have recourse to extremes is that they get into deeper difficulties, the greater the difference between the bodies which they take for comparison. For if one of the bodies is a thousand times as large as the other, surely these people must do some toiling and sweating before they can show that the velocity of one is a thousand times that of the other.

But, to come to the next point, we must now consider the ratio [of the speed] of motion of bodies of *different* material moving [with natural motion] in the same medium. Though such bodies may differ from each other in three ways — either in size but not in weight, or in weight but not in size, or in both weight and size — we must examine only the case of those that differ in weight but not in size. For the ratios of those that differ in the two other ways can be reduced to this one. Thus in the case of bodies differing in size but not in weight, if from the larger we take a part equal to the smaller, the bodies will then differ in weight, but not in size. And the *whole* of the larger body will, with the smaller body, maintain the same ratio [in the speed of their motions] as will the *part* taken from the larger body. For it has been proved that bodies of the same material, though they differ in size, move with the same velocity.

Similarly, in the case of bodies differing both in size and weight, if we take from the larger a part equal [in size] to the smaller, we shall again have two bodies differing in weight, but not in size. And the *part* [of the larger body] will, with the smaller, keep the same ratio [in the speed] of their motions, as will the *whole* of the larger body. For, once again, in the case of bodies of the same material, the part and the whole move with the same speed. It is therefore clear that, if we know the ratio of the speeds of those bodies that differ only in weight, but not in size, we also know the ratios of those that differ in every other way. And so, in order to find this ratio and to show, in opposition to Aristotle's view, that also for bodies of *different* material this ratio is not equal to the ratio of their weights, we shall prove certain propositions. On these depends the outcome not only of this investigation but also of the investigation of the ratio of the speeds of the same body moving in different media. And we shall consider both matters together.

Let us therefore proceed to investigate the ratio [of the speeds] of the same body moving in different media. And first let us examine whether or not Aristotle's view on this is sounder than the other view explained above. Now Aristotle held that in the case of the same body moving in different media the ratio of the speeds was equal to the ratio of the rarenesses of the media. Indeed, that is what he wrote quite clearly, saying (*Physics* 4.71): "That medium which is denser interferes with motion more. Thus *a* will move over path *b* in time *c*, and over path *d*, a rarer medium, in time *e*, [the times being] in the ratio of the hindering action of

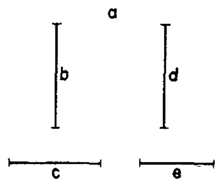

FIG. 7.

the media, provided the lengths of the paths are equal. Thus, if *b* is water and *d* air, *a* will move through *d* more swiftly than through *b* in proportion as air is less dense than water. Thus speed has to speed the same ratio as that between air and water. That is, if air is twice as rare as water, *a* will take twice as long to traverse path *b* as to traverse path *d*; and time *c* will be twice time *e*."

These are Aristotle's words, but surely they embrace a false viewpoint. And to make this perfectly clear I shall construct the following proof. If the ratio of the speeds is equal to the ratio of the rarenesses of the media, let there be a moving body *o* and a medium *a*, whose rareness is 4; let this medium be water, for example. Let the rareness of medium *b* be 16, greater, that is, than the rareness of *a*; and let us say, for example, that *b* is air. Now suppose that body *o* is such that it does not sink in water, but suppose that its velocity in medium *b* is 8. Hence, since the speed of *o* in medium *b* is 8, but in medium *a* is zero, some medium can surely be found in which the speed of *o* is 1. Let such a medium be *c*. Now since *o* moves more swiftly in medium *b* than in *c*, the rareness of *c* must be less than the rareness of *b*, and it must, according to our adversary, be less in proportion as the speed in medium *c* is less than the speed in medium *b*. But the speed in medium *b* was assumed to be eight times the speed in medium *c*. Therefore the rareness of medium *b* will be eight times the rareness of medium *c*. Hence the rareness of *c* will be 2. Therefore, *o* moves with speed 1 in the rareness of medium *c*, which is 2. But it was assumed that it cannot move in the rareness of medium

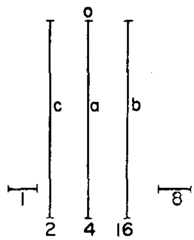

FIG. 8.

a, which is 4. Hence *o* will fail to move in the medium of greater rareness, though it moves in the medium of lesser rareness. This is completely absurd. Clearly, then, the speeds of the motions are not in the same ratio as the rarenesses of the media.

But even apart from other proof, can anyone fail to see the error in Aristotle's opinion? For if the speeds have the same ratio as the [rarenesses of the] media, then, conversely, the [rarenesses of the] media will have the same ratio as the speeds. Hence, since wood falls in air but not in water, and, consequently, the speed in air has no ratio to the speed in water, it follows that the rareness of air will have no ratio to the rareness of water. What can be more absurd than this? But someone might think that he would be giving a sufficient answer to my argument, if he said: "Though wood does not move downward in water, it does move upward; and the rareness of water has to the rareness of air the same ratio as the speed of the motion upward in water has to the speed of the motion downward in air." And he might believe that he had skillfully saved Aristotle by such an answer. But we shall destroy this subterfuge, too, by considering a body which moves neither up nor down in water. Such a body, for example, would be water itself. But water moves in air with considerable speed.

And so, having properly rejected Aristotle's view, let us now investigate the ratio [of the speeds] of the motion of the same body in different media. And first, in connection with upward motion, let us show that, when solids lighter than water are completely immersed in water, they are carried upward with a force measured by the difference between the weight of a volume of water equal to the volume of the submerged body and the weight of the body itself. Thus, let the first position of the water, before the body is immersed in it, have as its surface *ab*; and let the solid *cd* be forcibly immersed in it, the surface of the water being raised to *ef*. Since the raised water *eb* has a volume equal to that of the whole submerged body, and the body is assumed to be lighter than water, the weight of the water *eb* will be greater than the weight of *cd*. Then let *tb* represent that part of the water

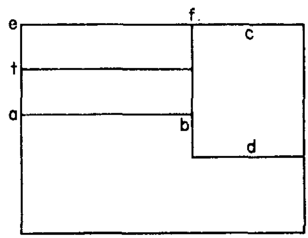

FIG. 9.

whose weight is equal to the weight of body *cd*. We must therefore prove that body *cd* is carried upward with a force equal to the weight of *tf* (for this is the amount by which water *eb* is heavier than water *tb*, that is to say, than body *cd*). Now since the weight of water *tb* is equal to the weight of *cd*, water *tb* will press upward so as to raise *cd* with the same force with which that body will resist being raised. Thus the weight of a part of the water that exerts pressure, namely *tb*, is equal to the resistance of the solid body. But the weight of all the water that exerts pressure, namely *eb*, exceeds the weight of water *tb* by the weight of water *tf*.

Thus the weight of all the water *eb* will exceed the resistance of solid *cd* by the weight of water *tf*. Therefore the weight of all the water that exerts pressure will impel the solid upward with a force equal to the weight of part *tf* of the water. Which was to be proved.

From this proof, first, it is clear that upward motion results from the weight not merely of the body, but of the medium, as we have shown; and, secondly, the whole purpose of our investigation can be achieved. For since we are investigating how much faster the same body rises in one medium than in another, whenever we know how fast it moves through each medium, we shall also know the interval between the two speeds. And this is what we seek. If, for example, a piece of wood whose weight is 4 moves upward in water, and the weight of a volume of water equal to that of the wood is 6, the wood will move with a speed that we may represent as 2. But if, now, the same piece of wood is carried upward in a medium heavier than water, a medium such that a volume of it equal to the volume of the wood has a weight of 10, the wood will rise in this medium with a speed that we may represent as 6. But it moved in the other medium with a speed 2. Therefore the two speeds will be to each other as 6 and 2, and not (as Aristotle held) as the weights or densities of the media, which are to each other as 10 and 6. It is clear, then, that in all cases the speeds of upward motion are to each other as the excess of weight of one medium over the weight of the moving body is to the excess of weight of the other medium over the weight of the body.

Therefore, if we wish to know at once the [relative] speeds of a given body in two different media, we take an amount of each medium equal to the volume of the body, and subtract from the weights [of such amounts] of each medium the weight of the body. The numbers found as remainders will be to each other as the speeds of the motions.

And we also obtain the answer to our second problem, namely, the ratio of the speeds of different bodies equal in volume but unequal in weight. For if each of them moves upward with a force measured by the difference between the weight of a volume of the medium equal to the volume of the body and the weight of the body itself, the numerical remainders, when the weights of the various bodies are subtracted from the weight of the aforesaid volume of the medium, will have the same ratio as the speeds. For example, if the weight of one body is 4, of a second body 6, and of the medium 8, the speed of the body whose weight is 4 will be 4, and the speed of the other body will be 2. These speeds, 4 and 2, do not have the same ratio, as the lightnesses of the bodies, 6 and 4. For the excesses of one number over two others will never have the same ratio to each other as the two numbers themselves; nor will the excesses of two numbers over another number have the same ratio to each other as the two numbers themselves. It is therefore clear that in motion upward the speeds of the different bodies are not in the same ratio as the lightnesses of the bodies.

It remains for us to show that also in the [natural] downward motion of bodies the ratio of the speeds is not equal to the ratio of the weights of the bodies; and at the same time to determine the ratio of the speeds of the same body moving in different media. All these results will easily be drawn from the following demonstration. I say, then, that a solid body heavier than water moves downward [in water] with a force measured by the difference in weight between an amount of water equal to the volume of the solid body and the body itself. Thus, let the water in its first position have the surface *de*; and let solid *bl*, heavier than water, be let down into it, the surface of the water rising to *ab*, so that water *ae* has a volume equal to the volume of the solid itself. Since the solid is assumed to be heavier than water, the weight of the water [*ae*] will be less than the weight of the solid. Thus, let *ao* be an amount of water that has a weight equal to the weight of *bl*. Now, since water *ae* is lighter than *ao* by the weight of *do*, we must prove that body *bl* moves downward with a force measured by the weight of water *do*.

Imagine a second solid body, lighter than water and joined to the first body; let its volume be equal to that of water *ao* and its weight equal to the weight of water *ae*. Let *lm* represent this body. Since the volume of *bl* is equal to that of *ae*, and the volume of *lm* is equal to that of *ao*, the volume of the combined bodies, *bl* and *lm*, is equal to the sum of the volumes of *ea* and *ao*.

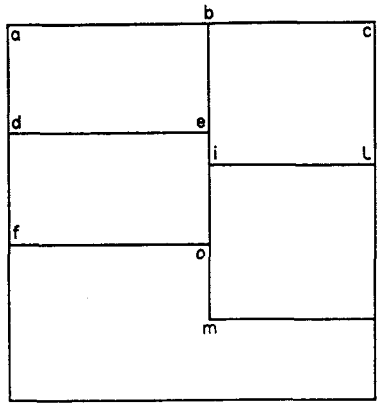

FIG. 10.

But the weight of water *ae* is equal to the weight of body *lm*; and the weight of water *ao* is equal to the weight of body *bl*. Therefore the whole weight of both bodies, *bl* and *lm*, is equal to the weight of water *oa* and *ae*. But the volume of the [combined] bodies [*bl* and *lm*] has been shown to be equal to the [combined] volume of water *oa* and *ae*. Hence by our first proposition, the bodies so joined will neither rise nor sink. Therefore the force of the downward pressure of body *bl* will be equal to the force of the upward pressure of *lm*. But, by the foregoing demonstration, magnitude *lm* tends to move upward with a force equal to the weight of water *do*. Therefore body *bl* will move downward with a force equal to the weight of water *do*. Which was to be proved.

Now if this demonstration is grasped, the answer to our problems can easily be discerned. For, clearly, in the case of the same body falling in different media, the ratio of the speeds of the motions is the same as the ratio of the amounts by which the weight of the body exceeds the weights [of an equal volume] of the respective media. Thus, if the weight of the body is 8, and the weight of a volume of one medium equal to the volume of the body is 6, the speed of the body can be represented by 2. And if the weight of a volume of the second medium equal to the volume of the body is 4, the speed of the body in this second medium can be represented by 4. Clearly, then, these speeds will be to each other as 2 and 4, and not as the densities or weights of the media, as Aristotle believed, i.e., as 6 and 4.

And similarly we have a clear answer to our second problem — to find the ratio of the speeds of bodies equal in size, but unequal in weight, moving [with natural motion] in the same medium. For the speeds of these bodies have the same ratio as do the amounts by which the weights of the bodies exceed the weight of the medium. For example, if there are two bodies equal in volume but unequal in weight, the weight of one of them being 8, and of the other 6, and if the weight of a volume of the medium equal to the volume of either body is 4, the speed of the first body will be 4 and of the second 2. These speeds will have a ratio of 4 to 2, not the same as the ratio between their weights, which is 8 to 6.

And from all that has been said here, it will not be difficult also to find the ratio in the case of bodies of different material moving in different media. For we examine first the ratio of the speeds of both bodies in the same medium. How this is to be done is clear from what has already been said. Then we consider what speed [the appropriate] one of the bodies has in the second medium (again with the help of what has been stated above): and we shall then have what is sought. For example, suppose there are two bodies, equal in size but unequal in weight, the weight of one being 12, and of the other 8, and we seek the ratio between the speed of the one whose weight is 12 sinking in water, and the speed of the one whose weight is 8 falling in air. Consider first how much faster the body weighing 12 sinks in water than the body weighing 8; then how much faster the body weighing 8 moves in air than in water, and we shall have what we are looking for. Or, alternatively, consider how much more swiftly the body weighing 12 falls in air than the body weighing 8; and then how much more slowly the body weighing 12 moves in water than in air.

These, then, are the general rules governing the ratio of the speeds of [natural] motion of bodies made of the same or of different material, in the same medium or in different media, and moving upward or downward. But note that a great difficulty arises at this point, because those ratios will not be observable by one who makes the experiment. For if one takes two different bodies, which have such properties that the first should fall twice as fast as the second, and if one then lets them fall from a tower, the first will not reach the ground appreciably faster or twice as fast. Indeed, if an observation is made, the lighter body will, at the beginning of the motion, move ahead of the heavier and will be swifter. This is not the place to consider how these contradictory and, so to speak, unnatural accidents come about (for they are accidental). In fact, certain things must be considered first which have not yet been examined. For we must first consider why natural motion is slower at its beginning.

CHAPTER 9

IN WHICH ALL THAT WAS DEMONSTRATED ABOVE IS CONSIDERED IN PHYSICAL TERMS, AND BODIES MOVING NATURALLY ARE REDUCED TO THE WEIGHTS OF A BALANCE. When a person has discovered the truth about something and has established it with great effort, then, on viewing his discoveries more carefully, he often realizes that what he has taken such pains to find might have been perceived with the greatest ease. For truth has the property that it is not so deeply concealed as many have thought; indeed, its traces shine brightly in various places and there are many paths by which it is approached. Yet it often happens that we do not see what is quite near at hand and clear. And we have a clear example of this right before us. For everything that was demonstrated and explained above so laboriously is shown us by nature so openly and clearly that nothing could be plainer or more obvious.

That this may be clear to everyone, let us consider how and why bodies moving upward [in natural motion] move with a force measured by the amount by which the weight of a volume of the medium (through which motion takes place) equal to the volume of the moving body exceeds the weight of the body itself. Consider a piece of wood that rises in water and floats on the surface. Now it is clear that the wood moves upward with just as much force as is necessary to submerge it forcibly in the water. If, therefore, we can find how much force is necessary to hold the wood under the water, we shall have what we are looking for. But if the wood were not lighter than water, that is, if its weight were the same as the weight of a volume of water equal to the volume of the wood, it would, of course, remain submerged, and it would not rise above the surface of the water. Therefore a force equal to the amount by which the weight of the aforesaid volume of water exceeds the weight of the piece of wood is sufficient to submerge the piece of wood. That is, we have found how much weight is required to submerge the piece of wood. But it was just determined that the wood moves upward with a force equal to that required to submerge it. And the weight just now found is what is required to submerge it. Therefore the wood moves upward with a force measured by the amount by which the weight of a volume of water equal to the volume of the wood exceeds the weight of the wood. And this is what was sought.

We must deal with downward motion by like reasoning. Thus we ask with what force a lead sphere moves downward in water. Now it is clear, to begin with, that the lead sphere moves downward with as much force as would be required to draw it upward. But if the sphere were made of water instead of lead, no force would be necessary to draw it upward, or, more precisely, the very smallest of forces. Therefore a weight equal to the amount by which the lead sphere exceeds an aqueous sphere of the same size measures the resistance of the lead sphere to being drawn upward. But the lead sphere also moves downward with the same force with which its resists being drawn upward. Therefore the lead sphere moves downward with a force equal to the weight by which it exceeds the weight of an aqueous sphere [of the same size].

One can see this same thing in the weights of a scale. For if the weights are in balance, and an additional weight is added to one side, then that side moves down, not in consequence of its whole weight, but only by reason of the weight by which it exceeds the weight on the other side. That is the same as if we were to say that the weight on this side moves down with a force measured by the amount by which the weight on the other side is less than it. And, for the same reason, the weight on the other side will move up with a force measured by the amount by which the weight on the first side is greater than it.

From what was said in this and in the previous chapter, we have the general conclusion that in the case of bodies of different material, provided that they are equal in size, the ratio of the speeds of their [natural downward] motions is the same as the ratio of their weights — and not their weights as such, but the weights found by weighing them in the medium in which the motion is to take place. Consider, for example, two bodies, *a* and *b*, equal in size but not in weight. Let the weight in air of *a* be 8 and of *b* 6. The speeds [of the natural motion] of these bodies

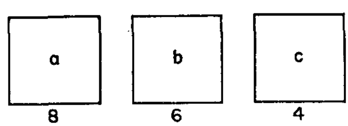

FIG. 11.

in water will not, as has been said before, have the ratio 8 to 6. For if we take a volume of water *c* equal to the volume of the bodies, and its weight is 4, the speed of body *a* will be represented by 4, and the speed of *b* by 2. And these speeds are in the ratio of 2 to 1, not in the ratio of 4 to 3, the ratio of the weights of the bodies in air. Yet the weights of these same bodies in water will also be in the ratio of 2 to 1; for the weight of *a* in water would only be 4.

This can be made clear as follows. If the weight of *a* in air were 4, it would be zero in water. For *a* would then be of the same weight as water, since 4 was assumed to be the weight in air of a volume of water *c* equal to the volume of *a*. But the weight of *c* in water would be zero, for it would move neither upward nor downward. Therefore the weight of *a* in water would be zero, if it were 4 in air. But because it is 8 in air, it will be 4 in water; and, by the same reasoning, the weight of *b* in water would be 2. Therefore their weights would be in the ratio of 2 to 1, as are also the speeds of their motions. And one must deal with lightness by a similar argument.

Now we conclude that, given the weights of two bodies in air, their weights in water can be found immediately. For having subtracted from each the weight of a volume of water equal to the volume of the solid bodies, we shall have as remainders the weights of these bodies in water. And similarly with other media.

Now from what has been said it should be clear to everyone that we do not have for any object its own proper weight. For if two objects are weighed, let us say, in water, who can say that the weights which we then obtain are the true weights of these objects, when, if these same objects are weighed in air, the weights will prove to be different from those [found in water] and will have a different ratio to each other? And if these objects could again be weighed in still another medium, e.g., fire, the weights would once more be different, and would have a different ratio to each other. And in this way the weights will always vary, along with the differences of the media. But if the objects could be weighed in a void, then we surely would find their exact weights, when no weight of the medium would diminish the weight of the objects. However, since the Peripatetics, following their leader, have said that in a void no motions could take place, and that therefore all things would be equally heavy, perhaps it will not be inappropriate to examine this opinion and to consider its foundations and its proofs. For this problem is one of the things that have to do with motion.

CHAPTER 10

IN WHICH, IN OPPOSITION TO ARISTOTLE, IT IS PROVED THAT, IF THERE WERE A VOID, MOTION IN IT WOULD NOT TAKE PLACE INSTANTANEOUSLY, BUT IN TIME. Aristotle, in Book 4 of the *Physics*, in his attempt to deny the existence of a void adduces many arguments. Those that are found beginning with section 64 are drawn from a consideration of motion. For since he assumes that motion cannot take place instantaneously, he tries to show that if a void existed, motion in it would take place instantaneously; and, since that is impossible, he concludes necessarily that a void is also impossible. But, since we are dealing with motion, we have decided to inquire whether it is true that, if a void existed, motion in it would take place instantaneously. And since our conclusion will be that motion in a void takes place in time, we shall first examine the contrary view and Aristotle's arguments.

In the first place, of the arguments adduced by Aristotle there is none that involves a necessary conclusion, but there is one which, at first sight, seems to lead to such a conclusion. This is the argument set forth in sections 71 and 72, in which Aristotle deduces the following inconsistency — that, on the assumption that motion can take place in time in a void, then the same body will move in the same time in a plenum and in a void. In order to be better able to refute this argument, we have decided to state it at this point.

Thus, Aristotle's first assumption, when he saw that the same body moved more swiftly through the rarer than through the denser medium, was this: that the ratio of the speed of motion in one medium to the speed in the second medium is equal to the ratio of the rareness of the first medium to the rareness of the second. He then reasoned as follows. Suppose body *a* traverses medium *b* in time *c*, and that it traverses a medium rarer than *b*, namely *d*, in time *e*. Clearly, the ratio of time *c* to time *e* is equal to the ratio of the density of *b* to the density of *d*. Suppose, then, that there is a void *f* and that body *a* traverses *f*, if it is possible, not in an instant, but in time *g*. And suppose that the ratio of the density of medium *d* to the density of some new medium is equal to the ratio of time *e* to time *g*. Then, from what has been established, body *a* will move through the new medium in time *g*, since [the density of] medium *d* has to that of the new medium

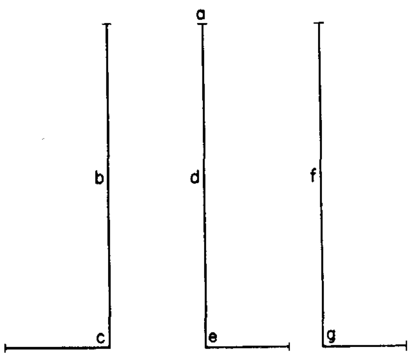

FIG. 12.

the same ratio as time *e* to time *g*. But in the same time *g* body *a* also moves through the void *f*. Therefore *a* will in the same time move over two equal paths, one a plenum, the other a void. But this is impossible. Therefore the body will not move through the void in time; and therefore the motion will be instantaneous.

Such is Aristotle's proof. And, indeed, his conclusions would have been sound and necessary, if he had proved his assumptions, or at least if these assumptions, even though unproved, had been true. But he was deceived in this, that he assumed as well-recognized axioms propositions which not only are not obvious to the senses, but have never been proved, and cannot be proved because they are completely false. For he assumed that the ratio of the speeds of the same body moving in different media is equal to the ratio of the rarenesses of the media. But that this is false has been fully proved above. In support of that proof, I shall add only this. Suppose it is true that the ratio of the rareness of air to the rareness of water is equal to the ratio of the speed of a body moving in air to the speed of the same body in water. Then, when a drop or some other quantity of water falls swiftly in air, but does not fall at all in water, since the speed in air has no ratio to the speed in water, it follows, according to Aristotle himself, that there will be no ratio between the rareness of air and the rareness of water. But that is ridiculous.

Therefore, it is clear that, when Aristotle argues in this way, we must answer him as follows. In the first place, as has been shown above, it is not true that differences in the slowness and speed of a given body arise from the greater or lesser density and rareness of the medium. But even if that were conceded, it is still not true that the ratio of the speeds of the motion of the body is equal to the ratio of the rarenesses of the media.

And as for Aristotle's statement in the same section that it is impossible for one number to have the same relation to another number as a number has to zero, this is, of course, true of geometric ratios [viz., a/b], and not merely in numbers but in every kind of quantity. Since, in the case of geometric ratios, it is necessarily true that the smaller magnitude can be added to itself a sufficient number of times so that it will ultimately exceed any magnitude whatever, it follows that this smaller magnitude is something, and not zero. For zero, no matter how often it is added to itself, will exceed no quantity. But Aristotle's conclusion does not apply to *arithmetic* relations [viz., the difference, $a-b$]. That is, in these cases, one number can have the same relation to another number as still another number has to zero. For, since [two pairs of] numbers are in the same arithmetic relation when the difference of the [two] larger is equal to the difference of the [two] smaller, it will, of course, be possible for one number to have the same [arithmetic] relation to another number, as still another number has to zero. Thus, we say that the [arithmetic] relation of 20 to 12 is the same as that of 8 to 0: for the excess of 20 over 12, i.e., 8, is equal to the excess of 8 over 0.

Therefore, if, as Aristotle held, the ratio of the speeds were equal to the ratio, in the geometric sense, of the rarenesses of the media, Aristotle's conclusion would have been valid, that motion in a void could not take place in time. For the ratio of the time in the plenum to the time in the void cannot be equal to the ratio of the rareness of the plenum to the rareness of the void, since the rareness of the void does not exist. But if the ratio of the speeds were made to depend on the aforesaid ratio, not in the geometric, but in the arithmetic sense [i.e., as a ratio of differences], no absurd conclusion would follow. And, in fact, the ratio of the speeds does depend, in an arithmetic sense, on the relation of the lightness of the first medium to that of the second. For the ratio of the speeds is equal, not to the ratio of the lightness of the first medium to that of the second, but, as has been proved, to the ratio of the excess of the weight of the body over the weight of the first medium to the excess of the weight of the body over the weight of the second medium.

So that this may be clearer, here is an example. Suppose there is a body *a* whose weight is 20, and two media unequal in weight,

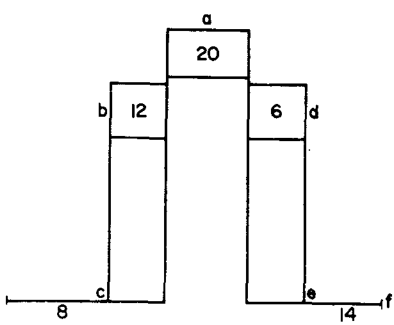

FIG. 13.

bc and *de*. Let the volume of *b* be equal to that of *a*, and the volume of *d* also equal to that of *a*. Since we are now discussing downward motion that takes place in a void, let the media be lighter than the body *a*, and let the weight of *b* be 12, and of *d* 6. It is clear, then, from what was proved above, that the ratio of the speed of body *a* in medium *bc* to the speed of the same body in medium *de* will be equal to the ratio of the excess of the weight of *a* over the weight of *b* to the excess of the weight of *a* over the weight of *d*, that is, as 8 is to 14. Thus if the speed of *a* in medium *bc* is 8, its speed in medium *de* would be 14. Now it is clear that the ratio of the speeds, 14 to 8, is not the same as the ratio (in the geometric sense) of the lightness of one medium to the lightness of the other. For the lightness of medium *de* is double that of medium *bc* (for since the weight of *b* is 12, and of *d* 6, i.e., since the weight of *b* is double the weight of *d*, the lightness of *d* will be double the lightness of *b*); but a speed of 14 is less than twice a speed of 8. Yet the speed 14 has to the speed 8 the same relation, in the arithmetic sense, as the lightness of *d* to the lightness of *b*, since the difference between 14 and 8 is 6, and 6 is also the difference between the lightness of *d* (12) and the lightness of *b* (6).

Furthermore, if medium *de* should be lighter, so that the weight of *d* is 5, the speed *f* will be 15 (for 15 will be the difference between the weight of body *a* and the weight of the medium *d*). And again the relation [i.e., arithmetic difference] of speed 15 and speed 8 will be the same as between the weight of medium *b* (12) and the weight of medium *d* (5), that is, the same as the relation of the lightness of *d* and the lightness of *b*. For the difference in each case will be 7. Furthermore, if the weight of *d* is only 4, the speed *f* will be 16: and the relation of speed 16 and speed 8 (with a difference of 8) is the same arithmetic relation as between the weight of *b* (12) and the weight of *d* (4), i.e., between the lightness of *d* and the lightness of *b*, the difference being also 8. If, again, medium *de* becomes lighter, and the weight of *d* is only 3, the speed *f* will now be 17. And between the speed *f* (17) and the speed 8 (a difference of 9), the difference is the same as between the weight of *b* (12) and the weight of *d* (3), i.e., as between the lightness of *d* and the lightness of *b*. If, again, medium *de* becomes lighter, and the weight of *d* is only 2, the speed *f* will now be 18.

And the arithmetic difference between that speed and the speed 8 will be the same as the difference between the weight of *b* (12) and the weight of *d* (2), i.e., between the lightness of *d* and the lightness of *b*. In each case the difference will be 10. If, again, medium *de* becomes lighter, and the weight of *d* is only 1, the speed *f* will now be 19. And there will be the same arithmetic difference between this speed and the speed 8 as between the weight of *b* (12) and the weight of *d* (1), i.e., between the lightness of *d* and the lightness of *b*. In each case the difference will be 11. Now if, finally, the weight of *d* is 0, so that the difference between the weight of body *a* and of the medium *d* is 20, the speed *f* will be 20; and the arithmetic difference between the speed *f* (20) and the speed 8 will be the same as that between the weight of *b* (12) and the weight of *d* (0), the difference in each case being 12.

It is clear, therefore, that the relation of speed to speed is the same as the relation of the lightness of one medium to the lightness of the other, not geometrically [i.e., as a quotient] but arithmetically [i.e., as a difference]. And since it is not absurd for this arithmetic relation [i.e., difference] to be the same between one quantity and a second quantity as between a third quantity and zero, it will similarly not be absurd for the relation of speed to speed to be the same, in this arithmetic sense, as the relation of a given lightness [of medium] to zero.

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

From this it can clearly be understood that in a plenum, such as that which surrounds us, things do not weigh their proper and natural weight, but they will always be lighter to the extent that they are in a heavier medium. Indeed, a body will be lighter by an amount equal to the weight, in a void, of a volume of the medium equal to the volume of the body. Thus, a lead sphere will be lighter in water than in a void by an amount equal to the weight, in a void, of an aqueous sphere of the same size as the lead sphere. And the lead sphere is lighter in air than in a void by an amount equal to the weight, in a void, of a sphere of air having the same size as the lead sphere. And so also in fire, and in other media. And since the speed of a body's motion depends on the weight the body has in the medium in which it moves, its motion will be swifter, the heavier the body is in relation to the various media.

But the following argument is invalid: "A void is a medium infinitely lighter than every plenum; therefore motion in it will be infinitely swifter than in a plenum; therefore such motion will be instantaneous." For it is true that a void is infinitely lighter than any [nonvacuous] medium; but we must not say that such a [nonvacuous] medium is of infinite weight. We must instead understand [the applicability of the term "infinite"] in this way, that between the lightness of air, for example, and a void there may exist an unlimited number of media lighter than air and heavier than a void. And if we understand the matter in this way, there may also exist, between the speed in air and the speed in a void, an unlimited number of speeds, greater than the speed in air and less than the speed in a void. And so also between the weight of a body in air and its weight in a void, an unlimited number of intermediate weights may exist, greater than the weight of the body in air, but less than its weight in a void.

And the same is true of every continuum. Thus between lines *a* and *b*, of which *a* is greater, an unlimited number of intermediate lines, smaller than *a*, but greater than *b* may exist (for since the amount by which *a* exceeds *b* is also a line, it will be infinitely divisible). But we must not say that line *a* is infinitely greater than line *b*, in the sense that even if *b* were to be added to itself without limit, it would not produce a line greater than *a*. And by similar reasoning, if we suppose *a* to be the speed in a void, and *b* the speed in air, an unlimited number of speeds, greater than *b* and smaller than *a*, will be able to exist between

FIG. 14.

a and *b*. Yet we must not conclude that *a* is infinitely greater than *b*, in the sense that the time in which [the motion with] speed *a* is accomplished, when added to itself any number of times without limit, can still never exceed the time corresponding to speed *b*, and that, therefore, the speed corresponding to time *a* is instantaneous.

It is therefore clear how the argument is to be understood. "The lightness of a void infinitely exceeds the lightness of a [non-vacuous] medium; therefore the speed in the void will infinitely exceed the speed in a plenum." All that is conceded. What is denied is the conclusion: "Therefore the speed [i.e., the motion] in the void will be instantaneous." For such motion can take place in time, but in a shorter time than the time corresponding to the speed in a plenum; so that between the time in the plenum and the time in the void an unlimited number of times, greater than the latter and smaller than the former, may exist. Hence it follows, not that motion in a void is instantaneous, but that it takes place in less time than the time of motion in any plenum.

Therefore, to put it briefly, my whole point is this. Suppose there is a heavy body *a*, whose proper and natural weight is 1000. Its weight in any plenum whatever will be less than 1000, and therefore the speed of its motion in any plenum will be less than

1000. Thus if we assume a medium such that the weight of a volume of it equal to the volume of *a* is only 1, then the weight of *a* in this medium will be 999. Therefore its speed too will be 999.

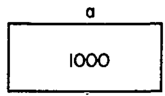

FIG. 15.

And the speed of *a* will be 1000 only in a medium in which its weight is 1000, and that will be nowhere except in a void.

This is the refutation of Aristotle's argument. And from this refutation it can readily be seen that motion in a void does not have to be instantaneous.

CHAPTER 10

WEIGHING AIR

A LITER of air weighs about 1.3 g, which is small in comparison with that of an ordinary container so that any accurate determination of its density even today requires considerable care. What is more, the buoyant force in the atmosphere is the same amount. Therefore, one has to produce an increment of weight of air either by compressing more into a container or by partially evacuating it. Galileo did the former; he used sand to adjust the balance finely and then determined the initial volume of the compressed air. In this way he found water to be about 400 times (actually about 767) heavier than air, in comparison with Aristotle's estimate of 10.

In this connection, Galileo discussed the general problem of determining the weight of a body in a vacuum by simply weighing it in air.

Dialogues Concerning Two New Sciences †

As to the other question, namely, how to determine the specific gravity of air, I have employed the following method. I took a rather large glass bottle with a narrow neck and attached to it a leather cover, binding it tightly about the neck of the bottle: in the top of this cover I inserted and firmly fastened the valve of a leather bottle, through which I forced into the glass bottle, by means of a syringe, a large quantity of air. And since air is easily condensed one can pump into the bottle two or three times its own volume of air. After this I took an accurate balance and weighed this bottle of compressed air with the utmost precision, adjusting the weight with fine sand. I next opened the valve and allowed the compressed air to escape; then replaced the flask upon the balance and found it perceptibly lighter: from the sand which had been used as a counterweight I now removed and laid aside as much as was necessary to again secure balance. Under

† Ref. (9), pp. 79–82.

these conditions there can be no doubt but that the weight of the sand thus laid aside represents the weight of the air which had been forced into the flask and had afterwards escaped. But after all this experiment tells me merely that the weight of the compressed air is the same as that of the sand removed from the balance; when however it comes to knowing certainly and definitely the weight of air as compared with that of water or any other heavy substance this I cannot hope to do without first measuring the volume [*quantità*] of compressed air; for this measurement I have devised the two following methods.

According to the first method one takes a bottle with a narrow neck similar to the previous one; over the mouth of this bottle is slipped a leather tube which is bound tightly about the neck of the flask; the other end of this tube embraces the valve attached to the first flask and is tightly bound about it. This second flask is provided with a hole in the bottom through which an iron rod can be placed so as to open, at will, the valve above mentioned and thus permit the surplus air of the first to escape after it has once been weighed: but this second bottle must be filled with water. Having prepared everything in the manner above described, open the valve with the rod; the air will rush into the flask containing the water and will drive it through the hole at the bottom, it being clear that the volume [*quanttià*] of water thus displaced is equal to the volume [*mole e quanttià*] of air escaped from the other vessel. Having set aside this displaced water, weigh the vessel from which the air has escaped (which is supposed to have been weighed previously while containing the compressed air), and remove the surplus of sand as described above; it is then manifest that the weight of this sand is precisely the weight of a volume [*mole*] of air equal to the volume of water displaced and set aside; this water we can weigh and find how many times its weight contains the weight of the removed sand, thus determining definitely how many times heavier water is than air; and we shall find, contrary to the opinion of Aristotle, that this is not 10 times, but, as our experiment shows, more nearly 400 times.

The second method is more expeditious and can be carried out with a single vessel fitted up as the first was. Here no air is added to that which the vessel naturally contains but water is forced into it without allowing any air to escape; the water thus introduced necessarily compresses the air. Having forced into the vessel as much water as possible, filling it, say, three-fourths full, which does not require any extraordinary effort, place it upon the balance and weight it accurately; next hold the vessel mouth up, open the valve, and allow the air to escape; the volume of the air thus escaping is precisely equal to the volume of water contained in the flask. Again weigh the vessel which will have diminished in weight on account of the escaped air; this loss in weight represents the weight of a volume of air equal to the volume of water contained in the vessel.

SIMP. No one can deny the cleverness and ingenuity of your devices; but while they appear to give complete intellectual satisfaction they confuse me in another direction. For since it is undoubtedly true that the elements when in their proper places have neither weight nor levity, I cannot understand how it is possible for that portion of air, which appeared to weigh, say, 4 drachms of sand, should really have such a weight in air as the sand which counterbalances it. It seems to me, therefore, that the experiment should be carried out, not in air, but in a medium in which the air could exhibit its property of weight if such it really has.

SALV. The objection of Simplicio is certainly to the point and must therefore either be unanswerable or demand an equally clear solution. It is perfectly evident that that air which, under compression, weighed as much as the sand, loses this weight when once allowed to escape into its own element, while, indeed, the sand retains its weight. Hence for this experiment it becomes necessary to select a place where air as well as sand can gravitate; because, as has been often remarked, the medium diminishes the weight of any substance immersed in it by an amount equal to the weight of the displaced medium; so that air in air loses all its weight. If therefore this experiment is to be made with accuracy it should be performed in a vacuum where every heavy body exhibits its momentum without the slightest diminution. If then, Simplicio, we were to weigh a portion of air in a vacuum would you then be satisfied and assured of the fact?

SIMP. Yes truly: but this is to wish or ask the impossible.

SALV. Your obligation will then be very great if, for your sake, I accomplish the impossible. But I do not want to sell you something which I have already given you; for in the previous experiment we weighed the air in vacuum and not in air or other medium. The fact that any fluid medium diminishes the weight of a mass immersed in it, is due, Simplicio, to the resistance which this medium offers to its being opened up, driven aside, and finally lifted up. The evidence for this is seen in the readiness with which the fluid rushes to fill up any space formerly occupied by the mass; if the medium were not affected by such an immersion then it would not react against the immersed body. Tell me now, when you have a flask, in air, filled with its natural amount of air and then proceed to pump into the vessel more air, does this extra charge in any way separate or divide or change the circumambient air? Does the vessel perhaps expand so that the surrounding medium is displaced in order to give more room? Certainly not. Therefore one is able to say that this extra charge of air is not immersed in the surrounding medium for it occupies no space in it, but is, as it were, in a vacuum. Indeed, it is really in a vacuum; for it diffuses into the vacuities which are not completely filled by the original and uncondensed air. In fact I do not see any difference between the enclosed and the surrounding media: for the surrounding medium does not press upon the enclosed medium and, *vice versa*, the enclosed medium exerts no pressure against the surrounding one; this same relationship exists in the case of any matter in a vacuum, as well as in the case of the extra charge of air compressed into the flask. The weight of this condensed air is therefore the same as that which it would have if set free in a vacuum. It is true of course that the weight of the sand used as a counterpoise would be a little greater *in vacuo* than in free air. We must, then, say that the air is slightly lighter than the sand required to counterbalance it, that is to say, by an amount equal to the weight *in vacuo* of a volume of air equal to the volume of the sand.

CHAPTER 11

FLOATING EBONY

ACCORDING to Archimedes' principle the buoyant force F on a body immersed in a fluid, i.e. its apparent loss of weight, is equal to the weight of the displaced fluid. Thus

$$\text{Buoyant force} = (d_F\, V_F)g,$$

where d_F and V_F are, respectively, the density and volume of the displaced fluid, and g is the acceleration due to gravity. Now the weight of a body is

$$\text{Weight} = (DV)g,$$

where D and V are, respectively, its own density and volume. Consider the unbalanced force, i.e. the difference between the weight of the body downward and the buoyant force on it upward, i.e.

$$\text{Weight}-\text{Buoyant force} = (DV-d_FV_F)g.$$

If the body is completely *submerged*, then $V=V_F$. Hence

$$\text{Weight}-\text{Buoyant force} = (D-d_F)Vg.$$

The weight, accordingly, will be greater or less than the buoyant force as D is greater or less than d_F. In other words, a more dense body will sink in such a fluid. If $D = d_F$, e.g. the same fluid, then the submerged body will remain at rest; as Galileo remarked, "Water hath no gravity in water". If D is less than D_F, then the body will rise until the displaced volume V_F is such that $DV-d_FV_F = 0$, the body will *float*. In this case, the resulting volumes are inversely proportional to the densities. Galileo expressed continually his amazement that a body can float in a volume of liquid even less than its own volume.

What is the effect of a body's shape? An empty brass *kettle*, for example, will float in water — despite the fact that brass is denser than water. As Galileo pointed out, in this instance, the floating body is not just brass, but virtually a combination of brass and the enclosed air.

A more puzzling effect is the floating of a *chip of solid ebony*, which is slightly denser than water, but which contains no air. Galileo did not correctly solve this problem. He did observe, however, that in such cases the liquid does not wet the body. If the same ebony is wet all over, it sinks. (His descriptive drawings were quite accurate.) It was not until the sixteenth century that surface tension was finally recognized as the missing physical factor.

Discourse on Bodies in Water†

Moreover, it seemed to me convenient to inform your Highness of all the sequel, concerning the controversy of which I treat, as it hath been advertised often already by others: and because the doctrine which I follow, in the discussion of the point in hand, is different from that of Aristotle; and interferes with his principles, I have considered that against the authority of that most famous man, which amongst many makes all suspected that comes not from the schools of the Peripatetics, it is far better to give one's reasons by the pen than by word of mouth, and therefore I resolved to write the present discourse: in which yet I hope to demonstrate that it was not out of capriciousness, or for that I had not read or understood Aristotle, that I sometimes swerve from his opinion, but because several reasons persuade me to it, and the same Aristotle hath taught me to fix my judgement on that which is grounded upon reason, and not on the bare authority of the master; and it is most certain according to the sentence of Alcinoos, that philosophating should be free. Nor is the resolution of our question in my judgement without some benefit to the universal, forasmuch as treating whether the figure of solids operates, or not, in their going, or not going to the bottom in water, in occurrences of building bridges or other fabrics on the water, which happen commonly in affairs of grand import, it may be of great avail to know the truth.

I say therefore, that being the last summer in company with certain learned men, it was said in the argumentation; that condensation was the propriety of cold, and there was alledged for instance, the example of ice: now I at that time said, that, in my judgement, the ice should be rather water rarified than condensed, and my reason was, because condensation begets diminution of mass, and augmentation of gravity, and rarification causeth greater lightness, and augmentation of mass: and water in freezing, increaseth in mass, and the ice made thereby is lighter than the water on which it swimmeth.

† Ref. (5), pp. 3–5, 22, 26–30.

What I say, is manifest, because, the medium subtracting from the whole gravity of solids the weight of such another mass of the said medium; as Archimedes proves in his first book De Insidentibus Humido; *whenever the mass of the said solid increaseth by distraction, the more shall the* medium *detract from its entire gravity; and less, when by compression it shall be condensed and reduced to a less mass.*

It was answered me, that that proceeded not from the greater levity, but from the figure, large and flat, which not being able to penetrate the resistance of the water, is the cause that it submergeth not. I replied, that any piece of ice, of whatsoever figure, swims upon the water, a manifest sign, that its being never so flat and broad, hath not any part in its floating: and added, that it was a manifest proof hereof to see a piece of ice of very broad figure being thrust to the bottom of the water, suddenly return to float on top, which had it been more grave, and had its swimming proceeded from its form, unable to penetrate the resistance of the *medium*, that would be altogether impossible; I concluded therefore, that the figure was in sort a cause of the natation or submersion of bodies, but the greater or less gravity in respect of the water: and therefore all bodies heavier than it of what figure soever they be, indifferently go to the bottom, and the lighter, though of any figure, float indifferently on the top: and I suppose that those which hold otherwise, were induced to that belief, by seeing how that diversity of forms or figures, greatly altereth the velocity, and tardity of motion; so that bodies of figure broad and thin, descend far more leisurely into the water, than those of a more compacted figure, though both made of the same matter: by which some might be induced to believe that the dilation of the figure might reduce it to such ampleness that it should not only retard by wholly impede and take away the motion, which I hold to be false. Upon this conclusion, in many days' discourse, was spoken much, and many things, and divers experiments produced, of which your Highness heard, and saw some, and in this discourse shall have all that which hath been produced against my assertion, and what hath been suggested to my thoughts on this matter, and for confirmation of my conclusion: which it shall suffice to remove that (as I esteem hitherto false) opinion, I shall think I have not unprofitably spent my pains and time, and although that come not to pass, yet ought I to promise another benefit to myself, namely, of attaining the knowledge of the truth, by hearing my fallacies confuted, and true demonstrations produced by those of the contrary opinion.

And to proceed with the greatest plainness and perspicuity that I can possible, it is, I conceive, necessary, first of all to declare what is the true, intrinsic, and total cause, of the ascending of some solid bodies in the water, and therein floating; or on the contrary, of their sinking and so much the rather in asmuch as I cannot satisfy myself in that which Aristotle hath left written on this subject.

I say then the cause why some solid bodies descend to the bottom of water, is the excess of their gravity, above the gravity of the water; and on the contrary, the excess of the water's gravity above the gravity of those, is the cause that others do not descend, rather that they rise from the bottom, and ascend to the surface. This was subtlely demonstrated by Archimedes in his book of the *Natation of Bodies*: conferred afterwards by a very grave author, but, if I err not invisibly, as below for defence of him, I shall endeavour to prove.

I, with a different method, and by other means, will endeavour to demonstrate the same, reducing the causes of such effects to more intrinsic and immediate principles, in which also are discovered the causes of some admirable and almost incredible accidents, as that would be, that a very little quantity of water, should be able, with its small weight, to raise and sustain a solid body, a hundred or a thousand times heavier than it.

And because demonstrative order so requires, I shall define certain terms, and afterwards explain some propositions, of which, as of things true and obvious, I may make use of to my present purpose.

Definition I

I then call equally grave in specie, *those matters of which equal masses weigh equally.*

As if for example, two balls, one of wax, and the other of some wood of equal mass, were also equal in weight, we say, that such wood, and the wax are *in specie* equally grave.

Definition II

But equally grave in absolute gravity, we call two solids, weighing equally, though of mass they be unequal.

As for example, a mass of lead, and another of wood, that weigh each ten pounds, I call equal in absolute gravity, though the mass of the wood be much greater than that of the lead.

And, consequently, less grave in specie.

Definition III

I call a matter more grave in specie *than another, of which a mass, equal to a mass of the other, shall weigh more.*

. . . a piece of wood which by its nature sinks not in water, shall not sink though it be turned and converted into the form of any vessel whatsoever, and then filled with water: and he that would readily see the experiment in some other tractable matter, and that is easily reduced into several figures, may take pure wax, and making it first into a ball or other solid figure, let him add to it so much lead as shall just carry it to the bottom, so that being a grain less it could not be able to sink it, and making it afterwards into the form of a dish, and filling it with water, he shall find that without the said lead it shall not sink, and that with the lead it shall descend with much slowness: and in short he shall satisfy himself, that the water included makes no alteration. I say not all this while, but that it is possible of wood to make barks, which being filled with water, sink; but that proceeds not through its gravity, increased by the water, but rather from the nails and other iron works, so that it no longer hath a body less grave than water, but one mixed of iron and wood, more grave than a like mass of water. Therefore let Signor Buonamico desist from desiring a reason of an effect, that is not in nature: yea if the sinking of the wooden vessel when it is full of water, may call in question the doctrine of Archimedes, which he would not have you to follow, is on the contrary consonant and agreeable to the doctrine of the Peripatetics, since it aptly assigns a reason why such a vessel must, when it is full of water, descend to the bottom; converting the argument the other way, we may with safety say that the doctrine of Archimedes is true, since it aptly agreeth with true experiments, and question the other, whose deductions are fastened upon erroneous conclusions. As for the other point hinted in this same instance, where it seems that Buonamico understands the same not only of a piece of wood, shaped in the form of a vessel, but also of massy wood, which filled, *scilicet*, as I believe, he would say, soaked and steeped in water, goes finally to the bottom that happens in some porous woods, which, while their porosity is replenished with air, or other matter less grave than water, are masses specifically less grave than the said water, like as is that vial of glass whilst it is full of air: but when, such light matter departing, there succeedeth water into the same porosities and cavities, there results a compound of water and glass more grave than a like mass of water: but the excess of its gravity consists in the matter of the glass, and not in the water, which cannot be graver than itself: so that which remains of the wood, the air of its cavities departing, if it shall be more grave *in specie* than water, fill but its porosities with water, and you shall have a compost of water and wood more grave than water, but not by virtue of the water received into and imbibed by the porosities, but of that matter of the wood which remains when the air is departed: and being such it shall, according to the doctrine of Archimedes, go to the bottom, like as before, according to the same doctrine it did swim.

As to that finally which presents itself in the fourth place, namely, that the ancients have been heretofore confuted by Aristotle, who denying positive and absolute levity, and truly esteeming all bodies to be grave, said, that that which moved upward was driven by the circumambient air, and therefore that also the doctrine of Archimedes, as an adherent to such an opinion was convicted and confuted: I answer first, that Signor

Buonamico in my judgement hath imposed upon Archimedes, and deduced from his words more than ever he intended by them, or may from his propositions be collected, in regard that Archimedes neither denies, nor admitteth positive levity, nor doth he so much as mention it: so that much less ought Buonamico to infer, that he hath denied that it might be the cause and principle of the ascension of fire, and other light bodies: having but only demonstrated, that solid bodies more grave than water descend in it, according to the excess of their gravity above the gravity of that, he demonstrates likewise, how the less grave ascend in the same water, according to its excess of gravity, above the gravity of them. So that the most that can be gathered from the demonstration of Archimedes is, that like as the excess of the gravity of the moveable above the gravity of the water, is the cause that it descends therein, so the excess of the gravity of the water above that of the moveable, is a sufficient cause why it descends not, but rather betakes itself to swim: not enquiring whether of moving upwards there is, or is not any other cause contrary to gravity: nor doth Archimedes discourse less properly than if one should say: If the South Wind shall assault the bark with greater *impetus* than is the violence with which the stream of the river carries it towards the South, the motion of it shall be towards the North: but if the *impetus* of the water shall overcome that of the wind, its motion shall be towards the South. The discourse is excellent and would be unworthily contradicted by such as should oppose it, saying: Thou mis-alledgest as cause of the motion of the bark towards the South, the *impetus* of the stream of the water above that of the South Wind; mis-alledgest I say, for it is the force of the North Wind opposite to the South, that is able to drive the bark towards the South.

Theorem V

The diversity of figures given to this or that solid, cannot any way be a cause of its absolute sinking or swimming.

So that if a solid being formed, for example, into a spherical figure, doth sink or swim in the water, I say, that being formed into any other figure, the same figure in the same water, shall sink or swim: nor can such its motion by the expansion or by other mutation of figure, be impeded or taken away.

The expansion of the figure may indeed retard its velocity, as well of ascent as descent, and more and more according as the said figure is reduced to a greater breadth and thinness: but that it may be reduced to such a form as that that same matter be wholly hindered from moving in the same water, that I hold to be impossible. In this I have met with great contradictors, who producing some experiments, and in particular a thin board of ebony, and a ball of the same wood, and showing how the ball in water descended to the bottom, and the board being put lightly upon the water submerged not, but rested; have held, and with the authority of Aristotle, confirmed themselves in their opinions, that the cause of that rest was the breadth of the figure, unable by its small weight to pierce and penetrate the resistance of the water's crassitude, which resistance is readily overcome by the other spherical figure.

This is the principal point in the present question, in which I persuade myself to be on the right side.

Therefore, beginning to investigate with the examination of exquisite experiments that really the figure doth not a jot alter the descent or ascent of the same solids, and having already demonstrated that the greater or less gravity of the solid in relation to the gravity of the *medium* is the cause of descent or ascent: whenever we would make proof of that, which about this effect the diversity of figure worketh, it is necessary to make the experiment with matter wherein variety of gravities hath no place. For making use of matters which may be different in their specific gravities, and meeting with varieties of effects of ascending and descending, we shall always be left unsatisfied whether that diversity derive itself really from the sole figure, or else from the divers gravity also. We may remedy this by taking one only matter, that is tractable and easily reduceable into every sort of figure. Moreover, it will be an excellent expedient to take a kind of matter, exactly alike in gravity unto the water: for that matter, as far as pertains to the gravity, is indifferent either to ascend or descend; so that we may presently observe any the least difference that derives itself from the diversity of figure.

Now to do this, wax is most apt, which, besides its incapacity of receiving any sensible alteration from its imbibing of water, is ductile or pliant, and the same piece is easily reduceable into all figures: and being *in specie* a very inconsiderable matter inferior in gravity to the water, by mixing therewith a little of the filings of lead it is reduced to a gravity exactly equal to that of the water.

This matter prepared, and, for example, a ball being made thereof as big as an orange or bigger, and that made so grave as to sink to the bottom, but so lightly, that taking thence one only grain of lead, it returns to the top, and being added, it submergeth to the bottom, let the same wax afterwards be made into a very broad and thin flake or cake; and then, returning to make the same experiment, you shall see that it being put to the bottom, it shall, with the grain of lead rest below, and that grain deducted, it shall ascend to the very surface, and added again it shall dive to the bottom. And this same effect shall happen always in all sort of figures, as well regular as irregular: nor shall you ever find any that will swim without the removal of the grain of lead, or sink to the bottom unless it be added: and, in short, about the going or not going to the bottom, you shall discover no diversity, although, indeed, you shall about the quick and slow descent: for the more expatiated and distended figures move more slowly as well in the diving to the bottom as in the rising to the top; and the other more contracted and compact figures, more speedily. Now I know not what may be expected from the diversity of figures, if the most contrary to one another operate not so much as doth a very small grain of lead, added or removed.

Methinks I hear some of the adversaries to raise a doubt upon my produced experiment. And first, that they offer to my consideration, that the figure, as a figure simply, and disjunct from the matter works not any effect, but requires to be conjoined with the matter; and, furthermore, not with every matter, but with those only, wherewith it may be able to execute the desired operation. Like as we see it verified by experience, that the acute and sharp angle is more apt to cut, than the obtuse; yet always provided, that both the one and the other, be joined with a matter apt to cut, as for example, with steel. Therefore, a knife with a fine and sharp edge, cuts bread or wood with much ease, which it will not do, if the edge be blunt and thick: but he that will instead of steel, take wax, and mould it into a knife, undoubtedly shall never know the effects of sharp and blunt edges: because neither of them will cut, the wax being unable by reason of its flexibility, to overcome the hardness of the wood and bread. And, therefore, applying the like discourse to our purpose, they say, that the difference of figure will show different effects, touching natation and submersion, but not conjoined with any kind of matter, but only with those matters which, by their gravity, are apt to resist the velocity of the water, whence he that would elect for the matter, cork or other light wood, unable, through its levity, to superate the crassitude of the water, and of that matter should form solids of divers figures, would in vain seek to find out what operation figure hath in natation or submersion; because all would swim, and that not through any property of this or that figure, but through the debility of the matter, wanting so much gravity, as is requisite to superate and overcome the density and crassitude of the water.

It is needful, therefore, if we would see the effect wrought by the diversity of figure, first to make choice of a matter of its nature apt to penetrate the crassitude of the water. And, for this effect, they have made choice of such a matter, as fit, that being readily reduced into spherical figure, goes to the bottom; and it is ebony, of which they afterwards making a small board or splinter, as thin as a lathe, have illustrated how that this, put upon the surface of the water, rests there without descending to the bottom: and making, on the other side, of the same wood a ball, no less than a hazel nut, they show, that this swims not, but descends. From which experiment, they think they may frankly conclude, that the breadth of the figure in the flat lathe or board, is the cause of its not descending to the bottom, forasmuch as a ball of the same matter, not different from the board in anything but in figure, submergeth in the same water to the bottom. The discourse and the experiment hath really so much of probability and likelihood of truth in it, that it would be no wonder, if many persuaded by a certain cursory observation, should yield credit to it; nevertheless, I think I am able to discover, how that it is not free from fallacy.

Beginning, therefore, to examine one by one, all the particulars that have been produced, I say, that figures, as simple figures, not only operate not in natural things, but neither are they ever separated from the corporeal substance: nor have I ever alledged them stripped of sensible matter, like as also I freely admit, that in our endeavouring to examine the diversity of accidents, dependent upon the variety of figures, it is necessary to apply them to matters, which obstruct not the various operations of those various figures: and I admit and grant, that I should do very ill, if I would experiment the influence of acuteness of edge with a knife of wax, applying it to cut an oak, because there is no acuteness in wax able to cut that very hard wood. But yet such an experiment of this knife, would not be besides the purpose, to cut curded milk, or other very yielding matter: yea, in such like matters, the wax is more commodious than steel; for finding the diversity depending upon angles, more or less acute, for that milk is indifferently cut with a razor, and with a knife, that hath a blunt edge. It needs, therefore, that regard be had, not only to the hardness, solidity or gravity of bodies, which under divers figures, are to divide and penetrate some matters, but it forceth also, that regard be had, on the other side, to the resistance of the matters, to be divided and penetrated. But since I have in making the experiment concerning our contest, chosen a matter which penetrates the resistance of the water; and in all figures descends to the bottom, the adversaries can charge me with no defect; yea, I have propounded so much a more excellent method than they, in as much as I have removed all other causes, of descending or not descending to the bottom, and retained the only sole and pure variety of figures, demonstrating that the same figures all descend with the only alteration of a grain in weight: which grain being removed, they return to float and swim; it is not true, therefore, (resuming the example by them introduced) that I have gone about to experiment the efficacy of acuteness, in cutting with matters unable to cut, but with matters proportioned to our occasion; since they are subjected to no other variety, than that alone which depends on the figure more or less acute.

But let us proceed a little farther, and observe, how that indeed the consideration, which, they say, ought to be had about the election of the matter, to the end, that it may be proportionate for the making of our experiment, is needlessly introduced, declaring by the example of cutting, that like as acuteness is insufficient to cut, unless when it is in a matter hard and apt to superate the resistance of the wood or other matter, which we intend to cut; so the aptitude of descending or not descending in water, ought and can only be known in those matters, that are able to overcome the renitence, and superate the crassitude of the water. Unto which, I say, that to make distinction and election, more of this than of that matter, on which to impress the figures for cutting or penetrating this or that body, as the solidity or obdurateness of the said bodies shall be greater or less, is very necessary: but withall I subjoin, that such distinction, election and caution would be superfluous and unprofitable, if the body to be cut or penetrated, should have no resistance, or should not at all withstand the cutting or penetration: and if the knife were to be used in cutting a mist or smoke, one of paper would be equally serviceable with one of *Damascus* steel: and so by reason the water hath not any resistance against the penetration of any solid body, all choice of matter is superfluous and needless, and the election which I said above to have been well made of a matter reciprocal in gravity to water, was not because it was necessary, for the overcoming of the crassitude of the water, but its gravity, with which only it resists the sinking of solid bodies: and for what concerneth the resistance of the crassitude, if we narrowly consider it, we shall find that all solid bodies, as well those that sink, as those that swim, are indifferently accomodated and apt to bring us to the knowledge of the truth in question. Nor will I be frightened out of the belief of these conclusions, by the experiments which may be produced against me, of many several woods, corks, galls, and, moreover, of subtle slates and plates of all sorts of stone and metal, apt by means of their natural gravity, to move towards the center of the earth, the which, nevertheless, being impotent, either through the figure (as the adversaries think) or through levity, to break and penetrate the continuity of the parts of the water, and to distract its union, do continue to swim without submerging in the least: nor on the other side, shall the authority of Aristotle move me, who in more than one place, affirmeth the contrary to this, which experience shows me.

CHAPTER 12

ANALYZING AN ALLOY

HIERO, King of Syracuse, asked his science adviser, Archimedes, how to ascertain if a crown made by the royal goldsmith contained only the royal gold or also some cheaper silver. No reliable account exists as to just how Archimedes actually solved this problem. In thinking about it, however, the 22-year old Galileo devised a balance utilizing the concept of specific gravity, the law of the lever, and the principle of buoyancy. (Archimedes had invented the first, attempted a proof of the second, and discovered the last.) Galileo's paper was written in Italian for popular consumption.

We shall consider this method from a modern point of view. Let the weight of a body A in air be A_a and its apparent weight in water be A_w. If the body is balanced in air with a given counterpoise C (cf. Galileo's own figure below), the *law of the lever* requires that

$$A_w \,.\, \overline{ca} = C \,.\, \overline{cb} \,,$$

where $\overline{ca}$ and $\overline{cb}$ are the lever arms of the body in air and of the counterpoise, respectively. Likewise, for the same counterpoise, but body balanced in water,

$$A_w \,.\, \overline{cg} = C \,.\, \overline{cb} \,,$$

where $\overline{cg}$ is the new lever arm of the body in this instance. Dividing these two expressions, we obtain

$$\frac{A_a}{A_w} = \frac{\overline{cg}}{\overline{ca}} \,.$$

The *specific gravity* SG of a body, by definition, is the ratio of its weight in air to that of an equal volume of water, which, by Archimedes' principle, is equal to the apparent loss of weight in water, i.e.

$$\mathrm{SG} = \frac{A_a}{A_a - A_w} \,.$$

Hence, the specific gravity SG_A for body A is

$$\mathrm{SG}_A = \frac{A_a}{A_a - A_w} = \frac{\overline{ca}}{\overline{ca} - \overline{cg}} \,.$$

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Likewise, for a gold body E of the same volume, with a balance point of the counterpoise at e,

$$\mathrm{SG}_E = \frac{\overline{ca}}{\overline{ca}-\overline{ce}} \ ;$$

and for a similar silver body F, with the counterpoise at f:

$$\overline{\mathrm{SG}}_F = \frac{\overline{ca}}{\overline{ca}-\overline{cf}} \ .$$

Let G be the unknown fraction, by weight, of gold in an alloy mixture of gold and silver, then $(1-G)$ will be the fraction of silver.

The weight W (strictly speaking, its mass) of a material M with volume V and *density* D, by definition, is given by

$$W_M = V_M \ . \ D_M \ .$$

Hence the specific gravity SG_M is

$$\mathrm{SG}_M = \frac{W_M}{W_W} = \frac{D_M}{D_W} \ ,$$

and

$$V_M = \frac{W_M}{D_M} = \frac{1}{\mathrm{SG}_M} \cdot \frac{W_M}{D_W}$$

In this case, we obtain for the body A

$$V_A = \frac{\overline{ca}-\overline{cg}}{\overline{ca}} \ . \ \frac{W_A}{D_W} \ ;$$

for the gold body E,

$$V_E = \frac{\overline{ca}-\overline{ce}}{\overline{ca}} \ . \ \frac{G_b\, W_A}{D_W} \ ;$$

and for the silver body F,

$$V_F = \frac{\overline{ca}-\overline{cf}}{\overline{ca}} \ . \ \frac{(1-F)W_A}{D_W} \ .$$

Assuming that the volumes of the constituents combined in the alloy are the same (not always true) as those in the free state, we have for the total volume of the body

$$V_A = V_E + V_F \ ,$$

or upon substitution,

$$\overline{ca}-\overline{cg} = G(\overline{ca}-\overline{ce})+(1-G)(\overline{ca}-\overline{cf}) \ .$$

Solving for G, the fraction of gold, we find

$$G = \frac{\overline{cf} - \overline{cg}}{\overline{cf} - \overline{ce}} = \frac{\overline{fg}}{\overline{fe}} .$$

Note: the problem has been solved without actually weighing the sample. The accuracy of this method would probably not have been as good as that obtainable by chemical analysis in the 16th century. As in Archimedes' time, of course, in this case the body would have had to be defaced by cutting out a piece.

The Little Balance†

Just as it is well known to anyone who takes the care to read ancient authors that Archimedes discovered the jeweler's theft in Hiero's crown, it seems to me the method which this great man must have followed in this discovery has up to now remained unknown. Some authors have written that he proceeded by immersing the crown in water, having previously and separately immersed equal amounts [in weight] of very pure gold and of silver, and, from the differences in their making the water rise or spill over, he came to recognize the mixture of gold and silver of which the crown was made. But this seems, so to say, a crude thing, far from scientific precision; and it will seem even more so to those who have read and understood the very subtle inventions of this divine man in his own writings; from which one most clearly realizes how inferior all other minds are to Archimedes's and what small hope is left to anyone of ever discovering things similar to his [discoveries]. I may well believe that, a rumor having spread that Archimedes had discovered the said theft by means of water, some author of that time may have then left a written record of this fact; and that the same [author], in order to add something to the little that he had heard, may have said that Archimedes used the water in that way which was later universally believed. But my knowing that this way was altogether false and lacking that precision which is needed in mathematical questions made me think several times how, by

† Ref. (2), pp. 134–40.

means of water, one could exactly determine the mixture of two metals. And at last, after having carefully gone over all that Archimedes demonstrates in his books *On Floating Bodies* and *Equilibrium*, a method came to my mind which very accurately solves our problem. I think it probable that this method is the same that Archimedes followed, since, besides being very accurate, it is based on demonstrations found by Archimedes himself.

This method consists in using a balance whose construction and use we shall presently explain, after having expounded what is needed to understand it. One must first know that solid bodies that sink in water weigh in water so much less than in air as is the weight in air of a volume of water equal to that of the body. This [principle] was demonstrated by Archimedes, but because his demonstration is very laborious I shall leave it aside, so as not to take too much time, and I shall demonstrate it by other means. Let us suppose, for instance, that a gold ball is immersed in water. If the ball were made of water it would have no weight at all because water inside water neither rises nor sinks. It is then clear that in water our gold ball weighs the amount by which the weight of the gold [in air] is greater than in water. The same can be said of other metals. And because metals are of different [specific] gravity, their weight in water will decrease in different proportions. Let us assume, for instance, that gold weighs twenty times as much as water; it is evident from what we said that gold will weigh less in water than in air by a twentieth of its total weight [in air]. Let us now suppose that silver, which is less heavy than gold, weighs twelve times as much as water; if silver is weighed in water its weight will decrease by a twelfth. Thus the weight of gold in water decreases less than that of silver, since the first decreases by a twentieth, the second by a twelfth.

Let us suspend a [piece of] metal on [one arm of] a scale of great precision, and on the other arm a counterpoise weighing as much as the piece of metal in air. If we now immerse the metal in water and leave the counterpoise in air, we must bring the said counterpoise closer to the point of suspension [of the balance beam] in order to balance the metal. Let, for instance, *ab* be the balance [beam] and *c* its point of suspension; let a piece of some metal be suspended at *b* and counterbalanced by the weight *d*. If we immerse the weight *b* in water the weight *d* at *a* in the air will weigh more [than *b* in water], and to make it weigh the same we should bring it closer to the point of suspension *c*, for instance to *e*. As many times as the distance *ac* will be greater than the distance *ae*, that many times will the metal weigh more than water.

Let us then assume that weight *b* is gold and that when this is weighed in water, the counterpoise goes back to *e*; then we do the same with very pure silver and when we weigh it in water its counterpoise goes in *f*. This point will be closer to *c* [than is *e*], as the experiment shows us, because silver is lighter than gold. The difference between the distance *af* and the distance *ae* will be the same as the difference between the [specific] gravity of gold and that of silver. But if we shall have a mixture of gold and silver it is clear that because this mixture is in part silver it will weigh less than pure gold, and because it is in part gold it will weigh more than pure silver. If therefore we weigh it in air first, and if then we want the same counterpoise to balance it when immersed in water, we shall have to shift said counterpoise closer to the point of suspension *c* than the point *e*, which is the mark for gold, and farther than *f*, which is the mark for pure silver, and therefore it will fall between the marks *e* and *f*. From the proportion in which the distance *ef* will be divided we shall accurately obtain the proportion of the two metals composing the mixture. So, for instance, let us assume that the mixture of gold and silver is at *b*, balanced in air by *d*, and that this counterweight goes to *g* when the mixture is immersed in water. I now say that the gold and silver that compose the mixture are in the same proportion as the distances *fg* and *ge*. We must however note that the distance *gf*, ending in the mark for silver, will show the amount of gold, and the distance *ge* ending in the mark for gold will indicate the quantity of silver; so that, if *fg* will be twice *ge*, the said mixture will be of two [parts] of gold and one of silver. And thus, proceeding in this same order in the analysis of other mixtures, we shall accurately determine the quantities of the [component] simple metals.

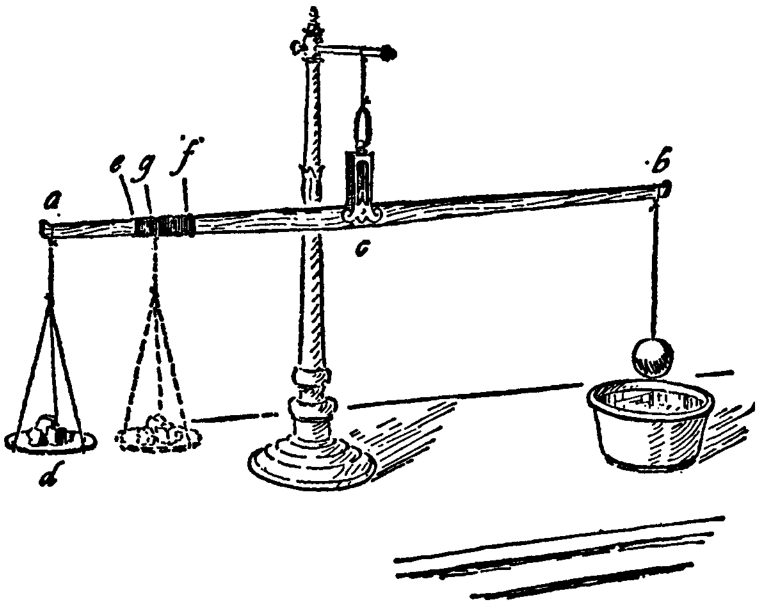


FIG. 16. *The Little Balance.* A drawing based on Galileo's description.

To construct this balance, take a [wooden] bar at least two *braccia* long — the longer the bar, the more accurate the instrument. Suspend it in its middle point; then adjust the arms so that they are in equilibrium, by thinning out whichever happens to be heavier; and on one of the arms mark the points where the counterpoises of the pure metals go when these are weighed in water, being careful to weigh the purest metals that can be found. Having done this, we must still find a way by which easily to obtain the proportions in which the distances between the marks for the pure metals are divided by the marks for the mixtures. This, in my opinion, may be achieved in the following way.

On the marks for the pure metals wind a single turn of very fine steel wire, and around the intervals between marks wind a brass wire, also very fine: these distances will be divided in many very small parts. Thus, for instance, on the marks *e*, *f* I wind only two turns of steel wire (and I do this to distinguish them from brass); and then I go on filling up the entire space between *e* and *f* by winding on it a very fine brass wire, which will divide the space *ef* into many small equal parts. When then I shall want to know the proportion between *fg* and *ge* I shall count the number of turns in *fg* and the number of turns in *ge*, and if I shall find, for instance, that the turns in *fg* are 40 and the turns in *ge* 21, I shall say that in the mixture there are 40 parts of gold and 21 of silver.

Here we must warn that a difficulty in counting arises: Since the wires are very fine, as is needed for precision, it is not possible to count them visually, because the eye is dazzled by such small spaces. To count them easily, therefore, take a most sharp stiletto and pass it slowly over said wires. Thus, partly through our hearing, partly through our hand feeling an obstacle at each turn of wire, we shall easily count said turns. And from their number, as I said before, we shall obtain the precise quantity of pure metals of which the mixture is composed. Note, however, that these metals are in inverse proportion to the distances: Thus, for instance, in a mixture of gold and silver the coils toward the mark for silver will give the quantity of gold, and the coils toward the mark for gold will indicate the quantity of silver; and the same is valid for other mixtures.

CHAPTER 13

THE SCREW AS A MACHINE

MAN has experienced the advantages of many simple machines from earliest known times. He sought subsequently to ascertain theoretically the machine that might be regarded as *fundamental*, the operating principle of which would be sufficient for deriving the specific laws of all other machines.

Galileo, following his acknowledged "master" Archimedes, chose the lever — which both considered (wrongly) could be understood physically merely in terms of mathematical symmetry. First principles, however, for explaining natural phenomena must be basically experiential.

A *screw* can be regarded as a totality of inclined-plane elements, which thus become fundamental for its comprehension. Galileo derived the law of the inclined plane from that of the lever. Although his manner is less ingenious than that of Simon Stevin (1548–1620), it is more natural and more profound. (Nemorius Jordanus (*c.* 1220), who had been the first to derive the law, also did so on the basis of machine experience itself.)

Consider a *bent lever* (cf. Galileo's Fig. 18) with a load (weight, say) L at the point F, having a lever arm $\overline{BK}$ and with an effort (applied force) E at A having a lever arm $\overline{BA}$ $(=\overline{FB})$, the fulcrum being at B. Now the *inclined plane* $\overline{GH}$, with length 1 $(=\overline{FA})$ and vertical height h $(=\overline{FK})$, is tangent to the circle with center B and radius $\overline{FG}$. In the case of equilibrium *law of the lever* requires that

$$\frac{E}{L} = \frac{\overline{BK}}{\overline{BA}} .$$

Since right triangle KBF is similar to right triangle HBF, we have for corresponding sides

$$\frac{\overline{BK}}{\overline{BF}} = \frac{\overline{KF}}{\overline{FH}} .$$

Upon substituting, we obtain

$$\frac{E}{L} = \frac{h}{l} ,$$

which is the *law of the inclined plane.*

Galileo, moreover, realized that the load L travels effectively only through the vertical height h, while the effort E is effective along the whole length l of the inclined plane. He concluded, "It is very important to consider along what lines the motions are made".

On Mechanics†

Of the Screw. Among all the mechanical instruments devised by human wit for various conveniences, it seems to me that for ingenuity and utility the screw takes first place, as something cleverly adapted not only to move but also to fix and to press with great force; and it is constructed in such a manner as to occupy but a very small space and yet to accomplish effects that the other instruments could perform only if made into large machines. The screw thus being among the most beautiful and useful of contrivances, we may rightly take the trouble to explain as clearly as we may both its origin and its nature. To do this we shall start from a theory which, though at first it may appear to be somewhat remote from the consideration of this instrument, is nevertheless its basis and foundation.

There can be no doubt that the constitution of nature with respect to the movements of heavy bodies is such that any body which retains heaviness within itself has a propensity, when free, to move toward the center; and not only by a straight perpendicular line, but also, when it cannot do otherwise, along any other line which, having some tilt toward the center, goes downward little by little. And thus we see, for instance, that water from some high place not only drops perpendicularly downward, but also runs about the surface of the earth on lines that are inclined, though but very little. This is seen in the course of rivers whose waters, though the bed is very little slanted, run freely dropping downward; which same effect, just as it is perceived in fluid bodies, appears also in hard solids, provided that their shapes and other external and accidental impediments do not prevent it. So that if we have a surface that is very smooth and polished, as

† Ref. (3), pp. 169–77.

would be that of a mirror, and a perfectly smooth and round ball of marble or glass or some such material capable of being polished, then if this ball is placed on that surface it will go moving along, provided that the surface has some little tilt, even the slightest; and it will remain still only on that surface which is most precisely leveled, and equidistant from the plane of the horizon. This, for example, might be the surface of a frozen lake or pond, upon which such a spherical body would stand still, though with a disposition to be moved by any extremely small force. For we have understood that if such a plane tilted only by a hair, the said ball would move spontaneously toward the lower part, and on the other hand it would have resistance toward the upper or rising part, nor could it be moved that way without some violence. Hence it is perfectly clear that on an exactly balanced surface the ball would remain indifferent and questioning between motion and rest, so that any the least force would be sufficient to move it, just as on the other hand any little resistance, such as that merely of the air that surrounds it, would be capable of holding it still.

From this we may take the following conclusion as an indubitable axiom: That heavy bodies, all external and adventitious inpediments being removed, can be moved in the plane of the horizon by any minimum force. But when the same heavy body must be driven upon an ascending plane, having a tendency to the contrary motion and commencing to oppose such an ascent, there will be required greater and greater violence the more elevation the said plane shall have. For example, the movable body *G* being placed on the line *AB* parallel to the horizon, it will stand there, as was said, indifferent to motion or to rest, so that it may be moved by the least force; but if we have the inclined planes *AC*, *AD*, and *AE*, upon these it will be driven only by violence, more of which is required to move it along the line *AD* than along *AC*, and still more along *AE* than *AD*. This comes from its having greater impetus to go downward along the line *EA* than along *DA*, and along *DA* than along *CA*. So that we may likewise conclude heavy bodies to have greater resistance to being moved upon variously inclined planes, according as one is more or less tilted than another; and finally the resistance to being raised will be greatest on the part of the heavy body in the perpendicular *AF*. But what proportion the force must have to the weight in order to draw it upon various inclined planes must be explained precisely before we proceed further, so that we may completely understand all that remains to be said.

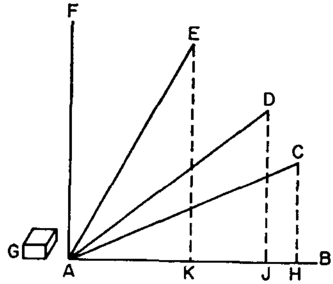


FIG. 17.

From the points *C*, *D*, and *E*, therefore, let fall the perpendiculars *CH*, *DJ*, and *EK* upon the horizontal line *AB*. It will be demonstrated that the same weight will be moved upon the inclined plane *AC* by less force than in the perpendicular *AF* (where it will be raised by a force equal to itself), in proportion as the perpendicular *CH* is less than *AC;* and upon the plane *AD* the force will have to the weight the same proportion as the perpendicular line *JD* has to *DA;* and finally in the plane *AE* the ratio of the force to the weight will be that of *KE* to *EA*.

This present theory was attempted also by Pappus of Alexandria in the eighth book of his *Mathematical Collections*, but in my opinion he missed the mark, being defeated by the assumption which he made when he supposed that the weight would have to be moved in the horizontal plane by a given force. This is false, no sensible force being required (neglecting accidental inpediments, which are not considered by the theoretician) to move the given weight horizontally, so that it is vain thus to seek the force with which it will be moved on the inclined plane. It will be better, given the force that would move the object perpendicularly upward (which would equal the weight of the object), to seek the force that will move it on the inclined plane. This we shall attempt to achieve, with an attack different from that of Pappus.

Consider, then, the circle *AJC* and in this the diameter *ABC* with center *B*, and two weights of equal *moments* at the extremities

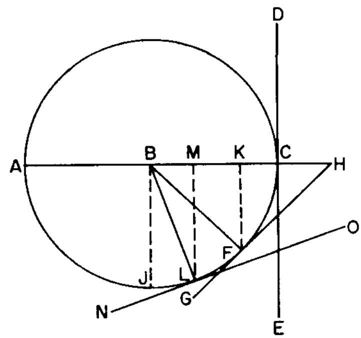


FIG. 18.

A and *C*, so that the line *AC* being a lever or balance movable about the center *B*, the weight *C* will be sustained by the weight *A*. But if we imagine the arm of the balance *BC* to be inclined downward along the line *BF* in such a way that the two lines *AB* and *BF* are fixed together at the point *B*, then the *moment* of the weight *C* will no longer be equal to the *moment* of the weight *A*, the distance of the point *F* from the line *BJ*, which goes from the support *B* to the center of the earth, being diminished.

Now if we draw from the point *F* a perpendicular to *BC*, which is *FK*, the *moment* of the weight at *F* will be as if it were hung from the line *KB*; and as the distance *KB* is made smaller with respect to the distance *BA*, the *moment* of the weight *F* is accordingly diminished from the *moment* of the weight *A*. Likewise, as the weight inclines more, as along the line *BL*, its *moment* will go on diminishing, and it will be as if it were hung from the distance *BM* along the line *ML*, in which point *L* a weight placed at *A* will sustain one as much less than itself as the distance *BA* is greater than the distance *BM*.

You see, then, how the weight placed at the end of the line *BC*, inclining downward along the circumference *CFLJ*, comes gradually to diminish its *moment* and its impetus to go downward, being sustained more and more by the lines *BF* and *BL*. But to consider this heavy body as descending and sustained now less and now more by the radii *BF* and *BL*, and as constrained to travel along the circumference *CFL*, is not different from imagining the same circumference *CFLJ* to be a surface of the same curvature placed under the same movable body, so that this body, being supported upon it, would be constrained to descend along it. For in either case the movable body traces out the same path, and it does not matter whether it is suspended from the center *B* and sustained by the radius of the circle, or whether this support is removed and it is supported by and travels upon the circumference *CFLJ*. Whence we may undoubtedly affirm that the heavy body descending from the point *C* along the circumference *CFLJ*, its *moment* of descent at the first point *C* is total and integral, since it is in no way supported by the circumference; and at this first point *C* it has no disposition to move differently from what it would freely do in the perpendicular tangent *DCE*. But if the movable body is located at the point *F*, then its heaviness is partly sustained by the circular path placed under it, and its *moment* downward is diminished in that proportion by which the line *BK* is exceeded by the line *BC*. Now when the movable body is at *F*, at the first point of its motion it is as if it were on an inclined plane according to the tangent line *GFH*, since the tilt of the circumference at the point *F* does not differ from the tilt of the tangent *FG*, apart from the insensible angle of contact.

And in the same way we shall find that at the point *L* the *moment* of the same movable body is diminished as the line *BM* is diminished from *BC*, so that in the tangent plane to the circle at *L*, represented by the line *NLO*, the *moment* of descent is lessened in the movable body in the same proportion. Therefore if on the plane *HG* the *moment* of the movable body is diminished from its total impetus (which it has in the perpendicular *DCE*) in the proportion of the line *KB* to the line *BC* or *BF*, the similarity of the triangles *KBF* and *KFH* making the proportion between the lines *KF* and *FH* the same as between *KB* and *BF*, we conclude that the whole and absolute *moment* that the movable body has in the perpendicular to the horizon is in the same proportion to that which it has on the inclined plane *HF* as the line *HF* is to the line *FK*, which is that of the length of the inclined plane to the perpendicular dropped from this on the horizontal. So that, passing to the present separate diagram, the *moment* downward of the movable body on the inclined plane *FH* has the same proportion to the total *moment* with which it presses down in the

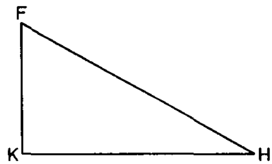


FIG. 19.

perpendicular *FK* as this line *KF* has to *FH*. This being the case, it is clear that the force that sustains the weight on the perpendicular *FK* must be equal to the weight, so that to sustain it on the inclined plane *FH* there will suffice one as much less than that as this perpendicular *FK* is less than the line *FH*. And since, as has been mentioned at other times, the force to move the weight need only insensibly exceed that which sustains it, we derive this general conclusion: That upon the inclined plane the force has the same proportion to the weight as the perpendicular dropped to the horizontal from the end of the plane has to the length of the plane.

Returning now to our original purpose, which was to investigate the nature of the screw, let us consider the triangle *ACB*, of which the line *AB* is horizontal, *BC* is perpendicular to it, and

AC is the inclined plane upon which the movable body *D* will be drawn by a force as much less than itself as the line *BC* is shorter than *CA*. Now to raise the same weight on the same plane *AC* with the triangle *CAB* standing still and the weight *D* being

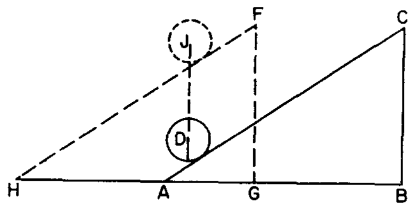


FIG. 20.

moved toward *C*, is the same thing as if the weight *D* were not moved from the perpendicular *DJ* while the triangle was being driven forward toward *H*, for when the triangle has reached the place *FHG* the movable body will have climbed to the altitude *AJ*.

Now finally the form and first essence of the screw is no other than such a triangle *ACB* which, driven forward, slips under the heavy body to be raised, and boosts it or jacks it up (as they say);

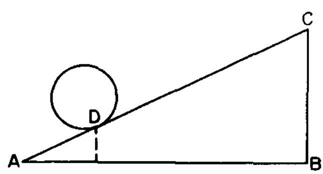


FIG. 21.

and such was its first origin. Whoever was its first inventor considered that as the triangle *ABC* coming forward raised the weight *D*, so an instrument could be constructed similar to the said triangle of some solid material which, driven forward, would elevate the given weight; but then considering better how such a machine could be reduced to a much smaller and more convenient form, he took the same triangle and wound it round the cylinder

ABCD in such a manner that the altitude *CB* of the triangle should be the height of the cylinder. Thus the ascending plane generates upon the cylinder the helical line denoted by the line *AEFGH*, which is commonly called the thread of the screw; and in this way there was created the instrument called by the Greeks *cochlea*, and by us the screw; which, turning round, comes to bear with its thread beneath the weight and easily raises it.

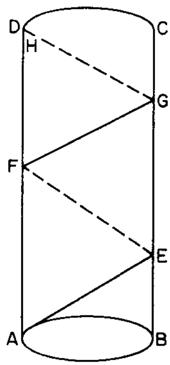


FIG. 22.

Now having demonstrated that upon the inclined plane the force has to the weight the same proportion as the perpendicular height of the inclined plane has to its length, we thus understand that the force is multiplied by the screw *ABCD* according to the ratio that the length of its entire thread *AEFGH* has to its height *CB*. In this way we learn that the more dense the threading of the screw by its helices, the more powerful it becomes, as being generated by a less steeply inclined plane, whose length is in greater proportion to its own perpendicular height. But let us not neglect to mention that if we wish to find the force of a given screw we do not have to measure the length of its entire thread, and the height of its whole cylinder, but it will suffice that we examine the number of times the distance between any two contiguous elements divides a single revolution of the thread.

This, for example, will be the number of times the distance *AF* is contained in the length of the turn *AEF*, for this is the same proportion that the entire height *CB* has to the whole thread.

When one understands all we have said about the nature of this instrument, I do not doubt that all its other properties can be understood without trouble — as, for instance, that instead of raising the weight upon the screw, a female thread is accommodated to it with a concave helix in which the male thread of the screw enters and is then turned round, raising and lifting this nut together with the weight attached to it.

Finally one must not ignore the consideration which from the beginning has been said to hold for all mechanical instruments, that is, that whatever is gained in force by their means is lost in time and in speed. Perhaps to someone this may not appear so clearly in the present instance, and it may even seem that the force is multiplied without the mover traveling farther than the body moved. For let us suppose in the triangle *ABC* the line *AB* to be horizontal, *AC* to be the inclined plane whose height is measured by the perpendicular *CB*, and a movable body to be placed on the plane *AC*, and linked by the cord *EDF* to a force

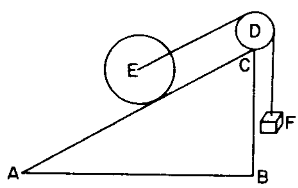


FIG. 23.

or weight placed at *F*. Then if its proportion to the heaviness of the weight *E* is the same as that of the line *BC* to *CA*, from what has been proven, the weight *F* will descend, drawing the movable body *E* along the inclined plane without the weight *F* measuring a greater space in descending than the movable body *E* measures along the line *AC*. But here it should be noticed that although the movable body *E* will have passed over all the line *AC* in the same time that the other heavy body *F* will have fallen through an equal interval, nevertheless the heavy body *E* will not have been removed from the common center of heavy things more than the distance along the perpendicular *CB*, while the heavy body *F* descending perpendicularly will have dropped by a space equal to the whole line *AC*. And since heavy bodies do not have any resistance to transverse motions except in proportion to their removal from the center of the earth, then the movable body *E* not being raised more than the distance *CB* in the whole motion *AC*, while *F* has dropped perpendicularly as much as the whole length of *AC*, we may rightly say that the travel of the force *F* has the same ratio to the travel of the force *E* as the line *AC* has to the line *CB*, or as the weight *E* has to the weight *F*. Therefore it is very important to consider along what lines the motions are made, and especially of inanimate heavy bodies, whose *moments* have their whole power and their entire resistance in the line perpendicular to the horizon; and in other lines, transversely rising or falling, they have only that power, impetus, or resistance which is greater or lesser according as the inclinations approach more or less to the perpendicular elevation.

CHAPTER 14

STRENGTH OF MATERIALS

(a) Scaling

Galileo began his most significant book on pure science with a practical problem that arose out of his consultation services for the famous Venetian Arsenal with its dockyards and its galleys. It was a question of scaling; namely, why is the scaffolding and bracing for launching a large vessel greater than that for a small one? From purely geometrical considerations a similar large machine should not be proportionately stronger than a small one. For example, the law of the lever is the same for geometrically similar levers; if all distances are doubled or halved, the mechanical advantage remains unchanged. Evidently there is a missing physical factor, viz. the strength of materials. Geometry as pure mathematics is not sufficient for solving all mechanical problems; some kind of physical geometry is requisite. The first new science, strictly speaking, was truly the first engineering science, namely, the *strength of materials.*

Dialogues Concerning Two New Sciences†

SALV. The constant activity which you Venetians display in your famous arsenal suggests to the studious mind a large field for investigation, especially that part of the work which involves mechanics; for in this department all types of instruments and machines are constantly being constructed by many artisans, among whom there must be some who, partly by inherited experience and partly by their own observations, have become highly expert and clever in explanation.

SAGR. You are quite right. Indeed, I myself, being curious by nature, frequently visit this place for the mere pleasure of observing the work of those who, on account of their superiority over

† Ref. (9), pp. 1–3.

other artisans, we call "first rank men." Conference with them has often helped me in the investigation of certain effects including not only those which are striking, but also those which are recondite and almost incredible. At times also I have been put to confusion and driven to despair of ever explaining something for which I could not account, but which my senses told me to be true. And notwithstanding the fact that what the old man told us a little while ago is proverbial and commonly accepted, yet it seemed to me altogether false, like many another saying which is current among the ignorant; for I think they introduce these expressions in order to give the appearance of knowing something about matters which they do not understand.

SALV. You refer, perhaps, to that last remark of his when we asked the reason why they employed stocks, scaffolding and bracing of larger dimensions for launching a big vessel than they do for a small one; and he answered that they did this in order to avoid the danger of the ship parting under its own heavy weight [*vasta mole*], a danger to which small boats are not subject?

SAGR. Yes, that is what I mean; and I refer especially to his last assertion which I have always regarded as a false, though current, opinion; namely, that in speaking of these and other similar machines one cannot argue from the small to the large, because many devices which succeed on a small scale do not work on a large scale. Now, since mechanics has its foundation in geometry, where mere size cuts no figure, I do not see that the properties of circles, triangles, cylinders, cones and other solid figures will change with their size. If, therefore, a large machine be constructed in such a way that its parts bear to one another the same ratio as in a smaller one, and if the smaller is sufficiently strong for the purpose for which it was designed, I do not see why the larger also should not be able to withstand any severe and destructive tests to which it may be subjected.

SALV. The common opinion is here absolutely wrong. Indeed, it is so far wrong that precisely the opposite is true, namely, that many machines can be constructed even more perfectly on a large scale than on a small; thus, for instance, a clock which indicates and strikes the hour can be made more accurate on a large scale than on a small. There are some intelligent people who maintain this same opinion, but on more reasonable grounds, when they cut loose from geometry and argue that the better performance of the large machine is owing to the imperfections and variations of the material. Here I trust you will not charge me with arrogance if I say that imperfections in the material, even those which are great enough to invalidate the clearest mathematical proof, are not sufficient to explain the deviations observed between machines in the concrete and in the abstract. Yet I shall say it and will affirm that, even if the imperfections did not exist and matter were absolutely perfect, unalterable and free from all accidental variations, still the mere fact that it is matter makes the larger machine, built of the same material and in the same proportion as the smaller, correspond with exactness to the smaller in every respect except that it will not be so strong or so resistant against violent treatment; the larger the machine, the greater its weakness. Since I assume matter to be unchangeable and always the same, it is clear that we are no less able to treat this constant and invariable property in a rigid manner than if it belonged to simple and pure mathematics. Therefore, Sagredo, you would do well to change the opinion which you, and perhaps also many other students of mechanics, have entertained concerning the ability of machines and structures to resist external disturbances, thinking that when they are built of the same material and maintain the same ratio between parts, they are able equally, or rather proportionally, to resist or yield to such external disturbances and blows. For we can demonstrate by geometry that the large machine is not proportionately stronger than the small. Finally, we may say that, for every machine and structure, whether artificial or natural, there is set a necessary limit beyond which neither art nor nature can pass; it is here understood, of course, that the material is the same and the proportion preserved.

SAGR. My brain already reels. My mind, like a cloud momentarily illuminated by a lightning-flash, is for an instant filled with an unusual light, which now beckons to me and which now suddenly mingles and obscures strange, crude ideas. From what you have said it appears to me impossible to build two similar structures of the same material, but of different sizes and have them proportionately strong; and if this were so, it would not be possible to find two single poles made of the same wood which shall be alike in strength and resistance but unlike in size.

(b) Galileo's Problem

What has become known as *Galileo's problem* concerns the resistance to fracture of a cantilever, i.e. a horizontal beam embedded in a wall (cf. Fig. 24). He assumed physically that the base of the fracture, where the beam is joined to the wall, is under a uniform tensile stress, which can be regarded as equivalent to a single resultant force acting towards the wall, at the center of the contact. He failed to realize that the fibers of the strained beam are not inextensible, and that there is a balance between forces of tension and those of compression (correctly noted by Edmé Mariotte (*c.* 1620–84), who specified them in 1680 with the law of Robert Hooke (1635–1703) for the relation of elastic stress and equilibrium displacement). This false assumption, however, did not invalidate most of his conclusions inasmuch as they involved only ratios of the strengths of beams of similar cross-sections.

Galileo's whole procedure was based on the law of the lever for equilibrium.

Let a load W be suspended from the end of a beam of span L and depth D (or diameter). Then for a beam of neglible weight, with the lower edge of the beam along the wall as an axis (fulcrum), we have according to the *law of the lever*

$$W \,.\, L = R \,.\, \frac{D}{2} \,,$$

where R is the "resistance" of the beam. Hence

$$\frac{R}{W} = \frac{2L}{D} \,.$$

Galileo indicated further how this conclusion has to be modified to include the weight of the beam. (He had previously derived the law of the lever for a beam having a significant weight.)

Dialogues Concerning Two New Sciences†

Proposition I

A prism or solid cylinder of glass, steel, wood or other breakable material which is capable of sustaining a very heavy weight

† Ref. (9), pp. 115–17.

when applied longitudinally is, as previously remarked, easily broken by the transverse application of a weight which may be much smaller in proportion as the length of the cylinder exceeds its thickness.

Let us imagine a solid prism ABCD fastened into a wall at the end AB, and supporting a weight E at the other end; understand also that the wall is vertical and that the prism or cylinder is fastened at right angles to the wall. It is clear that, if the cylinder breaks, fracture will occur at the point B where the edge of the mortise acts as a fulcrum for the lever BC, to which the force is applied; the thickness of the solid BA is the other arm of the lever along which is located the resistance. This resistance opposes the separation of the part BD, lying outside the wall, from that portion lying inside. From the preceding, it follows that the magnitude [*momento*] of the force applied at C bears to the magnitude [*momento*] of the resistance, found in the thickness of the prism, i.e., in the attachment of the base BA to its contiguous parts, the same ratio which the length CB bears to half the length BA; if now we define absolute resistance to fracture as that offered to a longitudinal pull (in which case the stretching force acts in the same direction as that through which the body is moved), then it follows that the absolute resistance of the prism BD is to the breaking load placed at the end of the lever BC in the same ratio as the length BC is to the half of AB in the case of a prism, or the semidiameter in the case of a cylinder. This is our first proposition. Observe that in what has here been said the weight of the solid BD itself has been left out of consideration, or rather, the prism has been assumed to be devoid of weight. But if the weight of the prism is to be taken account of in conjunction with the weight E, we must add to the weight E one half that of the prism BD: so that if, for example, the latter weighs two pounds and the weight E is ten pounds we must treat the weight E as if it were eleven pounds.

SIMP. Why not twelve?

SALV. The weight E, my dear Simplicio, hanging at the extreme end C acts upon the lever BC with its full moment of ten pounds:

so also would the solid BD if suspended at the same point exert its full moment of two pounds; but, as you know, this solid is uniformly distributed throughout its entire length, BC, so that the parts which lie near the end B are less effective than those more remote.

FIG. 24.

Accordingly if we strike a balance between the two, the weight of the entire prism may be considered as concentrated at its center of gravity which lies midway of the lever BC. But a weight hung at the extremity C exerts a moment twice as great as it would if suspended from the middle: therefore if we consider the moments of both as located at the end C we must add to the weight E one-half that of the prism.

SIMP. I understand perfectly; and moreover, if I mistake not, the force of the two weights BD and E, thus disposed, would exert the same moment as would the entire weight BD together with twice the weight E suspended at the middle of the lever BC.

SALV. Precisely so, and a fact worth remembering.

(c) Similar Beams

The interesting question posed at the beginning of the *Two New Sciences* involved the relative diameters of two cylindrical beams where one given beam of span L_1 and diameter D_1 is just able to support its own weight W_1 without fracture, and a second beam of span L_2 is just able to support its weight W_2, associated with an unknown diameter D_2. Weight being proportional to volume, accordingly,

$$W_1 \propto L_1 D_1^2 \ ,$$

and

$$W_2 \propto L_2 D_2^2 \ .$$

Therefore, the corresponding bending moments B_1 and B_2 are, respectively,

$$B_1 \propto L_1 D_1^2 \frac{L_1}{2} \propto L_1^2 D_1^2 \ ,$$

and

$$B_2 \propto L_2 D_2^2 \frac{L_2}{2} \propto L_2^2 D_2^2 \ .$$

Galileo had already proved (cf. Proposition IV, p. 119) that the maximum bending moment M of the resistance to fracture is proportional to the cube of the diameter of the base.

If $B = M$ max, then

$$D^3 \propto L^2 D^2 \ ,$$

i.e.

$$D \propto L^2 \ .$$

But similar beams have $D \propto L$; hence they cannot also have similar strengths. A given form, therefore, cannot be physically magnified either in nature — or in art; its size is naturally limited. There is generally a right size!

Dialogues Concerning Two New Sciences†

SAGR. The favor will be that much greater: nevertheless I hope you will oblige me by putting into written form the argument just given so that I may study it at my leisure.

† Ref. (9), pp. 129–33.

SALV. I shall gladly do so. Let A denote a cylinder of diameter DC and the largest capable of sustaining its own weight: the problem is to determine a larger cylinder which shall be at once the maximum and the unique one capable of sustaining its own weight.

Let E be such a cylinder, similar to A, having the assigned length, and having a diameter KL. Let MN be a third proportional to the two lengths DC and KL: let MN also be the diameter

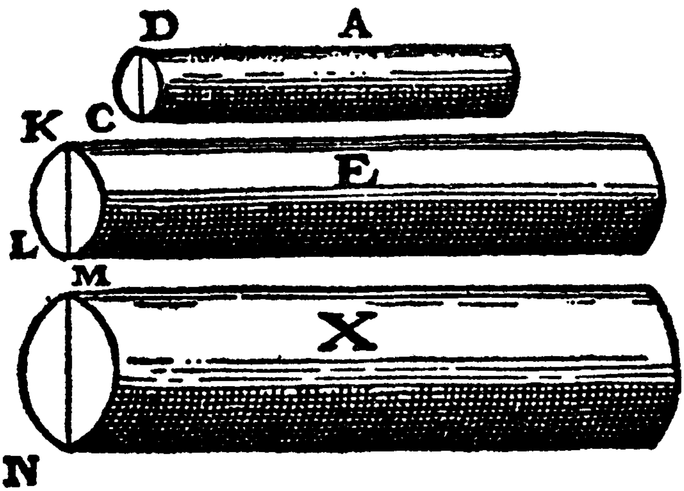


FIG. 25.

of another cylinder, X, having the same length as E: then, I say, X is the cylinder sought. Now since the resistance of the base DC is to the resistance of the base KL as the square of DC is to the square of KL, that is, as the square of KL is to the square of MN, or, as the cylinder E is to the cylinder X, that is, as the moment E is to the moment X; and since also the resistance [bending strength] of the base KL is to the resistance of the base MN as the cube of KL is to the cube of MN, that is, as the cube of DC is to the cube of KL, or, as the cylinder A is to the cylinder E, that is, as the moment of A is to the moment of E; hence it follows, *ex æquali in proportione perturbata*, that the moment of A is to the moment of X as the resistance of the base DC is to the resistance of the base MN; therefore moment and resistance are related to each other in prism X precisely as they are in prism A.

Let us now generalize the problem; then it will read as follows:

> Given a cylinder AC in which moment and resistance [bending strength] are related in any manner whatsoever; let DE be the length of another cylinder; then determine what its thickness must be in order that the relation between its moment and resistance shall be identical with that of the cylinder AC.

Using Fig. 25 in the same manner as above, we may say that, since the moment of the cylinder FE is to the moment of the portion DG as the square of ED is to the square of FG, that is, as the length DE is to I; and since the moment of the cylinder FG is to the moment of the cylinder AC as the square of FD is to the square of AB, or, as the square of ED is to the square of I, or, as the square of I is to the square of M, that is, as the length I is to O; it follows, *ex æquali*, that the moment of the cylinder FE is to the moment of the cylinder AC as the length DE is to O, that is, as the cube of DE is to the cube of I, or, as the cube of FD is to the cube of AB, that is, as the resistance of the base FD is to the resistance of the base AB; which was to be proven.

From what has already been demonstrated, you can plainly see the impossibility of increasing the size of structures to vast dimensions either in art or in nature; likewise the impossibility of building ships, palaces, or temples of enormous size in such a way that their oars, yards, beams, iron-bolts, and, in short, all their other parts will hold together; nor can nature produce trees of extraordinary size because the branches would break down under their own weight; so also it would be impossible to build up the bony structures of men, horses, or other animals so as to hold together and perform their normal functions if these animals were to be increased enormously in height; for this increase in height can be accomplished only by employing a material which is harder and stronger than usual, or by enlarging the size of the bones, thus changing their shape until the form and appearance of the animals suggest a monstrosity. This is perhaps what our wise Poet had in mind, when he says, in describing a huge giant:

"Impossible it is to reckon his height
"So beyond measure is his size."

To illustrate briefly, I have sketched a bone whose natural length has been increased three times and whose thickness has been multiplied until, for a correspondingly large animal, it would perform the same function which the small bone performs

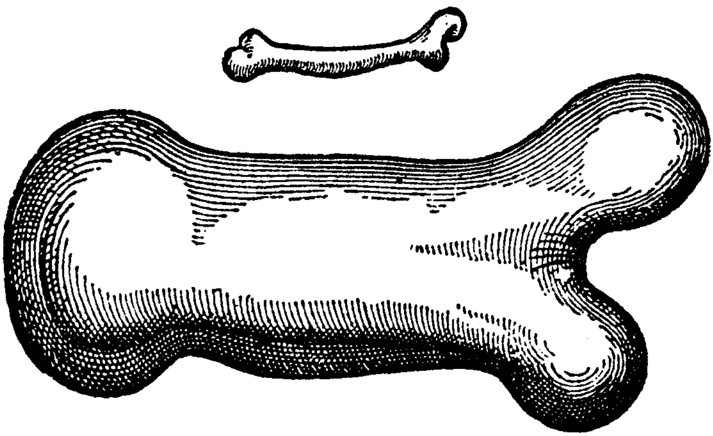


FIG. 26.

for its small animal. From the figures here shown you can see how out of proportion the enlarged bone appears. Clearly then if one wishes to maintain in a great giant the same proportion of limb as that found in an ordinary man he must either find a harder and stronger material for making the bones, or he must admit a diminution of strength in comparison with men of medium stature; for if his height be increased inordinately he will fall and be crushed under his own weight. Whereas, if the size of a body be diminished, the strength of that body is not diminished in the same proportion; indeed the smaller the body the greater its relative strength. Thus a small dog could probably carry on his back two or three dogs of his own size; but I believe that a horse could not carry even one of his own size.

SIMP. This may be so; but I am led to doubt it on account of the enormous size reached by certain fish, such as the whale which, I understand, is ten times as large as an elephant; yet they all support themselves.

SALV. Your question, Simplicio, suggests another principle, one which had hitherto escaped my attention and which enables giants and other animals of vast size to support themselves and to move about as well as smaller animals do. This result may be secured either by increasing the strength of the bones and other parts intended to carry not only their weight but also the superincumbent load; or, keeping the proportions of the bony structure constant, the skeleton will hold together in the same manner or even more easily, provided one diminishes, in the proper proportion, the weight of the bony material, of the flesh, and of anything else which the skeleton has to carry. It is this second principle which is employed by nature in the structure of fish, making their bones and muscles not merely light but entirely devoid of weight.

SIMP. The trend of your argument, Salviati, is evident. Since fish live in water which on account of its density [*corpulenza*] or, as others would say, heaviness (*gravità*) diminishes the weight [*peso*] of bodies immersed in it, you mean to say that, for this reason, the bodies of fish will be devoid of weight and will be supported without injury to their bones. But this is not all; for although the remainder of the body of the fish may be without weight, there can be no question but that their bones have weight. Take the case of a whale's rib, having the dimensions of a beam; who can deny its great weight or its tendency to go to the bottom when placed in water? One would, therefore, hardly expect these great masses to sustain themselves.

SALV. A very shrewd objection! And now, in reply, tell me whether you have ever seen fish stand motionless at will under water, neither descending to the bottom nor rising to the top, without the exertion of force by swimming?

SIMP. This is a well-known phenomenon.

SALV. The fact then that fish are able to remain motionless under water is a conclusive reason for thinking that the material of their bodies has the same specific gravity as that of water; accordingly, if in their make-up there are certain parts which are heavier than water there must be others which are lighter, for otherwise they would not produce equilibrium.

Hence, if the bones are heavier, it is necessary that the muscles or other constituents of the body should be lighter in order that their buoyancy may counterbalance the weight of the bones. In aquatic animals therefore circumstances are just reversed from what they are with land animals inasmuch as, in the latter, the bones sustain not only their own weight but also that of the flesh, while in the former it is the flesh which supports not only its own weight but also that of the bones. We must therefore cease to wonder why these enormously large animals inhabit the water rather than the land, that is to say, the air.

SIMP. I am convinced and I only wish to add that what we call land animals ought really to be called air animals, seeing that they live in the air, are surrounded by air, and breathe air.

SAGR. I have enjoyed Simplicio's discussion including both the question raised and its answer. Moreover I can easily understand that one of these giant fish, if pulled ashore, would not perhaps sustain itself for any great length of time, but would be crushed under its own mass as soon as the connections between the bones gave way.

SALV. I am inclined to your opinion; and, indeed, I almost think that the same thing would happen in the case of a very big ship which floats on the sea without going to pieces under its load of merchandise and armament, but which on dry land and in air would probably fall apart.

(d) A Cracked Column

In the beginning Galileo told a strange anecdote about a large marble column resting horizontally on two supports at its ends. In order to prevent sagging and rupture at the middle a careful mechanic had placed an additional support there. A few months later the column cracked — precisely in the middle.

Suppose a cylinder supporting its own weight has the maximum length possible without fracture. If it is supported at both ends, it will have twice the length of such a beam supported by only at one of its ends in a wall. Consequently another support, say at the middle, produces an additional force and bending moment so that rupture occurs.

Dialogues Concerning Two New Sciences†

Hitherto we have considered the moments and resistances of prisms and solid cylinders fixed at one end with a weight applied at the other end; three cases were discussed, namely, that in which the applied force was the only one acting, that in which the weight of the prism itself is also taken into consideration, and

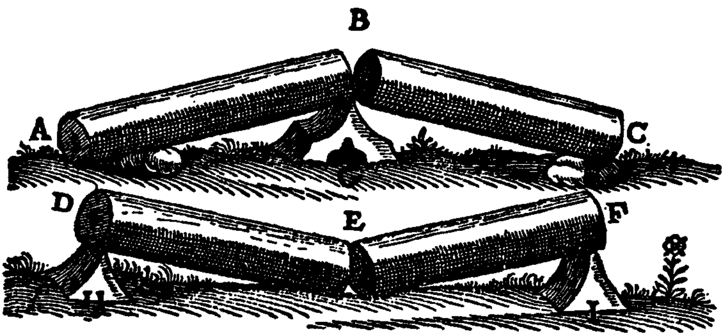


FIG. 27.

that in which the weight of the prism alone is taken into consideration. Let us now consider these same prisms and cylinders when supported at both ends or at a single point placed somewhere between the ends. In the first place, I remark that a

† Ref. (9), pp. 134–8.

cylinder carrying only its own weight and having the maximum length, beyond which it will break, will, when supported either in the middle or at both ends, have twice the length of one which is mortised into a wall and supported only at one end. This is very evident because, if we denote the cylinder by ABC and if we assume that one-half of it, AB, is the greatest possible length capable of supporting its own weight with one end fixed at B, then, for the same reason, if the cylinder is carried on the point G, the first half will be counterbalanced by the other half BC. So also in the case of the cylinder DEF, if its length be such that it will support only one-half this length when the end D is held fixed, or the other half when the end F is fixed, then it is evident that when supports, such as H and I, are placed under the ends D and F respectively the moment of any additional force or weight placed at E will produce fracture at this point.

A more intricate and difficult problem is the following: neglect the weight of a solid such as the preceding and find whether the same force or weight which produces fracture when applied at the middle of a cylinder, supported at both ends, will also break the cylinder when applied at some other point nearer one end than the other.

Thus, for example, if one wished to break a stick by holding it with one hand at each end and applying his knee at the middle, would the same force be required to break it in the same manner if the knee were applied, not at the middle, but at some point nearer to one end?

SAGR. This problem, I believe, has been touched upon by Aristotle in his *Questions in Mechanics*.

SALV. His inquiry however is not quite the same; for he seeks merely to discover why it is that a stick may be more easily broken by taking hold, one hand at each end of the stick, that is, far removed from the knee, than if the hands were closer together. He gives a general explanation, referring it to the lengthened lever arms which are secured by placing the hands at the ends of the stick. Our inquiry calls for something more: what we want to know is whether, when the hands are retained at the ends of the stick, the same force is required to break it wherever the knee be placed.

SAGR. At first glance this would appear to be so, because the two lever arms exert, in a certain way, the same moment, seeing that as one grows shorter the other grows correspondingly longer.

SALV. Now you see how readily one falls into error and what caution and circumspection are required to avoid it. What you have just said appears at first glance highly probable, but on closer examination it proves to be quite far from true; as will be seen from the fact that whether the knee — the fulcrum of the two levers — be placed in the middle or not makes such a difference that, if fracture is to be produced at any other point than the middle, the breaking force at the middle, even when multiplied four, ten, a hundred, or a thousand times would not suffice. To begin with we shall offer some general considerations and then pass to the determination of the ratio in which the breaking force must change in order to produce fracture at one point rather than another.

Let AB denote a wooden cylinder which is to be broken in the middle, over the supporting point C, and let DE represent an identical cylinder which is to be broken just over the supporting point F which is not in the middle. First of all it is clear that, since the distances AC and CB are equal, the forces applied at the extremities B and A must also be equal. Secondly since the distance DF is less than the distance AC the moment of any force acting at D is less than the moment of the same force at A, that is, applied at the distance CA; and the moments are less in the ratio of the length DF to AC; consequently it is necessary to increase the force [*momento*] at D in order to overcome, or even to balance, the resistance at F; but in comparison with the length AC the distance DF can be diminished indefinitely: in order therefore to counterbalance the resistance at F it will be necessary to increase indefinitely the force [*forza*] applied at D. On the other hand, in proportion as we increase the distance FE over that of CB, we must diminish the force at E in order to counterbalance the resistance at F; but the distance FE, measured in terms of CB, cannot be increased indefinitely by sliding the fulcrum F toward the end D; indeed, it cannot even be made double the length CB. Therefore the force required at E to balance the resistance at F will always be more than half that required at B. It is clear then that, as the fulcrum F approaches the end D, we must of necessity indefinitely increase the sum of the forces applied at E and D in order to balance, or overcome, the resistance at F.

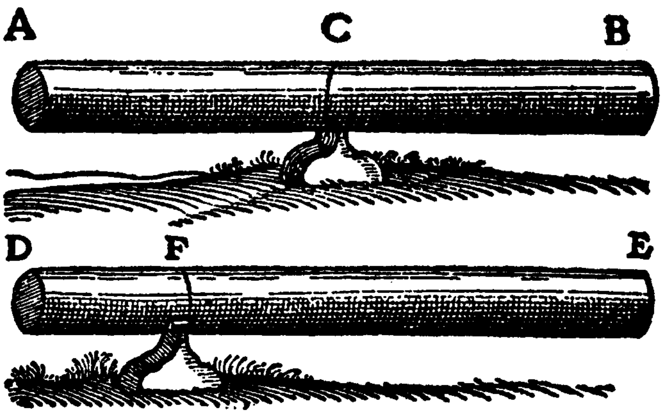


FIG. 28.

SAGR. What shall we say, Simplicio? Must we not confess that geometry is the most powerful of all instruments for sharpening the wit and training the mind to think correctly? Was not Plato perfectly right when he wished that his pupils should be first of all well grounded in mathematics? As for myself, I quite understood the property of the lever and how, by increasing or diminishing its length, one can increase or diminish the moment of force and of resistance; and yet, in the solution of the present problem I was not slightly, but greatly, deceived.

SIMP. Indeed I begin to understand that while logic is an excellent guide in discourse, it does not, as regards stimulation to discovery, compare with the power of sharp distinction which belongs to geometry.

SAGR. Logic, it appears to me, teaches us how to test the conclusiveness of any argument or demonstration already discovered and completed; but I do not believe that it teaches us to discover correct arguments and demonstrations. But it would be better if Salviati were to show us in just what proportion the forces must be increased in order to produce fracture as the fulcrum is moved from one point to another along one and the same wooden rod.

(e) Tubes

The Second Day of the *Two New Sciences* concluded with a comparison of the strength of a hollow tube and that of a solid one (same material, volume, and length). Now in each case the resistance moment is proportional to the product of the cross-sectional area and the diameter. But the cross-sectional area of each column is the same, namely, volume/length. Therefore, the resistance moment must be proportional to the diameter, which, of course, is greater for the hollow cylinder. Hence, a hollow cylinder is more resistant to fracture. Although Galileo actually underestimated the relative strength of a hollow cylinder, his investigations marked the beginning of this unquestionably "new science", the very foundation of engineering theory. He pointed clearly to the direction requisite for further development.

Dialogues Concerning Two New Sciences†

But, in order to bring our daily conference to an end, I wish to discuss the strength of hollow solids, which are employed in art — and still oftener in nature — in a thousand operations for the purpose of greatly increasing strength without adding to weight; examples of these are seen in the bones of birds and in many kinds of reeds which are light and highly resistant both to bending and breaking. For if a stem of straw which carries a head of wheat heavier than the entire stalk were made up of the same amount of material in solid form it would offer less resistance to bending and breaking. This is an experience which has been verified and confirmed in practice where it is found that a

† Ref. (9), pp. 150–1.

hollow lance or a tube of wood or metal is much stronger than would be a solid one of the same length and weight, one which would necessarily be thinner; men have discovered, therefore, that in order to make lances strong as well as light they must make them hollow. We shall now show that:

> In the case of two cylinders, one hollow the other solid but having equal volumes and equal lengths, their resistances [bending strengths] are to each other in the ratio of their diameters.

Let AE denote a hollow cylinder and IN a solid one of the same weight and length; then, I say, that the resistance against fracture exhibited by the tube AE bears to that of the solid

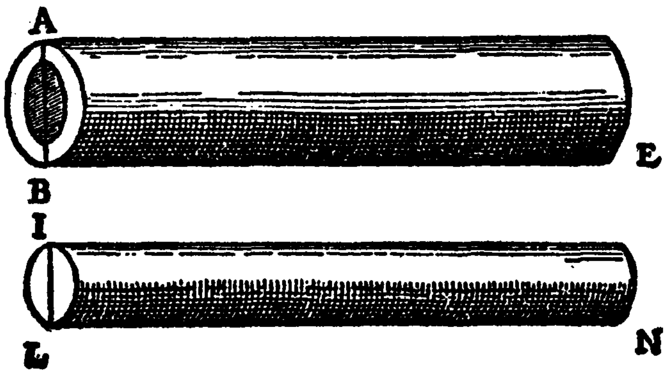


FIG. 29.

cylinder IN the same ratio as the diameter AB to the diameter IL. This is very evident; for since the tube and the solid cylinder IN have the same volume and length, the area of the circular base IL will be equal to that of the annulus AB which is the base of the tube AE. (By annulus is here meant the area which lies between two concentric circles of different radii.) Hence their resistances to a straight-away pull are equal; but in producing fracture by a transverse pull we employ, in the case of the cylinder IN, the length LN as one lever arm, the point L as a fulcrum, and the diameter LI, or its half, as the opposing lever arm: while in the case of the tube, the length BE which plays the part of the first lever arm is equal to LN, the opposing lever arm beyond the fulcrum, B, is the diameter AB, or its half. Manifestly then the resistance [bending strength] of the tube exceeds that of the solid cylinder in the proportion in which the diameter AB exceeds the diameter IL, which is the desired result. Thus the strength of a hollow tube exceeds that of a solid cylinder in the ratio of their diameters whenever the two are made of the same material and have the same weight and length.

CHAPTER 15

NATURAL OSCILLATIONS

(a) Simple Pendulum

Galileo was fascinated throughout his life by all phenomena involving oscillations — apparently beginning with his (legendary) observations of the swinging chandelier in the Cathedral of Pisa. His substitution of a *simple pendulum* for this ornate lamp is typical of his seeking a simplified physical model to investigate a complex phenomenon. Despite the desire of some historians to regard Galileo as a mathematical Platonist, Galileo's investigation of the simple pendulum appears to physicists to be a good example of the scientific method, namely, a search for related factors — not merely a simple linear proportion, not even a deduction from some *a priori* hypothesis, certainly not a so-called thought experiment. In the following passage he gave the basic law for comparing T_1 and T_2, the *periods* (time for a complete swing to and fro) of two pendula with lengths L_1 and L_2, respectively,

$$\frac{T_1}{T_2} = \sqrt{\frac{L_1}{L_2}} \, .$$

The very fact that Galileo expressed this relation in terms of an irrational quantity, a square root, indicates that he could not have been a traditional Pythagorean. He himself noted that one might prefer to express the relation as

$$\frac{T_1^2}{T_2^2} = \frac{L_1}{L_2} \, .$$

Nowadays the period would be given absolutely by the formula

$$T = 2\pi\sqrt{\frac{L_1}{g}} \, ,$$

where g is the acceleration due to gravity. It holds, however, only for small oscillations (for a displacement of 15° the error would be about 1.1%). Accordingly, there is some question as to whether Galileo's error (cf. ref. (7), p. 230) in assuming its correctness for any arc was due primarily to his poor time measurements or to his failure to examine carefully the periods for large arcs.

Already in his earlier notes (cf. ref. (3), p. 108) he had reported his observation that the oscillation of a lead bob lasts longer than that for a comparable wooden one (or cork; ref. (9), p. 85); he recognized also that the period is independent of the bob itself — except for any air resistance. He mentioned also (ref. (7), pp. 22, 26) that a bob would ascend to its initial height — were it not for this frictional loss. Here (ref. (7), p. 150), too, he illustrated the difference in persistence of motion for lead and for cotton.

He considered also the case of a cannon ball dropped through the center of the earth as analogous to the action of a pendulum (ref. (7), p. 236). He likened the oscillations of water in a container to that of a pendulum (ref. (7), p. 428). The regulation of time in a wheel clock, he observed (ref. (7), p. 449), could be adjusted in accordance with the pendulum law.

Dialogues Concerning Two New Sciences†

We come now to the other questions, relating to pendulums, a subject which may appear to many exceedingly arid, especially to those philosophers who are continually occupied with the more profound questions of nature. Nevertheless, the problem is one which I do not scorn. I am encouraged by the example of Aristotle whom I admire especially because he did not fail to discuss every subject which he thought in any degree worthy of consideration.

Impelled by your queries I may give you some of my ideas concerning certain problems in music, a splendid subject, upon which so many eminent men have written: among these is Aristotle himself who has discussed numerous interesting acoustical questions. Accordingly, if on the basis of some easy and tangible experiments, I shall explain some striking phenomena in the domain of sound, I trust my explanations will meet your approval.

SAGR. I shall receive them not only gratefully but eagerly. For, although I take pleasure in every kind of musical instrument and have paid considerable attention to harmony, I have never been able to fully understand why some combinations of tones are more pleasing than others, or why certain combinations not only fail to please but are even highly offensive. Then there is the old

† Ref. (9), pp. 94–8.

problem of two stretched strings in unison; when one of them is sounded, the other begins to vibrate and to emit its note; nor do I understand the different ratios of harmony [*forme delle consonanze*] and some other details.

SALV. Let us see whether we cannot derive from the pendulum a satisfactory solution of all these difficulties. And first, as to the question whether one and the same pendulum really performs its vibrations, large, medium, and small, all in exactly the same time, I shall rely upon what I have already heard from our Academician. He has clearly shown that the time of descent is the same along all chords, whatever the arcs which subtend them, as well along an arc of 180° (i.e., the whole diameter) as along one of 100°, 60°, 10°, 2°, ½°, or 4′. It is understood, of course, that these arcs all terminate at the lowest point of the circle, where it touches the horizontal plane.

If now we consider descent along arcs instead of their chords then, provided these do not exceed 90°, experiment shows that they are all traversed in equal times; but these times are greater for the chord than for the arc, an effect which is all the more remarkable because at first glance one would think just the opposite to be true. For since the terminal points of the two motions are the same and since the straight line included between these two points is the shortest distance between them, it would seem reasonable that motion along this line should be executed in the shortest time; but this is not the case, for the shortest time — and therefore the most rapid motion — is that employed along the arc of which this straight line is the chord.

As to the times of vibration of bodies suspended by threads of different lengths, they bear to each other the same proportion as the square roots of the lengths of the thread; or one might say the lengths are to each other as the squares of the times; so that if one wishes to make the vibration-time of one pendulum twice that of another, he must make its suspension four times as long. In like manner, if one pendulum has a suspension nine times as long as another, this second pendulum will execute three vibrations during each one of the first; from which it follows that the lengths of the suspending cords bear to each other the [inverse] ratio of the squares of the number of vibrations performed in the same time.

SAGR. Then, if I understand you correctly, I can easily measure the length of a string whose upper end is attached at any height whatever even if this end were invisible and I could see only the lower extremity. For if I attach to the lower end of this string a rather heavy weight and give it a to-and-fro motion, and if I ask a friend to count a number of its vibrations, while I, during the same time-interval, count the number of vibrations of a pendulum which is exactly one cubit in length, then knowing the number of vibrations which each pendulum makes in the given interval of time one can determine the length of the string. Suppose, for example, that my friend counts 20 vibrations of the long cord during the same time in which I count 240 of my string which is one cubit in length; taking the squares of the two numbers, 20 and 240, namely 400 and 57600, then, I say, the long string contains 57600 units of such length that my pendulum will contain 400 of them; and since the length of my string is one cubit, I shall divide 57600 by 400 and thus obtain 144. Accordingly I shall call the length of the string 144 cubits.

SALV. Nor will you miss it by as much as a hand's breadth, especially if you observe a large number of vibrations.

SAGR. You give me frequent occasion to admire the wealth and profusion of nature when, from such common and even trivial phenomena, you derive facts which are not only striking and new but which are often far removed from what we would have imagined. Thousands of times I have observed vibrations especially in churches where lamps, suspended by long cords, had been inadvertently set into motion; but the most which I could infer from these observations was that the view of those who think that such vibrations are maintained by the medium is highly improbable: for, in that case, the air must needs have considerable judgment and little else to do but kill time by pushing to and fro a pendent weight with perfect regularity. But I never dreamed of learning that one and the same body, when suspended from a string a hundred cubits long and pulled aside through an arc of 90° or even 1° or ½°, would employ the same time in passing through the least as through the largest of these arcs; and, indeed, it still strikes me as somewhat unlikely. Now I am waiting to hear how these same simple phenomena can furnish solutions for those acoustical problems — solutions which will be at least partly satisfactory.

SALV. First of all one must observe that each pendulum has its own time of vibration so definite and determinate that it is not possible to make it move with any other period [*altro periodo*] than that which nature has given it. For let any one take in his hand the cord to which the weight is attached and try, as much as he pleases, to increase or diminish the frequency [*frequenza*] of its vibrations; it will be time wasted. On the other hand, one can confer motion upon even a heavy pendulum which is at rest by simply blowing against it; by repeating these blasts with a frequency which is the same as that of the pendulum one can impart considerable motion. Suppose that by the first puff we have displaced the pendulum from the vertical by, say, half an inch; then if, after the pendulum has returned and is about to begin the second vibration, we add a second puff, we shall impart additional motion; and so on with other blasts provided they are applied at the right instant, and not when the pendulum is coming toward us since in this case the blast would impede rather than aid the motion. Continuing thus with many impulses [*impulsi*] we impart to the pendulum such momentum [*impeto*] that a greater impulse [*forza*] than that of a single blast will be needed to stop it.

SAGR. Even as a boy, I observed that one man alone by giving these impulses at the right instant was able to ring a bell so large that when four, or even six, men seized the rope and tried to stop it they were lifted from the ground, all of them together being unable to counterbalance the momentum which a single man, by properly-timed pulls, had given it.

(b) Vibrating Freely

When a one-dimensional string under tension T (gravity negligible) is plucked transversely, *waves* are propagated with a speed v

$$v = \sqrt{\frac{T}{\rho}} ,$$

where ρ is the mass per unit length. If the ends of the string are fixed, they reflect the progressive waves so that *stationary wave* patterns are set up, corresponding to different frequencies of vibration. The *fundamental* (i.e. the lowest frequency) occurs when the string (length L) vibrates as a whole (wavelength $= 2L$). Its period T_1 is the total distance over the speed, i.e.

$$T_1 = \frac{2L}{v} .$$

Hence, the fundamental (lowest) frequency $v_1(=1/T_1)$ is given by

$$v_1 = \frac{1}{2L}\sqrt{\frac{T}{\rho}} .$$

The higher frequencies are given generally by

$$v_1 = \frac{ni}{2L}\sqrt{\frac{T}{\rho}} , \quad i = 2, \ldots n ,$$

where n_i is an integer; these overtones are *harmonics*; for example, for $n = 2$ (corresponding to two vibrating segments) there occurs the octave, i.e. twice the fundamental frequency.

Evidently the frequency of a vibrating string may be raised by increasing the tension, or by decreasing the density (e.g. a thinner string for a given length), or by decreasing the length.

Père Marin Mersenne is usually credited with having found these *laws of a vibrating string*. Inasmuch as they were known to Galileo (cf. ref. (9), p. 100) even earlier, there is good reason to believe that he had already discovered them experientially — though not as precisely as Mersenne.

His analysis of the musical notes of a vibrating plate in terms of the spacing of the stationary patterns of shavings on it is an excellent illustration how his innate curiosity about any natural phenomenon led him to a deeper understanding of it. This incident predates the later discovery of the now well-known *Chladni figures* (Ernst Chladni (1756–1827)).

Dialogues Concerning Two New Sciences†

(c) Resonance

Any body will vibrate freely with certain characteristic frequencies. If an external stimulus has the same frequency, the body will readily respond in unison. It was Galileo who first recognized this phenomenon of *resonance.* He noted that the corresponding strings of one instrument could be set in motion by the vibrating strings of a neighboring one via sound waves in the air.

Dialogues Concerning Two New Sciences†

(d) Musical Intervals

The Greeks had observed that harmonious notes from a given vibrating string are always in the ratio of the small integers, describing the number of its vibrating segments; hence inversely as the lengths of the smallest segments. It was Galileo, however, who identified the pitch of a sound with the frequency of its originating vibration. A musical octave, of course, occurs when the strings have two segments (half the string's length) each vibrating with double the frequency.

From the law of vibrating strings we have

$$v \text{ fundamental} = \frac{1}{2L}\sqrt{\frac{T}{P}}\,,$$

and

$$v \text{ octave} = \frac{2}{2L}\sqrt{\frac{T}{P}}\,.$$

Hence, the frequency of the octave is double that of the fundamental. Galileo, accordingly, proposed to measure the *musical interval* between two notes by the ratio of their frequencies. He applied his knowledge of the law of vibrating strings to the general case of musical intervals for strings of different materials and dimensions. His explanation of such consonance is basically acceptable even today.

Galileo's investigations of such oscillations was the beginning of the modern science of acoustics.

Dialogues Concerning Two New Sciences†

SALV. Your illustration makes my meaning clear and is quite as well fitted, as what I have just said, to explain the wonderful phenomenon of the strings of the cittern [*cetera*] or of the spinet

† Ref. (9), pp. 98–104.

[*cimbalo*], namely, the fact that a vibrating string will set another string in motion and cause it to sound not only when the latter is in unison but even when it differs from the former by an octave or a fifth. A string which has been struck begins to vibrate and continues the motion as long as one hears the sound [*risonanza*]; these vibrations cause the immediately surrounding air to vibrate and quiver; then these ripples in the air expand far into space and strike not only all the strings of the same instrument but even those of neighboring instruments. Since that string which is tuned to unison with the one plucked is capable of vibrating with the same frequency, it acquires, at the first impulse, a slight oscillation; after receiving two, three, twenty, or more impulses, delivered at proper intervals, it finally accumulates a vibratory motion equal to that of the plucked string, as is clearly shown by equality of amplitude in their vibrations. This undulation expands through the air and sets into vibration not only strings, but also any other body which happens to have the same period as that of the plucked string. Accordingly if we attach to the side of an instrument small pieces of bristle or other flexible bodies, we shall observe that, when a spinet is sounded, only those pieces respond that have the same period as the string which has been struck; the remaining pieces do not vibrate in response to this string, nor do the former pieces respond to any other tone.

If one bows the base string on a viola rather smartly and brings near it a goblet of fine, thin glass having the same tone [*tuono*] as that of the string, this goblet will vibrate and audibly resound. That the undulations of the medium are widely dispersed about the sounding body is evinced by the fact that a glass of water may be made to emit a tone merely by the friction of the finger-tip upon the rim of the glass; for in this water is produced a series of regular waves. The same phenomenon is observed to better advantage by fixing the base of the goblet upon the bottom of a rather large vessel of water filled nearly to the edge of the goblet; for if, as before, we sound the glass by friction of the finger, we shall see ripples spreading with the utmost regularity and with high speed to large distances about the glass. I have often remarked, in thus sounding a rather large glass nearly full of water, that at first the waves are spaced with great uniformity, and when, as sometimes happens, the tone of the glass jumps an octave higher I have noted that at this moment each of the aforesaid waves divides into two; a phenomenon which shows clearly that the ratio involved in the octave [*forma dell' ottava*] is two.

SAGR. More than once have I observed this same thing, much to my delight and also to my profit. For a long time I have been perplexed about these different harmonies since the explanations hitherto given by those learned in music impress me as not sufficiently conclusive. They tell us that the diapason, i.e. the octave, involves the ratio of two, that the diapente which we call the fifth involves a ratio of 3:2, etc.; because if the open string of a monochord be sounded and afterwards a bridge be placed in the middle and the half length be sounded one hears the octave; and if the bridge be placed at 1/3 the length of the string, then on plucking first the open string and afterwards 2/3 of its length, the fifth is given; for this reason they say that the octave depends upon the ratio of two to one [*contenuta tra'l due e l'uno*] and the fifth upon the ratio of three to two. This explanation does not impress me as sufficient to establish 2 and 3/2 as the natural ratios of the octave and the fifth; and my reason for thinking so is as follows. There are three different ways in which the tone of a string may be sharpened, namely, by shortening it, by stretching it and by making it thinner. If the tension and size of the string remain constant one obtains the octave by shortening it to one-half, i.e., by sounding first the open string and then one-half of it; but if length and size remain constant and one attempts to produce the octave by stretching he will find that it does not suffice to double the stretching weight; it must be quadrupled; so that, if the fundamental note is produced by a weight of one pound, four will be required to bring out the octave.

And finally if the length and tension remain constant, while one changes the size of the string he will find that in order to produce the octave the size must be reduced to 1/4 that which gave the fundamental. And what I have said concerning the octave, namely, that its ratio as derived from the tension and size of the string is the square of that derived from the length, applies equally well to all other musical intervals [*intervalli musici*]. Thus if one wishes to produce a fifth by changing the length he finds that the ratio of the lengths must be sesquialteral, in other words he sounds first the open string, then two-thirds of it; but if he wishes to produce this same result by stretching or thinning the string, then it becomes necessary to square the ratio 3/2 that is by taking 9/4 [*dupla sesquiquarta*]; accordingly, if the fundamental requires a weight of 4 pounds, the higher note will be produced not by 6, but by 9 pounds; the same is true in regard to size, the string which gives the fundamental is larger than that which yields the fifth in the ratio of 9 to 4.

In view of these facts, I see no reason why those wise philosophers should adopt 2 rather than 4 as the ratio of the octave, or why in the case of the fifth they should employ the sesquialteral ratio, 3/2, rather than that of 9/4. Since it is impossible to count the vibrations of a sounding string on account of its high frequency, I should still have been in doubt as to whether a string, emitting the upper octave, made twice as many vibrations in the same time as one giving the fundamental, had it not been for the following fact, namely, that at the instant when the tone jumps to the octave, the waves which constantly accompany the vibrating glass divide up into smaller ones which are precisely half as long as the former.

SALV. This is a beautiful experiment enabling us to distinguish individually the waves which are produced by the vibrations of a sonorous body, which spread through the air, bringing to the tympanum of the ear a stimulus which the mind translates into sound. But since these waves in the water last only so long as the friction of the finger continues and are, even then, not constant but are always forming and disappearing, would it not be a fine thing if one had the ability to produce waves which would persist for a long while, even months and years, so as to easily measure and count them?

SAGR. Such an invention would, I assure you, command my admiration.

SALV. The device is one which I hit upon by accident; my part consists merely in the observation of it and in the appreciation of its value as a confirmation of something to which I had given profound consideration; and yet the device is, in itself, rather common. As I was scraping a brass plate with a sharp iron chisel in order to remove some spots from it and was running the chisel rather rapidly over it, I once or twice, during many strokes, heard the plate emit a rather strong and clear whistling sound; on looking at the plate more carefully, I noticed a long row of fine streaks parallel and equidistant from one another. Scraping with the chisel over and over again, I noticed that it was only when the plate emitted this hissing noise that any marks were left upon it; when the scraping was not accompanied by this sibilant note there was not the least trace of such marks. Repeating the trick several times and making the stroke, now with greater now with less speed, the whistling followed with a pitch which was correspondingly higher and lower. I noted also that the marks made when the tones were higher were closer together; but when the tones were deeper, they were farther apart. I also observed that when, during a single stroke, the speed increased toward the end the sound became sharper and the streaks grew closer together, but always in such a way as to remain sharply defined and equidistant. Besides whenever the stroke was accompanied by hissing I felt the chisel tremble in my grasp and a sort of shiver run through my hand. In short we see and hear in the case of the chisel precisely that which is seen and heard in the case of a whisper followed by a loud voice; for, when the breath is emitted without the production of a tone, one does not feel either in the throat or mouth any motion to speak of in comparison with that which is felt in the larynx and upper part of the throat when the voice is used, especially when the tones employed are low and strong.

At times I have also observed among the strings of the spinet two which were in unison with two of the tones produced by the aforesaid scraping; and among those which differed most in pitch I found two which were separated by an interval of a perfect fifth. Upon measuring the distance between the markings produced by the two scrapings it was found that the space which contained 45 of one contained 30 of the other, which is precisely the ratio assigned to the fifth.

But now before proceeding any farther I want to call your attention to the fact that, of the three methods for sharpening a tone, the one which you refer to as the fineness of the string should be attributed to its weight. So long as the material of the string is unchanged, the size and weight vary in the same ratio. Thus in the case of gut-strings, we obtain the octave by making one string 4 times as large as the other; so also in the case of brass one wire must have 4 times the size of the other; but if now we wish to obtain the octave of a gut-string, by use of brass wire, we must make it, not four times as large, but four times as heavy as the gut-string: as regards size therefore the metal string is not four times as big but four times as heavy. The wire may therefore be even thinner than the gut notwithstanding the fact that the latter gives the higher note. Hence if two spinets are strung, one with gold wire the other with brass, and if the corresponding strings each have the same length, diameter, and tension it follows that the instrument strung with gold will have a pitch about one-fifth lower than the other because gold has a density almost twice that of brass. And here it is to be noted that it is the weight rather than the size of a moving body which offers resistance to change of motion [*velocità del moto*] contrary to what one might at first glance think. For it seems reasonable to believe that a body which is large and light should suffer greater retardation of motion in thrusting aside the medium than would one which is thin and heavy; yet here exactly the opposite is true.

Returning now to the original subject of discussion, I assert that the ratio of a musical interval is not immediately determined either by the length, size, or tension of the strings but rather by the ratio of their frequencies, that is, by the number of pulses of air waves which strike the tympanum of the ear, causing it also to vibrate with the same frequency. This fact established, we may possibly explain why certain pairs of notes, differing in pitch produce a pleasing sensation, others a less pleasant effect, and still others a disagreeable sensation. Such an explanation would be tantamount to an explanation of the more or less perfect consonances and of dissonances. The unpleasant sensation produced by the latter arises, I think, from the discordant vibrations of two different tones which strike the ear out of time [*sproporzionatamente*]. Especially harsh is the dissonance between notes whose frequencies are incommensurable; such a case occurs when one has two strings in unison and sounds one of them open, together with a part of the other which bears the same ratio to its whole length as the side of a square bears to the diagonal; this yields a dissonance similar to the augmented fourth or diminished fifth [*tritono o semidiapente*].

Agreeable consonances are pairs of tones which strike the ear with a certain regularity; this regularity consists in the fact that the pulses delivered by the two tones, in the same interval of time, shall be commensurable in number, so as not to keep the ear drum in perpetual torment, bending in two different directions in order to yield to the ever-discordant impulses.

The first and most pleasing consonance is, therefore, the octave since, for every pulse given to the tympanum by the lower string, the sharp string delivers two; accordingly at every other vibration of the upper string both pulses are delivered simultaneously so that one-half the entire number of pulses are delivered in unison. But when two strings are in unison their vibrations always coincide and the effect is that of a single string; hence we do not refer to it as consonance. The fifth is also a pleasing interval since for every two vibrations of the lower string the upper one gives three, so that considering the entire number of pulses from the upper string one-third of them will strike in unison, i.e., between each pair of concordant vibrations there intervene two single vibrations; and when the interval is a fourth, three single vibrations intervene. In case the interval is a second where the ratio is 9/8 it is only every ninth vibration of the upper string which reaches the ear simultaneously with one of the lower; all the others are discordant and produce a harsh effect upon the recipient ear which interprets them as dissonances.

CHAPTER 16

FALLING BODIES

(a) How Fast?

Although the ancients used the notion of speed, they were more concerned with the fact that a body went from one place to another — and why, rather than how and how much. Comparisons of speeds were made only in the form of ratios; there was little interest in speed *per se*; for example, in its variation along the path of the moving body. Galileo, however, carefully defined uniform motion, particularly with respect to all intervals of time, large or small. (He did not have a clear idea of so-called *instantaneous speed*, which is defined mathematically in terms of a derivative, i.e. a limit; he did regard a body as passing through all degrees of speed, including zero speed as one infinitely slow.) His analysis was based upon four axioms which guaranteed that the distance traversed is a monotonically increasing function of the speed and time — a linear proportionality is tacitly assumed.

Galileo suggested a method for measuring the speed of light; namely, to determine the time for a signal to go from one spot to another several miles away. Despite the failure to record actually any time difference for a distance less than a mile, he believed from his observation of the spreading of a lightning flash among the clouds that a finite time is involved. Later the Accademia del Cimento repeated this experiment, again with a negative result. The *speed of sound*, however, was successfully determined by the Academy, viz. by noting the time for a discharged cannon at a known distance away to be heard after the flash had been observed.

Dialogues Concerning Two New Sciences†

UNIFORM MOTION

In dealing with steady or uniform motion, we need a single definition which I give as follows:

† Ref. (9), pp. 154–5, 42–4.

Definition

By steady or uniform motion, I mean one in which the distances traversed by the moving particle during any equal intervals of time, are themselves equal.

Caution

We must add to the old definition (which defined steady motion simply as one in which equal distances are traversed in equal times) the word "any," meaning by this, all equal intervals of time; for it may happen that the moving body will traverse equal distances during some equal intervals of time and yet the distances traversed during some small portion of these time-intervals may not be equal, even though the time-intervals be equal.

From the above definition, four axioms follow, namely:

Axiom I

In the case of one and the same uniform motion, the distance traversed during a longer interval of time is greater than the distance traversed during a shorter interval of time.

Axiom II

In the case of one and the same uniform motion, the time required to traverse a greater distance is longer than the time required for a less distance.

Axiom III

In one and the same interval of time, the distance traversed at a greater speed is larger than the distance traversed at a less speed.

Axiom IV

The speed required to traverse a longer distance is greater than that required to traverse a shorter distance during the same time-interval.

SAGR. But of what kind and how great must we consider this speed of light to be? Is it instantaneous or momentary or does it like other motions require time? Can we not decide this by experiment?

SIMP. Everyday experience shows that the propagation of light is instantaneous; for when we see a piece of artillery fired, at great distance, the flash reaches our eyes without lapse of time; but the sound reaches the ear only after a noticeable interval.

SAGR. Well, Simplicio, the only thing I am able to infer from this familiar bit of experience is that sound, in reaching our ear, travels more slowly than light; it does not inform me whether the coming of the light is instantaneous or whether, although extremely rapid, it still occupies time. An observation of this kind tells us nothing more than one in which it is claimed that "As soon as the sun reaches the horizon its light reaches our eyes"; but who will assure me that these rays had not reached this limit earlier than they reached our vision?

SALV. The small conclusiveness of these and other similar observations once led me to devise a method by which one might accurately ascertain whether illumination, i.e., the propagation of light, is really instantaneous. The fact that the speed of sound is as high as it is, assures us that the motion of light cannot fail to be extraordinarily swift. The experiment which I devised was as follows:

Let each of two persons take a light contained in a lantern, or other receptacle, such that by the interposition of the hand, the one can shut off or admit the light to the vision of the other. Next let them stand opposite each other at a distance of a few cubits and practice until they acquire such skill in uncovering and occulting their lights that the instant one sees the light of his companion he will uncover his own. After a few trials the response will be so prompt that without sensible error [*svario*] the uncovering of one light is immediately followed by the uncovering of the other, so that as soon as one exposes his light he will instantly see that of the other. Having acquired skill at this short distance let the two experimenters, equipped as before, take up positions separated by a distance of two or three miles and let them perform the same experiment at night, noting carefully whether the exposures and occultations occur in the same manner as at short distances; if they do, we may safely conclude that the propagation of light is instantaneous; but if time is required at a distance of three miles which, considering the going of one light and the coming of the other, really amounts to six, then the delay ought to be easily observable. If the experiment is to be made at still greater distances, say eight or ten miles, telescopes may be employed, each observer adjusting one for himself at the place where he is to make the experiment at night; then although the lights are not large and are therefore invisible to the naked eye at so great a distance, they can readily be covered and uncovered since by aid of the telescopes, once adjusted and fixed, they will become easily visible.

SAGR. This experiment strikes me as a clever and reliable invention. But tell us what you conclude from the results.

SALV. In fact I have tried the experiment only at a short distance, less than a mile, from which I have not been able to ascertain with certainty whether the appearance of the opposite light was instantaneous or not; but if not instantaneous it is extraordinarily rapid — I should call it momentary; and for the present I should compare it to motion which we see in the lightning flash between clouds eight or ten miles distant from us. We see the beginning of this light — I might say its head and source — located at a particular place among the clouds; but it immediately spreads to the surrounding ones, which seems to be an argument that at least some time is required for propagation; for if the illumination were instantaneous and not gradual, we should not be able to distinguish its origin — its center, so to speak — from its outlying portions. What a sea we are gradually slipping into without knowing it! With vacua and infinities and indivisibles and instantaneous motions, shall we ever be able, even by means of a thousand discussions, to reach dry land?

(b) Inertia

There has been much controversy as to Galileo's contribution to our modern conception of inertia. On the one hand, he has been praised for having been solely responsible for its introduction; on the other hand, he has been accused of not even understanding it. The truth, I suppose, is somewhere between these extreme views. Without question Galileo did not conceive of the generalized axiom that was later proposed by Isaac Newton (1642–1727) as the first law of motion; nor did he apply any such inertial principle to celestial motions.

Inklings of the inertial idea are found in his early notes "On Mechanics", where he associated rest and motion as two aspects of the same state — contrary to Aristotle's wide separation. In his notes "On Motion", he had previously suggested circular motion with constant speed as a neutral, third type of motion, neither natural nor violent. The first statement of the inertia principle was given (1607) in a letter to him; his first public reference in the second Sunspot letter (1613) — repeated (ref. (7), p. 147) in 1632, where he also specifically mentioned the forward lunges of a passenger in a boat that stops suddenly. It is in the *Two New Sciences*, however, that the physical concept of *inertia* is definitely formulated and practically utilized.

Galileo had no clear conception of Newton's second law of motion. Nevertheless, he did recognize that the *unbalanced force* on a body is associated with its acceleration and, in the case of the free fall of bodies in a medium, that it is proportional to the acceleration (or resulting speed from rest).

Dialogues Concerning Two New Sciences †

Furthermore we may remark that any velocity once imparted to a moving body will be rigidly maintained as long as the external causes of acceleration or retardation are removed, a condition which is found only on horizontal planes; for in the case of planes which slope downwards there is already present a cause of acceleration, while on planes sloping upward there is retardation; from this it follows that motion along a horizontal plane is perpetual; for, if the velocity be uniform, it cannot be diminished or slackened, much less destroyed. Further, although any velocity which a body may have acquired through natural fall is permanently maintained so far as its own nature [*suapte natura*] is concerned, yet it must be remembered that if, after descent along a plane inclined downwards, the body is deflected to a plane inclined upward, there is already existing in this latter plane a cause of retardation; for in any such plane this same body is subject to a natural acceleration downwards. Accordingly we have here the superposition of two different states, namely, the velocity acquired during the preceding fall which if acting alone

† Ref. (9), pp. 215-8, 244.

would carry the body at a uniform rate to infinity, and the velocity which results from a natural acceleration downwards common to all bodies. It seems altogether reasonable, therefore, if we wish to trace the future history of a body which has descended along some inclined plane and has been deflected along some plane inclined upwards, for us to assume that the maximum speed acquired during descent is permanently maintained during the ascent. In the ascent, however, there supervenes a natural inclination downwards, namely, a motion which, starting from rest, is accelerated at the usual rate. If perhaps this discussion is a little obscure, the following figure will help to make it clearer.

Let us suppose that the descent has been made along the downward sloping plane AB, from which the body is deflected so as to continue its motion along the upward sloping plane BC; and first let these planes be of equal length and placed so as to make equal angles with the horizontal line GH. Now it is well known that a body, starting from rest at A, and descending along AB, acquires a speed which is proportional to the time, which is a maximum at B, and which is maintained by the body so long as all causes of fresh acceleration or retardation are removed; the

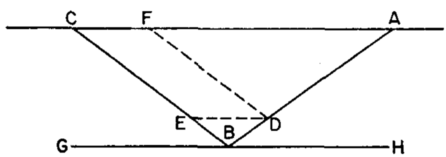


FIG. 30.

acceleration to which I refer is that to which the body would be subject if its motion were continued along the plane AB extended, while the retardation is that which the body would encounter if its motion were deflected along the plane BC inclined upwards; but, upon the horizontal plane GH, the body would maintain a uniform velocity equal to that which it had acquired at B after fall from A; moreover this velocity is such that, during an interval of time equal to the time of descent through AB, the body will traverse a horizontal distance equal to twice AB. Now let us imagine this same body to move with the same uniform speed along the plane BC so that here also during a time-interval equal to that of descent along AB, it will traverse along BC extended a distance twice AB; but let us suppose that, at the very instant the body begins its ascent it is subjected, by its very nature, to the same influences which surrounded it during its descent from A along AB, namely, it descends from rest under the same acceleration as that which was effective in AB, and it traverses, during an equal interval of time, the same distance along this second plane as it did along AB; it is clear that, by thus superposing upon the body a uniform motion of ascent and an accelerated motion of descent, it will be carried along the plane BC as far as the point C where these two velocities become equal.

If now we assume any two points D and E, equally distant from the vertex B, we may then infer that the descent along BD takes place in the same time as the ascent along BE. Draw DF

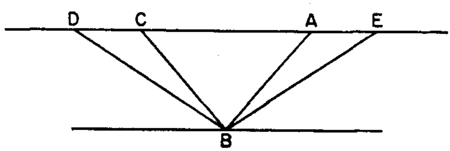


FIG. 31.

parallel to BC; we know that, after descent along AD, the body will ascend along DF; or, if, on reaching D, the body is carried along the horizontal DE, it will reach E with the same momentum [*impetus*] with which it left D; hence from E the body will ascend as far as C, proving that the velocity at E is the same as that at D.

From this we may logically infer that a body which descends along any inclined plane and continues its motion along a plane inclined upwards will, on account of the momentum acquired, ascend to an equal height above the horizontal; so that if the descent is along AB the body will be carried up the plane BC as far as the horizontal line ACD: and this is true whether the inclinations of the planes are the same or different, as in the case of the planes AB and BD. But by a previous postulate the speeds acquired by fall along variously inclined planes having the same vertical height are the same. If therefore the planes EB and BD have the same slope, the descent along EB will be able to drive the body along BD as far as D; and since this propulsion comes from the speed acquired on reaching the point B, it follows that this speed at B is the same whether the body has made its descent along AB or EB. Evidently then the body will be carried up BD whether the descent has been made along AB or along EB. The time of ascent along BD is however greater than that along BC, just as the descent along EB occupies more time that that along AB; moreover it has been demonstrated that the ratio between the lengths of these times is the same as that between the lengths of the planes.

SALVIATI. Once more, Simplicio is here on time; so let us without delay take up the question of motion. The text of our Author is as follows:

THE MOTION OF PROJECTILES

In the preceding pages we have discussed the properties of uniform motion and of motion naturally accelerated along planes of all inclinations. I now propose to set forth those properties which belong to a body whose motion is compounded of two other motions, namely, one uniform and one naturally accelerated; these properties, well worth knowing, I propose to demonstrate in a rigid manner. This is the kind of motion seen in a moving projectile; its origin I conceive to be as follows:

Imagine any particle projected along a horizontal plane without friction; then we know, from what has been more fully explained in the preceding pages, that this particle will move along this same plane with a motion which is uniform and perpetual, provided the plane has no limits. But if the plane is limited and elevated, then the moving particle, which we imagine to be a heavy one, will on passing over the edge of the plane acquire, in addition to its previous uniform and perpetual motion, a downward propensity due to its own weight; so that the resulting motion which I call projection [*projectio*], is compounded of one which is uniform and horizontal and of another which is vertical and naturally accelerated.

(c) A Thought Experiment

Aristotle had argued that the speed of a freely falling body should be in proportion to its weight, the "cause" of its fall. To test this conclusion, Galileo proposed a so-called (critical) thought-experiment. Consider the combination of a large weight with a small one. On the one hand, the large one should be retarded by the slower, small weight; on the other hand, the combination, having a still greater weight, should move faster even than the large one alone. Obviously, these inconsistent deductions must be based upon a false assumption, namely, the proportion between speed and weight. Galileo indicated physically how any such logical inconsistencies disappear if all bodies are assumed to fall freely with the same speed.

Dialogues Concerning Two New Sciences†

SALV. I greatly doubt that Aristotle ever tested by experiment whether it be true that two stones, one weighing ten times as much as the other, if allowed to fall, at the same instant, from a height of, say, 100 cubits, would so differ in speed that when the heavier had reached the ground, the other would not have fallen more than 10 cubits.

SIMP. His language would seem to indicate that he had tried the experiment, because he says: *We see the heavier*; now the word *see* shows that he had made the experiment.

SAGR. But I, Simplicio, who have made the test can assure you that a cannon ball weighing one or two hundred pounds, or even more, will not reach the ground by as much as a span ahead of a musket ball weighing only half a pound, provided both are dropped from a height of 200 cubits.

† Ref. (9), pp. 62–7.

SALV. But, even without further experiment, it is possible to prove clearly, by means of a short and conclusive argument, that a heavier body does not move more rapidly than a lighter one provided both bodies are of the same material and in short such as those mentioned by Aristotle. But tell me, Simplicio, whether you admit that each falling body acquires a definite speed fixed by nature, a velocity which cannot be increased or diminished except by the use of force [*violenza*] or resistance.

SIMP. There can be no doubt but that one and the same body moving in a single medium has a fixed velocity which is determined by nature and which cannot be increased except by the addition of momentum [*impeto*] or diminished except by some resistance which retards it.

SALV. If then we take two bodies whose natural speeds are different, it is clear that on uniting the two, the more rapid one will be partly retarded by the slower, and the slower will be somewhat hastened by the swifter. Do you not agree with me in this opinion?

SIMP. You are unquestionably right.

SALV. But if this is true, and if a large stone moves with a speed of, say, eight while a smaller moves with a speed of four, then when they are united, the system will move with a speed less than eight; but the two stones when tied together make a stone larger than that which before moved with a speed of eight. Hence the heavier body moves with less speed than the lighter; an effect which is contrary to your supposition. Thus you see how, from your assumption that the heavier body moves more rapidly than the lighter one, I infer that the heavier body moves more slowly.

SIMP. I am all at sea because it appears to me that the smaller stone when added to the larger increases its weight and by adding weight I do not see how it can fail to increase its speed or, at least, not to diminish it.

SALV. Here again you are in error, Simplicio, because it is not true that the smaller stone adds weight to the larger.

SIMP. This is, indeed, quite beyond my comprehension.

SALV. It will not be beyond you when I have once shown you the mistake under which you are laboring. Note that it is necessary to distinguish between heavy bodies in motion and the same bodies at rest. A large stone placed in a balance not only acquires additional weight by having another stone placed upon it, but even by the addition of a handful of hemp its weight is augmented six to ten ounces according to the quantity of hemp. But if you tie the hemp to the stone and allow them to fall freely from some height, do you believe that the hemp will press down upon the stone and thus accelerate its motion or do you think the motion will be retarded by a partial upward pressure? One always feels the pressure upon his shoulders when he prevents the motion of a load resting upon him; but if one descends just as rapidly as the load would fall how can it gravitate or press upon him? Do you not see that this would be the same as trying to strike a man with a lance when he is running away from you with a speed which is equal to, or even greater, than that with which you are following him? You must therefore conclude that, during free and natural fall, the small stone does not press upon the larger and consequently does not increase its weight as it does when at rest.

SIMP. But what if we should place the larger stone upon the smaller?

SALV. Its weight would be increased if the larger stone moved more rapidly; but we have already concluded that when the small stone moves more slowly it retards to some extent the speed of the larger, so that the combination of the two, which is a heavier body than the larger of the two stones, would move less rapidly, a conclusion which is contrary to your hypothesis. We infer therefore that large and small bodies move with the same speed provided they are of the same specific gravity.

(d) The Medium's Role

Nowadays in analyzing the free fall of a body we would begin by identifying less important influences, and then proceed to neglect them, if possible. Our everyday experience, for example, would lead us to regard the medium as having a less significant role than the attracting earth. Galileo, however, concentrated initially upon the medium itself, which seemed all-important in common phenomena such as the dropping of a heavy stone and the fluttering of a light leaf, or the fall of a solid gold metal and a thin gold leaf. He began, therefore, by investigating the behavior of bodies in different media.

Aristotle had supposed that a body's speed in a medium varies inversely as the medium's density (regarded as its "resistance" to the motion). On this basis, the same body should move much more rapidly in air than in water. Inasmuch as Aristotle had supposed water to be only 10 times denser than air, in general, a body should fall with one-tenth the speed in water as compared with that in air. If, on the other hand, a material with a density one-tenth that of water, and consequently with one-tenth the speed in water, were to fall in air, its speed there should be equivalent to that of similarly dense wood, which, however, actually rises in water. A real dilemma! The assumed role of the medium appeared questionable.

Let us consider the problem in modern terms. The unbalanced force F on an immersed body of density D and volume V in a medium of density d is

$$F = (DV - dV)g \ ,$$

where g is the acceleration due to gravity. By Newton's *second law of motion*

$$F = (DV)a \ ,$$

where DV is the mass of the body and a its resulting acceleration.

For the same body falling from rest in two media of densities d and d', we have for their resulting speeds v and by v', respectively,

$$\frac{v}{v'} = \frac{a}{a'} = \frac{D-d}{D-d'} \ .$$

For Galileo's particular example of a body ($D = 1.2$), in air ($d = 0$) and in water ($d = 1$), we have

$$\frac{v}{v'} = 6 \ .$$

For two bodies (1 and 2) of the same volume in the same medium, we have

$$\frac{(D_1-d)}{(D_2-d)} = \frac{D_1a_1}{D_2a_2} \ .$$

For free fall from rest, with negligible friction, the speed is proportional to the acceleration; hence in the same time

$$\frac{a_1}{a_2} = \frac{v_1}{v_2} \ .$$

Therefore,

$$\frac{v_1}{v_2} = \frac{D_1-d}{D_2-d} \cdot \frac{D_2}{D_1} \ ,$$

or,

$$\frac{v_1}{v_2} = \frac{1-d/D_1}{1-d/D_2} \ .$$

In the case of a medium of neglible density, say, air, the speeds of new bodies differing only in weight would be equal.
Let us take Galileo's example of a marble egg and a hen's egg falling in water. The densities are approximately 2.7, 1.2, and 1.0, respectively, in cgs units. Therefore

$$\frac{v_1}{v_2} = 8.5.$$

Hence the marble egg would fall much faster than the hen's egg and their distance apart would further increase with time. On the other hand, if the density of the medium were gradually decreased, the relative speed would approach unity and the distance apart zero. On this basis, Galileo concluded that "in a vacuum all bodies would fall with the same speed" — not an *a priori* assumption, but an experimental extrapolation.

Dialogues Concerning Two New Sciences†

SIMP. Your discussion is really admirable; yet I do not find it easy to believe that a bird-shot falls as swiftly as a cannon ball.

SALV. Why not say a grain of sand as rapidly as a grindstone? But, Simplicio, I trust you will not follow the example of many others who divert the discussion from its main intent and fasten upon some statement of mine which lacks a hair's-breadth of the truth and, under this hair, hide the fault of another which is as big as a ship's cable. Aristotle says that "an iron ball of one hundred pounds falling from a height of one hundred cubits reaches the ground before a one-pound ball has fallen a single cubit." I say that they arrive at the same time. You find, on making the experiment, that the larger outstrips the smaller by two finger-breadths, that is, when the larger has reached the ground, the other is short of it by two finger-breadths; now you would not hide behind these two fingers the ninety-nine cubits of Aristotle, nor would you mention my small error and at the same time pass over in silence his very large one. Aristotle declares that bodies of different weights, in the same medium, travel (in so far as their motion depends upon gravity) with speeds which are proportional to their weights; this he illustrates by use of bodies in which it is possible to perceive the pure and unadulterated

† Ref. (9), pp. 64–8, 71–7.

effect of gravity, eliminating other considerations, for example, figure as being of small importance [*minimi momenti*], influences which are greatly dependent upon the medium which modifies the single effect of gravity alone. Thus we observe that gold, the densest of all substances, when beaten out into a very thin leaf, goes floating through the air; the same thing happens with stone when ground into a very fine powder. But if you wish to maintain the general proposition you will have to show that the same ratio of speeds is preserved in the case of all heavy bodies, and that a stone of twenty pounds moves ten times as rapidly as one of two; but I claim that this is false and that, if they fall from a height of fifty or a hundred cubits, they will reach the earth at the same moment.

SIMP. Perhaps the result would be different if the fall took place not from a few cubits but from some thousands of cubits.

SALV. If this were what Aristotle meant you would burden him with another error which would amount to a falsehood; because, since there is no such sheer height available on earth, it is clear that Aristotle could not have made the experiment; yet he wishes to give us the impression of his having performed it when he speaks of such an effect as one which we see.

SIMP. In fact, Aristotle does not employ this principle, but uses the other one which is not, I believe, subject to these same difficulties.

SALV. But the one is as false as the other; and I am surprised that you yourself do not see the fallacy and that you do not perceive that if it were true that, in media of different densities and different resistances, such as water and air, one and the same body moved in air more rapidly than in water, in proportion as the density of water is greater than that of air, then it would follow that any body which falls through air ought also to fall through water. But this conclusion is false inasmuch as many bodies which descend in air not only do not descend in water, but actually rise.

SIMP. I do not understand the necessity of your inference; and in addition I will say that Aristotle discusses only those bodies which fall in both media, not those which fall in air but rise in water.

SALV. The arguments which you advance for the Philosopher are such as he himself would have certainly avoided so as not to aggravate his first mistake. But tell me now whether the density [*corpulenza*] of the water, or whatever it may be that retards the motion, bears a definite ratio to the density of air which is less retardative; and if so fix a value for it at your pleasure.

SIMP. Such a ratio does exist; let us assume it to be ten; then, for a body which falls in both these media, the speed in water will be ten times slower than in air.

SALV. I shall now take one of those bodies which fall in air but not in water, say a wooden ball, and I shall ask you to assign to it any speed you please for its descent through air.

SIMP. Let us suppose it moves with a speed of twenty.

SALV. Very well. Then it is clear that this speed bears to some smaller speed the same ratio as the density of water bears to that of air; and the value of this smaller speed is two. So that really if we follow exactly the assumption of Aristotle we ought to infer that the wooden ball which falls in air, a substance ten times less-resisting than water, with a speed of twenty would fall in water with a speed of two, instead of coming to the surface from the bottom as it does; unless perhaps you wish to reply, which I do not believe you will, that the rising of the wood through the water is the same as its falling with a speed of two. But since the wooden ball does not go to the bottom, I think you will agree with me that we can find a ball of another material, not wood, which does fall in water with a speed of two.

SIMP. Undoubtedly we can; but it must be of a substance considerably heavier than wood.

SALV. That is it exactly. But if this second ball falls in water with a speed of two, what will be its speed of descent in air? If you hold to the rule of Aristotle you must reply that it will move at the rate of twenty; but twenty is the speed which you yourself have already assigned to the wooden ball; hence this and the other heavier ball will each move through air with the same speed. But now how does the Philosopher harmonize this result with his other, namely, that bodies of different weight move through the same medium with different speeds — speeds which are proportional to their weights? But without going into the matter more deeply, how have these common and obvious properties escaped your notice? Have you not observed that two bodies which fall in water, one with a speed a hundred times as great as that of the other, will fall in air with speeds so nearly equal that one will not surpass the other by as much as one hundredth part? Thus, for example, an egg made of marble will descend in water one hundred times more rapidly than a hen's egg, while in air falling from a height of twenty cubits the one will fall short of the other by less than four finger-breadths. In short, a heavy body which sinks through ten cubits of water in three hours will traverse ten cubits of air in one or two pulse-beats; and if the heavy body be a ball of lead it will easily traverse the ten cubits of water in less than double the time required for ten cubits of air. And here, I am sure, Simplicio, you find no ground for difference or objection. We conclude, therefore, that the argument does not bear against the existence of a vacuum; but if it did, it would only do away with vacua of considerable size which neither I nor, in my opinion, the ancients ever believed to exist in nature, although they might possibly be produced by force [*violenza*] as may be gathered from various experiments whose description would here occupy too much time.

SAGR. Seeing that Simplicio is silent, I will take the opportunity of saying something. Since you have clearly demonstrated that bodies of different weights do not move in one and the same medium with velocities proportional to their weights, but that they all move with the same speed, understanding of course that they are of the same substance or at least of the same specific gravity; certainly not of different specific gravities, for I hardly think you would have us believe a ball of cork moves with the same speed as one of lead; and again since you have clearly demonstrated that one and the same body moving through differently resisting media does not acquire speeds which are inversely proportional to the resistances, I am curious to learn what are the ratios actually observed in these cases.

SALV. These are interesting questions and I have thought much concerning them. I will give you the method of approach and the result which I finally reached. Having once established the falsity of the proposition that one and the same body moving through differently resisting media acquires speeds which are inversely proportional to the resistances of these media, and having also disproved the statement that in the same medium bodies of different weight acquire velocities proportional to their weights (understanding that this applies also to bodies which differ merely in specific gravity), I then began to combine these two facts and to consider what would happen if bodies of different weight were placed in media of different resistances; and I found that the differences in speed were greater in those media which were more resistant, that is, less yielding. This difference was such that two bodies which differed scarcely at all in their speed through air would, in water, fall the one with a speed ten times as great as that of the other. Further, there are bodies which will fall rapidly in air, whereas if placed in water not only will not sink but will remain at rest or will even rise to the top: for it is possible to find some kinds of wood, such as knots and roots, which remain at rest in water but fall rapidly in air.

SALV. Returning from this digression, let us again take up our problem. We have already seen that the difference of speed between bodies of different specific gravities is most marked in those media which are the most resistant: thus, in a medium of quicksilver, gold not merely sinks to the bottom more rapidly than lead but it is the only substance that will descend at all; all other metals and stones rise to the surface and float. On the other hand the variation of speed in air between balls of gold, lead, copper, porphyry, and other heavy materials is so slight that in a fall of 100 cubits a ball of gold would surely not outstrip one of copper by as much as four fingers. Having observed this I came to the conclusion that in a medium totally devoid of resistance all bodies would fall with the same speed.

SIMP. This is a remarkable statement, Salviati. But I shall never believe that even in a vacuum, if motion in such a place were possible, a lock of wool and a bit of lead can fall with the same velocity.

SALV. A little more slowly, Simplicio. Your difficulty is not so recondite nor am I so imprudent as to warrant you in believing that I have not already considered this matter and found the proper solution. Hence for my justification and for your enlightenment hear what I have to say. Our problem is to find out what happens to bodies of different weight moving in a medium devoid of resistance, so that the only difference in speed is that which arises from inequality of weight. Since no medium except one entirely free from air and other bodies, be it ever so tenuous and yielding, can furnish our senses with the evidence we are looking for, and since such a medium is not available, we shall observe what happens in the rarest and least resistant media as compared with what happens in denser and more resistant media. Because if we find as a fact that the variation of speed among bodies of different specific gravities is less and less according as the medium becomes more and more yielding, and if finally in a medium of extreme tenuity, though not a perfect vacuum, we find that, in spite of great diversity of specific gravity [*peso*], the difference in speed is very small and almost inappreciable, then we are justified in believing it highly probable that in a vacuum all bodies would fall with the same speed. Let us, in view of this, consider what takes place in air, where for the sake of a definite figure and light material imagine an inflated bladder. The air in this bladder when surrounded by air will weigh little or nothing, since it can be only slightly compressed; its weight then is small being merely that of the skin which does not amount to the thousandth part of a mass of lead having the same size as the inflated bladder. Now, Simplicio, if we allow these two bodies to fall from a height of four or six cubits, by what distance do you imagine the lead will anticipate the bladder? You may be sure that the lead will not travel three times, or even twice, as swiftly as the bladder, although you would have made it move a thousand times as rapidly.

SIMP. It may be as you say during the first four or six cubits of the fall; but after the motion has continued a long while, I believe that the lead will have left the bladder behind not only six out of twelve parts of the distance but even eight or ten.

SALV. I quite agree with you and doubt not that, in very long distances, the lead might cover one hundred miles while the bladder was traversing one; but, my dear Simplicio, this phenomenon which you adduce against my proposition is precisely the one which confirms it. Let me once more explain that the variation of speed observed in bodies of different specific gravities is not caused by the difference of specific gravity but depends upon external circumstances and, in particular, upon the resistance of the medium, so that if this is removed all bodies would fall with the same velocity; and this result I deduce mainly from the fact which you have just admitted and which is very true, namely, that, in the case of bodies which differ widely in weight, their velocities differ more and more as the spaces traversed increase, something which would not occur if the effect depended upon differences of specific gravity. For since these specific gravities remain constant, the ratio between the distances traversed ought to remain constant whereas the fact is that this ratio keeps on increasing as the motion continues. Thus a very heavy body in a fall of one cubit will not anticipate a very light one by so much as the tenth part of this space; but in a fall of twelve cubits the heavy body would outstrip the other by one-third, and in a fall of one hundred cubits by 90/100, etc.

SIMP. Very well: but, following your own line of argument, if differences of weight in bodies of different specific gravities cannot produce a change in the ratio of their speeds, on the ground that their specific gravities do not change, how is it possible for the medium, which also we suppose to remain constant, to bring about any change in the ratio of these velocities?

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

Now seeing how great is the resistance which the air offers to the slight momentum [*momento*] of the bladder and how small that which it offers to the large weight [*peso*] of the lead, I am convinced that, if the medium were entirely removed, the advantage received by the bladder would be so great and that coming to the lead so small that their speeds would be equalized. Assuming this principle, that all falling bodies acquire equal speeds in a medium which, on account of a vacuum or something else, offers no resistance to the speed of the motion, we shall be able accordingly to determine the ratios of the speeds of both similar and dissimilar bodies moving either through one and the same medium or through different space-filling, and therefore resistant, media. This result we may obtain by observing how much the weight of the medium detracts from the weight of the moving body, which weight is the means employed by the falling body to open a path for itself and to push aside the parts of the medium, something which does not happen in a vacuum where, therefore, no difference [of speed] is to be expected from a difference of specific gravity. And since it is known that the effect of the medium is to diminish the weight of the body by the weight of the medium displaced, we may accomplish our purpose by diminishing in just this proportion the speeds of the falling bodies, which in a non-resisting medium we have assumed to be equal.

Thus, for example, imagine lead to be ten thousand times as heavy as air while ebony is only one thousand times as heavy. Here we have two substances whose speeds of fall in a medium devoid of resistance are equal: but, when air is the medium, it will subtract from the speed of the lead one part in ten thousand, and from the speed of the ebony one part in one thousand, i.e. ten parts in ten thousand. While therefore lead and ebony would fall from any given height in the same interval of time, provided the retarding effect of the air were removed, the lead will, in air, lose in speed one part in ten thousand; and the ebony, ten parts in ten thousand. In other words, if the elevation from which the bodies start be divided into ten thousand parts, the lead will reach the ground leaving the ebony behind by as much as ten, or at least nine, of these parts. Is it not clear then that a leaden ball allowed to fall from a tower two hundred cubits high will outstrip an ebony ball by less than four inches? Now ebony weighs a thousand times as much as air but this inflated bladder only four times as much; therefore air diminishes the inherent and natural speed of ebony by one part in a thousand; while that of the bladder which, if free from hindrance, would be the same, experiences a diminution in air amounting to one part in four. So that when the ebony ball, falling from the tower, has reached the earth, the bladder will have traversed only three-quarters of this distance. Lead is twelve times as heavy as water; but ivory is only twice as heavy. The speeds of these two substances which, when entirely unhindered, are equal will be diminished in water, that of lead by one part in twelve, that of ivory by half. Accordingly when the lead has fallen through eleven cubits of water the ivory will have fallen through only six. Employing this principle we shall, I believe, find a much closer agreement of experiment with our computation than with that of Aristotle.

In a similar manner we may find the ratio of the speeds of one and the same body in different fluid media, not by comparing the different resistances of the media, but by considering the excess of the specific gravity of the body above those of the media. Thus, for example, tin is one thousand times heavier than air and ten times heavier than water; hence, if we divide its unhindered speed into 1000 parts, air will rob it of one of these parts so that it will fall with a speed of 999, while in water its speed will be 900, seeing that water diminishes its weight by one part in ten while air by only one part in a thousand.

Again take a solid a little heavier than water, such as oak, a ball of which will weigh let us say 1000 drachms; suppose an equal volume of water to weigh 950, and an equal volume of air, 2; then it is clear that if the unhindered speed of the ball is 1000, its speed in air will be 998, but in water only 50, seeing that the water removes 950 of 1000 parts which the body weighs, leaving only 50.

Such a solid would therefore move almost twenty times as fast in air as in water, since its specific gravity exceeds that of water by one part in twenty. And here we must consider the fact that only those substances which have a specific gravity greater than water can fall through it — substances which must, therefore, be hundreds of times heavier than air; hence when we try to obtain the ratio of the speed in air to that in water, we may, without appreciable error, assume that air does not, to any considerable extent, diminish the free weight [*assoluta gravità*], and consequently the unhindered speed [*assoluta velocità*] of such substances. Having thus easily found the excess of the weight of these substances over that of water, we can say that their speed in air is to their speed in water as their free weight [*totale gravità*] is to the excess of this weight over that of water. For example, a ball of ivory weighs 20 ounces; an equal volume of water weighs 17 ounces; hence the speed of ivory in air bears to its speed in water the approximate ratio of 20:3.

SAGR. I have made a great step forward in this truly interesting subject upon which I have long labored in vain.

(e) Changing Speed

The quantitative definition of acceleration was original with Galileo. It grew out of his interest in a changing universe in contrast with the static conception of the Greeks, who were concerned with ends rather than with the means. He emphasized that his own concept was derived from experience. In the case of uniformly accelerated motion he defined the *acceleration* of a moving body to be given by

$$a = \frac{\Delta v}{\Delta t} \,,$$

where Δv is the increment of the body's speed during a time interval Δt. Hence for a body starting from rest

$$a = vt \;.$$

He confessed at one time that he himself had supposed incorrectly that $\Delta v \propto \Delta s$ (increment of distance s) rather than $\Delta v \propto \Delta t$.

Having stated clearly his definitions, Galileo then proceeded in typical geometrical fashion to deduce consequential theorems. Old familiar geometry still afforded the most rigorous reasoning, and was practised more frequently than the new algebra. Nowadays we are more accustomed to the latter, and particularly to the later analytic geometry.

His Theorem II, Proposition II (p. 174), related space intervals to the squares of the time involved, the so-called *law of falling bodies*. Starting with the definition of acceleration, we have for a body falling from rest

$$s = vt \,,$$

where v is the average speed, i.e.

$$v = \tfrac{1}{2}(gt-0) = \tfrac{1}{2}gt \;.$$

Hence, upon substitution,

$$s = \tfrac{1}{2}gt^2 \;.$$

Consider two successive distances, s_1 and s_2, covered during times t_1 and t_2, respectively. Then,

$$s_1 = \tfrac{1}{2}gt_1^2 \,,$$

and

$$s_2 = \tfrac{1}{2}gt_2^2 \;.$$

Hence

$$\Delta s_1 = s_2 - s_1 = \tfrac{1}{2}gt_2^2 - \tfrac{1}{2}gt_1^2 \,,$$

or,

$$\Delta s_1 = \tfrac{1}{2} g (t_1+t_2)(t_1-t_2) \ .$$

Suppose

$$t_1-t_2 = 1 \ ,$$

then

$$\Delta s_i = \tfrac{1}{2} g (t_i+t_i+_1); \quad t_i = 1, 2, \ldots$$

Now inasmuch as t_2 and t_1 are successive times, the sum $(t_i+t_i+_1)$ must always be an odd number so that successive increments of distance, traversed in equal time intervals, increase as the odd numbers beginning with 1.

Speed cannot be easily determined directly; it can be more precisely obtained indirectly from measurements of distance and of time. Even here time errors are considerable. Galileo ingeniously slowed down the motion by rolling balls down an inclined plane. In order to eliminate frictional contact between the ball and the plane, later he substituted pendula with bobs of lead and of cork. In this connection he was careful to use long, fine threads — again to reduce possible errors. Time measurement, however, presented a major difficulty; he determined *time* by weighing the water collected from a uniformly flowing source (probably with an accuracy of 1 sec).

In discovering that all bodies experience the same acceleration due to gravity Galileo did more than find a specific law for a particular body; he discovered a general law applicable to all astronomical bodies.

Galileo continually strove to increase the accuracy of his investigations. His description of the precautions needed to reduce friction has become a classic — typical of an experientially minded investigator. He emphasized again and again the need for repetition of measurements.

Galileo's formal deductive presentation with geometical reasoning, in the spirit of Archimedes, has caused many to regard his method of investigation as essentially *a priori*, i.e. hypothetico-deductive, with phenomena used primarily *a posteriori* for corroboration. The description of his inclined-plane experiment is prima facie evidence that experience was closely linked with theorizing in his procedure. In all his research Galileo exhibited a critical, informal interchange of theory and practice, of reasoning and experiment.

Galileo surmised that the speed acquired by a body falling freely down an inclined plane from a given height is independent of the inclination of the plane. Later a theorem along this line was inserted in the *Two New Sciences* — elaborated by Viviani, following a suggestion of Galileo.

Dialogues Concerning Two New Sciences†

NATURALLY ACCELERATED MOTION

The properties belonging to uniform motion have been discussed in the preceding section; but accelerated motion remains to be considered.

† Ref. (9), pp. 160–2, 167, 169–70, 174–9, 84–6, 183–4.

And first of all it seems desirable to find and explain a definition best fitting natural phenomena. For anyone may invent an arbitrary type of motion and discuss its properties; thus, for instance, some have imagined helices and conchoids as described by certain motions which are not met with in nature, and have very commendably established the properties which these curves possess in virtue of their definitions; but we have decided to consider the phenomena of bodies falling with an acceleration such as actually occurs in nature and to make this definition of accelerated motion exhibit the essential features of observed accelerated motions. And this, at last, after repeated efforts we trust we have succeeded in doing. In this belief we are confirmed mainly by the consideration that experimental results are seen to agree with and exactly correspond with those properties which have been, one after another, demonstrated by us. Finally, in the investigation of naturally accelerated motion we were led, by hand as it were, in following the habit and custom of nature herself, in all her various other processes, to employ only those means which are most common, simple and easy.

For I think no one believes that swimming or flying can be accomplished in a manner simpler or easier than that instinctively employed by fishes and birds.

When, therefore, I observe a stone initially at rest falling from an elevated position and continually acquiring new increments of speed, why should I not believe that such increases take place in a manner which is exceedingly simple and rather obvious to everybody? If now we examine the matter carefully we find no addition or increment more simple than that which repeats itself always in the same manner. This we readily understand when we consider the intimate relationship between time and motion; for just as uniformity of motion is defined by and conceived through equal times and equal spaces (thus we call a motion uniform when equal distances are traversed during equal time-intervals), so also we may, in a similar manner, through equal time-intervals, conceive additions of speed as taking place without complication; thus we may picture to our mind a motion as uniformly and continuously accelerated when, during any equal intervals of time whatever, equal increments of speed are given to it. Thus if any equal intervals of time whatever have elapsed, counting from the time at which the moving body left its position of rest and began to descend, the amount of speed acquired during the first two time-intervals will be double that acquired during the first time-interval alone; so the amount added during three of these time-intervals will be treble; and that in four, quadruple that of the first time-interval. To put the matter more clearly, if a body were to continue its motion with the same speed which it had acquired during the first time-interval and were to retain this same uniform speed, then its motion would be twice as slow as that which it would have if its velocity had been acquired during *two* time-intervals.

And thus, it seems, we shall not be far wrong if we put the increment of speed as proportional to the increment of time; hence the definition of motion which we are about to discuss may be stated as follows: A motion is said to be uniformly accelerated, when starting from rest, it acquires, during equal time-intervals, equal increments of speed.

SAGR. Although I can offer no rational objection to this or indeed to any other definition, devised by any author whomsoever, since all definitions are arbitrary, I may nevertheless without offense be allowed to doubt whether such a definition as the above, established in an abstract manner, corresponds to and describes that kind of accelerated motion which we meet in nature in the case of freely falling bodies. And since the Author apparently maintains that the motion described in his definition is that of freely falling bodies, I would like to clear my mind of certain difficulties in order that I may later apply myself more earnestly to the propositions and their demonstrations.

SALV. It is well that you and Simplicio raise these difficulties. They are, I imagine, the same which occurred to me when I first saw this treatise, and which were removed either by discussion with the Author himself, or by turning the matter over in my own mind.

SAGR. So far as I see at present, the definition might have been put a little more clearly perhaps without changing the fundamental idea, namely, uniformly accelerated motion is such that its speed increases in proportion to the space traversed; so that, for example, the speed acquired by a body in falling four cubits would be double that acquired in falling two cubits and this latter speed would be double that acquired in the first cubit. Because there is no doubt but that a heavy body falling from the height of six cubits has, and strikes with, a momentum [*impeto*] double that it had at the end of three cubits, triple that which it would have if it had fallen from two, and sextuple that which it would have had at the end of one.

SALV. It is very comforting to me to have had such a companion in error; and moreover let me tell you that your proposition seems so highly probable that our Author himself admitted, when I advanced this opinion to him, that he had for some time shared the same fallacy. But what most surprised me was to see two propositions so inherently probable that they commanded the assent of every one to whom they were presented, proven in a few simple words to be not only false, but impossible.

SAGR. But now, continuing the thread of our talk, it would seem that up to the present we have established the definition of uniformly accelerated motion which is expressed as follows:

> A motion is said to be equally or uniformly accelerated when, starting from rest, its momentum (*celeritatis momenta*) receives equal increments in equal times.

SALV. This definition established, the Author makes a single assumption, namely,

> The speeds acquired by one and the same body moving down planes of different inclinations are equal when the heights of these planes are equal.

By the height of an inclined plane we mean the perpendicular let fall from the upper end of the plane upon the horizontal line drawn through the lower end of the same plane. Thus, to illustrate, let the line AB be horizontal, and let the planes CA and CD be inclined to it; then the Author calls the perpendicular CB the "height" of the planes CA and CD; he supposes that the speeds acquired by one and the same body, descending along the planes CA and CD to the terminal points A and D are equal since the heights of these planes are the same, CB; and also it must be understood that this speed is that which would be acquired by the same body falling from C to B.

SAGR. Your assumption appears to me so reasonable that it ought to be conceded without question, provided of course there are no chance or outside resistances, and that the planes are hard and smooth, and that the figure of the moving body is perfectly round, so that neither plane nor moving body is rough.

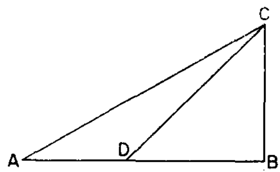


FIG. 32.

All resistance and opposition having been removed, my reason tells me at once that a heavy and perfectly round ball descending along the lines CA, CD, CB would reach the terminal points A, D, B, with equal momenta [*impeti eguali*].

SALV. Your words are very plausible; but I hope by experiment to increase the probability to an extent which shall be little short of a rigid demonstration.

THEOREM II, PROPOSITION II

The spaces described by a body falling from rest with a uniformly accelerated motion are to each other as the squares of the time-intervals employed in traversing these distances.

Let the time beginning with any instant A be represented by the straight line AB in which are taken any two time-intervals AD and AE. Let HI represent the distance through which the body, starting from rest at H, falls with uniform acceleration. If HL represents the space traversed during the time-interval AD, and HM that covered during the interval AE, then the space MH stands to the space LH in a ratio which is the square of the ratio of the time AE to the time AD; or we may say simply that the distances HM and HL are related as the squares of AE and AD.

Draw the line AC making any angle whatever with the line AB; and from the points D and E, draw the parallel lines DO

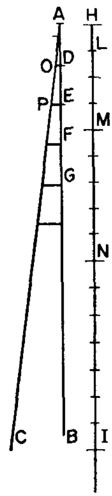


FIG. 33.

and EP; of these two lines, DO represents the greatest velocity attained during the interval AD, while EP represents the maximum velocity acquired during the interval AE. But it has just been proved that so far as distances traversed are concerned it is precisely the same whether a body falls from rest with a uniform acceleration or whether it falls during an equal time-interval with a constant speed which is one-half the maximum speed attained during the accelerated motion. It follows therefore that the distances HM and HL are the same as would be traversed, during the time-intervals AE and AD, by uniform velocities equal to one-half those represented by DO and EP respectively. If, therefore, one can show that the distances HM and HL are in the same ratio as the squares of the time intervals AE and AD, our proposition will be proven.

But in the fourth proposition of the first book it has been shown that the spaces traversed by two particles in uniform motion bear to one another a ratio which is equal to the product of the ratio of the velocities by the ratio of the times. But in this case the ratio of the velocities is the same as the ratio of the time-intervals (for the ratio of AE to AD is the same as that of $\frac{1}{2}$ EP to $\frac{1}{2}$ DO or of EP to DO). Hence the ratio of the spaces traversed is the same as the squared ratio of the time-intervals. Q. E. D.

Evidently then the ratio of the distances is the square of the ratio of the final velocities, that is, of the lines EP and DO, since these are to each other as AE to AD.

COROLLARY I

Hence it is clear that if we take any equal intervals of time whatever, counting from the beginning of the motion, such as AD, DE, EF, FG, in which the spaces HL, LM, MN, NI are traversed, these spaces will bear to one another the same ratio as the series of odd numbers, 1, 3, 5, 7; for this is the ratio of the differences of the squares of the lines [which represent time], differences which exceed one another by equal amounts, this excess being equal to the smallest line [viz. the one representing a single time-interval]: or we may say [that this is the ratio] of the differences of the squares of the natural numbers beginning with unity.

While, therefore, during equal intervals of time the velocities increase as the natural numbers, the increments in the distances traversed during these equal time-intervals are to one another as the odd numbers beginning with unity.

SAGR. Please suspend the discussion for a moment since there just occurs to me an idea which I want to illustrate by means of a diagram in order that it may be clearer both to you and to me.

Let the line AI represent the lapse of time measured from the initial instant A; through A draw the straight line AF making any angle whatever; join the terminal points I and F; divide the time AI in half at C; draw CB parallel to IF. Let us consider CB as the maximum value of the velocity which increases from

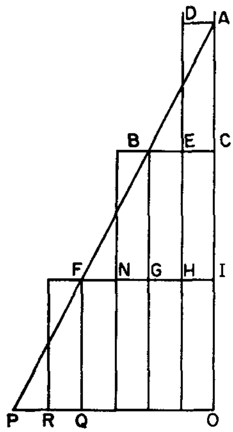


FIG. 34.

zero at the beginning, in simple proportionality to the intercepts on the triangle ABC of lines drawn parallel to BC; or what is the same thing, let us suppose the velocity to increase in proportion to the time; then I admit without question, in view of the preceding argument, that the space described by a body falling in the aforesaid manner will be equal to the space traversed by the same body during the same length of time travelling with a uniform speed equal to EC, the half of BC. Further let us imagine that the body has fallen with accelerated motion so that, at the instant C, it has the velocity BC. It is clear that if the body continued to descend with the same speed BC, without acceleration' it would in the next time-interval CI traverse double the distance covered during the interval AC, with the uniform speed EC which is half of BC; but since the falling body acquires equal increments of time, it follows that the velocity BC, during the next time-interval CI will be increased by an amount represented by the parallels of the triangle BFG which is equal to the triangle ABC. If, then, one adds to the velocity GI half of the velocity FG, the highest speed acquired by the accelerated motion and determined by the parallels of the triangle BFG, he will have the uniform velocity with which the same space would have been described in the time CI; and since this speed IN is three times as great as EC it follows that the space described during the interval CI is three times as great as that described during the interval AC. Let us imagine the motion extended over another equal time-interval IO, and the triangle extended to APO; it is then evident that if the motion continues during the interval IO, at the constant rate IF acquired by acceleration during the time AI, the space traversed during the interval IO will be four times that traversed during the first interval AC, because the speed IF is four times the speed EC. But if we enlarge our triangle so as to include FPQ which is equal to ABC, still assuming the acceleration to be constant, we shall add to the uniform speed an increment RQ, equal to EC; then the value of the equivalent uniform speed during the time-interval IO will be five times that during the first time-interval AC; therefore the space traversed will be quintuple that during the first interval AC. It is thus evident by simple computation that a moving body starting from rest and acquiring velocity at a rate proportional to the time, will, during equal intervals of time, traverse distances which are related to each other as the odd numbers beginning with unity, 1, 3, 5;* or considering the total space traversed, that covered in double time will be quadruple that covered during unit time; in triple time, the space is nine times as great as in unit time. And in general the spaces traversed are in the duplicate ratio of the times, i.e. in the ratio of the squares of the times.

SIMP. In truth, I find more pleasure in this simple and clear argument of Sagredo than in the Author's demonstration which to me appears rather obscure; so that I am convinced that matters are as described, once having accepted the definition of uniformly accelerated motion. But as to whether this acceleration is that which one meets in nature in the case of falling bodies, I am still doubtful; and it seems to me, not only for my own sake but also for all those who think as I do, that this would be the proper moment to introduce one of those experiments — and there are many of them, I understand — which illustrate in several ways the conclusions reached.

SALV. The request which you, as a man of science, make, is a very reasonable one; for this is the custom — and properly so — in those sciences where mathematical demonstrations are applied to natural phenomena, as is seen in the case of perspective, astronomy, mechanics, music, and others where the principles, once established by well-chosen experiments, become the foundations of the entire superstructure. I hope therefore it will not appear to be a waste of time if we discuss at considerable length this first and most fundamental question upon which hinge numerous consequences of which we have in this book only a small number, placed there by the Author, who has done so much to open a pathway hitherto closed to minds of speculative turn. So far as experiments go they have not been neglected by the Author; and often, in his company, I have attempted in the following manner to assure myself that the acceleration actually experienced by falling bodies is that above described.

A piece of wooden moulding or scantling, about 12 cubits long, half a cubit wide, and three finger-breadths thick, was taken; on its edge was cut a channel a little more than one finger in breadth; having made this groove very straight, smooth, and polished, and having lined it with parchment, also as smooth and polished as possible, we rolled along it a hard, smooth, and very round bronze ball. Having placed this board in a sloping position, by lifting one end some one or two cubits above the other, we rolled the ball, as I was just saying, along the channel, noting, in a manner presently to be described, the time required to make the descent. We repeated this experiment more than once in order to measure the time with an accuracy such that the deviation between two observations never exceeded one-tenth of a pulse-beat. Having performed this operation and having assured ourselves of its reliability, we now rolled the ball only one-quarter the length of the channel; and having measured the time of its descent, we found it precisely one-half of the former. Next we tried other distances, comparing the time for the whole length with that for the half, or with that for two-thirds, or three-fourths, or indeed for any fraction; in such experiments, repeated a full hundred times, we always found that the spaces traversed were to each other as the squares of the times, and this was true for all inclinations of the plane, i.e. of the channel, along which we rolled the ball. We also observed that the times of descent, for various inclinations of the plane, bore to one another precisely that ratio which, as we shall see later, the Author had predicted and demonstrated for them.

For the measurement of time, we employed a large vessel of water placed in an elevated position; to the bottom of this vessel was soldered a pipe of small diameter giving a thin jet of water, which we collected in a small glass during the time of each descent, whether for the whole length of the channel or for a part of its length; the water thus collected was weighed, after each descent, on a very accurate balance; the differences and ratios of these weights gave us the differences and ratios of the times, and this with such accuracy that although the operation was repeated many, many times, there was no appreciable discrepancy in the results.

SIMP. I would like to have been present at these experiments; but feeling confidence in the care with which you performed them, and in the fidelity with which you relate them, I am satisfied and accept them as true and valid.

SALV. The experiment made to ascertain whether two bodies, differing greatly in weight will fall from a given height with the same speed offers some difficulty; because, if the height is considerable, the retarding effect of the medium, which must be penetrated and thrust aside by the falling body, will be greater in the case of the small momentum of the very light body than in the case of the great force [*violenza*] of the heavy body; so that, in a long distance, the light body will be left behind; if the height be small, one may well doubt whether there is any difference; and if there be a difference it will be inappreciable.

It occurred to me therefore to repeat many times the fall through a small height in such a way that I might accumulate all those small intervals of time that elapse between the arrival of the heavy and light bodies respectively at their common terminus, so that this sum makes an interval of time which is not only observable, but easily observable. In order to employ the slowest speeds possible and thus reduce the change which the resisting medium produces upon the simple effect of gravity it occurred to me to allow the bodies to fall along a plane slightly inclined to the horizontal. For in such a plane, just as well as in a vertical plane, one may discover how bodies of different weight behave: and besides this, I also wished to rid myself of the resistance which might arise from contact of the moving body with the aforesaid inclined plane. Accordingly I took two balls, one of lead and one of cork, the former more than a hundred times heavier than the latter, and suspended them by means of two equal fine threads, each four or five cubits long. Pulling each ball aside from the perpendicular, I let them go at the same instant, and they, falling along the circumferences of circles having these equal strings for semi-diameters, passed beyond the perpendicular and returned along the same path. This free vibration [*per lor medesime le andate e le tornate*] repeated a hundred times showed clearly that the heavy body maintains so nearly the period of the light body that neither in a hundred swings nor even in a thousand will the former anticipate the latter by as much as a single moment [*minimo momento*], so perfectly do they keep step. We can also observe the effect of the medium which, by the resistance which it offers to motion, diminishes the vibration of the cork more than that of the lead, but without altering the frequency of either; even when the arc traversed by the cork did not exceed five or six degrees while that of the lead was fifty or sixty, the swings were performed in equal times.

SIMP. If this be so, why is not the speed of the lead greater than that of the cork, seeing that the former traverses sixty degrees in the same interval in which the latter covers scarcely six?

SALV. But what would you say, Simplicio, if both covered their paths in the same time when the cork, drawn aside through thirty degrees, traverses an arc of sixty, while the lead pulled aside only two degrees traverses an arc of four? Would not then the cork be proportionately swifter? And yet such is the experimental fact. But observe this: having pulled aside the pendulum of lead, say through an arc of fifty degrees, and set it free, it swings beyond the perpendicular almost fifty degrees, thus describing an arc of nearly one hundred degrees; on the return swing it describes a little smaller arc; and after a large number of such vibrations it finally comes to rest. Each vibration, whether of ninety, fifty, twenty, ten, or four degrees occupies the same time: accordingly the speed of the moving body keeps on diminishing since in equal intervals of time, it traverses arcs which grow smaller and smaller.

Precisely the same things happen with the pendulum of cork, suspended by a string of equal length, except that a smaller number of vibrations is required to bring it to rest, since on account of its lightness it is less able to overcome the resistance of the air; nevertheless the vibrations, whether large or small, are all performed in time-intervals which are not only equal among themselves, but also equal to the period of the lead pendulum. Hence it is true that, if while the lead is traversing an arc of fifty degrees the cork covers one of only ten, the cork moves more slowly than the lead; but on the other hand it is also true that the cork may cover an arc of fifty while the lead passes over one of only ten or six; thus, at different times, we have now the cork, now the lead, moving more rapidly. But if these same bodies traverse equal arcs in equal times we may rest assured that their speeds are equal.

SALV. Perfectly right. This point established, I pass to the demonstration of the following theorem:

> If a body falls freely along smooth planes inclined at any angle whatsoever, but of the same height, the speeds with which it reaches the bottom are the same.

First we must recall the fact that on a plane of any inclination whatever a body starting from rest gains speed or momentum [*la quantita dell'impeto*] in direct proportion to the time, in agreement with the definition of naturally accelerated motion given by the Author. Hence, as he has shown in the preceding proposition, the distances traversed are proportional to the squares of the times and therefore to the squares of the speeds. The speed relations are here the same as in the motion first studied [i.e. *vertical motion*], since in each case the gain of speed is proportional to the time.

Let AB be an inclined plane whose height above the level BC is AC. As we have seen above the force impelling [*l'impeto*] a body to fall along the vertical AC is to the force which drives the same

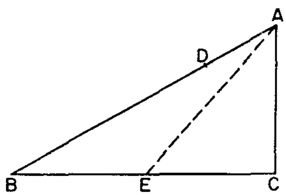


FIG. 35.

body along the inclined plane AB as AB is to AC. On the inclined AB, lay off AD a third proportional to AB and AC; then the force producing motion along AC is to that along AB (i.e. along AD) as the length AC is to the length AD. And therefore the body will traverse the space AD, along the incline AB, in the same time which it would occupy in falling the vertical distance AC, (since the forces [*momenti*] are in the same ratio as these distances); also the speed at C is to the speed at D as the distance AC is to the distance AD. But, according to the definition of accelerated motion, the speed at B is to the speed of the same body at D as the time required to traverse AB is to the time required for AD; and, according to the last corollary of the second proposition, the time of passing through the distance AB bears to the time of passing through AD the same ratio as the distance AC (a mean proportional between AB and AD) to AD. Accordingly the two speeds at B and C each bear to the speed at D the same ratio, namely, that of the distances AC and AD; hence they are equal. This is the theorem which I set out to prove.

(f) Projectiles

Undoubtedly the peak of Galileo's understanding of dynamics is found in his solution of the motion of a projectile — truly the climax of the *Two New Sciences*. Fundamental in this analysis was the new combination of a natural motion (free fall) with a violent one (projection). Although independent motions had previously been considered as acting simultaneously; for example, in the case of celestial orbits, they had always been of the same type, i.e. natural (circular) — or, at worse, merely hypothetical epicycles. On explaining the trajectory of a projectile, it had been customary to regard the occurrence of first one motion, say, the projection, and then that of, say, free fall — but never both together. Just as Galileo had refused to draw a line of demarcation between heavy and light bodies, between those at motion and those at rest, so here he considered both violent and natural motions occurring simultaneously.

What is more, the total speed in this instance was viewed as continuously changing, so that Galileo had to use both the qualitative concept of inertia and the quantitative idea of acceleration.

Let us consider a body projected upward with an initial speed v_0 at an angle α with the surface. We must solve the following equations of motion:

$$\frac{d^2x}{dt^2} = 0 \ ,$$

and

$$\frac{d^2y}{dt^2} = -g \ ,$$

where the vertical y-axis is directed positively upward. Now suppose that at time $t = 0, \dfrac{dx}{dt} = v_0 \cos \alpha$, and $\dfrac{dy}{dt} = v_0 \sin \alpha$. Upon integrating, we obtain at any future time

$$\frac{dx}{dt} = v_0 \cos \alpha \ ,$$

and

$$\frac{dy}{dt} = v_0 \sin \alpha - gt \ ,$$

Also suppose at $t = 0$, $x = y = 0$. Then at any time t,

$$x = (v_0 \cos) \ \alpha \ t \ ,$$

and

$$y = (v_0 \sin \alpha) \ t - \tfrac{1}{2} g t^2 \ .$$

Eliminating t, we obtain the path, i.e.

$$y = (\tan \alpha) \ x - \frac{g}{2v_0^2} \cos^2 \alpha \ . \ x^2)$$

which is the equation of a *parabola.* The *range* R (maximum horizontal displacement) is given by

$$R = \frac{v_0^2 \sin 2\alpha}{g} \ .$$

Evidently the maximum range occurs when $\alpha = 45°$, and the greatest height when $\alpha = 90°$. The first fact had long been known empirically to gunners. Galileo predicted additionally, however, that the range for $(45°+\beta)$ is the same as that for $(45°-\beta)$ — later confirmed.

Galileo discussed practical objections to his obviously idealized representation of a projectile, viz. convergence of the gravitational lines of force (not strictly parallel) and resistance (neglected) of the medium. His criterion for the use of such idealized models is still instructive: "to apply them with such limitations as experience will teach."

Unfortunately practical gunners, who up to then had refused to consider theoretical speculations seriously, now failed to take into account the theoretical limitations. For a century they dogmatically accepted Galileo's investigation without resistance in instances where the observed resistance was truly significant. Yet Galileo himself had already stressed the practical attainment of a *terminal speed,* which a freely falling body reaches in any resisting medium.

Dialogues Concerning Two New Sciences †

THEOREM I, PROPOSITION I

A projectile which is carried by a uniform horizontal motion compounded with a naturally accelerated vertical motion describes a path which is a semi-parabola.

SAGR. Here, Salviati, it will be necessary to stop a little while for my sake and, I believe, also for the benefit of Simplicio; for it so happens that I have not gone very far in my study of Apollonius and am merely aware of the fact that he treats of the parabola and other conic sections, without an understanding of which I hardly think one will be able to follow the proof of other propositions depending upon them. Since even in this first beautiful theorem the author finds it necessary to prove that the path of a projectile is a parabola, and since, as I imagine, we shall have to deal with only this kind of curves, it will be absolutely necessary to have a thorough acquaintance, if not with all the properties which Apollonius has demonstrated for these figures, at least with those which are needed for the present treatment.

SALV. You are quite too modest, pretending ignorance of facts which not long ago you acknowledged as well known — I mean at the time when we were discussing the strength of materials and needed to use a certain theorem of Appollonius which gave you no trouble.

SAGR. I may have chanced to know it or may possibly have assumed it, so long as needed, for that discussion; but now when we have to follow all these demonstrations about such curves we ought not, as they say, to swallow it whole, and thus waste time and energy.

SIMP. Now even though Sagredo is, as I believe, well equipped for all his needs, I do not understand even the elementary terms; for although our philosophers have treated the motion of projectiles, I do not recall their having described the path of a projectile except to state in a general way that it is always a curved line, unless the projection be vertically upwards. But

† Ref. (9), pp. 245–57.

if the little Euclid which I have learned since our previous discussion does not enable me to understand the demonstrations which are to follow, then I shall be obliged to accept the theorems on faith without fully comprehending them.

SALV. On the contrary, I desire that you should understand them from the Author himself, who, when he allowed me to see this work of his, was good enough to prove for me two of the principal properties of the parabola because I did not happen to have at hand the books of Apollonius. These properties, which are the only ones we shall need in the present discussion, he proved in such a way that no prerequisite knowledge was re-

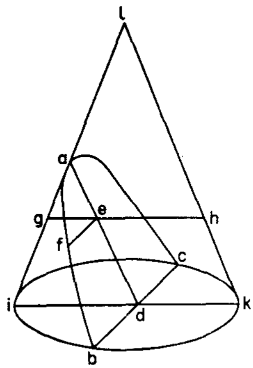


FIG. 36.

quired. These theorems are, indeed, given by Apollonius, but after many preceding ones, to follow which would take a long while. I wish to shorten our task by deriving the first property purely and simply from the mode of generation of the parabola and proving the second immediately from the first.

Beginning now with the first, imagine a right cone, erected upon the circular base *ibkc* with apex at *l*. The section of this cone made by a plane drawn parallel to the side *lk* is the curve which is called a *parabola*. The base of this parabola *bc* cuts at right angles the diameter *ik* of the cirle *ibkc*, and the axis *ad* is parallel to the side *lk*; now having taken any point *f* in the curve *bfa* draw the straight line *fe* parallel to *bd*; then, I say, the square of *bd* is to the square of *fe* in the same ratio as the axis *ad* is to the portion *ae*. Through the point *e* pass a plane parallel to the circle *ibkc*, producing in the cone a circular section whose diameter is the line *geh*. Since *bd* is at right angles to *ik* in the circle *ibk*, the square of *bd* is equal to the rectangle formed by *id* and *dk*; so also in the upper circle which passes through the points *gfh* the square of *fe* is equal to the rectangle formed by *ge* and *eh*; hence the square of *bd* is to the square of *fe* as the rectangule *id.dk* is to the rectangle *ge.eh*. And since the line *ed* is parallel to *hk*, the line *eh*, being parallel to *dk*, is equal to it; therefore the rectangle *id.dk* is to the rectangle *ge.eh* as *id* is to *ge*, that is, as *da* is to *ae*; whence also the rectangle *id.dk* is to the rectangle *ge.eh*, that is, the square of *bd* is to the square of *fe*, as the axis *da* is to the portion *ae*. Q. E. D.

The other proposition necessary for this discussion we demonstrate as follows. Let us draw a parabola whose axis *ca* is prolonged upwards to a point *d;* from any point *b* draw the line *bc* parallel to the base of the parabola; if now the point *d* is chosen so that *da*=*ca*, then, I say, the straight line drawn through the points *b* and *d* will be tangent to the parabola at *b*. For imagine, if possible, that this line cuts the parabola above or that its prolongation cuts it below, and through any point *g* in it draw the straight line *fge*. And since the square of *fe* is greater than the square of *ge*, the square of *fe* will bear a greater ratio to the square of *bc* than the square of *ge* to that of *bc;* and since, by the preceding proposition, the square of *fe* is to that of *bc* as the line *ea* is to *ca*, it follows that the line *ea* will bear to the line *ca* a greater ratio than the square of *ge* to that of *bc*, or, than the square of *ed* to that of *cd* (the sides of the triangles *deg* and *dcb* being proportional). But the line *ea* is to *ca*, or *da*, in the same ratio as four times the rectangle *ea.ad* is to four times the square of *ad*, or, what is the same, the square of *cd*, since this is four times the square of *ad;* hence four times the rectangle *ea.ad* bears to the square of *cd* a greater ratio than the square of *ed* to the square of *cd;* but that would make four times the rectangle *ea.ad* greater than the square of *ed;* which is false, the fact being just

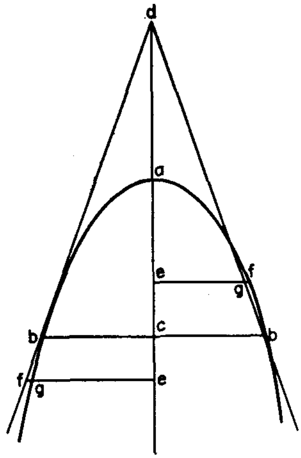


FIG. 37.

the opposite, because the two portions *ea* and *ad* of the line *ed* are not equal. Therefore the line *db* touches the parabola without cutting it. Q. E. D.

SIMP. Your demonstration proceeds too rapidly and, it seems to me, you keep on assuming that all of Euclid's theorems are as familiar and available to me as his first axioms, which is far from true. And now this fact which you spring upon us, that four times the rectangle *ea.ad* is less than the square of *de* because the two portions *ea* and *ad* of the line *de* are not equal brings me little composure of mind, but rather leaves me in suspense.

SALV. Indeed, all real mathematicians assume on the part of the reader perfect familiarity with at least the elements of Euclid; and here it is necessary in your case only to recall a proposition of the Second Book in which he proves that when a line is cut into equal and also into two unequal parts, the rectangle formed on the unequal parts is less than that formed on the equal (i.e. less than the square on half the line), by an amount which is the square of the difference between the equal and unequal segments. From this it is clear that the square of the whole line which is equal to four times the square of the half is greater than four times the rectangle of the unequal parts. In order to understand the following portions of this treatise it will be necessary to keep in mind the two elemental theorems from conic sections which we have just demonstrated; and these two theorems are indeed the only ones which the Author uses. We can now resume the text and see how he demonstrates his first proposition in which he shows that a body falling with a motion compounded of a uniform horizontal and a naturally accelerated [*naturale descendente*] one describes a semi-parabola.

Let us imagine an elevated horizontal line or plane *ab* along which a body moves with uniform speed from *a* to *b*. Suppose this plane to end abruptly at *b;* then at this point the body will, on account of its weight, acquire also a natural motion downwards along the perpendicular *bn*. Draw the line *be* along the

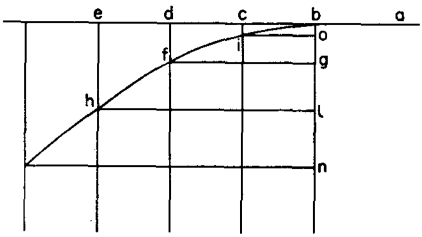


FIG. 38.

plane *ba* to represent the flow, or measure, of time; divide this line into a number of segments, *bc*, *cd*, *de*, representing equal intervals of time; from the points *b*, *c*, *d*, *e*, let fall lines which are parallel to the perpendicular *bn*. On the first of these lay off any distance *ci*, on the second a distance four times as long, *df;*

on the third, one nine times as long, *eh;* and so on, in proportion to the squares of *cb, db, eb,* or, we may say, in the squared ratio of these same lines. Accordingly we see that while the body moves from *b* to *c* with uniform speed, it also falls perpendicularly through the distance *ci,* and at the end of the time-interval *bc* finds itself at the point *i.* In like manner at the end of the time-interval *bd,* which is the double of *bc,* the vertical fall will be four times the first distance *ci;* for it has been shown in a previous discussion that the distance traversed by a freely falling body varies as the square of the time; in like manner the space *eh* traversed during the time *be* will be nine times *ci;* thus it is evident that the distances *eh, df, ci* will be to one another as the squares of the lines *be, bd, bc.* Now from the points *i, f, h* draw the straight lines *io, fg, hl* parallel to *be;* these lines *hl, fg, io* are equal to *eb, db* and *cb,* respectively; so also are the lines *bo, bg, bl* respectively equal to *ci, df,* and *eh.* The square of *hl* is to that of *fg* as the line *lb* is to *bg;* and the square of *fg* is to that of *io* as *gb* is to *bo;* therefore the points *i, f, h,* lie on one and the same parabola. In like manner it may be shown that, if we take equal time-intervals of any size whatever, and if we imagine the particle to be carried by a similar compound motion, the positions of this particle, at the ends of these time-intervals, will lie on one and the same parabola. Q. E. D.

SALV. This conclusion follows from the converse of the first of the two propositions given above. For, having drawn a parabola through the points *b* and *h,* any other two points, *f* and *i,* not falling on the parabola must lie either within or without; consequently the line *fg* is either longer or shorter than the line which terminates on the parabola. Therefore the square of *hl* will not bear to the square of *fg* the same ratio as the line *lb* to *bg,* but a greater or smaller; the fact is, however, that the square of *hl does* bear this same ratio to the square of *fg.* Hence the point *f* does lie on the parabola, and so do all the others.

SAGR. One cannot deny that the argument is new, subtle and conclusive, resting as it does upon this hypothesis, namely, that the horizontal motion remains uniform, that the vertical motion continues to be accelerated downwards in proportion to the square of the time, and that such motions and velocities as these combine without altering, disturbing, or hindering each other, so that as the motion proceeds the path of the projectile does not change into a different curve: but this, in my opinion, is impossible. For the axis of the parabola along which we imagine the natural motion of a falling body to take place stands perpendicular to a horizontal surface and ends at the center of the earth; and since the parabola deviates more and more from its axis no projectile can ever reach the center of the earth or, if it does, as seems necessary, then the path of the projectile must transform itself into some other curve very different from the parabola.

SIMP. To these difficulties, I may add others. One of these is that we suppose the horizontal plane, which slopes neither up nor down, to be represented by a straight line as if each point on this line were equally distant from the center, which is not the case; for as one starts from the middle [of the line] and goes toward either end, he departs farther and farther from the center [of the earth] and is therefore constantly going uphill. Whence it follows that the motion cannot remain uniform through any distance whatever, but must continually diminish. Besides, I do not see how it is possible to avoid the resistance of the medium which must destroy the uniformity of the horizontal motion and change the law of acceleration of falling bodies. These various difficulties render it highly improbable that a result dervied from such unreliable hypotheses should hold true in practice.

SALV. All these difficulties and objections which you urge are so well founded that it is impossible to remove them; and, as for me, I am ready to admit them all, which indeed I think our Author would also do. I grant that these conclusions proved in the abstract will be different when applied in the concrete and will be fallacious to this extent, that neither will the horizontal motion be uniform nor the natural acceleration be in the ratio assumed, nor the path of the projectile a parabola, etc. But, on the other hand, I ask you not to begrudge our Author that which other eminent men have assumed even if not strictly true. The authority of Archimedes alone will satisfy everybody. In his Mechanics and in his first quadrature of the parabola he takes for granted that the beam of a balance or steelyard is a straight line, every point of which is equidistant from the common center of all heavy bodies, and that the cords by which heavy bodies are suspended are parallel to each other.

Some consider this assumption permissible because, in practice, our instruments and the distances involved are so small in comparison with the enormous distance from the center of the earth that we may consider a minute of arc on a great circle as a straight line, and may regard the perpendiculars let fall from its two extremities as parallel. For if in actual practice one had to consider such small quantities, it would be necessary first of all to criticise the architects who presume, by use of a plumbline, to erect high towers with parallel sides. I may add that, in all their discussions, Archimedes and the others considered themselves as located at an infinite distance from the center of the earth, in which case their assumptions were not false, and therefore their conclusions were absolutely correct. When we wish to apply our proven conclusions to distances which, though finite, are very large, it is necessary for us to infer, on the basis of demonstrated truth, what correction is to be made for the fact that our distance from the center of the earth is not really infinite, but merely very great in comparison with the small dimensions of our apparatus. The largest of these will be the range of our projectiles — and even here we need consider only the artillery — which, however great, will never exceed four of those miles of which as many thousand separate us from the center of the earth; and since these paths terminate upon the surface of the earth only very slight changes can take place in their parabolic figure which, it is conceded, would be greatly altered if they terminated at the center of the earth.

As to the perturbation arising from the resistance of the medium this is more considerable and does not, on account of its manifold forms, submit to fixed laws and exact description. Thus if we consider only the resistance which the air offers to the motions studied by us, we shall see that it disturbs them all and disturbs them in an infinite variety of ways corresponding to the infinite variety in the form, weight, and velocity of the projectiles. For as to velocity, the greater this is, the greater will be the resistance offered by the air; a resistance which will be greater as the moving bodies become less dense [*men gravi*]. So that although the falling body ought to be displaced [*andare accelerandosi*] in proportion to the square of the duration of its motion, yet no matter how heavy the body, if it falls from a very considerable height, the resistance of the air will be such as to prevent any increase in speed and will render the motion uniform; and in proportion as the moving body is less dense [*men grave*] this uniformity will be so much the more quickly attained and after a shorter fall. Even horizontal motion which, if no impediment were offered, would be uniform and constant is altered by the resistance of the air and finally ceases; and here again the less dense [*più leggiero*] the body the quicker the process. Of these properties [*accidenti*] of weight, of velocity, and also of form [*figura*], infinite in number, it is not possible to give any exact description; hence, in order to handle this matter in a scientific way, it is necessary to cut loose from these difficulties; and having discovered and demonstrated the theorems, in the case of no resistance, to use them and apply them with such limitations as experience will teach. And the advantage of this method will not be small; for the material and shape of the projectile may be chosen, as dense and round as possible, so that it will encounter the least resistance in the medium. Nor will the spaces and velocities in general be so great but that we shall be easily able to correct them with precision.

In the case of those projectiles which we use, made of dense [*grave*] material and round in shape, or of lighter material and cylindrical in shape, such as arrows, thrown from a sling or crossbow, the deviation from an exact parabolic path is quite insensible. Indeed, if you will allow me a little greater liberty, I can show you, by two experiments, that the dimensions of our apparatus are so small that these external and incidental resistances, among which that of the medium is the most considerable, are scarcely observable.

I now proceed to the consideration of motions through the air, since it is with these that we are now especially concerned; the resistance of the air exhibits itself in two ways: first by offering greater impedance to less dense than to very dense bodies, and secondly by offering greater resistance to a body in rapid motion than to the same body in slow motion.

Regarding the first of these, consider the case of two balls having the same dimensions, but one weighing ten or twelve times as much as the other; one, say, of lead, the other of oak, both allowed to fall from an elevation of 150 or 200 cubits.

Experiment shows that they will reach the earth with slight difference in speed, showing us that in both cases the retardation caused by the air is small; for if both balls start at the same moment and at the same elevation, and if the leaden one be slightly retarded and the wooden one greatly retarded, then the former ought to reach the earth a considerable distance in advance of the latter, since it is ten times as heavy. But this does not happen; indeed, the gain in distance of one over the other does not amount to the hundredth part of the entire fall. And in the case of a ball of stone weighing only a third or half as much as one of lead, the difference in their times of reaching the earth will be scarcely noticeable. Now since the speed [*impeto*] acquired by a leaden ball in falling from a height of 200 cubits is so great that if the motion remained uniform the ball would, in an interval of time equal to that of the fall, traverse 400 cubits, and since this speed is so considerable in comparison with those which, by use of bows or other machines except fire arms, we are able to give to our projectiles, it follows that we may, without sensible error, regard as absolutely true those propositions which we are about to prove without considering the resistance of the medium.

Passing now to the second case, where we have to show that the resistance of the air for a rapidly moving body is not very much greater than for one moving slowly, ample proof is given by the following experiment. Attach to two threads of equal length — say four or five yards — two equal leaden balls and suspend them from the ceiling; now pull them aside from the perpendicular, the one through 80 or more degrees, the other through not more than four or five degrees; so that, when set free, the one falls, passes through the perpendicular, and describes large but slowly decreasing arcs of 160, 150, 140 degrees etc.; the other swinging through small and also slowly diminishing arcs of 10, 8, 6, degrees, etc.

In the first place it must be remarked that one pendulum passes through its arcs of 180°, 160°, etc., in the same time that the other swings through its 10°, 8°, etc., from which it follows that the speed of the first ball is 16 and 18 times greater than that of the second. Accordingly, if the air offers more resistance to the high speed than to the low, the frequency of vibration in the large arcs of 180° or 160°, etc., ought to be less than in the small arcs of 10°, 8°, 4°, etc., and even less than in arcs of 2°, or 1°; but this prediction is not verified by experiment; because if two persons start to count the vibrations, the one the large, the other the small, they will discover that after counting tens and even hundreds they will not differ by a single vibration, not even by a fraction of one.

This observation justifies the two following propositions, namely, that vibrations of very large and very small amplitude all occupy the same time and that the resistance of the air does not affect motions of high speed more than those of low speed, contrary to the opinion hitherto generally entertained.

SAGR. On the contrary, since we cannot deny that the air hinders both of these motions, both becoming slower and finally vanishing, we have to admit that the retardation occurs in the same proportion in each case. But how? How, indeed, could the resistance offered to the one body be greater than that offered to the other except by the impartation of more momentum and speed [*impeto e velocità*] to the fast body than to the slow? And if this is so the speed with which a body moves is at once the cause and measure [*cagione e misura*] of the resistance which it meets. Therefore, all motions, fast or slow, are hindered and diminished in the same proportion; a result, it seems to me, of no small importance.

SALV. We are able, therefore, in this second case to say that the errors, neglecting those which are accidental, in the results which we are about to demonstrate are small in the case of our machines where the velocities employed are mostly very great and the distances negligible in comparison with the semi-diameter of the earth or one of its great circles.

SIMP. I would like to hear your reason for putting the projectiles of fire arms, i.e., those using powder, in a different class from the projectiles employed in bows, slings, and crossbows, on the ground of their not being equally subject to change and resistance from the air.

SALV. I am led to this view by the excessive and, so to speak, supernatural violence with which such projectiles are launched; for, indeed, it appears to me that without exaggeration one might say that the speed of a ball fired either from a musket or from a piece of ordnance is supernatural. For if such a ball be allowed to fall from some great elevation its speed will, owing to the resistance of the air, not go on increasing indefinitely; that which happens to bodies of small density in falling through short distances — I mean the reduction of their motion to uniformity — will also happen to a ball of iron or lead after it has fallen a few thousand cubits; this terminal or final speed [*terminata velocita*] is the maximum which such a heavy body can naturally acquire in falling through the air. This speed I estimate to be much smaller than that impressed upon the ball by the burning powder.

An appropriate experiment will serve to demonstrate this fact. From a height of one hundred or more cubits fire a gun [*archibuso*] loaded with a lead bullet, vertically downwards upon a stone pavement; with the same gun shoot against a similar stone from a distance of one or two cubits, and observe which of the two balls is the more flattened. Now if the ball which has come from the greater elevation is found to be the less flattened of the two, this will show that the air has hindered and diminished the speed initially imparted to the bullet by the powder, and that the air will not permit a bullet to acquire so great a speed, no matter from what height it falls; for if the speed impressed upon the ball by the fire does not exceed that acquired by it in falling freely [*naturalmente*] then its downward blow ought to be greater rather than less.

This experiment I have not performed, but I am of the opinion that a musket-ball or cannon-shot, falling from a height as great as you please, will not deliver so strong a blow as it would if fired into a wall only a few cubits distant, i.e., at such a short range that the splitting or rending of the air will not be sufficient to rob the shot of that excess of supernatural violence given it by the powder.

The enormous momentum [*impeto*] of these violent shots may cause some deformation of the trajectory, making the beginning of the parabola flatter and less curved than the end; but, so far as our Author is concerned, this is a matter of small consequence in practical operations, the main one of which is the preparation of a table of ranges for shots of high elevation, giving the distance attained by the ball as a function of the angle of elevation; and since shots of this kind are fired from mortars [*mortari*] using small charges and imparting no supernatural momentum [*impeto sopranaturale*] they follow their prescribed paths very exactly.

(g) Galileian Relativity

Newton's *second law of motion* states that an unbalanced force F on a body with mass M will be equal to the time-rate of change of its momentum Mv, where v is the velocity of the body. Let $\Delta (Mv)$ be the change of momentum of the body in a time interval Δt. Then

$$F = \frac{\Delta(Mv)}{\Delta t} ,$$

where all measurements are made relative to a given frame of reference S. Suppose we measure also the velocity v_1 in a different frame of reference S' which has a constant velocity v_0 relative (horizontally) to S.

Then

$$v = v' + v_0 .$$

But

$$\Delta v = \Delta v'' \ .$$

Hence

$$F = \frac{\Delta(Mv')}{\Delta t} \ ,$$

i.e. the mathematical form of the describing equation is the same for both systems. The solutions, accordingly, will also be formally the same. For example, a body falling freely (vertically) in the first frame S will have identically the same path and speed in the second S'. In general, the motion will be described similarly in all frames of reference that differ only in constant velocity — later called Galileian relativity by physicists.

The Galileian *transformation equations* between measurements in system S' and those in system S can be shown to be

$$x' = x + v_0 t \ ,$$
$$y' = y \ ,$$
$$z' = Z \ ,$$
$$t' = t \ .$$

Dialogue Concerning Two Chief World Systems — Ptolemaic and Copernican †

For a final indication of the nullity of the experiments brought forth, this seems to me the place to show you a way to test them all very easily. Shut yourself up with some friend in the main cabin below decks on some large ship, and have with you there some flies, butterflies, and other small flying animals. Have a large bowl of water with some fish in it; hang up a bottle that empties drop by drop into a wide vessel beneath it. With the ship standing still, observe carefully how the little animals fly with equal speed to all sides of the cabin. The fish swim indifferently in all directions; the drops fall into the vessel beneath; and, in throwing something to your friend, you need throw it no more strongly in one direction than another, the distances being equal; jumping with your feet together, you pass equal spaces in every direction. When you have observed all these things carefully (though there is no doubt that when the ship is standing still everything must happen in this way), have the ship proceed with any speed you like, so long as the motion is uniform

† Ref. (7), pp. 186–7, 250.

and not fluctuating this way and that. You will discover not the least change in all the effects named, nor could you tell from any of them whether the ship was moving or standing still. In jumping, you will pass on the floor the same spaces as before, nor will you make larger jumps toward the stern than toward the prow even though the ship is moving quite rapidly, despite the fact that during the time that you are in the air the floor under you will be going in a direction opposite to your jump. In throwing something to your companion, you will need no more force to get it to him whether he is in the direction of the bow or the stern, with yourself situated opposite. The droplets will fall as before into the vessel beneath without dropping toward the stern, although while the drops are in the air the ship runs many spans. The fish in their water will swim toward the front of their bowl with no more effort than toward the back, and will go with equal ease to bait placed anywhere around the edges of the bowl. Finally the butterflies and flies will continue their flights indifferently toward every side, nor will it ever happen that they are concentrated toward the stern, as if tired out from keeping up with the course of the ship, from which they will have been separated during long intervals by keeping themselves in the air. And if smoke is made by burning some incense, it will be seen going up in the form of a little cloud, remaining still and moving no more toward one side than the other. The cause of all these correspondences of effects is the fact that the ship's motion is common to all the things contained in it, and to the air also.

Now transfer this argument to the whirling of the earth and to the rock placed on top of the tower, whose motion you cannot discern because in common with the rock you possess from the earth that motion which is required for following the tower; you do not need to move your eyes. Next, if you add to the rock a downward motion which is peculiar to it and not shared by you, and which is mixed with this circular motion, the circular portion of the motion which is common to the stone and the eye continues to be imperceptible. The straight motion alone is sensible, for to follow that you must move your eyes downward.

I wish I could tell this philosopher, in order to remove him from error, to take with him a very deep vase filled with water some time when he goes sailing, having prepared in advance a ball of wax or some other material which would descend very slowly to the bottom — so that in a minute it would scarcely sink a yard. Then, making the boat go as fast as he could, so that it might travel more than a hundred yards in a minute, he should gently immerse this ball in the water and let it descend freely, carefully observing its motion. And from the first, he would see it going straight toward that point on the bottom of the vase to which it would tend if the boat were standing still. To his eye and in relation to the vase its motion would appear perfectly straight and perpendicular, and yet no one could deny that it was a compound of straight (down) and circular (around the watery element).

Now these things take place in motion which is not natural, and in materials with which we can experiment also in a state of rest or moving in the opposite direction, yet we can discover no difference in the appearances, and it seems that our senses are deceived. Then what can we be expected to detect as to the earth, which, whether it is in motion or at rest, has always been in the same state? And when is it that we are supposed to test by experiment whether there is any difference to be discovered among these events of local motion in their different states of motion and of rest, if the earth remains forever in one or the other of these two states?

CHAPTER 17

SPOTS ON THE SUN

GALILEO's unique contribution to the understanding of sunspot phenomena was his identification of their motion with the dark spots as rotation of the sun. He regarded them (wrongly) as clouds rather than depressions. The existence of these changing imperfections in a heavenly body was prima facie evidence of the falsity of the Greek notion of a fixed heaven made up possibly of some unchangeable quintessence.

History and Demonstrations Concerning Sunspots and their Phenomena†

I have remained silent also until I might hope to give some satisfaction to your inquiry about the solar spots, concerning which you have sent me some brief essays by the mysterious "Apelles." The difficulty of this matter, combined with my inability to make many continued observations, has kept (and still keeps) my judgment in suspense. And I, indeed, must be more cautious and circumspect than most other people in pronouncing upon anything new. As Your Excellency well knows, certain recent discoveries that depart from common and popular opinions have been noisily denied and impugned, obliging me to hide in silence every new idea of mine until I have more than proved it. Even the most trivial error is charged to me as a capital fault by the enemies of innovation, making it seem better to remain with the herd in error than to stand alone in reasoning correctly. I might add that I am quite content to be last and to come forth with a correct idea, rather than to get ahead of other

† Ref. (4), pp. 90–1, 98–9, 106–9.

people and later be compelled to retract what might be said sooner, indeed, but with less consideration.

These considerations have made me slow to respond to Your Excellency's requests and still make me hesitate to do more than advance a rather negative case by appearing to know rather what sunspots are not than what they really are, it being much harder for me to discover the truth than to refute what is false. But in order to satisfy Your Excellency's wishes in part at least, I shall consider those things which seem to me worthy of notice in the three letters of this man Apelles, as you require, and in particular what he has to say with regard to determining the essence, the location, and the motion of these spots.

First of all, I have no doubt whatever that they are real objects and not mere appearances or illusions of the eye or of the lenses of the telescope, as Your Excellency's friend well establishes in his first letter. I have observed them for about eighteen months, having shown them to various friends of mine, and at this time last year I had many prelates and other gentlemen at Rome observe them there. It is also true that the spots do not remain stationary upon the body of the sun, but appear to move in relation to it with regular motions, as your author has noted in that same letter. Yet to me it appears that this motion is in the opposite direction from what Apelles says — that is, they move from west to east, slanting from south to north, and not from east to west and north to south. This may be clearly perceived in the observations he himself describes, which compare in this regard with my own observations and with what I have seen of those made by other people. The spots seen at sunset are observed to change place from one evening to the next, descending from the part of the sun then uppermost, and the morning spots ascend from the part then below; and they appear first in the more southerly parts of the sun's body and disappear or separate from it in the more northerly regions. Thus the spots describe lines on the face of the sun similar to those along which Venus and Mercury proceed when those planets come between the sun and our eyes. Hence they move with respect to the sun as do

Venus and Mercury and the other planets, which motion is from west to east and obliquely to the horizon from south to north. If Apelles assumes that the spots do not revolve about the sun, but merely pass beneath it, then their motion may be properly called "from east to west." But assuming that the spots circle about the sun, being now beyond it and now this side of it, their rotation should be said to be from west to east, since that is the direction in which they move when they are in the more distant portions of their orbits.

. . . . .

It now remains for us to consider the judgment of Apelles concerning the essence and substance of these spots, which in sum is that they are neither clouds nor comets, but stars that go circling about the sun. I confess to Your Excellency that I am not yet sufficiently certain to affirm any positive conclusion about their nature. The substance of the spots might even be any of a thousand things unknown and unimaginable to us, while the phenomena commonly observed in them — their shapes, their opacity, and their movement — may lie partly or wholly outside the realm of our general knowledge. Therefore I see nothing discreditable to any philosopher in confessing that he does not know, and cannot know, what the material of the solar spots may be. But if, proceeding on a basis of analogy with materials known and familiar to us, one may suggest something that they may be from their appearance, my view would be exactly opposite to that of Apelles. To me it seems that none of the essentials belonging to stars are in any way adapted to the spots, while on the other hand I find in them nothing at all which does not resemble our own clouds. This may be seen by arguing as follows.

Sunspots are generated and decay in longer and shorter periods; some condense and others greatly expand from day to day; they change their shapes, and some of these are most irregular; here their obscurity is greater and there less. They must be simply enormous in bulk, being either on the sun or very close to it. By their uneven opacity they are capable of impeding the sunlight in differing degrees; and sometimes many spots are produced, sometimes few, sometimes none at all.

Now of all the things found with us, only clouds are vast and immense, are produced and dissolved in brief times, endure for long or short periods, expand and contract, easily change shape, and are more dense and opaque in some places and less so in others. Indeed, all other materials not only lack these properties but are far from having them. Moreover there is no doubt that if the earth shone with its own light and not by that of the sun, then to anyone who looked at it from afar it would exhibit congruent appearances. For as now this country and now that was covered by clouds, it would appear to be strewn with dark spots that would impede the terrestrial splendor more or less according to the greater or less density of their parts. These spots would be seen darker here and less dark there, now more numerous and again less so, now spread out and now restricted; and if the earth revolved upon an axis, they would follow its motion. And since clouds are of no great depth with respect to the breadth in which they are normally extended, those seen at the center of the visible hemisphere would appear quite broad, while those toward the edges would look narrower. In a word, no phenomena would be perceived that are not likewise seen in sunspots.

. . . . .

I therefore repeat and more positively confirm to Your Excellency that the dark spots seen in the solar disk by means of the telescope are not at all distant from its surface, but are either contiguous to it or separated by an interval so small as to be quite imperceptible. Nor are they stars or other permanent bodies, but some are always being produced and others dissolved. They vary in duration from one or two days to thirty or forty. For the most part they are of most irregular shape, and their shapes continually change, some quickly and violently, others more slowly and moderately. They also vary in darkness, appearing sometimes to condense and sometimes to spread out and rarefy. In addition to changing shape, some of them divide into three or four, and often several unite into one; this happens less near the edge of the sun's disk than in its central parts. Besides all these disordered movements they have in common a general uniform motion across the face of the sun in parallel lines. From special characteristics of this motion one may learn that the sun is absolutely spherical, that it rotates from west to east around its own center, carries the spots along with it in parallel circles, and completes an entire revolution in about one lunar month. Also worth noting is the fact that the spots always fall in one zone of the solar body, lying between the two circles which bound the declinations of the planets — that is, they fall within 28° or 29° of the sun's equator.

The different densities and degrees of darkness of the spots, their changes of shape, and their collecting and separating are evident directly to our sight, without any need of reasoning, as a glance at the diagrams which I am enclosing will show. But that the spots are contiguous to the sun and are carried around by its rotation can only be deduced and concluded by reasoning from certain particular events which our observations yield.

First, to see twenty or thirty spots at a time move with one common movement is a strong reason for believing that each does not go wandering about by itself, in the manner of the planets going around the sun. In order to explain this, let us define the poles in the solar globe and its circles of longitude and latitude as we do in the celestial sphere. If the sun is spherical and rotates, there will be two points at rest called the poles, and all other points on its surface will describe parallel circles which are larger or smaller according to their distance from the poles. The largest of all will be the central circle, equally distant from the two poles. The dimension of the spots along these circles will be called their breadth, and by their length we shall mean their dimension extending toward the poles and determined by a line perpendicular to that which determines their breadth.

These terms defined, let us consider the specific events observed in the sunspots from which one may arrive at a knowledge of their positions and movements. To begin with, the spots at their first appearance and final disappearance near the edges of the sun generally seem to have very little breadth, but to have the same length that they show in the central parts of the sun's disk. Those who understand what is meant by foreshortening on a spherical surface will see this to be a manifest argument that the sun is a globe, that the spots are close to its surface, and that as they are carried on that surface toward the center they will always grow in breadth while preserving the same length. All of them do not thin out equally to a hairsbreadth when close to the circumference, but this is because they are not all simple spots on the surface, but also have a certain height. Some have more thickness and some have less, just as our clouds, which may spread out for tens or hundreds of miles in length and breadth and may have greater or less thickness; yet these are not more than a few hundred or perhaps a thousand yards thick. And the thickness of the sunspots, though small in comparison with their other two dimensions, may be much greater in one spot than another, so that the thinnest spots when close to the edge of the sun look extremely slender — especially as the inner part of this edge is brightly lighted — while the thicker spots appear broader. But many of them are reduced to a threadlike thinness, and this could not happen at all if their motion across the face of the sun took place at even a short distance from the solar globe. For this maximum thinning takes place at the point of greatest foreshortening, and it would occur outside the face of the sun if the spots were any perceptible distance away from its surface.

In the second place, one must observe the apparent travel of the spots day by day. The spaces passed by the same spot in equal times become always less as the spot is situated nearer the edge of the sun. Careful observation shows also that these increases and decreases of travel are quite in proportion to the versed sines of equal arcs, as would happen only in circular motion contiguous to the sun itself. In circles even slightly distant from it, the spaces passed in equal times would appear to differ very little against the sun's surface.

A third thing which strongly confirms this conclusion may be deduced from the spaces between one spot and another. Some of these separations remain constant, others greatly increase toward the center of the solar disk, being quite narrow elsewhere, and insensible near the edge; still others show extreme variability. The events are such that they could be met with only in circular motion made by different points on a rotating globe. Spots located close together along the same parallel of solar latitude seem almost to touch each other at their first emergence; if farther apart, they will at any rate be much closer near the edge than near the center of the sun. As they move away from the edge, they are seen to separate more and more; at the center, they have their maximum separation; and as they move on from there they approach each other again. Accurate observation of the ratios of these separations and approaches shows that they can occur only upon the very surface of the solar globe.

. . . . .

That the spots are very thin in comparison with their length and breadth may be deduced from the gaps between them, for they are often distinct all the way out to the very limb of the sun. This would not happen if they were very high and thick, especially when quite close together. Likewise separations among groups of very small spots have been seen all the way to the edge, though much foreshortened by the curvature of the surface. Some may say from this that such spots must be surfaces of little or no thickness, since when close to the edge of the disk the bright spaces between them are not foreshortened more than their own breadths are diminished, which it seems could not happen if their height were appreciable. But I say this is not a necessary consequence, because one must consider also the brilliance of the sunlight which illuminates the spots edgewise . . . I could give many examples, but in order to avoid prolixity I shall save this to write of in another place.

I should be mentioned that the spots are not completely fixed and motionless on the face of the sun, but continually change in shape, collect together, and disperse. But this variation is small in relation to the general rotation of the sun, and should not trouble anyone who will judiciously weigh the general movement against the small accidental variation. And just as all the phenomena in these observations agree exactly with the spots' being contiguous to the surface of the sun, and with this surface being spherical rather than any other shape, and with their being carried around by the rotation of the sun itself, so the same phenomena are opposed to every other theory that may be proposed to explain them.

CHAPTER 18

NEW MOONS

THERE has been considerable controversy among modern historians as to who should be credited with the invention of the telescope. Galileo made no such claim for himself. His own technical skill, however, was evident in his ability to design and construct a telescope merely on hearsay. What is more, he continually strove to improve the instrument, first by making use of specially ground lenses (not merely available spectacle lenses) and later by increasing the magnification. His curiosity turned his terrestrial spy-glass toward heaven (if not the first to do so, then at least the first to appreciate scientifically the view). The earth's moon was revealed as having a rough surface, contrary to Aristotelian notions. He accounted for "the new moon in the old moon's arms" as earthshine, and gave a reasonable estimate of height (4 miles) of the mountains on the moon — quantitative evidence that the moon is not a perfect sphere.

His discovery of bright *planetary moons* about Jupiter is a fascinating example of a scientific investigation that began with his casually noting an interesting phenomenon, but then continued with his determinedly seeking to understand it. The later discovery of *moonlike phases* of the planet Venus was even less a matter of luck. Galileo's work with the telescope belies a modern portraiture of him as a Platonic dreamer or even as a pure mathematician. His whole research approach was essentially experiential.

Such discoveries, to be sure, did not confirm the Copernican hypothesis. They did, however, give credence to the existence of a *universe*, i.e. not a duoverse with a perfect heaven and an imperfect earth, and to the probability that the earth is not the center of motion for all heavenly bodies.

The Starry Messenger†

About ten months ago a report reached my ears that a certain Fleming had constructed a spyglass by means of which visible objects, though very distant from the eye of the observer, were distinctly seen as if nearby. Of this truly remarkable effect several

† Ref. (4), pp. 28–33, 40–1, 51–7.

experiences were related, to which some persons gave credence while others denied them. A few days later the report was confirmed to me in a letter from a noble Frenchman at Paris, Jacques Badovere, which caused me to apply myself wholeheartedly to inquire into the means by which I might arrive at the invention of a similar instrument. This I did shortly afterwards, my basis being the theory of refraction. First I prepared a tube of lead, at the ends of which I fitted two glass lenses, both plane on one side while on the other side one was spherically convex and the other concave. Then placing my eye near the concave lens I perceived objects satisfactorily large and near, for they appeared three times closer and nine times larger than when seen with the naked eye alone. Next I constructed another one, more accurate, which represented objects as enlarged more than sixty times. Finally, sparing neither labor nor expense, I succeeded in constructing for myself so excellent an instrument that objects seen by means of it appeared nearly one thousand times larger and over thirty times closer than when regarded with our natural vision.

It would be superfluous to enumerate the number and importance of the advantages of such an instrument at sea as well as on land. But forsaking terrestrial observations, I turned to celestial ones, and first I saw the moon from as near at hand as if it were scarcely two terrestrial radii away. After that I observed often with wondering delight both the planets and the fixed stars, and since I saw these latter to be very crowded, I began to seek (and eventually found) a method by which I might measure their distances apart.

Here it is appropriate to convey certain cautions to all who intend to undertake observations of this sort, for in the first place it is necessary to prepare quite a perfect telescope, which will show all objects bright, distinct, and free from any haziness, while magnifying them at least four hundred times and thus showing them twenty times closer. Unless the instrument is of this kind it will be vain to attempt to observe all the things which I have seen in the heavens, and which will presently be set forth. Now in order to determine without much trouble the magnifying power of an instrument, trace on paper the contour of two circles or two squares of which one is four hundred times as large as the other, as it will be when the diameter of one is twenty times that of the other. Then, with both these figures attached to the same wall, observe them simultaneously from a distance, looking at the smaller one through the telescope and at the larger one with the other eye unaided. This may be done without inconvenience while holding both eyes open at the same time; the two figures will appear to be of the same size if the instrument magnifies objects in the desired proportion.

Such an instrument having been prepared, we seek a method of measuring distances apart. This we shall accomplish by the following contrivance.

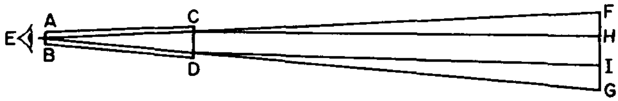


FIG. 39.

Let ABCD be the tube and E be the eye of the observer. Then if there were no lenses in the tube, the rays would reach the object FG along the straight lines ECF and EDG. But when the lenses have been inserted, the rays go along the refracted lines ECH and EDI; thus they are brought closer together, and those which were previously directed freely to the object FG now include only the portion of it HI. The ratio of the distance EH to the line HI then being found, one may by means of a table of sines determine the size of the angle formed at the eye by the object HI, which we shall find to be but a few minutes of arc. Now, if to the lens CD we fit thin plates, some pierced with larger and some with smaller apertures, putting now one plate and now another over the lens as required, we may form at pleasure different angles subtending more or fewer minutes of arc, and by this means we may easily measure the intervals between stars which are but a few minutes apart, with no greater error than one or two minutes. And for the present let it suffice that we have touched lightly on these matters and scarcely more than mentioned them, as on some other occasion we shall explain the entire theory of this instrument.

Now let us review the observations made during the past two months, once more inviting the attention of all who are eager for true philosophy to the first steps of such important contemplations. Let us speak first of that surface of the moon which faces us. For greater clarity I distinguish two parts of this surface, a lighter and a darker; the lighter part seems to surround and to pervade the whole hemisphere, while the darker part discolors the moon's surface like a kind of cloud, and makes it appear covered with spots. Now those spots which are fairly dark and rather large are plain to everyone and have been seen throughout the ages; these I shall call the "large" or "ancient" spots, distinguishing them from others that are smaller in size but so numerous as to occur all over the lunar surface, and especially the lighter part. The latter spots had never been seen by anyone before me. From observations of these spots repeated many times I have been led to the opinion and conviction that the surface of the moon is not smooth, uniform, and precisely spherical as a great number of philosophers believe it (and the other heavenly bodies) to be, but is uneven, rough, and full of cavities and prominences, being not unlike the face of the earth, relieved by chains of mountains and deep valleys. The things I have seen by which I was enabled to draw this conclusion are as follows.

On the fourth or fifth day after new moon, when the moon is seen with brilliant horns, the boundary which divides the dark part from the light does not extend uniformly in an oval line as would happen on a perfectly spherical solid, but traces out an uneven, rough, and very wavy line as shown in the figure below. Indeed, many luminous excrescences extend beyond the boundary into the darker portion, while on the other hand some dark patches invade the illuminated part. Moreover a great quantity of small blackish spots, entirely separated from the dark region, are scattered almost all over the area illuminated by the sun with the exception only of that part which is occupied by the large and ancient spots. Let us note, however, that the said small spots always agree in having their blackened parts directed toward the sun, while on the side opposite the sun they are crowned with bright contours like shining summits. There is a similar sight on earth about sunrise, when we behold the valleys not yet flooded with light though the mountains surrounding them are already ablaze with glowing splendor on the side opposite the sun. And just as the shadows in the hollows on earth diminish in size as the sun rises higher, so these spots on the moon lose their blackness as the illuminated region grows larger and larger.

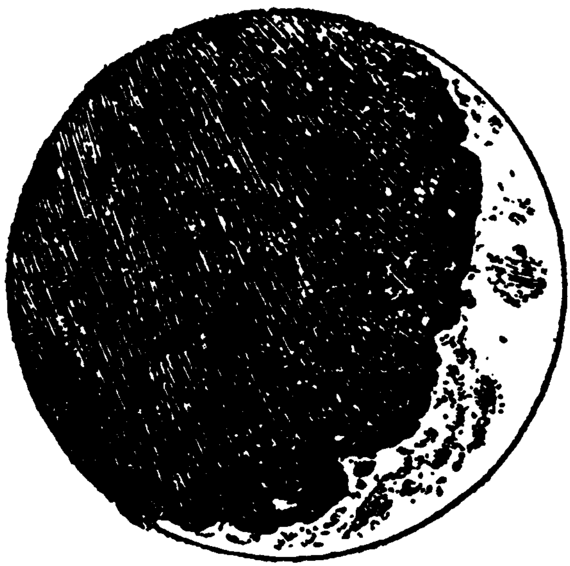


FIG. 40.

Again, not only are the boundaries of shadow and light in the moon seen to be uneven and wavy, but still more astonishingly many bright points appear within the darkened portion of the moon, completely divided and separated from the illuminated part and at a considerable distance from it. After a time these gradually increase in size and brightness, and an hour or two later they become joined with the rest of the lighted part which has now increased in size. Meanwhile more and more peaks shoot up as if sprouting now here, now there, lighting up within the shadowed portion; these become larger, and finally they too are united with that same luminous surface which extends ever further. An illustration of this is to be seen in the figure above. And on the earth, before the rising of the sun, are not the highest peaks of the mountains illuminated by the sun's rays while the plains remain in shadow? Does not the light go on spreading while the larger central parts of those mountains are becoming illuminated? And when the sun has finally risen, does not the illumination of plains and hills finally become one? But on the moon the variety of elevations and depressions appears to surpass in every way the roughness of the terrestrial surface, as we shall demonstrate further on.

That the lighter surface of the moon is everywhere dotted with protuberances and gaps has, I think, been made sufficiently clear from the appearances already explained. It remains for me to speak of their dimensions, and to show that the earth's irregularities are far less than those of the moon. I mean that they are absolutely less, and not merely in relation to the sizes of the respective globes. This is plainly demonstrated as follows.

I had often observed, in various situations of the moon with respect to the sun, that some summits within the shadowy portion appeared lighted, though lying some distance from the boundary of the light. By comparing this separation to the whole diameter of the moon, I found that it sometimes exceeded one-twentieth of the diameter. Accordingly, let CAF be a great circle of the lunar body, E its center, and CF a diameter, which is to the diameter of the earth as two is to seven.

Since according to very precise observations the diameter of the earth is seven thousand miles, CF will be two thousand, CE one thousand, and one-twentieth of CF will be one hundred miles. Now let CF be the diameter of the great circle which divides the light part of the moon from the dark part (for because of the very great distance of the sun from the moon, this does not differ appreciably from a great circle), and let A be distant from C by one-twentieth of this. Draw the radius EA, which, when produced, cuts the tangent line GCD (representing the illuminating ray) in the point D. Then the arc CA, or rather the straight line CD, will consist of one hundred units whereof CE contains one thousand, and the sum of the squares of DC and CE will be 1,010,000 This is equal to the square of DE; hence ED will exceed 1,004, and AD will be more than four of those units of which CE contains one thousand. Therefore the altitude AD on the moon, which represents a summit reaching up to the solar ray GCD and standing at the distance CD from C, exceeds four miles. But on the earth we have no mountains which reach to a perpendicular height of even one mile. Hence it is quite clear that the prominences on the moon are loftier than those on the earth.

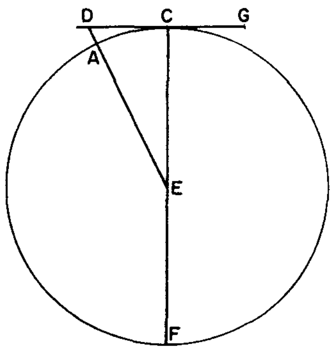


FIG. 41.

On the seventh day of January in this present year 1610, at the first hour of night, when I was viewing the heavenly bodies with a telescope, Jupiter presented itself to me; and because I had prepared a very excellent instrument for myself, I perceived (as I had not before, on account of the weakness of my previous instrument) that beside the planet there were three starlets, small indeed, but very bright. Though I believed them to be among the host of fixed stars, they aroused my curiosity somewhat by appearing to lie in an exact straight line parallel to the ecliptic, and by their being more splendid than others of their size. Their arrangement with respect to Jupiter and each other was the following:

East * * ○ * *West*

that is, there were two stars on the eastern side and one to the west. The most easterly star and the western one appeared larger than the other. I paid no attention to the distances between them and Jupiter, for at the outset I thought them to be fixed stars, as I have said. But returning to the same investigation on January eighth — led by what, I do not know — I found a very different arrangement. The three starlets were now all to the west of Jupiter, closer together, and at equal intervals from one another as shown in the following sketch:

East ○ * * * *West*

At this time, though I did not yet turn my attention to the way the stars had come together, I began to concern myself with the question how Jupiter could be east of all these stars when on the previous day it had been west of two of them. I commenced to wonder whether Jupiter was not moving eastward at that time, contrary to the computations of the astronomers, and had got in front of them by that motion. Hence it was with great interest that I awaited the next night. But I was disappointed in my hopes, for the sky was then covered with clouds everywhere.

On the tenth of January, however, the stars appeared in this position with respect to Jupiter:

East * * ○ *West*

that is, there were but two of them, both easterly, the third (as I supposed) being hidden behind Jupiter. As at first, they were in the same straight line with Jupiter and were arranged precisely in the line of the zodiac. Noticing this, and knowing that there was no way in which such alterations could be attributed to Jupiter's motion, yet being certain that these were still the same stars I had observed (in fact no other was to be found along the line of the zodiac for a long way on either side of Jupiter), my perplexity was now transformed into amazement. I was sure that the apparent changes belonged not to Jupiter but to the observed stars, and I resolved to pursue this investigation with greater care and attention.

And thus, on the eleventh of January, I saw the following disposition:

East ✳ ✳ ○ *West*

There were two stars, both to the east, the central one being three times as far from Jupiter as from the one farther east. The latter star was nearly double the size of the former, whereas on the night before they had appeared approximately equal.

I had now decided beyond all question that there existed in the heavens three stars wandering about Jupiter as do Venus and Mercury about the sun, and this became plainer than daylight from observations on similar occasions which followed. Nor were there just three such stars; four wanderers complete their revolutions about Jupiter, and of their alterations as observed more precisely later on we shall give a description here. Also I measured the distances between them by means of the telescope, using the method explained before. Moreover I recorded the times of the observations, especially when more than one was made during the same night — for the revolutions of these planets are so speedily completed that it is usually possible to take even their hourly variations.

Thus on the twelfth of January at the first hour of night I saw the stars arranged in this way:

East ✳ ✳○ ✳ *West*

The most easterly star was larger than the western one, though both were easily visible and quite bright. Each was about two minutes of arc distant from Jupiter. The third star was invisible at first, but commenced to appear after two hours; it almost touched Jupiter on the east, and was quite small. All were on the same straight line directed along the ecliptic.

On the thirteenth of January four stars were seen by me for the first time, in this situation relative to Jupiter:

East * ○ * * * *West*

Three were westerly and one was to the east; they formed a straight line except that the middle western star departed slightly toward the north. The eastern star was two minutes of arc away from Jupiter, and the intervals of the rest from one another and from Jupiter were about one minute. All the stars appeared to be of the same magnitude, and though small were very bright, much brighter than fixed stars of the same size.

. . . . .

On the twenty-sixth of February, midway in the first hour of night, there were only two stars:

East * ○ * *West*

One was to the east, ten minutes from Jupiter; the other to the west, six minutes away. The eastern one was somewhat smaller than the western. But at the fifth hour three stars were seen:

East * ○ * * *West*

In addition to the two already noticed, a third was discovered to the west near Jupiter; it had at first been hidden behind Jupiter and was now one minute away. The eastern one appeared farther away than before, being eleven minutes from Jupiter.

This night for the first time I wanted to observe the progress of Jupiter and its accompanying planets along the line of the zodiac in relation to some fixed star, and such a star was seen to the east, eleven minutes distant from the easterly starlet and a little removed toward the south, in the following manner:

East * ○ * * *West*

★

On the twenty-seventh of February, four minutes after the first hour, the stars appeared in this configuration:

East ✳ ⋆○ ✳✳ *West*

★

The most easterly was ten minutes from Jupiter; the next, thirty seconds; the next to the west was two minutes thirty seconds from Jupiter, and the most westerly was one minute from that. Those nearest Jupiter appeared very small, while the end ones were plainly visible, especially the westernmost. They marked out an exactly straight line along the course of the ecliptic. The progress of these planets toward the east is seen quite clearly by reference to the fixed star mentioned, since Jupiter and its accompanying planets were closer to it, as may be seen in the figure above. At the fifth hour, the eastern star closer to Jupiter was one minute away.

At the first hour on February twenty-eighth, two stars only were seen; one easterly, distant nine minutes from Jupiter, and one to the west, two minutes away. They were easily visible and on the same straight line. The fixed star, perpendicular to this line, now fell under the eastern planet as in this figure:

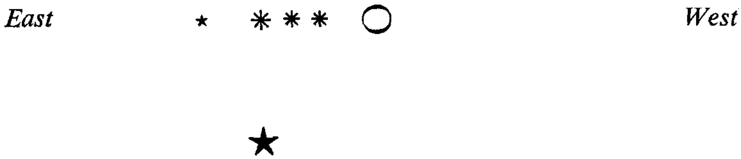
East ✳ ○ ✳ *West*

★

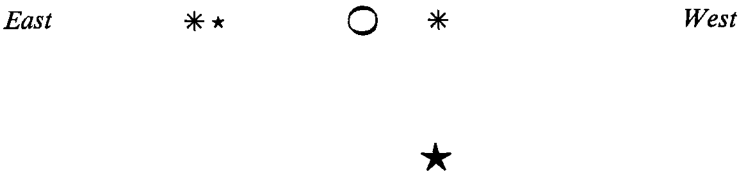
At the fifth hour a third star, two minutes east of Jupiter, was seen in this position:

East ✳ ✳ ○ ✳ *West*

On the first of March, forty minutes after sunset, four stars all to the east were seen, of which the nearest to Jupiter was two minutes away, the next was one minute from this, the third two seconds from that and brighter than any of the others; from this in turn the most easterly was four minutes distant, and it was smaller than the rest. They marked out almost a straight line, but the third one counting from Jupiter was a little to the north. The fixed star formed an equilateral triangle with Jupiter and the most easterly star, as in this figure:



On March second, half an hour after sunset, there were three planets, two to the east and one to the west, in this configuration:



The most easterly was seven minutes from Jupiter and thirty seconds from its neighbor; the western one was two minutes away from Jupiter. The end stars were very bright and were larger than that in the middle, which appeared very small. The most easterly star appeared a little elevated toward the north from the straight line through the other planets and Jupiter. The fixed star previously mentioned was eight minutes from the western planet along the line drawn from it perpendicularly to the straight line through all the planets, as shown above.

I have reported these relations of Jupiter and its companions with the fixed star so that anyone may comprehend that the progress of those planets, both in longitude and latitude, agrees exactly with the movements derived from planetary tables.

Such are the observations concerning the four Medicean planets recently first discovered by me, and although from these data their periods have not yet been reconstructed in numerical form, it is legitimate at least to put in evidence some facts worthy of note. Above all, since they sometimes follow and sometimes precede Jupiter by the same intervals, and they remain within very limited distances either to east or west of Jupiter, accompanying that planet in both its retrograde and direct movements in a constant manner, no one can doubt that they complete their revolutions about Jupiter and at the same time effect all together a twelve-year period about the center of the universe. That they also revolve in unequal circles is manifestly deduced from the fact that at the greatest elongation from Jupiter it is never possible to see two of these planets in conjunction, whereas in the vicinity of Jupiter they are found united two, three, and sometimes all four together. It is also observed that the revolutions are swifter in those planets which describe smaller circles about Jupiter, since the stars closest to Jupiter are usually seen to the east when on the previous day they appeared to the west, and vice versa, while the planet which traces the largest orbit appears upon accurate observation of its returns to have a semimonthly period.

Here we have a fine and elegant argument for quieting the doubts of those who, while accepting with tranquil mind the revolutions of the planets about the sun in the Copernican system, are mightily disturbed to have the moon alone revolve about the earth and accompany it in an annual rotation about the sun. Some have believed that this structure of the universe should be rejected as impossible. But now we have not just one planet rotating about another while both run through a great orbit around the sun; our own eyes show us four stars which wander around Jupiter as does the moon around the earth, while all together trace out a grand revolution about the sun in the space of twelve years.

CHAPTER 19

PARALLAX OF A STAR

LOOKING out a window we note that a tree hides something behind it farther away. If we shift to the right or to the left, the background seems to also move to the right or left, respectively. The actual displacement depends upon how far we move, and how close the tree is. If, say, the tree is just in front of a house, the displacement will be relatively small.

Imagine a star nearer to the earth than others behind it. As the earth revolves annually about the sun, we move now to its right, now to its left. The near star, therefore, should appear periodically displaced relative to the others — it should show parallax ("change beside"). Astronomers, however, failed for centuries to detect any such displacement due to the earth's motion — owing to the great distance of all stars. Stellar parallax was not detected until 1837–40, when Bessel repeated accurate measurements of 61 Cygni, a fifth magnitude star which had been found to have a very large proper motion (indicative of proximity to the earth). For this star, his observations yielded a parallax of only 0.35.

Dialogue Concerning Two Chief World Systems — Ptolemaic and Copernican†

SAGR. Getting back to the point, I invite Simplicio to consider how the approach and retreat which the earth makes with respect to some fixed star near the pole may be made as if by a straight line, for such is the diameter of the earth's orbit. Hence the attempt to compare the rising and falling of the polestar due to motion along such a diameter with that due to motion over the small circle of the earth strongly indicates a lack of understanding.

SIMP. But we are still in the same difficulty, since not even the small variation which ought to exist is to be found, and if the variation is null, then the annual motion attributed to the earth along its orbit must also be admitted to be null.

†Ref. (7), pp. 377–9, 382–6.

SAGR. Now I shall let Salviati resume, who I believe would not shrug off as nonexistent the rising or dropping of the polestar or of some other fixed star. I say this even though such events may not be known to anyone, and were assumed by Copernicus himself to be, I shall not say null, but unobservable because of their smallness.

SALV. I said earlier that I do not believe anyone has set himself the task of observing whether variations which might depend upon an annual movement of the earth are to be perceived in any fixed star at the various seasons of the year, and I added that I doubt whether anyone has very clearly understood just what variations should appear, or among what stars. Therefore it will be good for us to examine this point carefully.

I have indeed found authors writing in general terms that the annual motion of the earth should not be admitted because it is improbable that visible changes would not then be seen in the fixed stars. Not having heard anyone go on to say what, in particular, these visible changes ought to be, and in what stars, I think it quite reasonable to suppose that those who say generally that the fixed stars remain unchanged have not understood (and perhaps have not even tried to find out) the nature of these alterations, or what it is that they mean ought to be seen. In making this judgment I have been influenced by knowing that the annual movement attributed to the earth by Copernicus, if made perceptible in the steller sphere, would not produce visible alterations equally among all stars, but would necessarily make great changes in some, less in others, still less in yet others, and finally none in some stars, however great the size of the circle assumed for this annual motion. The alterations which should be seen, then, are of two sorts; one is an apparent change in size of these stars, and the other is a variation in their altitudes at the meridian, which implies as a consequence the varying of places of rising and setting, of distances from the zenith, etc.

SAGR. I think that what I see coming is like a ball of string so snarled that without God's help I may never manage to disentangle it; for to confess my deficiencies to Salviati, I have often thought about this without ever getting hold of the loose end of it. I say this not so much in reference to things pertaining to the fixed stars as to an even more terrifying task that you have brought to my mind by mentioning these meridian altitudes, latitudes of rising, distances from the zenith, etc. The reeling of my brain has its origin in what I shall now tell you.

Copernicus assumes the stellar sphere to be motionless, with the sun likewise motionless in the center of it. Therefore all alterations in the sun or in the fixed stars which may appear to us must necessarily belong to the earth; that is, be ours. But the sun rises and sets along a very great arc on our meridian — almost forty-seven degrees — and its deviations in rising and setting vary by still greater arcs along the oblique horizons. Now how can the earth be so remarkably tilted and elevated with respect to the sun, and not at all so with regard to the fixed stars — or so little as to be imperceptible? This is the knot which has never passed through my comb, and if you untie it for me I shall consider you greater than an Alexander.

SALV. These difficulties do credit to Sagredo's ingenuity; the question is one which Copernicus himself despaired of explaining in such a way as to make it intelligible, as will be seen both from his own admission of its obscurity and from his setting out twice to explain it, in two different ways. And without affectation I admit not having understood his explanation myself, until I had made it intelligible in still another way which is quite plain and clear, and this only after a long and laborious application of my mind.

SIMP. Aristotle saw the same objection, and made use of it to disprove some of the ancients who would have had it that the earth was a planet. Against them he reasoned that if it were, it would be necessary for it, like the other planets, to have more than one movement to produce these variations in the risings and settings of the fixed stars as well as in their meridian altitudes. And since he raised the difficulty without solving it, it must necessarily be very difficult of solution, if not entirely impossible.

SALV. The strength and force of the knotting make the untying the more beautiful and admirable, but this I do not promise you today; you must excuse me until tomorrow. For the present, let us go on considering and explaining these alterations and differences which ought to be perceived in the fixed stars on account of the annual movement, as we were just saying.

SAGR. I see it clearly now, thanks to your having awakened my mind, first by telling me positively that a fallacy existed, and next by commencing to interrogate me in general about the means of my recognizing the stoppings and retrograde motions of the planets. Now, this is known by comparing the planets with the fixed stars, in relation to which they are seen to vary their movements now westward, now eastward, and sometimes to remain practically motionless. But beyond the stellar sphere there is not another sphere, immensely more remote and visible to us, with which we might compare the fixed stars. Hence not a trace could we discover in them of anything corresponding to what appears among the planets. I believe that this is what you were so anxious to draw from my mouth.

SALV. And there it is, with the addition of your most subtle insight to boot. And if I, with my little joke, opened your mind, you with yours have reminded me that it is not entirely impossible for something some time to become observable among the fixed stars by which it might be discovered what the annual motion does reside in. Then they, too, no less than the planets and the sun itself, would appear in court to give witness to such motion in favor of the earth. For I do not believe that the stars are spread over a spherical surface at equal distances from one center; I suppose their distances from us to vary so much that some are two or three times as remote as others. Thus if some tiny star were found by the telescope quite close to some of the larger ones, and if that one were therefore very very remote, it might happen that some sensible alterations would take place in it corresponding to those of the outer planets.

So much for the moment with regard to the special case of stars placed in the ecliptic. Let us now go to the fixed stars outside the ecliptic, and assume a great circle vertical to its plane, for example a circle that would correspond in the stellar sphere to the solstitial colure. This we shall mark CEH, and it will be a meridian at the same time. Let us take in it a star outside the ecliptic, which can be E here. Now this will indeed vary its elevation with the movement of the earth, because from the earth at A it will be seen along the ray AE, with the elevation of the angle EAC, but from the earth at B it will be seen along the ray BE, with an angle of elevation EBC. This is greater than EAC, on account of its being an exterior angle of the triangle EAB, while the other is the opposite interior angle. Hence the distance of the star E from the ecliptic would be seen to be changed, and also its meridian altitude would be greater in position B than in the place A, in proportion as the angle EBC exceeds EAC; that is, by the angle AEB. For the side AB of the triangle EAB being produced to C, the exterior angle EBC (being equal to the two opposite interior angles E and A) exceeds A by the size of the angle E. And if we take another star in the same meridian farther from the ecliptic — let this be the star H — then this will be even greater in variation when seen from the two positions A and B, according as the angle AHB becomes greater than the angle E. This angle will continue to increase in proportion as the star observed gets farther from the ecliptic, until finally the maximum alteration will appear in that star which is placed at the very pole of the ecliptic. For a complete understanding, this may be demonstrated as follows:

Let the diameter of the earth's orbit be AB, whose center is G, and assume it to be extended out to the stellar sphere in the points D and C. From the center G, let the axis GF of the ecliptic be erected as far as the same sphere, in which a meridian DFC vertical to the plane of the ecliptic is assumed to be described. Taking, in the arc FC, any points H and E as places of fixed stars, add the lines FA, FB, AH, HG, HB, AE, GE, and BE. Then AFB is the angle of difference (or we may say the parallax) of the star placed at the pole F; that of the star at H is the angle AHB, and for the star at E it is the angle AEB. I say that the angle of difference of the polestar F is the maximum; of the others, those closest to this maximum are larger than those more distant from it. That is, the angle F is greater than the angle H, and this is greater than the angle E.

Suppose a circle described about the triangle FAB. Since the angle F is acute, its base AB being less than the diameter DC of the semicircle DFC, it will fall in the larger portion of the circumscribed circle cut by the base AB. And since AB is divided in the center and at right angles to FG, the center of the circumscribed circle will be in the line FG. Let this be the point I. Now of all the lines drawn to the circumference of the circumscribed circle from the point G, which is not its center, the greatest is that which

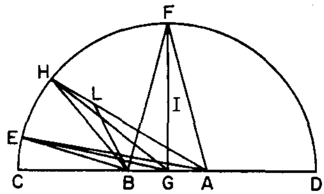


FIG. 42.

passes through the center. Hence FG will be greater than any other line drawn through G to the circumference of the same circle, and therefore this circumference will cut the line GH, which is equal to the line GF, and cutting GH it will also cut AH. Let it cut that in L, and add the line LB. Then the two angles AFB and ALB will be equal, being included in the same portion of the circumscribed circle. But ALB, an exterior angle, is greater than the interior angle H; therefore angle F is greater than angle H.

By the same method we may show that the angle H is greater than the angle E, because the center of the circle described about the triangle AHB is on the perpendicular GF, to which the line GH is closer than the line GE; hence its circumference cuts GE and also AE, from which the proposition is obvious.

From this we conclude that the alteration of appearance (which, using the proper technical term, we may call the parallax of the fixed stars) is greater or less according as the stars observed are more or less close to the pole of the ecliptic, and that finally for stars on the ecliptic itself the alteration is reduced to nothing. Next, as to the earth approaching and retreating from the stars by its motion, those stars which are on the ecliptic are made nearer or farther by the entire diameter of the earth's orbit, as we have already seen. For those which lie near the pole of the ecliptic, this approach and retreat is almost nothing, while for others the alteration is made greater as the stars become closer to the ecliptic.

In the third place we may see that this alteration of appearance is greater or less according as the observed star is closer to or more remote from us. For if we draw another meridian less distant from the earth (which shall be DFI here), a star placed at F and seen along the same ray AFE with the earth at A, when it is later observed from the earth at B will be seen along the ray BF, and will make the angle of difference BFA greater than the first one, AEB, being exterior to the triangle BFE.

SAGR. I have listened to your discourse with great pleasure, and with profit too; now, to make sure that I have understood everything, I shall state briefly the heart of your conclusions.

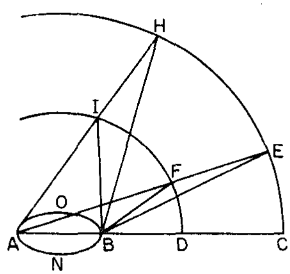


FIG. 43.

It seems to me that you have explained to us two sorts of differing appearances as being those which because of the annual motion of the earth we might observe in the fixed stars. One is their variation in apparent size as we, carried by the earth, approach them or recede from them; the other (which likewise depends upon this same approach and retreat) is their appearing to us to be now more elevated and now less so on the same meridian. Besides this you tell us (and I thoroughly understand) that these two alterations do not occur equally in all stars, but to a greater extent in some, to a lesser in others, and not at all in still others. The approach and retreat by which the same star ought to appear larger at one time and smaller at another is imperceptible and practically nonexistent for stars which are close to the pole of the ecliptic, but it is great for the stars placed in the ecliptic itself, being intermediate for those in between. The reverse is true of the other alteration; that is, the elevation or lowering is nil for stars along the ecliptic and large for those encircling the pole of the ecliptic, being intermediate for those in the middle.

Furthermore, both these alterations are more perceptible in the closest stars, less sensible in those more distant, and would ultimately vanish for those extremely remote.

So much for my part. The next thing, so far as I can see, is to convince Simplicio. I think he will not easily be reconciled to admitting such alterations as these to be imperceptible, stemming as they do from such a vast movement of the earth and from a change that carries the earth to places twice as far apart as our distance from the sun.

SIMP. Really, to be quite frank, I do feel a great repugnance against having to concede the distance of the fixed stars to be so great that the alterations just explained would have to remain entirely imperceptible in them.

SALV. Do not completely despair, Simplicio; perhaps there is yet some way of tempering your difficulties. First of all, that the apparent size of the stars is not seen to alter visibly need not appear entirely improbable to you when you see that men's estimates in such a matter may be so grossly in error, particularly when looking at brilliant objects. Looking, for example, at a burning torch from a distance of two hundred paces, and then coming closer by three or four yards, do you believe that you yourself would perceive it as larger? For my part, I should certainly not discover this even if I approached by twenty or thirty paces; sometimes I have even happened to see such a light at a distance, and been unable to decide whether it was coming toward me or going away, when in fact it was approaching. Now what of this? If the same approach and retreat of Saturn (I mean double the distance from the sun to us) is almost entirely imperceptible, and if it is scarcely noticeable in Jupiter, what could it amount to in the fixed stars, which I believe you would not hesitate to place twice as far away as Saturn? In Mars, which while approaching us . . .

SIMP. Please do not labor this point, for I am indeed convinced that what you have said about the unaltered appearance of the apparent sizes of the fixed stars may very well be the case. But what shall we say to that other difficulty which arises from no variation at all being seen in their changing aspects?

CHAPTER 20

NATURE—GOD'S HANDIWORK

ALTHOUGH Galileo was not deeply spiritual, he was sincerely religious and a loyal churchman. The celebrated Galileo case, therefore, does not truly revolve about the perennial issues of science versus religion *per se*. On the contrary, he himself showed genuine understanding of this problem in his own attempt to reconcile public natural science with his personal religious faith. Believing in God as the Author of both nature and the Bible, he refused to regard any apparent disagreement as more than a natural human shortcoming due to necessarily incomplete human understanding. His attitude, I believe, would be generally acceptable to many modern Christians.

What took place historically was an inevitable clash between the limited private opinion of an individual and the limited accepted judgment of a social group, of which he was a member. The controversy was theological only in the sense that the orthodox theology of that period was couched in Aristotelian philosophy, which was characterized by such universal breadth and organic coherence that the disturbance of any part was propagated throughout the whole. Furthermore, the entire matter was complicated by the friction of narrow personalities: Peripatetics and Platonists, Dominicans and Jesuits, the Pope and even Galileo himself. Neither "truth" nor academic freedom was basically at stake; the affair was primarily sociological. The history of science itself, as well as the history of the Church, has had similar incidents when an individual's idea has been lost in a social morass.

Galileo's personal views on the relative roles of science and of the Bible are clearly stated in his letter to the dowager Grand Duchess. He looked upon scientific evidence as one aspect of divine wisdom, but at the same time he warned of human fallibility in interpreting ancient biblical writings. He urged tolerant confidence in the fact that "two truths cannot contradict each other". His presentation revealed a deep understanding of the Scriptures and a broad reading of the Church Fathers. Individuals today still have to resolve their own continually changing conflicts that inevitably arise because of having to incorporate new experiences in necessarily incomplete old understandings. Clues can be obtained from studying the behavior of other religious men of science like Galileo.

Letter to Madame Christine of Lorraine, Grand Duchess of Tuscany, Concerning the Use of Biblical Quotation in Matters of Science†

The reason produced for condemning the opinion that the earth moves and the sun stands still is that in many places in the Bible one may read that the sun moves and the earth stands still. Since the Bible cannot err, it follows as a necessary consequence that anyone takes an erroneous and heretical position who maintains that the sun is inherently motionless and the earth movable.

With regard to this argument, I think in the first place that it is very pious to say and prudent to affirm that the holy Bible can never speak untruth — whenever its true meaning is understood. But I believe nobody will deny that it is often very abstruse, and may say things which are quite different from what its bare words signify. Hence in expounding the Bible if one were always to confine oneself to the unadorned grammatical meaning, one might fall into error. Not only contradictions and propositions far from true might thus be made to appear in the Bible, but even grave heresies and follies. Thus it would be necessary to assign to God feet, hands, and eyes, as well as corporeal and human affections, such as anger, repentance, hatred, and sometimes even the forgetting of things past and ignorance of those to come. These propositions uttered by the Holy Ghost were set down in that manner by the sacred scribes in order to accommodate them to the capacities of the common people, who are rude and unlearned. For the sake of those who deserve to be separated from the herd, it is necessary that wise expositors should produce the true senses of such passages, together with the special reasons for which they were set down in these words. This doctrine is so widespread and so definite with all theologians that it would be superfluous to adduce evidence for it.

Hence I think that I may reasonably conclude that whenever the Bible has occasion to speak of any physical conclusion

† Ref. (4), pp. 181–3, 185–7.

(especially those which are very abstruse and hard to understand), the rule has been observed of avoiding confusion in the minds of the common people which would render them contumacious toward the higher mysteries. Now the Bible, merely to condescend to popular capacity, has not hesitated to obscure some very important pronouncements, attributing to God himself some qualities extremely remote from (and even contrary to) His essence. Who, then, would positively declare that this principle has been set aside, and the Bible has confined itself rigorously to the bare and restricted sense of its words, when speaking but casually of the earth, of water, of the sun, or of any other created thing? Especially in view of the fact that these things in no way concern the primary purpose of the sacred writings, which is the service of God and the salvation of souls — matters infinitely beyond the comprehension of the common people.

This being granted, I think that in discussions of physical problems we ought to begin not from the authority of scriptural passages, but from sense-experiences and necessary demonstrations; for the holy Bible and the phenomena of nature proceed alike from the divine Word, the former as the dictate of the Holy Ghost and the latter as the observant executrix of God's commands. It is necessary for the Bible, in order to be accommodated to the understanding of every man, to speak many things which appear to differ from the absolute truth so far as the bare meaning of the words is concerned. But Nature, on the other hand, is inexorable and immutable; she never transgresses the laws imposed upon her, or cares a whit whether her abstruse reasons and methods of operation are understandable to men. For that reason it appears that nothing physical which sense-experience sets before our eyes, or which necessary demonstrations prove to us, ought to be called in question (much less condemned) upon the testimony of biblical passages which may have some different meaning beneath their words. For the Bible is not chained in every expression to conditions as strict as those which govern all physical effects; nor is God any less excellently revealed in Nature's actions than in the sacred statements of the Bible. Perhaps this is what Tertullian meant by these words:

"We conclude that God is known first through Nature, and then again, more particularly, by doctrine; by Nature in His works, and by doctrine in His revealed word."

From this I do not mean to infer that we need not have an extraordinary esteem for the passages of holy Scripture. On the contrary, having arrived at any certainties in physics, we ought to utilize these as the most appropriate aids in the true exposition of the Bible and in the investigation of those meanings which are necessarily contained therein, for these must be concordant with demonstrated truths. I should judge that the authority of the Bible was designed to persuade men of those articles and propositions which, surpassing all human reasoning, could not be made credible by science, or by any other means than through the very mouth of the Holy Spirit.

Yet even in those propositions which are not matters of faith, this authority ought to be preferred over that of all human writings which are supported only by bare assertions or probable arguments, and not set forth in a demonstrative way. This I hold to be necessary and proper to the same extent that divine wisdom surpasses all human judgment and conjecture.

But I do not feel obliged to believe that that same God who has endowed us with senses, reason, and intellect has intended to forgo their use and by some other means to give us knowledge which we can attain by them.

The same disregard of these sacred authors toward beliefs about the phenomena of the celestial bodies is repeated to us by St. Augustine in his next chapter. On the question whether we are to believe that the heaven moves or stands still, he writes thus:

"Some of the brethren raise a question concerning the motion of heaven, whether it is fixed or moved. If it is moved, they say, how is it a firmament? If it stands still, how do these stars which are held fixed in it go round from east to west, the more northerly performing shorter circuits near the pole, so that heaven (if there is another pole unknown to us) may seem to revolve upon some axis, or (if there is no other pole) may be thought to move as a discus? To these men I reply that it would require many subtle and profound reasonings to find out which of these things is actually so; but to undertake this and discuss it is consistent neither with my leisure nor with the duty of those whom I desire to instruct in essential matters more directly conducing to their salvation and to the benefit of the holy Church."

From these things it follows as a necessary consequence that, since the Holy Ghost did not intend to teach us whether heaven moves or stands still, whether its shape is spherical or like a discus or extended in a plane, nor whether the earth is located at its center or off to one side, then so much the less was it intended to settle for us any other conclusion of the same kind. And the motion or rest of the earth and the sun is so closely linked with the things just named, that without a determination of the one, neither side can be taken in the other matters. Now if the Holy Spirit has purposely neglected to teach us propositions of this sort as irrelevant to the highest goal (that is, to our salvation), how can anyone affirm that it is obligatory to take sides on them, and that one belief is required by faith, while the other side is erroneous? Can an opinion be heretical and yet have no concern with the salvation of souls? Can the Holy Ghost be asserted not to have intended teaching us something that does concern our salvation? I would say here something that was heard from an ecclesiastic of the most eminent degree: "That the intention of the Holy Ghost is to teach us how one goes to heaven, not how heaven goes."

But let us again consider the degree to which necessary demonstrations and sense experiences ought to be respected in physical conclusions, and the authority they have enjoyed at the hands of holy and learned theologians. From among a hundred attestations I have selected the following:

"We must also take heed, in handling the doctrine of Moses, that we altogether avoid saying positively and confidently anything which contradicts manifest experiences and the reasoning of philosophy or the other sciences. For since every truth is in agreement with all other truth, the truth of Holy Writ cannot be contrary to the solid reasons and experiences of human knowledge."

And in St. Augustine we read: "If anyone shall set the authority of Holy Writ against clear and manifest reason, he who does this knows not what he has undertaken; for he opposes to the truth not the meaning of the Bible, which is beyond his comprehension, but rather his own interpretation; not what is in the Bible, but what he has found in himself and imagines to be there."

This granted, and it being true that two truths cannot contradict one another, it is the function of wise expositors to seek out the true senses of scriptural texts. These will unquestionably accord with the physical conclusions which manifest sense and necessary demonstrations have previously made certain to us. Now the Bible, as has been remarked, admits in many places expositions that are remote from the signification of the words for reasons we have already given. Moreover, we are unable to affirm that all interpreters of the Bible speak by divine inspiration, for if that were so there would exist no differences between them about the sense of a given passage. Hence I should think it would be the part of prudence not to permit anyone to usurp scriptural texts and force them in some way to maintain any physical conclusion to be true, when at some future time the senses and demonstrative or necessary reasons may show the contrary. Who indeed will set bounds to human ingenuity? Who will assert that everything in the universe capable of being perceived is already discovered and known? Let us rather confess quite truly that "Those truths which we know are very few in comparison with those which we do not know."

We have it from the very mouth of the Holy Ghost that God delivered up the world to disputations, *so that man cannot find out the work that God hath done from the beginning even to the end.* In my opinion no one, in contradiction to that dictum, should close the road to free philosophizing about mundane and physical things, as if everything had already been discovered and revealed with certainty. Nor should it be considered rash not to be satisfied with those opinions which have become common. No one should be scorned in physical disputes for not holding to the opinions which happen to please other people best, especially concerning problems which have been debated among the greatest philosophers for thousands of years.

OUTLINE OF LIFE AND WORKS

1564 Born February 15th in Pisa, Italy (father a cloth merchant of lower nobility).
1574 Moved to Florence, Italy.
1579 Attended monastery school of Sta Maria di Vallombrosa (about 25 miles east of Florence) for one year.
1581 Matriculated at the University of Pisa.
1583 Discovered the isochronism of the swinging chandelier in the Cathedral of Pisa.
1585 Returned with family to Florence.
1585 Investigated the concept of center of gravity.
1586 Constructed a hydrostatic balance and wrote a paper on *The Little Balance* (*La Bilancetta*) and its use (published 1644).
1587 First visit (educational) to Rome (met Jesuit astronomer, Father Christopher Clavius, at the Romano Collegio).
1589 Appointed to the Chair of Mathematics at the University of Pisa.
1590 Discovered the cycloid.
1590–1 Experimented from the Leaning Tower of Pisa (legendary). Wrote lecture notes, *On Motion* (*De Motu*), applying Archimedes' principle to motion in a medium; adhered to Aristotle's doctrine of natural places (and the medieval notion of impetus) although criticizing some of his other ideas (published in 1883).
1591 Death of father, Vincenzio Galilei.
1592 Appointed to the Chair of Mathematics at the University of Padua.
1594 Caught chill resulting in an arthritic condition during his whole life.
1597 Designed and constructed a popular "geometric and military compass" (like a modern proportional compass or divider).
1600 Wrote lecture notes, *On Mechanics* (*Le Meccaniche*), a systematic summary about the statics of simple machines as then known (early manuscript versions of 1593 and 1594; published in French in 1634, by M. Mersenne, and in 1649 in Italian).
1600 Favorite daughter, Virginia (later Sister Maria Celeste of San Matteo convent in Arcetri), born of Venetian mistress, Marina Gamba, who he kept in Padua.
1601 Daughter Livia (later Sister Archangela of San Matteo convent) born.
1602 Investigated magnetism.
1604 Wrote letter to Fra Paolo Sarpi, stating correctly the law of falling bodies, but giving an incorrect proof.
1604 Lectured on the (super) nova, noting its appearance among the fixed stars.

1605 Tutored Cosimo de' Medici, son of Ferdinand I, Grand Duke of Tuscany, during summer vacation at Florence.

1606 Constructed a thermoscope.

1606 Son, Vincenzio, born.

1606 Published his first book, *Instructions on the Use of the Geometric and Military Compass* (*Le Operazioni del Compasso Geometrico et Militare*).

1607 Published a polemic *Defense against the Calumnies and Impostures of Baldesar Capra* (*Difesa Contro alle Calunnie e imposture di Baldesar Capra*), a Padua student from Milan, who plagiarized Galileo's work and was later condemned for doing so.

1609 Designed and constructed a telescope (Galileian, opera glass).

1610 Used the telescope to view the sky, thereby discovering the myriads of stars in the Milky Way, the moon's mountains, Jupiter's moons (four), Venus phases, and Saturn's ring (indefinite).

1610 Published *The Sidereal Messenger* (*Sidereus Nuncius*), describing his astronomical investigations (good reading for all students today).

1610 Appointed Chief Mathematician and Philosopher to Cosimo II, Grand Duke of Tuscany, at Florence.

1611 Second visit (triumphant) to Rome; observed sun spot; elected sixth member of the Accademia dei Lincei (L.—comparable to later English FRS).

1612 Published *Discourse on Bodies in Water* (*Discorso . . . intorno alle cose, che stanno in su acqua*), describing experimental investigations on floating bodies (ice, ebony chips, wax models, etc.)—first public dispute on natural philosophy (physics) with an informal "league" of peripatetic philosophers—worth reading by physics teachers.

1613 Published, under Lincean auspices, three *Letters on Sunspots* to Mark Welser at Augsburg in opposition to the views of the Jesuit Father Christopher Scheiner—the beginning of a bitter controversy involving priority of sun-spot discovery (neither was rightfully first); contained his first (1612) public endorsement of the Copernican hypothesis.

1613 Wrote a letter to a former pupil the Benedictine, Benedetto Castelli, holder of the Chair of Mathematics at the University of Pisa, about his personal views as to the relation of science and religion; copied by someone for a Dominican priest, Niccolo Lorini, of the Convent of San Marco in Florence (famous for Savonarola and Fra Angelico).

1614–31 Dwelt at the Villa L'Ombrellino at Bellosguardo (west across the Arno) as a guest of Lorenzo Segni.

1614 Denounced as anti-Christian by the Dominican Fra Thomaso Caccini from the pulpit of the Convent of Santa Maria Novella—the beginning of a planned attack by this arch-villain of the celebrated Galileo case.

1615 Denounced as a heretic to the Holy Office at Rome by Father Lorini on the basis of the letter to Castelli.

1615 Revised formally Castelli letter as *Letter to Madame Christina of Lorraine, Grand Duchess of Tuscany* (*Lettero alla Granduchessa di Toscano, Crestina di Lorena*) giving his personal views (generally acceptable—and worth reading today) of apparent conflicts between Biblical statements and scientific findings (published 1636).

1615–16 Third visit (promotional) to Rome, to win friends for the Copernican theory; matter referred officially by Pope Paul V to the Qualifiers of the Congregation of the Index, who ruled that the idea of a central sun was philosophically absurd and formally heretical, and that the idea of a moving earth was censurable in philosophy and erroneous in faith; through Robert Cardinal Bellarmine, Galileo was forbidden, at least, to hold or teach the Copernican theory (the record is not clear as to the specific injunction).

1623 Death of Cosimo II; accession of Ferdinand II.

1623 Maffeo Cardinale Berberini (a friend of Galileo) became Pope Urban VIII.

1623 Published *The Assayer* (*Il Saggiatore*), an outstanding polemic against the opinions of an anonymous Jesuit (Father Horatio Grassi) about the nature of comets (three had appeared in 1618), and a philosophical defense (manifesto) of science (e.g. need for intellectual freedom, distinction of primary and secondary qualities, use of mathematics, etc.).

1624 Fourth visit (friendly) to Rome—six audiences with Pope Urban VIII; designed a compound microscope (?).

1626 Investigated magnetism.

1630 Fifth visit (license) to Rome to obtain permission to publish "The Two Chief World Systems"; death of Galileo's influential friend Prince Federico Cesi, head of the Lincean Academy.

1631 Moved to Martellini's villa, "Il Giojello" (east across the Arno), near the San Matteo convent.

1632 Published *The Two Chief World Systems* (*Dialogo . . . sopra i due Massimi Sistemi del Mondo, Tolemaico, e Copernicano*), a clever literary dialog by an Aristotelean philosopher, Simplicio, and a scientific Florentine, Filippo Salviati, who compare the relative arguments for the Ptolemaic and Copernican theories in an effort to win the neutral, urbane Venetian, Giovanni Francesco Sagredo (discussed telescope data, the rotation and revolution of the earth stellar parallax). The Copernican case, based chiefly upon an explanation (false) of tidal phenomena, is obviously presented more convincingly even than the Pope's unanswerable logic (told by Simplicio at the end)—the obiter dicta include magnetism, relativity of motion, errors in measurement, etc.

1632 Ordered by the Inquisition to stand trial at Rome (sale of "Dialogue" suspended).

1633 Sixth visit (trial) to Rome; examined three times by Inquisition; sentenced (22 June), largely on basis of his failure to comply with 1616 injunction (record not clear); confessed and abjured in Dominican Convent of Santa Maria sopra la Minerva.

1633 Lived at Siena in custody of a former pupil, Archbishop Ascanio Piccolomini; returned to Arcetri home under house arrest.
1634 Death of daughter Virginia.
1637 Blinded in both eyes (glaucoma?).
1638 Wrote letter to Daniello Antonini on his observation of lunar librations.
1638 Published, at Leyden, the *Two New Sciences* (*Discorsi a dimostrazioni matematiche, intorno a due nuove scienze Attenenti alla meccanica e i movimenti locali*, completed 1636); viz., a novel approach to the strength of materials (including a discussion of resistance to fracture, the weight of air, the existence of a vacuum, resistance of a medium, terminal speed, acoustics, speed of light, etc.), and his mature view of terrestrial motions (i.e., velocity, acceleration, inertia, falling bodies, motion on an inclined plane, projectiles, etc.)—discussed by the same characters as in *The Two Chief World Systems* in a more scientific (quite clear) though less literary manner—should be read by all physics students.
1638 Visited by the 29-year-old John Milton.
1639 Refused freedom by Pope.
1639 Received the 18-year-old Vincenzio Viviani as his "last disciple".
1641 Welcomed Evangelista Torricelli (a student of Castelli) as a collaborator (his successor).
1641 Investigated applicability of pendulum motion to a clock.
1642 Died (8 January); buried in Cappella del Companile del Noviziato in Santa Croce.
1736 Transferred to present tomb in Santa Croce (funds left by Viviani), under authorization of Pope Clement XII (Florentine).
1835 Works removed from the Index by Pope Gregory XVI.
1893 Encyclical Providentissimus Deus of Pope Leo XIII states the official position of the Roman Church on the relations of science and religion —little different from Galileo's posture.
1965 Mentioned graciously by Pope Paul VI on visit to Santa Croce.