THE WORLD OF MATHEMATICS

Volume 2

EDITED BY
JAMES NEWMAN

PARTS V-VII

Mathematics and the Physical World

Mathematics and Social Science

The Laws of Chance

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PART V

Mathematics and the Physical World

COMMENTARY ON

GALILEO GALILEI

MODERN science was founded by men who asked more searching questions than their predecessors. The essence of the scientific revolution of the sixteenth and seventeenth centuries is a change in mental outlook rather than a flowering of invention and Galileo, more than any other single thinker, was responsible for that change.

Galileo has been called the first of the moderns. "As we read his writings we instinctively feel at home; we know that we have reached the method of physical science which is still in use." [1] Galileo's primary interest was to discover *how* rather than *why* things work. He did not depreciate the role of theory and was himself unrivaled in framing bold hypotheses. But he recognized that theory must conform to the results of observation, that the schemes of Nature are not drawn up for our easy comprehension. "Nature nothing careth," he says, "whether her abstruse reasons and methods of operating be or be not exposed to the capacity of men." He insisted on the supremacy of the "irreducible and stubborn facts" however "unreasonable" they might seem.[2] "I know very well," says Salviati, a character representing Galileo himself in the *Dialogues Concerning the Two Principal Systems of the World,* "that one sole experiment, or concludent demonstration, produced on the contrary part, sufficeth to batter to the ground . . . a thousand . . . probable Arguments."

The origins of modern science can of course be traced much further back—at least to the thirteenth- and fourteenth-century philosophers, Robert Grosseteste, Adam Marsh, Nicole Oresme, Albertus Magnus, William of Occam.[3] Recent historical researches have broadened our understanding of the evolution of scientific thought, proved its continuity (history, like nature, evidently abhors making jumps) and helped to kill the already tottering myth that the science of the Middle Ages was little more than commentary and sterile exegesis.[4] The enlarged perspective

[1] Sir William Dampier, *A History of Science, and Its Relations with Philosophy and Religion,* Fourth Edition, Cambridge (England), 1949, p. 129.

[2] It is a great mistake, as Whitehead points out, "to conceive this historical revolt as an appeal to reason. On the contrary it was through and through an anti-intellectualist movement. It was the return to the contemplation of brute fact; and it was based on a recoil from the inflexible rationality of medieval thought." *Science and the Modern World,* Chapter I.

[3] See for example Herbert Butterfield, *The Origins of Modern Science,* London, 1949; A. C. Crombie, *Robert Grosseteste and the Origins of Experimental Science,* Oxford, 1953; A. C. Crombie, *Augustine to Galileo, the History of Science,* A.D. *400–1650,* London, 1952; A. R. Hall, *The Scientific Revolution, 1500–1800,* London, 1954.

[4] In this discussion I have drawn on material of mine published in the pages of *Scientific American*; in particular on a review of the Butterfield book cited in the preceding note (*Scientific American,* July 1950, p. 56 *et seq.*) I am much indebted to *The Origins of Modern Science* for its masterly presentation of the period treated above.

does not, however, diminish one's admiration for the stupendous achievements of Galileo. It is in his approach to the problems of motion that his imaginative powers are most wonderfully exhibited. Let us examine briefly the ideas he had to overthrow and the system he had to create in order to found a rational science of mechanics. According to Aristotle, all heavy bodies had a "natural" motion toward the center of the universe, which, for medieval thinkers was the center of the earth. All other motion was "violent" motion, because it required a constant motive force, and because it contravened the tendency of bodies to sink to their natural place. The acceleration of falling bodies was explained on the ground that they moved more "jubilantly"—somewhat like a horse—as they got nearer home. The planetary spheres, seen to be exempt from the "natural" tendency, were kept wheeling in their great arcs by the labors of a sublime Intelligence or Prime Mover. Except for falling bodies things moved only when and as long as effort was expended to keep them moving. They moved fast when the mover worked hard; their motion was impeded by friction; they stopped when the mover stopped. For the motion of terrestrial objects Aristotle had in mind the example of the horse and cart; in the celestial regions his mechanics left "the door halfway open for spirits already."

On the whole, Aristotle's theory of motion squared well enough with common experience, and his teachings prevailed for more than fifteen centuries. Eventually, however, men began to discover small but disturbing inconsistencies between experimental data and the Aristotelian dictates. There was the anomaly of the misbehaving arrow which, according to the horse and cart concept of motion, should have fallen to earth the instant it lost contact with the bowstring. Nor was the traditional explanation of the acceleration of falling bodies swallowed forever without protest. In each case the paradox was met by an ingenious modification of the accepted system (this was known, from Plato's celebrated phrase, as "saving the phenomena"); yet every such tailoring, however skillful, was a source of controversy and raised new suspicions as to the validity of all Aristotle's teachings.

In the fourteenth century Jean Buridan and others at the University of Paris developed a "theory of impetus" which proved to be a major factor in dethroning Aristotelian mechanics. This theory, later picked up by Leonardo da Vinci, held that a projectile kept moving by virtue of a something "inside the body itself" which it had acquired in the course of getting under way. Falling bodies accelerated because "impetus" was continually being added to the constant fall produced by the original weight. The importance of the theory lay in the fact that men for the first time were presented with the idea of motion as a lingering aftereffect derived from an initial impulse. This was midway to the modern view, pretty clearly expressed in Galileo, that a body "continues its motion in a straight line until something intervenes to halt or slacken or deflect it."

What was needed to complete the journey was an extraordinary transposition of ideas from the real to an imaginary world. The ghosts of Plato and Pythagoras returned triumphantly to point the way. Modern mechanics describes quite well how real bodies behave in the real world; its principles and laws are derived, however, from a nonexistent conceptual world of pure, clean, empty, boundless Euclidean space, in which perfect geometric bodies execute perfect geometric figures. Until the great thinkers, operating, in Butterfield's words, "on the margin of contemporary thought," were able to establish the mathematical hypothesis of this ideal Platonic world, and to draw their mathematical consequences, it was impossible for them to construct a rational science of mechanics applicable to the physical world of experience. This was the forward leap of imagination required—the new look at familiar things in an unfamiliar way, to see what in fact there was to be seen, rather than what some classical or medieval writer had said ought to be seen. Buridan, Nicole Oresme and Albert of Saxony with their theory of impetus; Galileo with his beautiful systematization of everyday mechanical occurrences and his ability to picture such situations as the behavior of perfectly spherical balls moving on perfectly horizontal planes; Tycho Brahe with his immense and valuable observational labors in astronomy; Copernicus with the *De Revolutionibus Orbium* and heliocentric hypothesis; Kepler with his laws of planetary motion and his passionate search for harmony and "sphericity"; Descartes with his discourse on method, his determination to have all science as closely knit as mathematics, his wedding of algebra to geometry; Huygens with his mathematical analysis of circular motion and centrifugal force; Gilbert with his *terella*, his theory of magnetism and gravitation; Viète, Stevin and Napier with their aids to simplicity of mathematical notation and operations: each took a part in the grand renovation not only of the physical sciences but of the whole manner of thinking about the furniture of the outside world. They made possible the culminating intellectual creation of the seventeenth century, the clockwork universe of Newton in which marbles and planets rolled about as a result of the orderly interplay of gravitational forces, in which motion was as "natural" as rest, and in which God, once having wound the clock, had no further duties.

Galileo was the principal figure in this drama of changing ideas.[5] He

[5] "Taking his achievements in mechanics as a whole, we must admit that in the progress from the pre-Galileon to the post-Newtonian period, Galileo's contribution extends more than halfway. And it must be remembered that he was the pioneer. Newton said no more than the truth when he declared that if he saw further than other men it was because he stood on the shoulders of giants. Galileo in these matters had no giants on whom to mount; the only giants he encountered were those who had first to be destroyed before vision of any kind became possible." Herbert Dingle, *The Scientific Adventure* ("Galileo Galilei (1564 1642)"), London, 1952, p. 106.

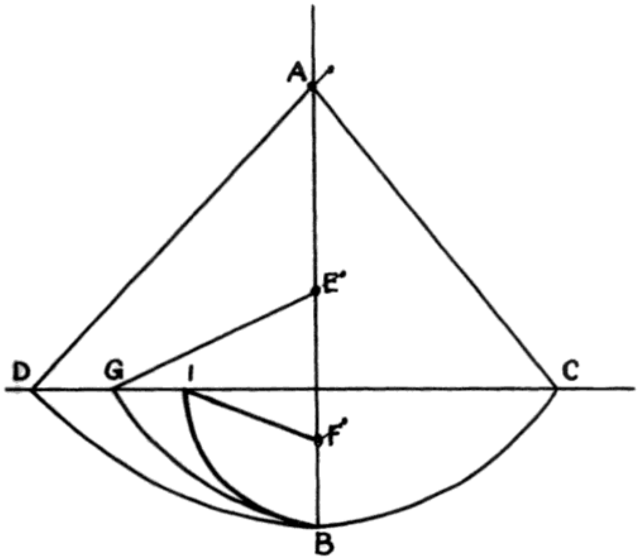
was the first to grasp the importance of the concept of *acceleration* in dynamics. Acceleration means change of velocity, in magnitude or direction. As against Aristotle's view that motion required a force to maintain it, Galileo held that it is not motion but rather "the creation or destruction" of motion or a change in its direction—i.e. acceleration, which requires the application of external force. He discovered the law of falling bodies. This law, as Bertrand Russell remarks, given the concept of "acceleration," is of "the utmost simplicity." [6] A falling body moves with constant acceleration except for the resistance of the air. At first Galileo supposed that the speed of a falling body was proportional to the distance fallen through. When this hypothesis proved unsatisfactory, he modified it to read that speed was proportional to the time of fall. The mathematical consequences of this assumption he was able partially to verify by experiment.

Because free-falling bodies attained a velocity beyond the capacity of the measuring instruments then available, Galileo approached the problem of verification by experiments in which the effect of gravity was "diluted." He proved that a body moving down an inclined plane of given height attains a velocity independent of the angle of slope, and that its terminal speed is the same as if it had fallen through the same vertical height. The trials on the inclined plane thus confirmed his law. The famous story about his dropping different weights from the Tower of Pisa to disprove Aristotle's contention that heavy objects fall faster than light ones is probably untrue; but it follows from Galileo's law that all bodies, heavy or light, are subject to the same acceleration. He himself had no doubt on this score—whether or not he actually corroborated the principle by experiment—but it was not until after his death, when the air pump had been invented, that a complete proof was given by causing bodies to fall in a vacuum.

Experimenting with a pendulum, Galileo obtained further evidence to sustain the principle of "persistence of motion." The swinging bob of a pendulum is analogous to a body falling down an inclined plane. A ball rolling down a plane, assuming negligible friction, will climb another plane to a height equal to that of its starting point. And so, as Galileo found, with the bob. If released at one horizontal level C (as in the diagram), it will ascend to the same height DC, whether it moves by the arc BD or, when the string is caught by nails at E or F, by the steeper arcs BG or BI.[7] It was a short step from his work on the problem of persistence of motion to Newton's first law of motion, also known as the law of inertia.

Another of Galileo's important discoveries in dynamics resulted from his study of the path of projectiles. That a cannon ball moves forward and

[6] Bertrand Russell, *A History of Western Philosophy*, New York, 1945, p. 532.
[7] See Crombie (*Augustine to Galileo*), *op. cit.*, pp. 299–300.



also falls was obvious, but how these motions were combined was not understood. Galileo showed that the trajectory of the projectile could be resolved into two simultaneous motions: one horizontal, with the velocity (disregarding the small air resistance) remaining unchanged, the other vertical, conforming to the law of falling bodies, i.e., 16 feet in the first second, 48 feet in the second second, 80 feet in the third, and so on. Combining the two motions makes the path a parabola. Galileo's principle of the persistence of motion, and his method of dissecting compound motions solved the apparent anomalies gleefully urged by those opposed to the Copernican system. It could now be explained why an object dropped from the mast of a ship fell to the foot of the mast and was not left behind by the ship's motion; why a stone dropped from a tower landed at its base and not to the west of it, even though the rotating earth had moved towards the east while the stone fell. The stone, as Galileo realized, shares the velocity of rotation of the earth, and retains it on the way down.[8]

It was Galileo's way to turn back and forth from hypothesis and deduc-

[8] "In fact if the tower were high enough, there would be the opposite effect to that expected by the opponents of Copernicus. The top of the tower, being further from the center of the earth than the bottom, is moving faster, and therefore the stone should fall slightly to the east of the foot of the tower. This effect, however, would be too slight to be measurable." Russell, *op. cit.*, p. 534.

tion to experiment: no one before him attained a comparable skill in blending experiments with mathematical abstractions. In all his investigations he followed the procedure epitomized in a famous passage of Francis Bacon: "to educe and form axioms from experience . . . to deduce and derive new experiments from axioms. . . . For our road does not lie on a level, but ascends and descends; first ascending to axioms, then descending to works." In the true Platonic tradition he was convinced that the mathematical models which led him to observations were the "enduring reality, the substance, underlying phenomena." [9]

"Philosophy," he wrote in his polemic treatise, *Il Saggiatore*, "is written in that vast book which stands forever open before our eyes, I mean the universe; but it cannot be read until we have learnt the language and become familiar with the characters in which it is written. It is written in mathematical language, and the letters are triangles, circles and other geometrical figures, without which means it is humanly impossible to comprehend a single word."

*   *   *   *   *

The facts of Galileo's life are well known and I shall restrict myself to a bare summary. He was born in 1564, the year Michelangelo died, and he died in 1642, the year in which Newton was born. ("I commend these facts," says Bertrand Russell, "to those (if any) who still believe in metempsychosis.") His father was a noble of Florence and Galileo was well educated, first in medicine, at the University of Pisa, then in mathematics and physics. One of his first discoveries was that of the isochronism of the pendulum; he was seventeen when the sight of a lamp set swinging in the cathedral of Pisa—which he measured by his pulse beats—inspired this conjecture. Another of his early works was the invention of a hydrostatic balance. For eighteen years (1592–1610) he held the chair of mathematics at Padua. He was an enormously popular lecturer, and "such was the charm of his demonstrations that a hall capable of containing 2000 people had eventually to be assigned for the accommoda-

[9] "Galileo's Platonism was of the same kind as that which had led to Archimedes being known in the sixteenth century as the 'Platonic philosopher,' and with Galileo mathematical abstractions got their validity as statements about Nature by being solutions of particular physical problems. By using this method of abstracting from immediate and direct experience, and by correlating observed events by means of mathematical relations which could not themselves be observed, he was led to experiments of which he could not have thought in terms of the old commonsense empiricism. A good example of this is his work on the pendulum.

"By abstracting from the inessentials of the situation, 'the opposition of the air, and line, or other accidents,' he was able to demonstrate the law of the pendulum, that the period of oscillation is independent of the arc of swing and simply proportional to the square-root of the length. This having been proved, he could then reintroduce the previously excluded factors. He showed, for instance, that the reason why a real pendulum, of which the thread was not weightless, came to rest, was not simply because of air resistance, but because each particle of the thread acted as a small pendulum. Since they were at different distances from the point of suspension, they had different frequencies and therefore inhibited each other." Crombie, *op. cit.*, (*Augustine to Galileo*), pp. 295–296.

tion of the overflowing audiences which they attracted." [10] Galileo made first-class contributions to hydrostatics, to the mechanics of fluids and to acoustics. He designed the first pendulum clock, invented the first thermometer (a glass bulb and tube, filled with air and water, with the end of the open tube dipping in a vessel of water), and constructed, from his own knowledge of refraction, one of the first telescopes and a compound microscope.[11] Although he was early drawn to the Copernican system, it was not until 1604, on the appearance of a new star, that he publicly renounced the Aristotelian axiom of the "incorruptibility of the heavens"; a short time later he entirely abandoned the Ptolemaic principles. Having greatly improved on his first telescope, Galileo made a series of discoveries which opened a new era in the history of astronomy. He observed the mountains in the moon and roughly measured their height; the visibility of "the old moon in the new moon's arms" he explained by earth-shine; [12] he discovered four of the eleven satellites of Jupiter,[13] innumerable stars and nebulae, sun spots, the phases of Venus predicted by Copernicus, the librations of the moon.

In 1615, after Galileo had removed from Padua to Florence, his advocacy of the Copernican doctrines began to bring him into conflict with the Church. At first the warnings were mild. "Write freely," he was told by a high ecclesiastic, Monsignor Dini, "but keep outside the sacristy." He made two visits to Rome to explain his position; the second on the accession of Pope Urban VIII, who received him warmly. But the publication in 1632 of his powerfully argued, beautifully written masterpiece, *Dialogue on the Great World Systems*, an evaluation, "sparkling with malice," of the comparative merits of the old and new theories of celestial motion, brought a head-on collision with the Inquisition.[14] Galileo was

[10] *Encyclopaedia Britannica*, Eleventh Edition, article on Galileo.

[11] It is the Dutchman, Jan Lippershey, to whom priority in the invention of these instruments (1600) is usually attributed. See H. C. King, *The History of the Telescope*, Cambridge, 1955.

A small excursion on Galileo's work with the microscope will perhaps be permitted. "When the Frenchman, Jean Tarde, called on Galileo in 1614, he said 'Galileo told me that the tube of a telescope for observing the stars is no more than two feet in length; but to see objects well, which are very near, and which on account of their smaller size are hardly visible to the naked eye, the tube must be two or three times longer. He tells me that with this long tube he has seen flies which look as big as a lamb, are covered all over with hair, and have very pointed nails, by means of which they keep themselves up and walk on glass although hanging feet upwards.'" *Galileo, Opera, Ed. Naz.* Vol. 19, p. 589, as quoted in Crombie, *op. cit.*, p. 352.

[12] Sir Oliver Lodge, *Pioneers of Science*, London, 1928, p. 100.

[13] It is not always possible to prove to a philosopher the existence of a thing by bringing it into plain view. The professor of philosophy at Padua "refused to look through Galileo's telescope, and his colleague at Pisa labored before the Grand Duke with logical arguments, 'as if with magical incantations to charm the new planets out of the sky.'" Dampier, *op. cit.*, p. 130.

[14] In 1953 appeared two new excellent editions of the Dialogue, last translated into English by Sir Thomas Salusbury in 1661: (1) *Galileo's Dialogue on the Great World Systems*, edited by Giorgio de Santillana, Chicago, 1953 (based on the Salusbury translation); (2) *Galileo Galilei—Dialogue Concerning the Two Chief World*

summoned to Rome and tried for heresy. Before his spirit was broken he observed: "In these and other positions certainly no man doubts but His Holiness the Pope hath always an absolute power of admitting or condemning them; but it is not in the power of any creature to make them to be true or false or otherwise than of their own nature and in fact they are." Long questioning—though it is unlikely physical torture was applied—brought him to his knees. He was forced to recant, to recite penitential psalms, and was sentenced to house imprisonment for the rest of his life. He retired to a villa at Arcetri, near Florence, where he continued, though much enfeebled and isolated, to write and meditate. In 1637 he became blind and thereafter the rigor of his confinement was relaxed so as to permit him to have visitors. Among those who came, it is said, was John Milton. He died aged seventy-eight.

I have taken a substantial excerpt from the *Dialogues Concerning Two New Sciences* (*Discorsi e Dimostriazioni Matematiche Intorno a due nuove scienze*),[15] a work completed in 1636. It presents his mature and final reflections on the science of mechanics and is a monument of literature and science. On the margin of Galileo's own copy of the *Dialogue on the Great World Systems* appears a note in his handwriting which sums up his lifelong, passionate and courageous dedication to the unending struggle of reason against authority:

"In the matter of introducing novelties. And who can doubt that it will lead to the worst disorders when minds created free by God are compelled to submit slavishly to an outside will? When we are told to deny our senses and subject them to the whim of others? When people devoid of whatsoever competence are made judges over experts and are granted authority to treat them as they please? These are the novelties which are apt to bring about the ruin of commonwealths and the subversion of the state."

Systems—Ptolemaic and Copernican, translated by Stillman Drake, Foreword by Albert Einstein, Berkeley and Los Angeles, 1953. For the most detailed and searching modern account of Galileo's clash with the Church, and trial for heresy, see Giorgio de Santillana, *The Crime of Galileo*, Chicago, 1955.

[15] The standard English translation is by Henry Crew and Alfonso de Salvio, The Macmillan Company, New York, 1914. A reprint has recently (1952) been issued by Dover Publications, Inc., New York.

[My uncle Toby] proceeded next to Galileo and Torricellius, wherein, by certain Geometrical rules, infallibly laid down, he found the precise part to be a "Parabola"—or else an "Hyperbola,"—and that the parameter, or "latus rectum," of the conic section of the said path, was to the quantity and amplitude in a direct ratio, as the whole line to the sine of double the angle of incidence, formed by the breech upon an horizontal line;—and that the semiparameter,—stop! my dear uncle Toby—stop!

—LAWRENCE STERNE

In questions of science the authority of a thousand is not worth the humble reasoning of a single individual. —GALILEO GALILEI

1 Mathematics of Motion

By GALILEO GALILEI

THIRD DAY

CHANGE OF POSITION [*De Motu Locali*]

MY purpose is to set forth a very new science dealing with a very ancient subject. There is, in nature, perhaps nothing older than motion, concerning which the books written by philosophers are neither few nor small; nevertheless I have discovered by experiment some properties of it which are worth knowing and which have not hitherto been either observed or demonstrated. Some superficial observations have been made, as, for instance, that the free motion [*naturalem motum*] of a heavy falling body is continuously accelerated;[1] but to just what extent this acceleration occurs has not yet been announced; for so far as I know, no one has yet pointed out that the distances traversed, during equal intervals of time, by a body falling from rest, stand to one another in the same ratio as the odd numbers beginning with unity.[2]

It has been observed that missiles and projectiles describe a curved path of some sort; however no one has pointed out the fact that this path is a parabola. But this and other facts, not few in number or less worth knowing, I have succeeded in proving; and what I consider more important, there have been opened up to this vast and most excellent science, of which my work is merely the beginning, ways and means by which other minds more acute than mine will explore its remote corners. . . .

NATURALLY ACCELERATED MOTION

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

[1] "Natural motion" of the author has here been translated into "free motion"—since this is the term used to-day to distinguish the "natural" from the "violent" motions of the Renaissance. [*Trans.*]

[2] A theorem demonstrated in Corollary I, p. 746. [*Trans.*]

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.

SAGREDO. 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.

SALVIATI. 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. When I think of a heavy body falling from rest, that is, starting with zero speed and gaining speed in proportion to the time from the beginning of the motion; such a motion as would, for instance, in eight beats of the pulse acquire eight degrees of speed; having at the end of the fourth beat acquired four degrees; at the end of the second, two; at the end of the first, one: and since time is divisible without limit, it follows from all these considerations that if the earlier speed of a body is less than its present speed in a constant ratio, then there is no degree of speed however small (or, one may say, no degree of slowness however great) with which we may not find this body travelling after starting from infinite slowness, i.e., from rest. So that if that speed which it had at the end of the fourth beat was such that, if kept uniform, the body would traverse two miles in an hour, and if keeping the speed which it had at the end of the second beat, it would traverse one mile an hour, we must infer that, as the instant of starting is more and more nearly approached, the body moves so slowly that, if it kept on moving at this rate, it would not traverse a mile in an hour, or in a day, or in a year or in a thousand years; indeed, it would not traverse a span in an even greater time; a phenomenon which baffles the imagination, while our senses show us that a heavy falling body suddenly acquires great speed.

SALV. This is one of the difficulties which I also at the beginning, experienced, but which I shortly afterwards removed; and the removal was effected by the very experiment which creates the difficulty for you. You say the experiment appears to show that immediately after a heavy body starts from rest it acquires a very considerable speed: and I say that the same experiment makes clear the fact that the initial motions of a falling body, no matter how heavy, are very slow and gentle. Place a heavy body upon a yielding material, and leave it there without any pressure except that owing to its own weight; it is clear that if one lifts this body a cubit or two and allows it to fall upon the same material, it will, with this impulse, exert a new and greater pressure than that caused by its mere weight; and this effect is brought about by the [weight of the] falling body together with the velocity acquired during the fall, an effect which will be greater and greater according to the height of the fall, that is according as the velocity of the falling body becomes greater. From the quality and intensity of the blow we are thus enabled to accurately estimate the speed of a falling body. But tell me, gentlemen, is it not true that if a block be allowed to fall upon a stake from a height of four cubits and drives it into the earth, say, four finger-breadths, that coming from a height of two cubits it will drive the stake a much less distance, and from the height of one cubit a still less distance; and finally if the block be lifted only one finger-breadth how much more will it accomplish than if merely laid on top of the stake without percussion? Certainly very little. If it be lifted only the thickness of a leaf, the effect will be altogether imperceptible. And since the effect of the blow depends upon the velocity of this striking body, can any one doubt the motion is very slow and the speed more than small whenever the effect [of the blow] is imperceptible? See now the power of truth; the same experiment which at first glance seemed to show one thing, when more carefully examined, assures us of the contrary.

But without depending upon the above experiment, which is doubtless very conclusive, it seems to me that it ought not to be difficult to establish such a fact by reasoning alone. Imagine a heavy stone held in the air at rest; the support is removed and the stone set free; then since it is heavier than the air it begins to fall, and not with uniform motion but slowly at the beginning and with a continuously accelerated motion. Now since velocity can be increased and diminished without limit, what reason is there to believe that such a moving body starting with infinite slowness, that is, from rest, immediately acquires a speed of ten degrees rather than one of four, or of two, or of one, or of a half, or of a hundredth; or, indeed, of any of the infinite number of small values [of speed]? Pray listen. I hardly think you will refuse to grant that the gain of speed of the stone falling from rest follows the same sequence as the diminution and loss of this same speed when, by some impelling force, the stone is thrown to its former elevation: but even if you do not grant this, I do not see how you can doubt that the ascending stone, diminishing in speed, must before coming to rest pass through every possible degree of slowness.

SIMPLICIO. But if the number of degrees of greater and greater slowness is limitless, they will never be all exhausted, therefore such an ascending heavy body will never reach rest, but will continue to move without limit always at a slower rate; but this is not the observed fact.

SALV. This would happen, Simplicio, if the moving body were to maintain its speed for any length of time at each degree of velocity; but it merely passes each point without delaying more than an instant: and since each time-interval however small may be divided into an infinite number of instants, these will always be sufficient [in number] to correspond to the infinite degrees of diminished velocity.

That such a heavy rising body does not remain for any length of time at any given degree of velocity is evident from the following: because if, some time-interval having been assigned, the body moves with the same speed in the last as in the first instant of that time-interval, it could from this second degree of elevation be in like manner raised through an equal height, just as it was transferred from the first elevation to the second, and by the same reasoning would pass from the second to the third and would finally continue in uniform motion forever. . . .

SALV. The present does not seem to be the proper time to investigate the cause of the acceleration of natural motion concerning which various opinions have been expressed by various philosophers, some explaining it by attraction to the center, others to repulsion between the very small parts of the body, while still others attribute it to a certain stress in the surrounding medium which closes in behind the falling body and drives it from one of its positions to another. Now, all these fantasies, and others too, ought to be examined; but it is not really worth while. At present it is the purpose of our Author merely to investigate and to demonstrate some of the properties of accelerated motion (whatever the cause of this acceleration may be)—meaning thereby a motion, such that the momentum of its velocity [*i momenti della sua velocità*] goes on increasing after departure from rest, in simple proportionality to the time, which is the same as saying that in equal time-intervals the body receives equal increments of velocity; and if we find the properties [of accelerated motion] which will be demonstrated later are realized in freely falling and accelerated bodies, we may conclude that the assumed definition includes such a motion of falling bodies and that their speed [*accelerazione*] goes on increasing as the time and the duration of the motion.

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 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 everyone to whom they were presented, proven in a few simple words to be not only false, but impossible.

SIMP. I am one of those who accept the proposition, and believe that a falling body acquires force [*vires*] in its descent, its velocity increasing in proportion to the space, and that the momentum [*momento*] of the falling body is doubled when it falls from a doubled height; these propositions, it appears to me, ought to be conceded without hesitation or controversy.

SALV. And yet they are as false and impossible as that motion should be completed instantaneously; and here is a very clear demonstration of it. If the velocities are in proportion to the spaces traversed, or to be traversed, then these spaces are traversed in equal intervals of time; if, therefore, the velocity with which the falling body traverses a space of eight feet were double that with which it covered the first four feet (just as the one distance is double the other) then the time-intervals required for these passages would be equal. But for one and the same body to fall eight feet and four feet in the same time is possible only in the case of instantaneous [discontinuous] motion; but observation shows us that the motion of a falling body occupies time, and less of it in covering a distance of four feet than of eight feet; therefore it is not true that its velocity increases in proportion to the space.

The falsity of the other proposition may be shown with equal clearness. For if we consider a single striking body the difference of momentum in its blows can depend only upon difference of velocity; for if the striking body falling from a double height were to deliver a blow of double momentum, it would be necessary for this body to strike with a doubled velocity; but with this doubled speed it would traverse a doubled space in the same time-interval; observation however shows that the time required for fall from the greater height is longer.

SAGR. You present these recondite matters with too much evidence and ease; this great facility makes them less appreciated than they would be had they been presented in a more abstruse manner. For, in my opinion, people esteem more lightly that knowledge which they acquire with so little labor than that acquired through long and obscure discussion.

SALV. If those who demonstrate with brevity and clearness the fallacy of many popular beliefs were treated with contempt instead of gratitude the injury would be quite bearable; but on the other hand it is very unpleasant and annoying to see men, who claim to be peers of anyone in a certain field of study, take for granted certain conclusions which later are quickly and easily shown by another to be false. I do not describe such a feeling as one of envy, which usually degenerates into hatred and anger against those who discover such fallacies; I would call it a strong desire to maintain old errors, rather than accept newly discovered truths. This desire at times induces them to unite against these truths, although at heart believing in them, merely for the purpose of lowering the esteem in which certain others are held by the unthinking crowd. Indeed, I have heard from our Academician many such fallacies held as true but easily refutable; some of these I have in mind.

SAGR. You must not withhold them from us, but, at the proper time, tell us about them even though an extra session be necessary. 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

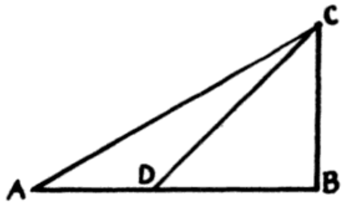


FIGURE 1

the figure of the moving body is perfectly round, so that neither plane nor moving body is rough. 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.

Imagine this page to represent a vertical wall, with a nail driven into it; and from the nail let there be suspended a lead bullet of one or two ounces by means of a fine vertical thread, AB, say from four to six feet long, on this wall draw a horizontal line DC, at right angles to the vertical thread AB, which hangs about two finger-breadths in front of the wall. Now bring the thread AB with the attached ball into the position AC and set it free; first it will be observed to descend along the arc CBD, to pass the point B, and to travel along the arc BD, till it almost reaches the horizontal CD, a slight shortage being caused by the resistance of the air and the string; from this we may rightly infer that the ball in its descent through the arc CB acquired a momentum [*impeto*] on reaching B, which was just sufficient to carry it through a similar arc BD to the same height. Having repeated this experiment many times, let us now drive a nail into the wall close to the perpendicular AB, say at E or F, so that it projects out some five or six finger-breadths in order that the thread, again carrying the bullet through the arc CB, may strike upon the nail E when the bullet reaches B, and thus compel it to traverse the arc BG, described about E as center. From this we can see what can be done by the same momentum [*impeto*] which previously starting at the same point B carried the same body through the arc BD to the horizontal CD. Now, gentlemen, you will observe with pleasure that the ball swings to the point G in the horizontal, and you would see the same thing happen if the obstacle were placed at some lower point, say at F, about which the ball would describe the arc BI, the rise of the ball always terminating exactly on the line CD. But when the nail is placed so low that the remainder of the thread below it will not reach to the height CD

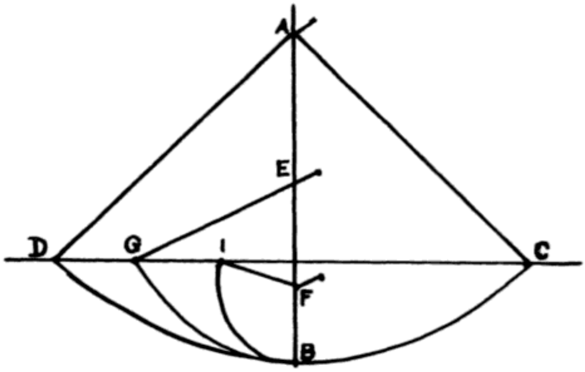


FIGURE 2

(which would happen if the nail were placed nearer B than to the intersection of AB with the horizontal CD) then the thread leaps over the nail and twists itself about it.

This experiment leaves no room for doubt as to the truth of our supposition; for since the two arcs CB and DB are equal and similarly placed, the momentum [*momento*] acquired by the fall through the arc CB is the same as that gained by fall through the arc DB; but the momentum [*momento*] acquired at B, owing to fall through CB, is able to lift the same body [*mobile*] through the arc BD; therefore, the momentum acquired in the fall BD is equal to that which lifts the same body through the same arc from B to D; so, in general, every momentum acquired by fall through an arc is equal to that which can lift the same body through the same arc. But all these momenta [*momenti*] which cause a rise through the arcs BD, BG, and BI are equal, since they are produced by the same momentum, gained by fall through CB, as experiment shows. Therefore all the momenta gained by fall through the arcs DB, GB, IB are equal.

SAGR. The argument seems to me so conclusive and the experiment so well adapted to establish the hypothesis that we may, indeed, consider it as demonstrated.

SALV. I do not wish, Sagredo, that we trouble ourselves too much about this matter, since we are going to apply this principle mainly in motions which occur on plane surfaces, and not upon curved, along which acceleration varies in a manner greatly different from that which we have assumed for planes.

So that, although the above experiment shows us that the descent of the moving body through the arc CB confers upon it momentum [*momento*] just sufficient to carry it to the same height through any of the arcs BD, BG, BI, we are not able, by similar means, to show that the event would be identical in the case of a perfectly round ball descending along planes whose inclinations are respectively the same as the chords of these arcs. It seems likely, on the other hand, that, since these planes form angles at the point B, they will present an obstacle to the ball which has descended along the chord CB, and starts to rise along the chord BD, BG, BI.

In striking these planes some of its momentum [*impeto*] will be lost and it will not be able to rise to the height of the line CD; but this obstacle, which interferes with the experiment, once removed, it is clear that the momentum [*impeto*] (which gains in strength with descent) will be able to carry the body to the same height. Let us then, for the present, take this as a postulate, the absolute truth of which will be established when we find that the inferences from it correspond to and agree perfectly with experiment. The Author having assumed this single principle passes next to the propositions which he clearly demonstrates; the first of these is as follows:

THEOREM I, PROPOSITION I

The time in which any space is traversed by a body starting from rest and uniformly accelerated is equal to the time in which that same space would be traversed by the same body moving at a uniform speed whose value is the mean of the highest speed and the speed just before acceleration began.

Let us represent by the line AB the time in which the space CD is traversed by a body which starts from rest at C and is uniformly accelerated; let the final and highest value of the speed gained during the interval AB be represented by the line EB drawn at right angles to AB; draw the line AE, then all lines drawn from equidistant points on AB and parallel to BE will represent the increasing values of the speed, beginning with the instant A. Let the point F bisect the line EB; draw FG parallel to BA, and GA parallel to FB, thus forming a parallelogram AGFB which will be equal in area to the triangle AEB, since the side GF bisects the side AE at the point I; for if the parallel lines in the triangle AEB are extended to GI, then the sum of all the parallels contained in the quadrilateral is equal to the sum of those contained in the triangle AEB; for those in the triangle IEF are equal to those contained in the triangle GIA, while those included in the trapezium AIFB are common. Since each and every instant of time in the time-interval AB has its corresponding point on the line AB, from which points parallels drawn in and limited by the triangle AEB represent the increasing values of the growing velocity, and since parallels contained within the rectangle represent the values of a speed which is not increasing, but constant, it appears, in like manner, that the momenta [*momenti*] assumed by the moving body may also be

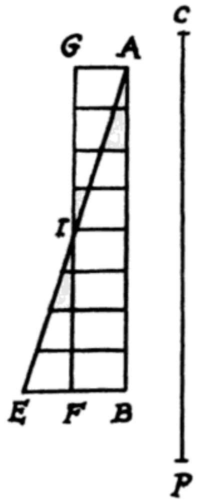


FIGURE 3

represented, in the case of the accelerated motion, by the increasing parallels of the triangle AEB, and, in the case of the uniform motion, by the parallels of the rectangle GB. For, what the momenta may lack in the first part of the accelerated motion (the deficiency of the momenta being represented by the parallels of the triangle AGI) is made up by the momenta represented by the parallels of the triangle IEF.

Hence it is clear that equal spaces will be traversed in equal times by two bodies, one of which, starting from rest, moves with a uniform acceleration, while the momentum of the other, moving with uniform speed, is one-half its maximum momentum under accelerated motion.

Q. E. D.

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

FIGURE 4

from the points D and E, draw the parallel lines DO 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 ½ EP to ½ 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. . . .

SIMP. 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. Then we can proceed without discussion.

COROLLARY II

Secondly, it follows that, starting from any initial point, if we take any two distances, traversed in any time-intervals whatsoever, these time-intervals bear to one another the same ratio as one of the distances to the mean proportional of the two distances.

For if we take two distances ST and SY measured from the initial

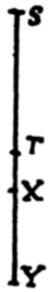


FIGURE 5

point S, the mean proportional of which is SX, the time of fall through ST is to the time of fall through SY as ST is to SX; or one may say the time of fall through SY is to the time of fall through ST as SY is to SX. Now since it has been shown that the spaces traversed are in the same ratio as the squares of the times; and since, moreover, the ratio of the space SY to the space ST is the square of the ratio SY to SX, it follows that the ratio of the times of fall through SY and ST is the ratio of the respective distances SY and SX.

SCHOLIUM

The above corollary has been proven for the case of vertical fall; but it holds also for planes inclined at any angle; for it is to be assumed that along these planes the velocity increases in the same ratio, that is, in proportion to the time, or, if you prefer, as the series of natural numbers. . . .

THEOREM III, PROPOSITION III

If one and the same body, starting from rest, falls along an inclined plane and also along a vertical, each having the same height, the times of descent will be to each other as the lengths of the inclined plane and the vertical.

Let AC be the inclined plane and AB the perpendicular, each having the same vertical height above the horizontal, namely, BA; then I say, the time of descent of one and the same body along the plane AC bears a ratio to the time of fall along the perpendicular AB, which is the same as the ratio of the length AC to the length AB. Let DG, EI and LF be any lines parallel to the horizontal CB; then it follows from what has preceded that a body starting from A will acquire the same speed at the point G as at D, since in each case the vertical fall is the same; in like manner the speeds at I and E will be the same; so also those at L and F. And in general the speeds at the two extremities of any parallel drawn from any point on AB to the corresponding point on AC will be equal.

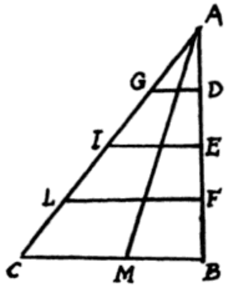


FIGURE 6

Thus the two distances AC and AB are traversed at the same speed. But it has already been proved that if two distances are traversed by a body moving with equal speeds, then the ratio of the times of descent will be the ratio of the distances themselves; therefore, the time of descent along AC is to that along AB as the length of the plane AC is to the vertical distance AB. Q. E. D.

SAGR. It seems to me that the above could have been proved clearly and briefly on the basis of a proposition already demonstrated, namely, that the distance traversed in the case of accelerated motion along AC or AB is the same as that covered by a uniform speed whose value is one-half the maximum speed, CB; the two distances AC and AB having been traversed at the same uniform speed it is evident, from Proposition I, that the times of descent will be to each other as the distances.

COROLLARY

Hence we may infer that the times of descent along planes having different inclinations, but the same vertical height stand to one another in the same ratio as the lengths of the planes. For consider any plane AM extending from A to the horizontal CB; then it may be demonstrated in the same manner that the time of descent along AM is to the time along AB as the distance AM is to AB; but since the time along AB is to that along AC as the length AB is to the length AC, it follows, *ex æquali*, that as AM is to AC so is the time along AM to the time along AC.

THEOREM IV, PROPOSITION IV

The times of descent along planes of the same length but of different inclinations are to each other in the inverse ratio of the square roots of their heights.

From a single point B draw the planes BA and BC, having the same length but different inclinations; let AE and CD be horizontal lines drawn to meet the perpendicular BD; and let BE represent the height of the plane AB, and BD the height of BC; also let BI be a mean proportional to BD and BE; then the ratio of BD to BI is equal to the square root of the ratio of BD to BE. Now, I say, the ratio of the times of descent along BA and BC is the ratio of BD to BI; so that the time of descent along BA is related to the height of the other plane BC, namely BD as the time

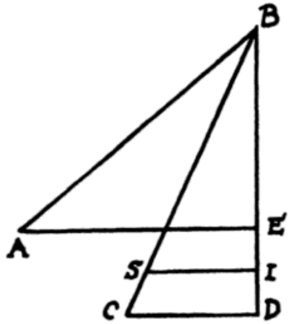


FIGURE 7

along BC is related to the height BI. Now it must be proved that the time of descent along BA is to that along BC as the length BD is to the length BI.

Draw IS parallel to DC; and since it has been shown that the time of fall along BA is to that along the vertical BE as BA is to BE; and also that the time along BE is to that along BD as BE is to BI; and likewise that the time along BD is to that along BC as BD is to BC, or as BI to BS; it follows, *ex æquali*, that the time along BA is to that along BC as BA to BS, or BC to BS. However, BC is to BS as BD is to BI; hence follows our proposition.

THEOREM V, PROPOSITION V

The times of descent along planes of different length, slope and height bear to one another a ratio which is equal to the product of the ratio of the lengths by the square root of the inverse ratio of their heights.

Draw the planes AB and AC, having different inclinations, lengths, and heights. My theorem then is that the ratio of the time of descent along AC to that along AB is equal to the product of the ratio of AC to AB by the square root of the inverse ratio of their heights.

For let AD be a perpendicular to which are drawn the horizontal lines BG and CD; also let AL be a mean proportional to the heights AG and AD; from the point L draw a horizontal line meeting AC in F; accordingly AF will be a mean proportional between AC and AE. Now since the time of descent along AC is to that along AE as the length AF is to AE;

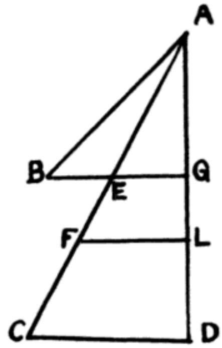


FIGURE 8

and since the time along AE is to that along AB as AE is to AB, it is clear that the time along AC is to that along AB as AF is to AB.

Thus it remains to be shown that the ratio of AF to AB is equal to the product of the ratio of AC to AB by the ratio of AG to AL, which is the inverse ratio of the square roots of the heights DA and GA. Now it is evident that, if we consider the line AC in connection with AF and AB, the ratio of AF to AC is the same as that of AL to AD, or AG to AL which is the square root of the ratio of the heights AG and AD; but the ratio of AC to AB is the ratio of the lengths themselves. Hence follows the theorem.

THEOREM VI, PROPOSITION VI

If from the highest or lowest point in a vertical circle there be drawn any inclined planes meeting the circumference the times of descent along these chords are each equal to the other.

On the horizontal line GH construct a vertical circle. From its lowest point—the point of tangency with the horizontal—draw the diameter FA and from the highest point, A, draw inclined planes to B and C, any points whatever on the circumference; then the times of descent along these are equal. Draw BD and CE perpendicular to the diameter; make AI a mean proportional between the heights of the planes, AE and AD; and since the rectangles FA.AE and FA.AD are respectively equal to the squares of AC and AB, while the rectangle FA.AE is to the rectangle FA.AD as AE is to AD, it follows that the square of AC is to the square of AB as the length AE is to the length AD. But since the length AE is to AD as the square of AI is to the square of AD, it follows that the squares on the lines AC and AB are to each other as the squares on the lines AI and AD, and hence also the length AC is to the length AB as AI

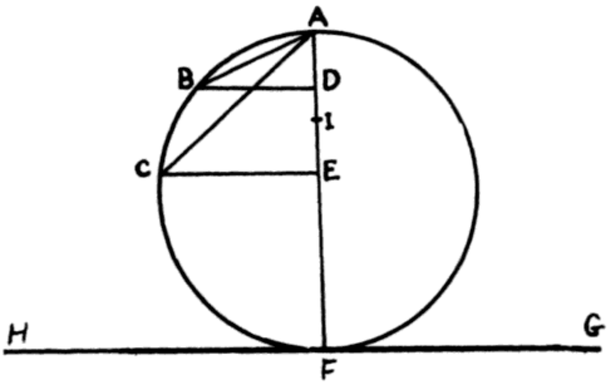


FIGURE 9

is to AD. But it has previously been demonstrated that the ratio of the time of descent along AC to that along AB is equal to the product of the two ratios AC to AB and AD to AI; but this last ratio is the same as that of AB to AC. Therefore the ratio of the time of descent along AC to that along AB is the product of the two ratios, AC to AB and AB to AC. The ratio of these times is therefore unity. Hence follows our proposition.

By use of the principles of mechanics [*ex mechanicis*] one may obtain the same result. . . .

SCHOLIUM

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 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,

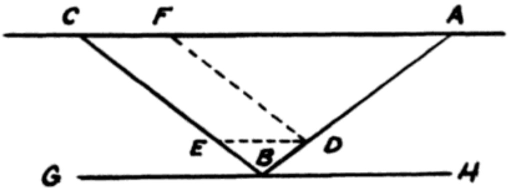


FIGURE 10

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 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 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

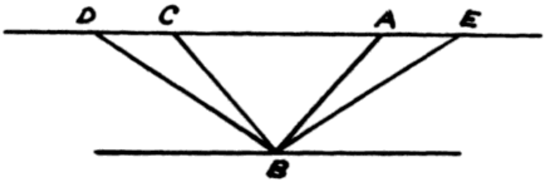


FIGURE 11

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 than 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. . . .

FOURTH DAY

SALV. 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. We now proceed to demonstrate some of its properties, the first of which is as follows:

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 Apollonius 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 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 required. 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 circle *ibkc*, and the axis *ad* is parallel to the side *lk*; now having taken any

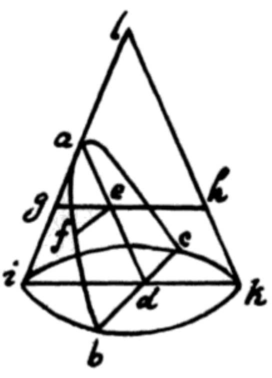


FIGURE 12

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 rectangle *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*. 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

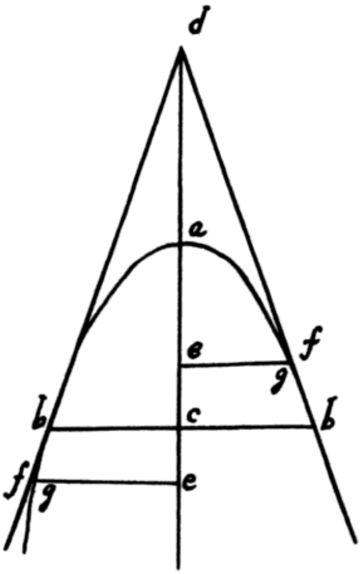


FIGURE 13

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 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 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,*

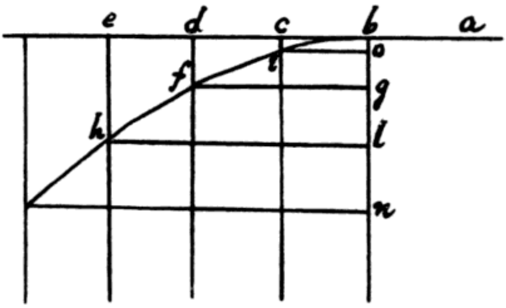


FIGURE 14

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,[3] 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 derived 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

[3] A very near approach to Newton's Second Law of Motion. [*Trans.*]

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 [*piu 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 of 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 velocità*] 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 soprana-turale*] they follow their prescribed paths very exactly.

But now let us proceed with the discussion in which the Author invites us to the study and investigation of the motion of a body [*impeto del mo-bile*] when that motion is compounded of two others; and first the case in which the two are uniform, the one horizontal, the other vertical.

THEOREM II, PROPOSITION II

When the motion of a body is the resultant of two uniform motions, one horizontal, the other perpendicular, the square of the resultant momentum is equal to the sum of the squares of the two component momenta.

Let us imagine any body urged by two uniform motions and let *ab* represent the vertical displacement, while *bc* represents the displacement which, in the same interval of time, takes place in a horizontal direction.

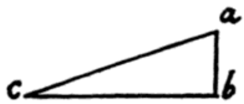


FIGURE 15

If then the distances *ab* and *bc* are traversed, during the same time-interval, with uniform motions the corresponding momenta will be to each other as the distances *ab* and *bc* are to each other; but the body which is urged by these two motions describes the diagonal *ac*; its momentum is proportional to *ac*. Also the square of *ac* is equal to the sum of the squares of *ab* and *bc*. Hence the square of the resultant momentum is equal to the sum of the squares of the two momenta *ab* and *bc*. Q. E. D.

SIMP. At this point there is just one slight difficulty which needs to be cleared up; for it seems to me that the conclusion just reached contradicts a previous proposition in which it is claimed that the speed [*impeto*] of a body coming from *a* to *b* is equal to that in coming from *a* to *c*; while now you conclude that the speed [*impeto*] at *c* is greater than that at *b*.

SALV. Both propositions, Simplicio, are true, yet there is a great difference between them. Here we are speaking of a body urged by a single motion which is the resultant of two uniform motions, while there we were speaking of two bodies each urged with naturally accelerated motions, one along the vertical *ab* the other along the inclined plane *ac*. Besides the time-intervals were there not supposed to be equal, that along the incline *ac* being greater than that along the vertical *ab*; but the motions of which we now speak, those along *ab*, *bc*, *ac*, are uniform and simultaneous.

SIMP. Pardon me; I am satisfied; pray go on.

SALV. Our Author next undertakes to explain what happens when a body is urged by a motion compounded of one which is horizontal and uniform and of another which is vertical but naturally accelerated; from these two components results the path of a projectile, which is a parabola. The problem is to determine the speed [*impeto*] of the projectile at each point. With this purpose in view our Author sets forth as follows the manner, or rather the method, of measuring such speed [*impeto*] along the path which is taken by a heavy body starting from rest and falling with a naturally accelerated motion.

THEOREM III, PROPOSITION III

Let the motion take place along the line *ab*, starting from rest at *a*, and in this line choose any point *c*. Let *ac* represent the time, or the measure of the time, required for the body to fall through the space *ac*; let *ac* also represent the velocity [*impetus seu momentum*] at *c* acquired by a fall through the distance *ac*. In the line *ab* select any other point *b*. The problem now is to determine the velocity at *b* acquired by a body in falling through the distance *ab* and to express this in terms of the velocity at *c*, the measure of which is the length *ac*. Take *as* a mean proportional between *ac* and *ab*. We shall prove that the velocity at *b* is to that at *c* as the length *as* is to the length *ac*. Draw the horizontal line *cd*, having twice the length of *ac*, and *be*, having twice the length of *ba*. It then fol-

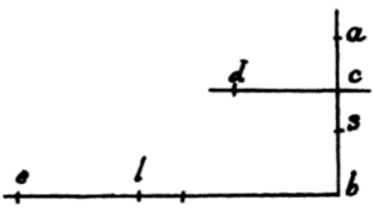


FIGURE 16

lows, from the preceding theorems, that a body falling through the distance *ac*, and turned so as to move along the horizontal *cd* with a uniform speed equal to that acquired on reaching *c* will traverse the distance *cd* in the same interval of time as that required to fall with accelerated motion from *a* to *c*. Likewise *be* will be traversed in the same time as *ba*. But the time of descent through *ab* is *as*; hence the horizontal distance *be* is also traversed in the time *as*. Take a point *l* such that the time *as* is to the time *ac* as *be* is to *bl*; since the motion along *be* is uniform, the distance *bl*, if traversed with the speed [*momentum celeritatis*] acquired at *b*, will occupy the time *ac*; but in this same time-interval, *ac*, the distance *cd* is traversed with the speed acquired in *c*. Now two speeds are to each other as the distances traversed in equal intervals of time. Hence the speed at *c* is to the speed at *b* as *cd* is to *bl*. But since *dc* is to *be* as their halves, namely, as *ca* is to *ba*, and since *be* is to *bl* as *ba* is to *sa*; it follows that *dc* is to *bl* as *ca* is to *sa*. In other words, the speed at *c* is to that at *b* as *ca* is to *sa*, that is, as the time of fall through *ab*.

The method of measuring the speed of a body along the direction of its fall is thus clear; the speed is assumed to increase directly as the time. . . .

PROBLEM I, PROPOSITION IV

SALV. Concerning motions and their velocities or momenta [*movimenti e lor velocità o impeti*] whether uniform or naturally accelerated, one cannot speak definitely until he has established a measure for such velocities and also for time. As for time we have the already widely adopted hours, first minutes and second minutes. So for velocities, just as for intervals of time, there is need of a common standard which shall be understood and accepted by everyone, and which shall be the same for all. As has already been stated, the Author considers the velocity of a freely falling body adapted to this purpose, since this velocity increases according to the same law in all parts of the world; thus for instance the speed acquired by a leaden ball of a pound weight starting from rest and falling vertically through the height of, say, a spear's length is the same in all places; it is therefore excellently adapted for representing the momentum [*impeto*] acquired in the case of natural fall.

It still remains for us to discover a method of measuring momentum in the case of uniform motion in such a way that all who discuss the subject will form the same conception of its size and velocity [*grandezza e velocità*]. This will prevent one person from imagining it larger, another smaller, than it really is; so that in the composition of a given uniform motion with one which is accelerated different men may not obtain different values for the resultant. In order to determine and represent such a momentum and particular speed [*impeto e velocità particolare*] our Author has found no better method than to use the momentum acquired by a body in naturally accelerated motion. The speed of a body which has in this manner acquired any momentum whatever will, when converted into uniform motion, retain precisely such a speed as, during a time-interval equal to that of the fall, will carry the body through a distance equal to twice that of the fall. But since this matter is one which is fundamental in our discussion it is well that we make it perfectly clear by means of some particular example.

Let us consider the speed and momentum acquired by a body falling through the height, say, of a spear [*picca*] as a standard which we may use in the measurement of other speeds and momenta as occasion demands; assume for instance that the time of such a fall is four seconds [*minuti secondi d'ora*]; now in order to measure the speed acquired from a fall through any other height, whether greater or less, one must not conclude that these speeds bear to one another the same ratio as the heights of fall; for instance, it is not true that a fall through four times a given height confers a speed four times as great as that acquired by descent through the given height; because the speed of a naturally accelerated motion does not vary in proportion to the time. As has been shown above, the ratio of the spaces is equal to the square of the ratio of the times.

If, then, as is often done for the sake of brevity, we take the same limited straight line as the measure of the speed, and of the time, and also of the space traversed during that time, it follows that the duration of fall and the speed acquired by the same body in passing over any other distance, is not represented by this second distance, but by a mean proportional between the two distances. This I can better illustrate by an example.

FIGURE 17

In the vertical line *ac*, lay off the portion *ab* to represent the distance traversed by a body falling freely with accelerated motion: the time of fall may be represented by any limited straight line, but for the sake of brevity, we shall represent it by the same length *ab*; this length may also be employed as a measure of the momentum and speed acquired during the motion; in short, let *ab* be a measure of the various physical quantities which enter this discussion.

Having agreed arbitrarily upon *ab* as a measure of these three different quantities, namely, space, time, and momentum, our next task is to find the time required for fall through a given vertical distance *ac*, also the momentum acquired at the terminal point *c*, both of which are to be expressed in terms of the time and momentum represented by *ab*. These two required quantities are obtained by laying off *ad*, a mean proportional between *ab* and *ac*; in other words, the time of fall from *a* to *c* is represented by *ad* on the same scale on which we agreed that the time of fall from *a* to *b* should be represented by *ab*. In like manner we may say that the momentum [*impeto o grado di velocità*] acquired at *c* is related to that acquired at *b*, in the same manner that the line *ad* is related to *ab*, since the velocity varies directly as the time, a conclusion, which although employed as a postulate in Proposition III, is here amplified by the Author.

This point being clear and well-established we pass to the consideration of the momentum [*impeto*] in the case of two compound motions, one of which is compounded of a uniform horizontal and a uniform vertical motion, while the other is compounded of a uniform horizontal and a naturally accelerated vertical motion. If both components are uniform, and one at right angles to the other, we have already seen that the square of the resultant is obtained by adding the squares of the components [p. 765] as will be clear from the following illustration.

Let us imagine a body to move along the vertical *ab* with a uniform momentum [*impeto*] of 3, and on reaching *b* to move toward *c* with a momentum [*velocità ed impeto*] of 4, so that during the same time-interval it will traverse 3 cubits along the vertical and 4 along the horizontal. But

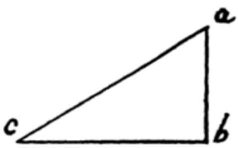


FIGURE 18

a particle which moves with the resultant velocity [*velocità*] will, in the same time, traverse the diagonal *ac*, whose length is not 7 cubits—the sum of *ab* (3) and *bc* (4)—but 5, which is *in potenza* equal to the sum of 3 and 4, that is, the squares of 3 and 4 when added make 25, which is the square of *ac*, and is equal to the sum of the squares of *ab* and *bc*. Hence *ac* is represented by the side—or we may say the root—of a square whose area is 25, namely 5.

As a fixed and certain rule for obtaining the momentum which results from two uniform momenta, one vertical, the other horizontal, we have therefore the following: take the square of each, add these together, and extract the square root of the sum, which will be the momentum resulting from the two. Thus, in the above example, the body which in virtue of its vertical motion would strike the horizontal plane with a momentum [*forza*] of 3, would owing to its horizontal motion alone strike at *c* with a momentum of 4; but if the body strikes with a momentum which is the resultant of these two, its blow will be that of a body moving with a momentum [*velocità e forza*] of 5; and such a blow will be the same at all points of the diagonal *ac*, since its components are always the same and never increase or diminish.

Let us now pass to the consideration of a uniform horizontal motion compounded with the vertical motion of a freely falling body starting from rest. It is at once clear that the diagonal which represents the motion compounded of these two is not a straight line, but, as has been demonstrated, a semi-parabola, in which the momentum [*impeto*] is always increasing because the speed [*velocità*] of the vertical component is always increasing. Wherefore, to determine the momentum [*impeto*] at any given point in the parabolic diagonal, it is necessary first to fix upon the uniform horizontal momentum [*impeto*] and then, treating the body as one falling freely, to find the vertical momentum at the given point; this latter can be determined only by taking into account the duration of fall, a consideration which does not enter into the composition of two uniform motions where the velocities and momenta are always the same; but here where one of the component motions has an initial value of zero and increases its speed [*velocità*] in direct proportion to the time, it follows that the time must determine the speed [*velocità*] at the assigned point. It only remains to obtain the momentum resulting from these two components (as in the case of uniform motions) by placing the square of the resultant equal to the sum of the squares of the two components. . . .

COMMENTARY ON

THE BERNOULLIS

IN EIGHT generations the Bach family produced at least two dozen eminent musicians and several dozen more of sufficient repute to find their way into musical dictionaries. So numerous and so eminent were they that, according to the *Britannica*, musicians were known as "Bachs" in Erfurt even when there were no longer any members of the family in the town. What the Bachs were to music, the Bernoulli clan was to science. In the course of a century eight of its members pursued mathematical studies, several attaining the foremost rank in various branches of this science as well as in related disciplines. From this group came a "swarm of descendants about half of whom were gifted above the average and nearly all of whom, down to the present day, have been superior human beings." [1]

The Bernoullis were a Protestant family driven from Antwerp in the last quarter of the sixteenth century by religious persecution. In 1583 they found asylum in Frankfurt; after a few years they moved to Basel in Switzerland. Nicolaus Bernoulli (1623–1708) was a wealthy merchant and a town councilor. This in itself is not a noteworthy achievement; he deserves rather to be remembered for his three sons Jacob, Nicolaus and John, and their descendants.[2] It is peculiar that nothing is ever said about the women the Bernoullis married; they must have made at least a genetic contribution to this illustrious spawn.

Jacob (I), for eighteen years professor of mathematics at Basel, had started out at his father's insistence as a theologian, but soon succumbed to his passion for science. He became a master of the calculus, developing

[1] E. T. Bell, *Men of Mathematics*, N. Y., 1937, p. 131. "No fewer than 120 of the descendants of the mathematical Bernoullis have been traced genealogically, and of this considerable posterity the majority achieved distinction—sometimes amounting to eminence—in the law, scholarship, science, literature, the learned professions, administration and the arts. None were failures."

[2] There is a confusion, understandable, about the Bernoulli genealogical lines, and another, less understandable, about their names. Jacob, for example, is also known as Jakob, Jacques and James; Johannes, as Johann, John and Jean. I shall use the familiar forms in the following table:

Nicolaus Senior (1623–1708)

- Jacob (I) (1654–1705)
- Nicolaus (I) (1662–1716)
  - Nicolaus (II) (1687–1759)
- John (I) (1667–1748)
  - Nicolaus (III) (1695–1726)
  - Daniel (1700–1782)
  - John (II) (1710–1790)
    - John (III) (1746–1807)
    - Jacob (II) (1759–1789)

and applying it successfully to a considerable number of problems. Among his more celebrated investigations were those into the properties of the curve known as the catenary (it is formed by a heavy chain hanging freely from its two extremities), into isoperimetrical figures (those enclosing, for any given perimeter, the greatest area) and into various spiral curves. His other works include a great treatise on probability, the *Ars Conjectandi* (a selection from it appears elsewhere in these pages: see pp. 1452–1455), *A Method of Teaching Mathematics to the Blind*, based on his experience teaching the elements of science to a blind girl at Geneva, and many verses in Latin, German and French, regarded as "elegant" in their time but now forgotten. Jacob, according to Francis Galton, suffered from "a bilious and melancholic temperament"; [3] his brother John did nothing to soothe it. John also was an exceptional mathematician. He was more prolific than Jacob, made many independent and important mathematical discoveries, and enlarged scientific knowledge in chemistry, physics and astronomy. The Bernoullis, Galton says, "were mostly quarrelsome and unamiable"; John was a prime example. He was violent, abusive, jealous, and, when necessary, dishonest. He claimed a reward Jacob had offered for a solution of the isoperimetrical problem. The solution he presented was wrong; he waited until Jacob died and then published another solution which he knew to be wrong—a fact he admitted seventeen years later. His son Daniel, again a brilliant mathematician, had the temerity to win a French Academy of Sciences prize which his father had sought. John gave him a special reward by throwing him out of the house.[4] These agreeable traits were "lived out," as psychoanalysts might observe, and thus did nothing to shorten John's life. He died at the age of eighty, retaining his powers and his meanness to the end.

It is with Daniel Bernoulli that we are here mainly concerned. He was a second son, born at Groningen—where his father was then professor of mathematics—in 1700. His father did everything possible to turn him from mathematical pursuits. The program consisted of cruel mistreatment, when Daniel was a child, to destroy his self-confidence, and of later attempts to force him into business. John should have known this wouldn't work; the Bernoullis were tough as well as dedicated. When he was eleven, Daniel got instruction in geometry from his brother Nicolaus.[5] He studied

[3] Francis Galton, *Hereditary Genius*; London, reprint of 1950, p. 195.

[4] E. T. Bell, *op. cit.*, p. 134.

[5] Nicolaus Bernoulli (1695–1726) was no exception to the Bernoulli rule of extraordinariness. At the age of eight he could speak German, Dutch, French and Latin; at sixteen he became a Doctor of Philosophy at the University of Basel; he was appointed professor of mathematics at St. Petersburg at the same time as Daniel. His early death (of a "lingering fever") prevented him from developing his evident powers. As the eldest son he was better treated by his father than Daniel; at any rate he was permitted, even encouraged, to study mathematics. When he was twenty-one his father pronounced him "worthy of receiving the torch of science from his own hands."

medicine, became a physician and finally, at the age of twenty-five, accepted an appointment as professor of mathematics at St. Petersburg. In 1733 he returned to Basel to become professor of anatomy, botany, and later, "experimental and speculative philosophy," i.e., physics. He remained at this post until he was almost eighty, publishing a large number of important memoirs on physical problems and doing first-rate work in probability theory, calculus, differential equations and related fields. He won, or divided equally, no less than ten prizes put up by the French Academy of Sciences, including the one which so infuriated his father. Late in life, he particularly enjoyed recalling that, in his youth, a stranger once answered his self-introduction, "I am Daniel Bernoulli," with an "incredulous and mocking" "and I am Isaac Newton."

Bernoulli's most famous book is the *Hydrodynamica*, in which he laid the foundations, theoretical and practical, for the "equilibrium, pressure, reaction and varied velocities" of fluids. The *Hydrodynamica* is notable also for presenting the first formulation of the kinetic theory of gases, a keystone of modern physics. Bernoulli showed that, if a gas be imagined to consist of "very minute corpuscles," "practically infinite in number," "driven hither and thither with a very rapid motion," their myriad collisions with one another and impact on the walls of the containing vessel would explain the phenomenon of pressure. Moreover, if the volume of the container were slowly decreased by sliding in one end like a piston, the gas would be compressed, the number of collisions of the corpuscles would be increased per unit of time, and the pressure would rise. The same effect would follow from heating the gas; heat, as Bernoulli perceived, being nothing more than "an increasing internal motion of the particles." This "astonishing prevision of a state of physics which was not actually reached for 110 years" (notably by Joule, who calculated the statistical averages of the enormous number of molecular collisions and thus derived Boyle's law—pressure $\times$ volume $=$ constant—from the laws of impact) was fully sustained by Bernoulli's remarkable experimental and theoretical labors.[6] He provided an algebraic formulation of the relation between impacts and pressure; he even calculated the magnitude of the pressure increase resulting from decreased volume and found it corresponded to the hypothesis Boyle had confirmed by experiment, that "the pressures and expansions are in reciprocal proportions." [7]

The following selection covers these topics; it is from the tenth section of the *Hydrodynamica*, (1738).

[6] Lloyd W. Taylor, *Physics—The Pioneer Science*, Boston, 1941, p. 109.

[7] "Bernoulli in effect had made in his thinking two enormous jumps, for which the temper of his time was not ready for over three generations: first the equivalence between heat and energy, and, secondly, the idea that a well-defined relationship, such as Boyle's simple law, could be deduced from the chaotic picture of randomly moving particles." Gerald Holton, *Introduction to Concepts and Theories in Physical Science*, Cambridge (Mass.), 1952, p. 376.

Daniel Bernoulli has been called the father of mathematical physics.
—ERIC TEMPLE BELL

So many of the properties of matter, especially when in the gaseous form, can be deduced from the hypothesis that their minute parts are in rapid motion, the velocity increasing with the temperature, that the precise nature of this motion becomes a subject of rational curiosity. Daniel Bernoulli, Herapath, Joule, Krönig, Clausius, &c., have shewn that the relations between pressure, temperature and density in a perfect gas can be explained by supposing the particles to move with uniform velocity in straight lines, striking against the sides of the containing vessel and thus producing pressure. —JAMES CLERK MAXWELL (*Illustrations of the Dynamical Theory of Gases*)

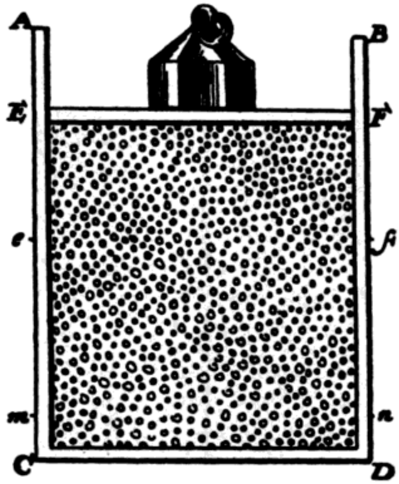
2 Kinetic Theory of Gases

By DANIEL BERNOULLI

1. IN the consideration of elastic fluids we may assign to them such a constitution as will be consistent with all their known properties, that so we may approach the study of their other properties, which have not yet been sufficiently investigated. The particular properties of elastic fluids are as follows: 1. They are heavy; 2. they expand in all directions unless they are restrained; and 3. they are continually more and more compressed when the force of compression increases. Air is a body of this sort, to which especially the present investigation pertains.

2. Consider a cylindrical vessel *ACDB* (Figure 44) set vertically, and a movable piston *EF* in it, on which is placed a weight *P*: let the cavity *ECDF* contain very minute corpuscles, which are driven hither and thither with a very rapid motion; so that these corpuscles, when they strike against the piston *EF* and sustain it by their repeated impacts, form an elastic fluid which will expand of itself if the weight *P* is removed or diminished, which will be condensed if the weight is increased, and which gravitates toward the horizontal bottom *CD* just as if it were endowed with no elastic powers: for whether the corpuscles are at rest or are agitated they do not lose their weight, so that the bottom sustains not only the weight but the elasticity of the fluid. Such therefore is the fluid which we shall substitute for air. Its properties agree with those which we have already assumed for elastic fluids, and by them we shall explain other properties which have been found for air and shall point out others which have not yet been sufficiently considered.

3. We consider the corpuscles which are contained in the cylindrical cavity as practically infinite in number, and when they occupy the space *ECDF* we assume that they constitute ordinary air, to which as a standard



all our measurements are to be referred: and so the weight P holding the piston in the position EF does not differ from the pressure of the superincumbent atmosphere, which therefore we shall designate by P in what follows.

It should be noticed that this pressure is not exactly equal to the absolute weight of a vertical cylinder of air resting on the piston EF, as hitherto most authors have asserted without sufficient consideration; rather it is equal to the fourth proportional to the surface of the earth, to the size of the piston EF, and to the weight of all the atmosphere on the surface of the earth.

4. We shall now investigate the weight π, which is sufficient to condense the air $ECDF$ into the space $eCDf$, on the assumption that the velocity of the particles is the same in both conditions of the air, the natural condition as well as the condensed. Let $EC = 1$ and $eC = s$. When the piston EF is moved to ef, it appears that a greater effort is made by the fluid for two reasons: first, because the number of particles is now greater in the ratio of the space in which they are contained, and secondly, because each particle repeats its impacts more often. That we may properly calculate the increment which depends on the first cause we may consider the particles as if they were at rest. We shall set the number of them which are contiguous to the piston in the position $EF = n$; then the like number when the piston is in the position ef will be $= n : \left(\dfrac{eC}{EC}\right)^{2/3}$ or $= n : s^{2/3}$.

It should be noticed that the fluid is no more condensed in the lower part than in the upper part, because the weight P is infinitely greater than the weight of the fluid itself: hence it is plain that for this reason the force of the fluid is in the ratio of the numbers n and $n : s^{2/3}$ that is, as $s^{2/3}$ is to 1. Now in reference to the other increment arising from the second cause, this is found by considering the motion of the particles, and it appears that their impacts are made more often by as much as the particles are closer together: therefore the numbers of the impacts will be reciprocally as the mean distances between the surfaces of the particles, and these mean distances will be thus determined.

We assume that the particles are spheres. We represent by D the mean distance between the centers of the spheres when the piston is in the position EF, and by d the diameter of a sphere. Then the mean distance between the surfaces of the spheres will be $D - d$. But it is evident that when the piston is in the position ef, the mean distance between the centers of the spheres $= D\sqrt[3]{s}$ and therefore the mean distance between the surfaces of the spheres $= D\sqrt[3]{s} - d$. Therefore, with respect to the second cause, the force of the natural air in $ECDF$ will be to the force of the compressed air in $eCDf$ as $\dfrac{1}{D-d}$ to $\dfrac{1}{D\sqrt[3]{s}-d}$, or as $D\sqrt[3]{s} - d$ to $D - d$. When both causes are joined the predicted forces will be as $s^{2/3} \times (D\sqrt[3]{s} - d)$ to $D - d$.

For the ratio of D to d we may substitute one which is easier to understand: for if we think of the piston EF as depressed by an infinite weight, so that it descends to the position mn, in which all the particles are in contact, and if we represent the line mC by m, we shall have D is to d as 1 is to $\sqrt[3]{m}$. If we substitute this in the ratio above, we shall find that the force of the natural air in $ECDF$ is to the force of the compressed air in $eCDf$ as $s^{2/3} \times (\sqrt[3]{s} - \sqrt[3]{m})$ is to $1 - \sqrt[3]{m}$, or as $s - \sqrt[3]{mss}$ is to $1 - \sqrt[3]{m}$. Therefore $\pi = \dfrac{1 - \sqrt[3]{m}}{s - \sqrt[3]{mss}} \times P$.

5. From all the facts known we may conclude that natural air can be very much condensed and compressed into a practically infinitely small space; so that we may set $m = 0$, and hence $\pi = P/s$; so that the compressing weights are almost in the inverse ratio of the spaces which air occupies when compressed by different amounts. This law has been proved by many experiments. It certainly may be safely adopted for air that is less dense than natural air; whether it holds for considerably denser air I have not sufficiently investigated: nor have there yet been experiments instituted with the accuracy which is necessary in this case. There is special need of an experiment to find the value of m, but this experiment must be most accurately carried out and with air under very high pressure;

and the temperature of the air while it is being compressed must be carefully kept constant.

6. The elasticity of air is not only increased by condensation but by heat supplied to it, and since it is admitted that heat may be considered as an increasing internal motion of the particles, it follows that if the elasticity of air of which the volume does not change is increased, this indicates a more intense motion in the particles of air; which fits in well with our hypothesis; for it is plain that so much the greater weight P is needed to keep the air in the condition $ECDF$, as the aerial particles are agitated by the greater velocity. It is not difficult to see that the weight P should be in the duplicate ratio of this velocity because, when the velocity increases, not only the number of impacts but also the intensity of each of them increases equally, and each of them is proportional to the weight P.

Therefore, if the velocity of the particles is called v, the weight which is able to sustain the piston in the position $EF = vvP$ and in the position

$$ef = \frac{1 - \sqrt[3]{m}}{s - \sqrt[3]{mss}} \times vvP, \text{ or approximately } = \frac{vvP}{s},$$ because as we have seen

the number m is very small in comparison with unity or with the number s.

7. This theorem, as I have presented it in the preceding paragraph, in which it is shown that in air of any density but at a fixed temperature, the elasticities are proportional to the densities, and further that the increments of elasticity which are produced by equal changes of temperature are proportional to the densities, this theorem, I say, D. Amontons discovered by experiment and presented it in the Memoirs of the Royal Academy of Sciences of Paris in 1702.

COMMENTARY ON

A Great Prize, a Long-Suffering Inventor and the First Accurate Clock

THE reckoning of latitude—the distance north or south from the equator of a point on the earth's surface—was well understood by the ancients. Since the Pole Star holds approximately the same position in the heavens throughout every night, and since the earliest sailors observed that it dipped toward the horizon as they sailed south, they measured its angle with the horizon (its altitude), using an astrolabe, cross-staff or other angle-measuring device, and thus fixed their position with reference to the equator. Over the centuries the ancient methods were modified and improved, but their basic features are retained by sea and air navigators to the present day.[1]

Longitude, the measure of distance east or west from an arbitrary line, presented much greater difficulties and defied exact calculation until the eighteenth century. The combined efforts of astronomers, physicists, mathematicians and clock makers were required to solve this important problem upon which scientific cartography, sound navigation and systematic exploration and discovery depended. Curiously enough, it was not until almost half a century after Columbus had made his "long voyage" that anyone connected the fixing of longitude with the construction of reliable and portable timekeepers. The relationship between clocks and longitude is doubtless obvious to many readers, but I had better be safe and explain what is involved. Longitude is determined, in effect, by translating space into time. Take as a baseline from which east-west distances are to be measured a meridian (half a great circle included between the poles) passing through a convenient place; the modern convention fixes on Greenwich. Designate this line as the zero or prime meridian and imagine other meridians marking off the globe at intervals of 15°. Since it takes the earth twenty-four hours to complete a rotation of 360°, each meridian may be regarded as separated from its immediate neighbors to the east and west by one hour. Finding your longitude then, is "merely a matter

[1] "The quadrants, sextants and octants, developed throughout the centuries, were little more than segments of the ancient astrolabe, refined and adapted to meet the special requirements of surveyors and navigators. The modern Nautical Almanac, with its complex and multifarious tables that make it possible to find the latitude at any hour of the day or night, is nothing more than the sum total of ancient astrology, streamlined and perfected by astronomical instruments, including telescopes." Lloyd A. Brown, *The Story of Maps*, Boston, 1949, p. 180.

of comparing noons with Greenwich. You are just as long a distance from Greenwich as your noon is long a time from the Greenwich noon."[2] Nowadays Greenwich noon is ascertained by radio; but a clock set to run on Greenwich time will do the job almost as well. Starting out on a sea voyage, you take along a chronometer set to Greenwich time; after sailing west for a few days you observe, let us suppose, that when the sun is directly overhead (12 noon), your Greenwich chronometer says 3 P.M. This means that the sun has required three hours to "move" from directly above Greenwich to directly above the spot where you find yourself. To be accurate, it means that the earth has turned for three hours. Thus you have reached a point three times 15° west longitude. To give another illustration, if it is noon where you are when it is midnight in Greenwich, you are halfway around the globe, at longitude 180°.

It is easy to see, therefore, that precise clocks were needed to calculate longitude. The search for a reliable method of keeping time at sea produced its share of fantastic as well as sensible suggestions. Sir Kenelm Digby, for example, invented a "powder of sympathy" which, by a method I shall not attempt to repeat, caused a dog on shipboard to "yelp the hour on the dot." This was not the final answer to the problem. John Harrison, a Yorkshire carpenter, did better with his famous No. 4 chronometer, which took fifty years to make but lost only one second per month in trials at sea. Parliament had offered a prize of £20,000 for a dependable chronometer and Harrison—this "very ingenious and sober man," a contemporary called him—quite properly claimed the money. He thereupon became the victim of a series of unsurpassed chicaneries perpetrated by scientists and politicians in concert. He got his reward but only after a parliamentary crisis and direct intervention by the King.

The story of "The Longitude," a chronicle of science, politics, mathematics, human determination and brilliant craftsmanship, is admirably told by Lloyd A. Brown, a leading cartographer, in *The Story of Maps.* The following material is selected from his book.

[2] David Greenhood, *Down to Earth: Mapping for Everybody*, New York, 1951, p. 15.

The Art of Navigation is to be perfected by the Solution of this Problem. To find, at any Time, the Longitude of a Place at Sea. A Public Reward is promised for the Discovery. Let him obtain it who is able.

—Bernhard Varenius (*Geographia Generalis, 1650*)

3 The Longitude

By LLOYD A. BROWN

SCIENTIFIC cartography was born in France in the reign of Louis XIV (1638–1715), the offspring of astronomy and mathematics. The principles and methods which had been used and talked about for over two thousand years were unchanged; the ideal of Hipparchus and Ptolemy, to locate each place on earth scientifically, according to its latitude and longitude, was still current. But something new had been introduced into the picture in the form of two pieces of apparatus—a telescope and a timekeeper. The result was a revolution in map making and a start towards an accurate picture of the earth. With the aid of these two mechanical contrivances it was possible, for the first time, to solve the problem of how to determine longitude, both on land and at sea.

The importance of longitude, the distance of a place east or west from any other given place, was fully appreciated by the more literate navigators and cartographers of history, but reactions to the question of how to find it varied from total indifference to complete despondency. Pigafetta, who sailed with Magellan, said that the great explorer spent many hours studying the problem of longitude, "but," he wrote, "the pilots content themselves with knowledge of the latitude, and are so proud [of themselves], they will not hear speak of the longitude." Many explorers of the time felt the same way, and rather than add to their mathematical burdens and observations, they were content to let well enough alone. However, "there be some," says an early writer, "that are very inquisitive to have a way to get the longitude, but that is too tedious for seamen, since it requireth the deep knowledge of astronomy, wherefor I would not have any man think that the longitude is to be found at sea by any instrument; so let no seamen trouble themselves with any such rule, but (according to their accustomed manner) let them keep a perfect account and reckoning of the way of their ship." What he meant was, let them keep their dead reckoning with a traverse board, setting down the ship's estimated daily speed and her course.[1]

Like the elixir of life and the pot of gold, longitude was a will-o'-the-wisp which most men refused to pursue and others talked about with awe.

[1] W. R. Martin, article "Navigation" in *Encyclopaedia Britannica*, 11th Edition.

"Some doo understand," wrote Richard Eden, "that the Knowledge of the Longitude myght be founde, a thynge doubtlesse greatly to be desyred, and hytherto not certaynly knowen, although Sebastian Cabot, on his death-bed told me that he had the knowledge thereof by divine revelation, yet so, that he myght not teache any man. But," adds Eden, with a certain amount of scorn, "I thinke that the good olde man, in that extreme age, somewhat doted, and had not yet even in the article of death, vtterly shaken off all wordly vayne glorie." [2]

Regardless of pessimism and indifference, the need for a method of finding the longitude was fast becoming urgent. The real trouble began in 1493, less than two months after Columbus returned to Spain from his first voyage to the west. On May 4 of that year, Pope Alexander VI issued the Bull of Demarcation to settle the dispute between Spain and Portugal, the two foremost maritime rivals in Europe. With perfect equanimity His Holiness drew a meridian line from pole to pole on a chart of the Western Ocean one hundred leagues from the Azores. To Spain he assigned all lands not already belonging to any other Christian prince which had been or would be discovered west of the line, and to Portugal all discoveries to the east of it; a masterful stroke of diplomacy, except for the fact that no one knew where the line fell. Naturally both countries suspected the worst, and in later negotiations each accused the other of pushing the line a little in the wrong direction. For all practical purposes, the term "100 leagues west of the Azores" was meaningless, as was the Line of Demarcation and all other meridians in the New World laid down from a line of reference in the Old.

Meanwhile armed convoys heavily laden with the wealth of the Indies ploughed the seas in total darkness so far as their longitude was concerned. Every cargo was worth a fortune and all the risk involved, but too many ships were lost. There were endless delays because a navigator was never sure whether he had overreached an island or was in imminent danger of arriving in the middle of the night without adequate preparations made for landing. The terrible uncertainty was wearing. In 1598 Philip III of Spain offered a perpetual pension of 6000 ducats, together with a life pension of 2000 ducats and an additional gratuity of 1000 more to the "discoverer of longitude." Moreover, there would be smaller sums available in advance for sound ideas that might lead to the discovery and for partially completed inventions that promised tangible results, and no questions asked. It was the clarion call for every crank, lunatic and undernourished inventor in the land to begin research on "the fixed point" or the "East and West navigation" as it was called. In a short time the

[2] See Richard Eden's "Epistle Dedicatory" in his translation of John Taisnier's *A very necessarie and profitable book concerning nauigation* . . . London, 1579 (?). (Quoted from *Bibliotheca Americana. A catalogue of books . . . in the library of the late John Carter Brown.* Providence, 1875, Part I, No. 310.)

Spanish government was so deluged with wild, impractical schemes and Philip was so bored with the whole thing that when an Italian named Galileo wrote the court in 1616 about another idea, the king was unimpressed. After a long, sporadic correspondence covering sixteen years, Galileo reluctantly gave up the idea of selling his scheme to the court of Spain.[3]

Portugal and Venice posted rewards, and drew the same motley array of talent and the same results as Spain. Holland offered a prize of 30,000 scudi to the inventor of a reliable method of finding the longitude at sea, and Willem Blaeu, map publisher, was one of the experts chosen by the States General to pass on all such inventions. In August, 1636, Galileo came forward again and offered his plan to Holland, this time through his Paris friend Diodati, as he did not care to have his correspondence investigated by the Inquisition. He told the Dutch authorities that some years before, with the aid of his telescope, he had discovered what might be a remarkable celestial timekeeper—Jupiter. He, Galileo, had first seen the four satellites, the "Cosmian Stars" (*Sidera Medicea*, as he called them), and had studied their movements. Around and around they went, first on one side of the planet, then on the other, now disappearing, then reappearing. In 1612, two years after he first saw them, he had drawn up tables, plotting the positions of the satellites at various hours of the night. These, he found, could be drawn up several months in advance and used to determine mean time at two different places at once. Since then, he had spent twenty-four years perfecting his tables of the satellites, and now he was ready to offer them to Holland, together with minute instructions for the use of any who wished to find the longitude at sea or on land.[4]

The States General and the four commissioners appointed to investigate the merits of Galileo's proposition were impressed, and requested further details. They awarded him a golden chain as a mark of respect and Hortensius, one of the commissioners, was elected to make the journey to Italy where he could discuss the matter with Galileo in person. But the Holy Office got wind of things and the trip was abandoned. In 1641 after a lapse of nearly three years, negotiations were renewed by the Dutch scientist Christian Huygens, but Galileo died a short time after and the idea of using the satellites of Jupiter was set aside.[5]

In the two thousand year search for a solution of the longitude problem

[3] J. J. Fahie, *Galileo. His life and work*, New York, 1903, pp. 172, 372 ff.; Rupert T. Gould, *The marine chronometer: Its history and development*, London, 1923, pp. 11, 12.

[4] J. J. Fahie, *op. cit.*, pp. 372 ff. Galileo named the satellites of Jupiter the "Cosmian Stars" after Cosmo Medici (Cosmo II, grandduke of Tuscany). See Galileo's *Opere* edited by Eugenio Alberi, 16 vols., Firenze, 1842–56. Tome III contains his "Sydereus Nuncius," pp. 59–99, describing his observations of Jupiter's satellites, and his suggestion that they be used in the determination of longitude.

[5] J. J. Fahie, *op. cit.*, pp. 373–75.

it was never a foregone conclusion that the key lay in the transportation of timekeepers. But among the optimistic who believed that a solution could somehow be found, it was agreed that it would have to come from the stars, especially for longitude at sea, where there was nothing else to observe. It might be found in the stars alone or the stars in combination with some terrestrial phenomenon. However, certain fundamental principles were apparent to all who concerned themselves with the problem. Assuming that the earth was a perfect sphere divided for convenience into 360 degrees, a mean solar day of 24 hours was equivalent to 360 degrees of arc, and 1 hour of the solar day was equivalent to 15 degrees of arc or 15 degrees of longitude. Likewise, 1 degree of longitude was the equivalent of 4 minutes of time. Finer measurements of time and longitude (minutes and seconds of time, minutes and seconds of arc) had been for centuries the stuff that dreams were made of. Surveys of the earth in an east-west direction, expressed in leagues, miles or some other unit of linear measure, would have no significance unless they could be translated into degrees and minutes of arc, fractional parts of the circumference of the earth. And how big was the earth?

The circumference of the earth and the length of a degree (1/360th part of it) had been calculated by Eratosthenes and others but the values obtained were questionable. Hipparchus had worked out the difference between a solar day and a sidereal day (the interval between two successive returns of a fixed star to the meridian), and had plotted a list of 44 stars scattered across the sky at intervals of right ascension equal to exactly one hour, so that one or more of them would be on the meridian at the beginning of every sidereal hour. He had gone a step further and adopted a meridian line through Rhodes, suggesting that longitudes of other places could be determined with reference to his prime meridian by the simultaneous observation of the moon's eclipses. This proposal assumed the existence of a reliable timekeeper which was doubtless nonexistent.

The most popular theoretical method of finding longitude, suggested by the voyages of Columbus, Cabot, Magellan, Tasman and other explorers, was to plot the variation or declination of the compass needle from the true north. This variation could be found by taking a bearing on the polestar and noting on the graduated compass card the number of points, half and quarter points (degree and minutes of arc) the needle pointed east or west of the pole. Columbus had noted this change of compass variation on his first voyage, and later navigators had confirmed the existence of a "line of no variation" passing through both poles and the fact that variation changed direction on either side of it. This being so, and assuming that the variation changed at a uniform rate with a change of longitude, it was logical to assume that here at last was a solution to the whole problem. All you had to do was compare the amount of variation at your place of observation with the tabulated variation at places whose longitude had already been determined. It was this fond hope that induced Edmund Halley and others to compile elaborate charts showing the supposed lines of equal variation throughout the world. However, it was by no means that simple, as Gellibrand and others discovered. Variation does not change uniformly with a change of longitude; likewise, changes in variation occur very slowly; so slowly, in fact, that precise east-west measurements are impractical, especially at sea. And, too, it was found that lines of equal variation do not always run north and south; some run nearly east and west. However, in spite of the flaws that cropped up, one by one, the method had strong supporters for many years, but finally died a painful, lingering death.[6]

In addition to discovering Jupiter's satellites, Galileo made a second important contribution to the solution of longitude by his studies of the pendulum and its behavior, for the application of the swinging weight to the mechanism of a clock was the first step towards the development of an accurate timekeeper.[7] The passage of time was noted by the ancients and their astronomical observations were "timed" with sundials, sandglasses and water clocks but little is known about how the latter were controlled. Bernard Walther, a pupil of Regiomontanus, seems to have been the first to time his observations with a clock driven by weights. He stated that on the 16th of January, 1484, he observed the rising of the planet Mercury, and immediately attached the weight to a clock having an hour-wheel with fifty-six teeth. By sunrise one hour and thirty-five teeth had passed, so that the elapsed time was an hour and thirty-seven minutes, according to his calculations. The next important phase in the development of a timekeeper was the attachment of a pendulum as a driving force. This clock was developed by Christian Huygens, Dutch physicist and astronomer, the son of Constantine Huygens. He built the first one in 1656 in order to increase the accuracy of his astronomical observations, and later presented it to the States General of Holland on the 16th of June, 1657. The following year he published a full description of the principles involved in the mechanism of his timekeeper and the physical laws governing the pendulum. It was a classic piece of writing, and established Huygens as one of the leading European scientists of the day.[8]

By 1666 there were many able scientists scattered throughout Europe. Their activities covered the entire fields of physics, chemistry, astronomy, mathematics, and natural history, experimental and applied. For the most

[6] R. T. Gould, *op. cit.*, pp. 4 and 4 *n.*

[7] See Galileo's *Dialogues concerning two new sciences, by Galileo Galilei*, translated by Henry Crew and Alfonso de Salvio, introduction by Antonio Favaro, New York, 1914, pp. 84, 95, 170, 254.

[8] See John L. E. Dreyer's article "Time, Measurement of" (in the *Encyclopaedia Britannica*, 11th edition, pp. 983d, 984a).

part they worked independently and their interests were widely diversified. Occasionally the various learned societies bestowed honorary memberships on worthy colleagues in foreign countries, and papers read in the various societies were exchanged with fellow scientists in foreign lands. The stage was set for the transition of cartography from an art to a science. The apparatus was at hand and the men to use it.

Pleading for the improvement of maps and surveys, Thomas Burnet made a useful distinction between the popular commercial map publications of the day and what he considered should be the goal of future map makers. "I do not doubt," he wrote, "but that it would be of very good use to have *natural* Maps of the Earth . . . as well as civil. . . . Our common Maps I call *Civil*, which note the distinction of Countries and of Cities, and represent the Artificial Earth as inhabited and cultivated: But natural Maps leave out all that, and represent the Earth as it would be if there were not an Inhabitant upon it, nor ever had been; the Skeleton of the Earth, as I may so say, with the site of all its parts. Methinks also every Prince should have such a Draught of his Country and Dominions, to see how the ground lies in the several parts of them, which highest, which lowest; what respect they have to one another, and to the Sea; how the Rivers flow, and why; how the Mountains lie, how Heaths, and how the Marches. Such a Map or Survey would be useful both in time of War and Peace, and many good observations might be made by it, not only as to Natural History and Philosophy, but also in order to the perfect improvement of a Country." [9]

These sentiments regarding "natural" maps were fully appreciated and shared by the powers in France, who proceeded to do something about it. All that was needed was an agency to acquire the services and direct the work of the available scientific talent, and someone to foot the bills. The agency was taken care of by the creation of the Académie Royale des Sciences, and the man who stood prepared to foot the bills for better maps was His Majesty Louis XIV, king of France.

Louis XIV ascended the throne when he was five years old, but had to wait sixteen years before he could take the reins of government. He had to sit back and watch the affairs of state being handled by the queen-mother and his minister, Cardinal Mazarin. He saw the royal authority weakened by domestic troubles and the last stages of the Thirty Years' War. Having suffered through one humiliation after another without being able to do anything about it, Louis resolved, when he reached the age of twenty-one, to rule as well as reign in France. He would be his own first minister. Foremost among his few trusted advisors was Jean Baptiste Colbert, minister for home affairs, who became, in a short time, the chief power behind the throne. Colbert, an ambitious and industrious man with

[9] Thomas Burnet: *The theory of the earth* . . . London, 1684, p. 144.

expensive tastes, contrived to indulge himself in literary and artistic extravagances while adding to the stature and glory of his monarch. As for the affairs of state over which he exercised control, there were two enterprises in particular which entitle Colbert to an important place in the history of France. The first was the establishment of the French Marine under a monarch who cared little for naval exploits or the importance of sea power in the growth and defense of his realm; the second was the founding, in 1666, of the Académie Royale des Sciences, now the Institut de France.[10]

The Académie Royale was Colbert's favorite project. An amateur scientist, he realized the potential value of a distinguished scientific body close to the throne, and with his unusual skill and seemingly unlimited funds he set out to make France pre-eminent in science as it was in the arts and the art of war. He scoured Europe in search of the top men in every branch of science. He addressed personal invitations to such figures as Gottfried Wilhelm von Leibnitz, German philosopher and mathematician; Niklaas Hartsoeker, Dutch naturalist and optician; Ehrenfried von Tschirnhausen, German mathematician and manufacturer of optical lenses and mirrors; Joannes Hévélius, one of Europe's foremost astronomers; Vincenzo Viviani, Italian mathematician and engineer; Isaac Newton, England's budding mathematical genius. The pensions that went with the invitations were without precedent, surpassing in generosity those established by Cardinal Richelieu for the members of the Académie Française, and those granted by Charles II for the Royal Society of London. Additional funds were available for research, and security and comfort were assured to those scientists who would agree to work in Paris, surrounded by the most brilliant court in Europe. Colbert's ambition to make France foremost in science was realized, though some of the invitations were declined with thanks. Christian Huygens joined the Académie in 1666 and received his pension of 6000 livres a year until 1681, when he returned to Holland. Olaus Römer, Dutch astronomer, also accepted. These celebrities were followed by Marin de la Chambre who became physician to Louis XIV; Samuel Duclos and Claude Bourdelin in chemistry; Jean Pecquet and Louis Gayant in anatomy; Nicholas Marchant in botany.[11]

In spite of the broad scope of its activities, the avowed purpose of founding the Académie Royale, according to His Majesty, was to correct and improve maps and sailing charts. And the solution of the major problems of chronology, geography and navigation, whose practical importance was incontestable, lay in the further study and application of astronomy.[12]

[10] See Charles J. E. Wolf, *Histoire de l'observatoire de Paris de sa fondation à 1793*, Paris, 1902; also *L'Institut de France* by Gaston Darboux, Henry Roujon and George Picot, Paris, 1907 ("Les Grandes Institutions de France").
[11] C. J. E. Wolf, *op. cit.*, pp. 5 ff.
[12] *Mémoires de l'Académie Royale des Sciences*, Vol. VIII, Paris, 1730.

To this end, astronomical observations and conferences were begun in January, 1667. The Abbé Jean Picard, Adrian Auzout, Jacques Buot and Christian Huygens were temporarily installed in a house near the Cordeliers, the garden of which was taken over for astronomical observations. There the scientists set up a great quadrant, a mammoth sextant and a highly refined version of the sundial. They also constructed a meridian line. Sometimes observations were made in the garden of the Louvre. On the whole, facilities for astronomical research were far from good, and there was considerable grumbling among the academicians.

As early as 1665, before the Académie was founded, Auzout had written Colbert an impassioned memorandum asking for an observatory, reminding him that the progress of astronomy in France would be as nothing without one. When Colbert finally made up his mind, in 1667, and the king approved the money, events moved rapidly. The site chosen for the observatory was at Faubourg St. Jacques, well out in the country, away from the lights and disconcerting noises of Paris. Colbert decided that the Observatory of Paris should surpass in beauty and utility any that had been built to date, even those in Denmark, England and China, one which would reflect the magnificence of a king who did things on a grand scale. He called in Claude Perrault, who had designed the palace at Versailles with accommodations for 6000 guests, and told him what he and his Académie wanted. The building should be spacious; it should have ample laboratory space and comfortable living quarters for the resident astronomers and their families.[13]

On the 21st of June, 1667, the day of the summer solstice, the members of the Académie assembled at Faubourg St. Jacques, and with great pomp and circumstance made observations for the purpose of "locating" the new observatory and establishing a meridian line through its center, a line which was to become the official meridian of Paris. The building was to have two octagonal towers flanking the southern facade, and eight azimuths were carefully computed so that the towers would have astronomical as well as architectural significance. Then, without waiting for their new quarters, the resident members of the Académie went back to work, attacking the many unsolved riddles of physics and natural history, as well as astronomy and mathematics. They designed and built much of the apparatus for the new observatory. They made vast improvements in the telescope as an astronomical tool; they solved mechanical and physical problems connected with the pendulum and what gravity does to it, helping Huygens get the few remaining "bugs" out of his pendulum timekeeper. They concentrated on the study of the earth, its size and shape and place in the universe; they investigated the nature and behavior of the moon and other celestial bodies; they worked towards the establishment of a

[13] C. J. E. Wolf, *op. cit.*, p. 4.

standard meridian of longitude for all nations, the meridian of Paris running through the middle of their observatory. They worked on the problem of establishing the linear value of a degree of longitude which would be a universally acceptable constant. In all these matters the Académie Royale des Sciences was fortunate in having at its disposal the vast resources of the court of France as well as the personal patronage of Louis XIV.

An accurate method of determining longitude was first on the agenda of the Académie Royale, for obviously no great improvement could be made in maps and charts until such a method was found. Like Spain and the Netherlands, France stood ready to honor and reward the man who could solve the problem. In 1667, an unnamed German inventor addressed himself to Louis XIV, stating that he had solved the problem of determining longitude at sea. The king promptly granted him a patent (brevet) on his invention, sight unseen, and paid him 60,000 livres in cash. More than this, His Majesty contracted to pay the inventor 8000 livres a year (Huygens was getting 6000!) for the rest of his life, and to pay him four sous on every ton of cargo moved in a ship using the new device, reserving for himself only the right to withdraw from the contract in consideration of 100,000 livres. All this His Majesty would grant, but on one condition: the inventor must demonstrate his invention before Colbert, Abraham Duquesne, Lieutenant-General of His Majesty's naval forces, and Messrs. Huygens, Carcavi, Roberval, Picard and Auzout of the Académie Royale des Sciences.[14]

The invention proved to be nothing more than a variation on an old theme, an ingenious combination of water wheel and odometer to be inserted in a hole drilled in the keel of a ship. The passage of water under the keel would turn the water wheel, and the distance traversed by the ship in a given period would be recorded on the odometer. The inventor also claimed that by some strange device best known to himself his machine would make any necessary compensations for tides and cross-currents of one kind and another; it was, in fact, an ideal and perfect solution to the longitude problem. The royal examiners studied the apparatus, praised its ingenuity and then submitted their report to the king in writing. They calmly pointed out, among other things, that if a ship were moving with a current, it might be most stationary with respect to the water under the keel and yet be carried along over an appreciable amount of longitude while the water wheel remained motionless. If, on the other hand, the ship were breasting a current, the odometer would register considerable progress when actually the ship might be getting nowhere. The German

[14] *Histoire de l'Académie Royale des Sciences*, Vol. I, pp. 45–46.

inventor departed from Paris richer by 60,000 livres and the members of the Académie went back to work.[15]

In 1669, after three years of intensive study, the scientists of the Académie Royale had gathered together considerable data on the celestial bodies, and had studied every method that had been suggested for the determination of longitude. The measurement of lunar distances from the stars and the sun was considered impractical because of the complicated mathematics involved. Lunar eclipses might be all right except for the infrequency of the phenomenon and the slowness of eclipses, which increase the chance of error in the observer. Moreover, lunar eclipses were utterly impractical at sea. Meridional transits of the moon were also tried with indifferent success. What the astronomers were looking for was a celestial body whose distance from the earth was so great that it would present the same appearance from any point of observation. Also wanted was a celestial body which would move in a constantly predictable fashion, exhibiting at the same time a changing picture that could be observed and timed simultaneously from different places on the earth. Such a body was Jupiter, whose four satellites, discovered by Galileo, they had observed and studied. The serious consideration of Jupiter as a possible solution of the longitude problem brought to mind a publication that had come out in 1668 written by an Italian named Cassini. While the members of the Académie continued their study of Jupiter's satellites with an eye to utilizing their frequent eclipses as a method of determining longitude, Colbert investigated the possibilities of luring Cassini to Paris.

Giovanni Domenico Cassini was born in Perinaldo, a village in the Comté of Nice, June 8, 1625, the son of an Italian gentleman. After completing his elementary schooling under a preceptor he studied theology and law under the Jesuits at Genoa and was graduated with honors. He developed a decided love of books, and while browsing in a library one day he came across a book on astrology. The work amused him, and after studying it he began to entertain his friends by predicting coming events. His phenomenal success as an astrologer plus his intellectual honesty made him very suspicious of his new-found talent, and he promptly abandoned the hocus-pocus of astrology for the less dramatic study of astron-

[15] Justin Winsor, *Narrative and Critical History of America,* Boston, 1889, Vol. II, pp. 98–99 has an interesting note on the "log." In Pigafetta's journal (January, 1521), he mentions the use of a chain dragged astern on Magellan's ships to measure their speed. The "log" as we know it was described in Bourne's *Regiment of the Sea*, 1573, and Humphrey Cole is said to have invented it. In Eden's translation of Taisnier he speaks of an artifice "not yet divulgate, which, placed in the pompe of a shyp, whyther the water hath recourse, and moved by the motion of the shypp, with wheels and weyghts, doth exactly shewe what space the shyp hath gone." See the article "Navigation" in the *Encyclopaedia Britannica*, 9th edition. For further comments on the history of the log, see L. C. Wroth, *The Way of a Ship*, Portland, Maine, 1937, pp. 72–74.

omy. He made such rapid progress and displayed such remarkable aptitude, that in 1650, when he was only twenty-five years old, he was chosen by the Senate of Bologna to fill the first chair of astronomy at the university, vacant since the death of the celebrated mathematician Bonaventura Cavalieri. The Senate never regretted their choice.[16]

One of Cassini's first duties was to serve as scientific consultant to the Church for the precise determination of Holy Days, an important application of chronology and longitude. He retraced the meridian line at the Cathedral of Saint Petronius constructed in 1575 by Ignazio Dante, and added a great mural quadrant which took him two years to build. In 1655 when it was completed, he invited all the astronomers in Italy to observe the winter solstice and examine the new tables of the sun by which the equinoxes, the solstices and numerous Holy Days could now be accurately determined.

Cassini was next appointed by the Senate of Bologna and Pope Alexander VII to determine the difference in level between Bologna and Ferrara, relative to the navigation of the Po and Reno rivers. He not only did a thorough job of surveying, but wrote a detailed report on the two rivers and their peculiarities as well. The Pope next engaged Cassini, in the capacity of a hydraulic engineer, to straighten out an old dispute between himself and the Duke of Tuscany relative to the diversion of the precious water of the Chiana River, alternate affluent of the Arno and the Tiber. Having settled the dispute to the satisfaction of the parties concerned, he was appointed surveyor of fortifications at Perugia, Pont Felix and Fort Urbino, and was made superintendent of the waters of the Po, a river vital to the conservation and prosperity of the country. In his spare time Cassini busied himself with the study of insects and to satisfy his curiosity repeated several experiments on the transfusion of blood from one animal to another, a daring procedure that was causing a flurry of excitement in the scientific world. But his major hobby was astronomy and his favorite planet was Jupiter. While he worked on the Chiana he spent many evenings at Citta della Piève observing Jupiter's satellites. His telescope was better than Galileo's and with it he was able to make some additional discoveries. He noted that the plane of the revolving satellites was such that the satellites passed across Jupiter's disc close to the equator; he noted the size of the orbit of each satellite. He was certain he could see a number of fixed spots on Jupiter's orb, and on the strength of his findings he began to time the rotation of the planet as well as the satellites, using a fairly reliable pendulum clock.[17]

[16] *Oeuvres de Fontenelle. Eloges.* Paris, 1825, Vol. I, p. 254.

[17] Joseph François Michaud: *Biographie Universelle*, Paris, 1854–65. Cassini gave Jupiter's rotation as $9^h\ 56^m$. The *correct* time is not yet known with certainty. Slightly different results are obtained by using different markings. The value $9^h\ 55^m$ is frequently used in modern texts.

After sixteen years of patient toil and constant observations, Cassini published his tables (*Ephemerides*) of the eclipses of Jupiter's satellites for the year 1668, giving on one page the appearance of the planet in a diagram with the satellites grouped around it and on the opposite page the time of the eclipse (immersion) of each satellite in hours, minutes and seconds, and the time of each emersion.[18]

Cassini, then forty-three years old, had become widely known as a scholar and skilled astronomer, and when a copy of his *Ephemerides* reached Paris Colbert decided he must get him for the Observatory and the Académie Royale. In this instance, however, it took considerable diplomacy as well as gold to get the man he wanted, for Cassini was then in the employ of Pope Clement IX, and neither Louis XIV nor Colbert cared to offend or displease His Holiness. Three distinguished scholars, Vaillant, Auzout and Count Graziani, were selected to negotiate with the Pope and the Senate of Bologna for the temporary loan of Cassini, who was to receive 9000 livres a year as long as he remained in France. The arrangements were finally completed, and Cassini arrived in Paris on the 4th of April, 1669. Two days later he was presented to the king. Although Cassini had no intention of staying indefinitely, Colbert was insistent, and in spite of the remonstrances of the Pope and the Senate of Bologna, Cassini became a naturalized citizen of France in 1673, and was thereafter known as Jean Domenique Cassini.[19]

Observations were in full swing when Cassini took his place among the savants of the Académie Royale who were expert mechanics as well as physicists and mathematicians. Huygens and Auzout had ground new lenses and mirrors, and had built vastly improved telescopes for the observatory. With the new instruments Huygens had already made some phenomenal discoveries. He had observed the rotation period of Saturn, discovered Saturn's rings and the first of the satellites. Auzout had built other instruments and applied to them an improved filar micrometer, a measuring device all but forgotten since its invention by Gascoyne (Gascoigne) about 1639. After Cassini's arrival, more apparatus was ordered, including the best telescopes available in Europe, made by Campani in Italy.[20]

One of the first important steps toward the correction of maps and charts was the remeasurement of the circumference of the earth and the establishment of a new value for a degree of arc in terms of linear measure. There was still a great deal of uncertainty as to the size of the earth,

[18] The first edition of Cassini's work was published under the title *Ephemerides Bononiensés Mediceorvm sydervm ex hypothesibvs, et tabvlis Io: Dominici Cassini* . . . Bononiae [Bologne], 1668.

[19] C. J. E. Wolf, *op. cit.*, p. 6.

[20] For a detailed inventory of the equipment built and purchased by the Académie Royale, see C. J. E. Wolf, *op. cit.*

and the astronomers were reluctant to base their new data on a fundamental value which might negate all observations made with reference to it. After poring over the writings of Hipparchus, Poseidonius, Ptolemy and later authorities such as Snell, and after studying the methods these men had used, the Académie worked out a detailed plan for measuring the earth, and in 1669 assigned Jean Picard to do the job.

The measurement of the earth at the equator, from east to west, was out of the question; no satisfactory method of doing it was known. Therefore the method used by Eratosthenes was selected, but with several important modifications and with apparatus that the ancients could only have dreamed of. Picard was to survey a line by triangulation running approximately north and south between two terminal points; he would then measure the arc between the two points (that is, the difference in latitude) by astronomical observations. After looking over the country around Paris, Picard decided he could run his line nearly northward to the environs of Picardy without encountering serious obstructions such as heavy woods and high hills.[21]

Picard selected as his first terminal point the "Pavillon" at Malvoisine near Paris, and for his second point the clock tower in Sourdon near Amiens, a distance of about thirty-two French leagues. Thirteen great triangles were surveyed between the two points, and for the purpose Picard used a stoutly reinforced iron quadrant with a thirty-eight inch radius fixed on a heavy standard. The usual pinhole alidades used for sighting were replaced by two telescopes with oculars fitted with cross hairs, an improved design of the instrument used by Tycho Brahe in Denmark. The limb of the quadrant was graduated into minutes and seconds by transversals. For measuring star altitudes involving relatively acute angles, Picard used a tall zenith sector made of copper and iron with an amplitude of about 18°. Attached to one radius of the sector was a telescope ten feet long. Also part of his equipment were two pendulum clocks, one regulated to beat seconds, the other half-seconds. For general observations and for observing the satellites of Jupiter, he carried three telescopes: a small one about five feet long and two larger ones, fourteen and eighteen feet long. Picard was well satisfied with his equipment. In describing his specially fitted quadrant, he said it did the work so accurately that during the two years it took to measure the arc of the meridian, there was never an error of more than a minute of arc in any of the angles measured on the entire circumference of the horizon, and that in many cases, on checking the instrument for accuracy, it was found to be absolutely true. And as for the

[21] For a complete account of Picard's measurement of the earth, including tables of data and a historical summary, see the *Mémoires de l'Académie Royale des Sciences*, Vol. VII, Pt. I, Paris, 1729. See also the article "Earth, Figure of the" by Alexander Ross Clarke and Frederick Robert Helmert in the *Encyclopaedia Britannica*, 11th edition, p. 801.

pendulum clocks he carried, Picard was pleased to report that they "marked the seconds with greater accuracy than most clocks mark the half hours." [22]

When the results of Picard's survey were tabulated, the distance between his two terminal points was found to be 68,430 toises 3 pieds. The difference in latitude between them was measured, not by taking the altitude of the sun at the two terminal points, but by measuring the angle between the zenith and a star in the kneecap of Cassiopeia, first at Malvoisine and then at Sourdon. The difference was 1° 11′ 57″. From these figures the value of a degree of longitude was calculated as 57,064 toises 3 pieds. But on checking from a second base line of verification which was surveyed in the same general direction as the first, this value was revised to 57,060 toises, and the diameter of the earth was announced as 6,538,594 toises. All measurements of longitude made by the Académie Royale were based on this value, equivalent to about 7801 miles, a remarkably close result.[23]

In 1676, after the astronomers had revised and enlarged his *Ephemerides* of 1668, Cassini suggested that the corrected data might now be used for the determination of longitude, and Jupiter might be given a trial as a celestial clock. The idea was approved by his colleagues and experimental observations were begun, based on a technique developed at the Observatory and on experience acquired by a recent expedition to Cayenne for the observation of the planet Mars. The scientists were unusually optimistic as the work began, and in a rare burst of enthusiasm one of them wrote, *"Si ce n'est pas -là le véritable secret des Longitudes, au-moins en approche-t-il de bien près."*

Because of his tremendous energy, skill and patience, Cassini had by this time assumed the leadership of the scientists working at the observatory, even though he did not have the title of Director. He carried on an extensive correspondence with astronomers in other countries, particularly in Italy where the best instruments were available and where he and his work were well known. Astronomers in foreign parts responded with enthusiasm when they learned of the work that was being done at the Paris Observatory. New data began to pour in faster than the resident astronomers could appraise and tabulate it. Using telescopes and the satellites of Jupiter, hundreds of cities and towns were now being located for the first time with reference to a prime meridian and to each other. All of the standard maps of Europe, it seemed, would have to be scrapped.[24]

[22] *Mémoires de l'Académie Royale des Sciences,* Vol. VII, Pt. I. Pagination varies widely in different editions of this series.

[23] *Ibid.*, pp. 306–07, gives a table of the linear measures used by Picard in making his computations. Measurements were made between the zenith and a star in the kneecap of Cassiopeia, probably δ (Al Rukbah). See the *Mémoires de l'Académie Royale des Sciences,* Vol. VII, Pt. II, Paris, 1730, p. 305.

[24] For Cassini's status in the Académie Royale, see C. J. E. Wolf, *op. cit.*

With so much new information available, Cassini conceived the idea of compiling a large-scale map of the world (planisphere) on which revised geographical information could be laid down as it came in from various parts of the world, especially the longitudes of different places, hitherto unknown or hopelessly incorrect. For this purpose the third floor of the west tower of the Observatory was selected. There was plenty of space, and the octagonal walls of the room had been oriented by compass and quadrant when the foundation of the building was laid. The planisphere, on an azimuthal projection with the North Pole at the center, was executed in ink by Sédileau and de Chazelles on the floor of the tower under the watchful eye of Cassini. The circular map was twenty-four feet in diameter, with meridians radiating from the center to the periphery, like the spokes of a wheel, at intervals of 10°. The prime meridian of longitude (through the island of Ferro) was drawn from the center at any angle "half way between the two south windows of the tower" to the point where it bisected the circumference of the map. The map was graduated into degrees from zero to 360 in a counterclockwise direction around the circle. The parallels of latitude were laid down in concentric circles at intervals of 10°, starting with zero at the equator and numbering both ways. For convenient and rapid "spotting" of places, a cord was attached to a pin fastened to the center of the map with a small rider on it, so that by swinging the cord around to the proper longitude and the rider up or down to the proper latitude, a place could be spotted very quickly.

On this great planisphere the land masses were of course badly distorted, but it did not matter. What interested the Académie was the precise location, according to latitude and longitude, of the important places on the earth's surface, places that could be utilized in the future for bases of surveying operations. For this reason it was much more important to have the names of a few places strategically located and widely distributed, according to longitude, than it was to include a great many places that were scientifically unimportant. For the same reason, most of the cities and towns that boasted an astronomical observatory, regardless of how small, were spotted on the map.

The planisphere was highly praised by all who saw it. The king came to see it with Colbert and all the court. His Majesty graciously allowed Cassini, Picard and de la Hire to demonstrate the various astronomical instruments used by the members of the Académie to study the heavens and to determine longitude by remote control, as it were. They showed him their great planisphere and explained how the locations of different places were being corrected on the basis of data sent in from the outside world. It was enough to make even Louis pause.[25]

[25] *Ibid.*, pp. 62–65. For an account of the great planisphere, see the *Histoire de l'Académie Royale des Sciences*, Vol. I, pp. 225–26; C. J. E. Wolf, *op. cit.* A facsimile

What effect the king's visit had on future events it is difficult to estimate, but in the next few years a great deal of surveying was done. Many surveying expeditions were sent out from the Observatory, and the astronomers went progressively further afield. Jean Richer led an expedition to Cayenne and Jean Mathieu de Chazelles went to Egypt. Jesuit missionaries observed at Madagascar and in Siam. Edmund Halley, who was in close touch with the work going on in France, made a series of observations at the Cape of Good Hope. Thevenot, the historian and explorer, communicated data on several lunar eclipses observed at Goa. About this time, Louis-Abel Fontenay, a Jesuit professor of mathematics at the College of Louis le Grand, was preparing to leave for China. Hearing of the work being done by Cassini and his colleagues, Fontenay volunteered to make as many observations as he could without interfering with his missionary duties. Cassini trained him and sent him on his way prepared to contribute data on the longitudes to the Orient. Thus the importance of remapping the world and the feasibility of the method devised by the Académie Royale began to dawn on the scholars of Europe, and many foreigners volunteered to contribute data. Meanwhile Colbert raised more money and Cassini sent more men into the field.

One of the longest and most difficult expeditions organized by the Académie Royale was led by Messrs. Varin and des Hayes, two of His Majesty's engineers for hydrography, to the island of Gorée and the West Indies. It was also one of the most important for the determination of longitudes in the Western Hemisphere, involving as it did the long jump across the Atlantic Ocean, a span where some of the most egregious errors in longitude had been made. Cassini's original plan, approved by the king, was to launch the expedition from Ferro, on the extreme southwest of the Canary Islands, an island frequently used by cartographers as a prime meridian of longitude. But as there was some difficulty about procuring passage for the expedition, it was decided to take a departure from Gorée, a small island off Cape Verde on the west coast of Africa, where a French colony had recently been established by the Royal Company of Africa.[26]

Before their departure, Varin and des Hayes spent considerable time at the Observatory, where they were thoroughly trained by Cassini and where they could make trial observations to perfect their technique. They received their final instructions in the latter part of 1681, and set out for

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of one of the printed versions of the map, reduced, was issued with Christian Sandler's *Die Reformation der Kartographie um 1700*, Munich, 1905; a second one, colored, was published in 1941 by the University of Michigan from the original in the William L. Clements Library. For bibliographical notes regarding the publication of the map, see L. A. Brown, *Jean Domenique Cassini* . . . pp. 62–73.

[26] The island of Ferro, the most southwest of the Canary Islands, was a common prime meridian among cartographers as late as 1880. It was considered the dividing line between the Eastern and Western hemispheres! (See Lippincott's *A complete pronouncing gazetteer or geographical dictionary of the world*. Philadelphia, 1883.)

Rouen equipped with a two and a half foot quadrant, a pendulum clock, and a nineteen foot telescope. Among the smaller pieces of apparatus they carried were a thermometer, a barometer and a compass. From Rouen they moved to Dieppe, where they were held up more than a month by storm weather and contrary winds. With time on their hands, they made a series of observations to determine the latitude and longitude of the city. The two men finally arrived at Gorée in March, 1682, and there they were joined by M. de Glos, a young man trained and recommended by Cassini. De Glos brought along a six foot sextant, an eighteen foot telescope, a small zenith sector, an astronomical ring and another pendulum clock. Although the primary object of the expedition was to determine longitudes by observing the eclipses of the satellites of Jupiter, the three men had orders to observe the variation of the compass at every point in their travels, especially during the ocean voyage, and to make thermometrical and barometric observations whenever possible; in short, they were to gather all possible scientific data that came their way. From Gorée the expedition sailed for Guadeloupe and Martinique, and for the next year extensive observations were made. The three men returned to Paris in March, 1683.[27]

Cassini's instructions to the party were given in writing. They furnish a clear picture of the best seventeenth century research methods and at the same time explain just how terrestrial longitudes were determined by timing the eclipses of the satellites of Jupiter. The object was simple enough: to find the difference in *mean* or *local* time between a prime meridian such as Ferro or Paris and a second place such as Guadeloupe, the difference in time being equivalent to the difference in longitude. Two pendulum clocks were carried on the expedition, and before leaving they were carefully regulated at the Observatory. The pendulum of one was adjusted so that it would keep *mean* time, that is twenty-four hours a day. The second clock was set to keep *sidereal* or *star* time ($23^h\ 56^m\ 4^s$).[28] The rate of going for the two timekeepers was carefully tabulated over a long period so that the observers might know in advance what to expect when the temperature, let us say, went up or down ten degrees in a twenty-four hour period. These adjustments were made by raising or lowering the pendulum bob to speed up or slow down the clock. After the necessary adjustments were made, the position of the pendulum bobs on the rods was marked and the clocks were taken apart for shipping.

Having arrived at the place where observations were to be made, the

[27] L. A. Brown: *Jean Domenique Cassini* . . . pp. 42–44.

[28] Cassini suggested two methods of adjusting a clock to *mean* time. The first was to make a series of observations of the sun (equal altitudes) and afterwards correct them with tables of the equation of time, and second, to regulate one clock to keep sidereal time by observing two successive transits of a star and correct the second clock from it.

astronomers selected a convenient, unobstructed space and set up their instruments. They fixed their pendulum bobs in position and started their clocks, setting them at the approximate hour of the day. The next operation was to establish a meridian line, running true north and south, at the place of observation. This was done in several ways, each method being used as a check against the accuracy of the others. The first was to take a series of equal altitude observations of the sun, a process which would also give a check on the accuracy of the clock which was to keep mean time. To do this, the altitude of the sun was taken with a quadrant or sextant approximately three (or four) hours before apparent noon. At the moment the sight was taken the hour, minute and second were recorded in the log. An afternoon sight was taken when the sun had descended to precisely the same angle recorded in the morning observation. Again the time was taken at the instant of observation. The difference in time between the two observations divided by two and added to the morning time gave the hour, minute and second indicated at apparent noon. This observation was repeated two days in succession, and the difference in minutes and seconds recorded by the clock on the two days (always different because of the declination of the sun) divided by two and added to the first gave the observers what the clock did in twenty-four hours; in other words, it gave them mean time. A very simple check on the arrival of apparent noon, when the sun reaches the meridian, was to drop a plumb line from the fixed quadrant and note the shadow on the ground as each observation was taken. These observations were repeated daily so that the observers always knew their local time.

The second pendulum clock was much simpler to adjust. All they had to do was set up a telescope in the plane of the meridian, sight it on a fixed star and time two successive transits of the star. When the pendulum was finally adjusted so that $23^{h}\ 56^{m}\ 4^{s}$ elapsed between two successive transits, the job was done. The latitude of the place of observation was equally simple to determine. The altitude of the sun at apparent noon was taken with a quadrant and the angle, referred to the tables of declination, gave the observers their latitude. A check on the latitude was made at night, by observing the height of the polestar.

With the meridian line established, and a clock regulated to keep mean time, the next thing was to observe and time the eclipses of the satellites of Jupiter, at least two of which are eclipsed every two days. As Cassini pointed out, this was not always a simple matter, because not all eclipses are visible from the same place and because bad weather often vitiates the observations. Observations called for a very fussy technique.

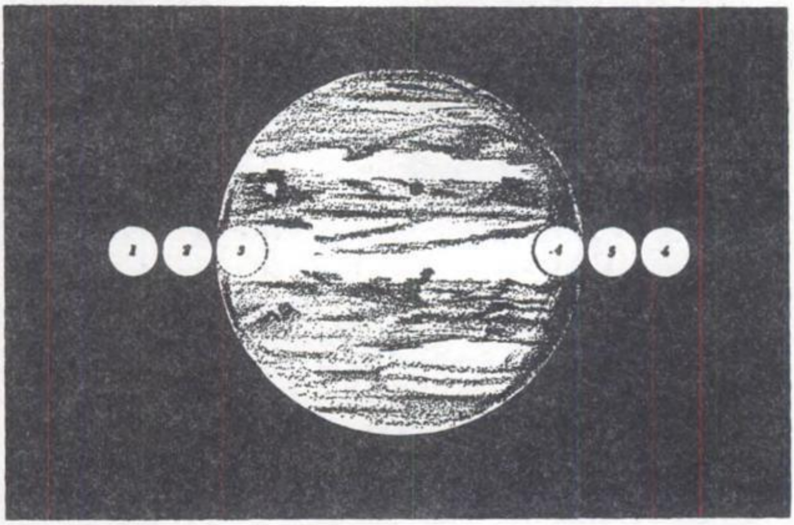
The most satisfactory time observations of Jupiter, in Cassini's opinion, could be made of the immersions and emersions of the first satellite. Six phases of the eclipse should be timed: during the immersion of the satellite

(1) when the satellite is at a distance from the limb of Jupiter equal to its own diameter; (2) when the satellite just touches Jupiter; (3) when it first becomes entirely hidden by Jupiter's disc. During the emersion of the satellite (4) the instant the satellite begans to reappear; (5) when it becomes detached from Jupiter's disc; (6) when the satellite has moved away from Jupiter a distance equal to its own diameter. To observe and time these phases was a two-man job: one to observe and one to keep a record of the time in minutes and seconds. If an observer had to work alone, Cassini recommended the "eye and ear" method of timing observations, which is still good observational practice. The observer begins to count out loud "one-five-hundred, two-five-hundred, three-five-hundred" and so on, the instant the eclipse begins, and he continues to count until he can get to his clock and note the time. Then by subtracting his count from the clock reading, he has the time at which the observation was made.

The emersion of the satellite, Cassini warned, always requires very careful observation, because you see nothing while you are waiting for it. At the instant you see a faint light in the region where the satellite should reappear, you should begin counting without leaving the telescope until you are sure you are seeing the actual emersion. You may make several false starts before you actually see and can time the actual emersion. Other observations worth using, according to Cassini, were the conjunctions of two satellites going in opposite directions. A conjunction was said to occur when the centers of the two satellites were in a straight perpendicular line. In all important observations requiring great accuracy, Cassini recommended a dress rehearsal the day before and at the same hour, so that if the instruments did not behave or the star was found to be in a difficult position, all necessary adjustments would have been made in advance.[29]

In addition to the observations for the determination of longitude, all expeditions sent out from the Paris Observatory were cautioned to note any variation in the functioning of their pendulum clocks. This did not mean normal variations caused by changes in temperature. Such variations could be predicted in advance by testing the metal pendulum rods: determining the coefficient of expansion at various temperatures. What they were watching for was a change caused by a variation in gravity. There were two reasons involved, one practical and one theoretical. The pendulum was an extremely important engine, since it was the driving force of the best clocks then in use. And too, the whole subject of gravitation, whose leading exponents were Christian Huygens, Isaac Newton and

[29] Cassini's "Instructions" were printed in full in the *Mémoires de l'Académie Royale des Sciences*, Vol. VIII, Paris, 1730. For a translation into English see L. A. Brown, *Jean Domenique Cassini. . .* pp. 48–60.



The planet Jupiter showing the six positions of the first satellite used by seventeenth-century astronomers to determine the difference in longitude between two places.

Robert Hooke, was causing a stir in the scientific world. The idea of using the pendulum experimentally for studying gravitation came from Hooke, and the theories of Newton and Huygens might well be proved or disproved by a series of experiments in the field. What no one knew was that these field trials would result in the discovery that the earth is not a perfect sphere but an oblate spheroid, a sphere flattened at the poles.

What effect, if any, did a change of latitude produce in the oscillations of a pendulum if the temperature remained unchanged? Many scientists said none, and experiments seemed to prove it. Members of the Académie had transported timekeepers to Copenhagen and The Hague to try them at different latitudes, and a series of experiments had been conducted in London. The results were all negative; at every place a pendulum of a given length (39.1 inches) beat seconds or made 3600 oscillations an hour. However, there was one exception. In 1672, Jean Richer had made an expedition to Cayenne (4° 56′ 5″ N.) to observe the opposition of Mars. On the whole the expedition was a success, but Richer had had trouble with his timekeeper. Although the length of the pendulum had been carefully adjusted at the Observatory before he sailed, Richer found that in Cayenne his clock lost about two minutes and a half a day, and that in order to get it to keep mean time he had to shorten the pendulum (raise the bob) by more than a "ligne" (about 1/12 of an inch). All this was very trying to Cassini, who was a meticulous observer. "It is suspected," he wrote, "that this resulted from some error in the observation."

Had he not been a gentleman as well as a scholar, he would have said that Richer was just plain careless.[30]

The following year, 1673, Huygens published his masterpiece on the oscillation of the pendulum, in which he set down for the first time a sound theory on the subject of centrifugal force, principles which Newton later applied to his theoretical investigation of the earth.[31] The first opportunity to confirm the fallacy of Richer's observations on the behavior of his timekeeper came when Varin and des Hayes sailed for Martinique (14° 48′ N.) and Guadeloupe (between 15° 47′ and 16° 30′ N.). Cassini cautioned them to check their pendulums with the greatest possible care and they did. But, unfortunately, their clocks behaved badly, and they, too, had to shorten the pendulums in order to make them beat mean time. Cassini was still dubious, but not Isaac Newton. In the third book of his *Principia* he concluded that this variation of the pendulum in the vicinity of the equator must be caused either by a diminution of gravity resulting from a bulging of the earth at the equator, or from the strong, counteracting effect of centrifugal force in that region.[32]

The discoveries made by the Académie Royale des Sciences set a fast pace in the scientific world and pointed the way towards many others. The method of finding longitude by means of the eclipses of Jupiter's satellites had proved to be feasible and accurate, but it was not accepted by foreign countries without a struggle. Tables of Jupiter's satellites were finally included in the English *Nautical Almanac* and remained there in good standing for many years, along with tables of lunar distances and other star data associated with rival methods of finding longitude. It was generally conceded, however, that Jupiter could not be used for finding longitude at sea, in spite of Galileo's assertions to the contrary. Many inventors besides the great Italian had come up with ingenious and wholly impractical devices to provide a steady platform on shipboard from which astronomical observations could be made. But the fact remained that the sea was too boisterous and unpredictable for astronomers and their apparatus.

England made her official entry in the race for the longitude when Charles II ordered the construction of a Royal Observatory, for the advancement of navigation and nautical astronomy, in Greenwich Park, overlooking the Thames and the plain of Essex.[33] In England things moved slowly at first, but they moved. The king was determined to have the tables of the heavenly bodies corrected for the use of his seamen and

[30] L. A. Brown: *Jean Domenique Cassini* . . . p. 57.

[31] Christian Huygens: *Horologivm oscillatorivm; sive de motu pendulorvm ad horologia aptato demonstrationes geometricae*, Paris, 1673.

[32] Isaac Newton: *Philosophiae Naturalis Principia Mathematica*, 3 vols., London, 1687.

[33] R. T. Gould, *op. cit.*, p. 9; also Henry S. Richardson's *Greenwich: its history, antiquities, improvements and public buildings*, London, 1834.

so appointed John Flamsteed "astronomical observator" by a royal warrant dated March 4, 1675, at the handsome salary of £100 a year, out of which he paid £10 in taxes. He had to provide his own instruments, and as an additional check to any delusions of grandeur he might have, he was ordered to give instruction to two boys from Christ's Hospital. Stark necessity made him take several private pupils as well. Dogged by ill health and the irritations common to the life of a public servant, Flamsteed was nevertheless buoyed up by the society of Newton, Halley, Hooke and the scientists of the Académie Royale, with whom he corresponded. A perfectionist of the first magnitude, Flamsteed was doomed to a life of unhappiness by his unwillingness to publish his findings before he had had a chance to check them for accuracy. To Flamsteed, no demand was sufficiently urgent to justify such scientific transgression.

Flamsteed worked under constant pressure. Everybody, it seemed, wanted data of one sort of another, and wanted it in a hurry. Newton needed full information on "places of the moon" in order to perfect his lunar theory. British scientists, as a group, had set aside the French method of finding longitude and all other methods requiring the use of sustained observations at sea. They were approaching the problem from another angle, and demanded complete tables of lunar distances and a complete catalogue of star places. Flamsteed did as he was told, and for fifteen years (1689–1704) spent most of his time at the pedestrian task of compiling the first Greenwich star catalogue and tables of the moon, meanwhile reluctantly doling out to his impatient peers small doses of what he considered incomplete if not inaccurate data.[34]

The loudest clamors for information came from the Admiralty and from the waterfront. In 1689 war broke out with France. In 1690 (June 30) the English fleet was defeated by the French at the battle of Beachy Head. Lord Torrington, the English admiral, was tried by court-martial and acquitted, but nevertheless dismissed from the service. In 1691 several ships of war were lost off Plymouth because the navigators mistook the Deadman for Berry Head. In 1707 Sir Cloudesley Shovel, returning with his fleet from Gibraltar, ran into dirty weather. After twelve days of groping in a heavy overcast, all hands were in doubt as to the fleet's position. The Admiral called for the opinion of his navigators, and with one exception they agreed that the fleet was well to the west of Ushant, off the Brittany peninsula. The fleet stood on, but that night, in a heavy fog, they ran into the Scilly Islands off the southwest coast of England. Four ships and two thousand men were lost, including the Admiral. There was a story current, long after, that a seaman on the flagship had estimated from his own dead reckoning that the fleet was in a dangerous position. He had the temerity to point this out to his superiors, who

[34] See Francis Baily's *Account of the Rev. John Flamsteed*, London, 1835.

sentenced him forthwith to be hanged at the yardarm for mutiny. The longitude had to be found! [35]

There was never a shortage of inventive genius in England, and many fertile minds were directed towards the problem of finding longitude at sea. In 1687 two proposals were made by an unknown inventor which were novel, to say the least. He had discovered that a glass filled to the brim with water would run over at the instant of new and full moon, so that the longitude could be determined with precision at least twice a month. His second method was far superior to the first, he thought, and involved the use of a popular nostrum concocted by Sir Kenelm Digby called the "powder of sympathy." This miraculous healer cured open wounds of all kinds, but unlike ordinary and inferior brands of medicine, the powder of sympathy was applied, not to the wound but to the weapon that inflicted it. Digby used to describe how he made one of his patients jump sympathetically merely by putting a dressing he had taken from the patient's wound into a basin containing some of his curative powder. The inventor who suggested using Digby's powder as an aid to navigation proposed that before sailing every ship should be furnished with a wounded dog. A reliable observer on shore, equipped with a standard clock and a bandage from the dog's wound, would do the rest. Every hour, on the dot, he would immerse the dog's bandage in a solution of the powder of sympathy and the dog on shipboard would yelp the hour.[36]

Another serious proposal was made in 1714 by William Whiston, a clergyman, and Humphrey Ditton, a mathematician. These men suggested that a number of lightships be anchored in the principal shipping lanes at regular intervals across the Atlantic ocean. The lightships would fire at regular intervals a star shell timed to explode at 6440 feet. Sea captains could easily calculate their distance from the nearest lightship merely by timing the interval between the flash and the report. This system would be especially convenient in the North Atlantic, they pointed out, where the depth never exceeded 300 fathoms! For obvious reasons, the proposal of Whiston and Ditton was not carried out, but they started something. Their plan was published, and thanks to the publicity it received in various periodicals, a petition was submitted to Parliament on March 25, 1714, by "several Captains of Her Majesty's Ships, Merchants of London, and Commanders of Merchantmen," setting forth the great importance of finding the longitude and praying that a public reward be offered for some practicable method of doing it.[37] Not only the petition but the

[35] R. T. Gould, *op. cit.*, p. 2.

[36] See *Curious Enquiries*, London, 1687; R. T. Gould, *op. cit.*, p. 11. The title of Sir Kenelm Digby's famous work, which appeared in French as well as English, was *A late discourse . . . touching the cure of wounds by the powder of sympathy; with instructions how to make the said powder* . . . Second edition, augmented, London, 1658.

[37] See Whiston and Ditton's *A new method for discovering the longitude*. London,

proposal of Whiston and Ditton were referred to a committee, who in turn consulted a number of eminent scientists including Newton and Halley.

That same year Newton prepared a statement which he read to the committee. He said, "That, for determining the Longitude at Sea, there have been several Projects, true in the Theory, but difficult to execute." Newton did not favor the use of the eclipses of the satellites of Jupiter, and as for the scheme proposed by Whiston and Ditton, he pointed out that it was rather a method of "keeping an Account of the Longitude at Sea, than for finding it, if at any time it should be lost." Among the methods that are difficult to execute, he went on. "One is, by a Watch to keep time exactly: But, by reason of the Motion of a Ship, the Variation of Heat and Cold, Wet and Dry, and the Difference of Gravity in Different Latitudes, such a Watch hath not yet been made." That was the trouble: such a watch had not been made.[38]

The idea of transporting a timekeeper for the purpose of finding longitude was not new, and the futility of the scheme was just as old. To the ancients it was just a dream. When Gemma Frisius suggested it in 1530 there were mechanical clocks, but they were a fairly new invention, and crudely built, which made the idea improbable if not impossible.[39] The idea of transporting "some true Horologie or Watch, apt to be carried in journeying, which by an Astrolabe is to be rectified . . ." was again stated by Blundeville in 1622, but still there was no watch which was "true" in the sense of being accurate enough to use for determining longitude.[40] If a timekeeper was the answer, it would have to be very accurate indeed. According to Picard's value, a degree of longitude was equal to about sixty-eight miles at the equator, or four minutes, by the clock. One minute of time meant seventeen miles—towards or away from danger. And if on a six weeks' voyage a navigator wanted to get his longitude within half a degree (thirty-four miles) the rate of his timekeeper must not gain or lose more than two minutes in forty-two days, or *three seconds a day.*

Fortified by these calculations, which spelled the impossible, and the report of the committee, Parliament passed a bill (1714) "for providing a publick reward for such person or persons as shall discover the Longitude." It was the largest reward ever offered, and stated that for any practical invention the following sum would be paid: [41]

£10,000 for any device that would determine the longitude within 1 degree.

1714. The petition appeared in various periodicals: *The Guardian*, July 14; *The Englishman*, Dec. 19, 1713 (R. T. Gould, *op. cit.*, p. 13).

[38] R. T. Gould, *op. cit.*, p. 13.

[39] Gemma Frisius: *De principiis astronomiae et cosmographiae*, Antwerp, 1530.

[40] Thomas Blundeville, *M. Blundeville his exercises . . .*, Sixth Edition, London, 1622, p. 390.

[41] 12 Anne, Cap. 15; R. T. Gould, *op. cit.*, p. 13.

£15,000 for any device that would determine the longitude within 40 minutes.

£20,000 for any device that would determine the longitude within 30 minutes (2 minutes of time or 34 miles).

As though aware of the absurdity of their terms, Parliament authorized the formation of a permanent commission—the Board of Longitude—and empowered it to pay one half of any of the above rewards as soon as a majority of its members were satisfied that any proposed method was practicable and useful, and that it would give security to ships within eighty miles of danger, meaning land. The other half of any reward would be paid as soon as a ship using the device should sail from Britain to a port in the West Indies without erring in her longitude more than the amounts specified. Moreover, the Board was authorized to grant a smaller reward for a less accurate method, provided it was practicable, and to spend a sum not to exceed £2000 on experiments which might lead to a useful invention.

For fifty years this handsome reward stood untouched, a prize for the impossible, the butt of English humorists and satirists. Magazines and newspapers used it as a stock cliché. The Board of Longitude failed to see the joke. Day in and day out they were hounded by fools and charlatans, the perpetual motion lads and the geniuses who could quarter a circle and trisect an angle. To handle the flood of crackpots, they employed a secretary who handed out stereotyped replies to stereotyped proposals. The members of the Board met three times a year at the Admirality, contributing their services and their time to the Crown. They took their responsibilities seriously and frequently called in consultants to help them appraise a promising invention. They were generous with grants-in-aid to struggling inventors with sound ideas, but what they demanded was results.[42] Neither the Board nor any one else knew exactly what they were looking for, but what everyone knew was that the longitude problem had stopped the best minds in Europe, including Newton, Halley, Huygens, von Leibnitz and all the rest. It was solved, finally, by a ticking machine in a box, the invention of an uneducated Yorkshire carpenter named John Harrison. The device was the marine chronometer.

Early clocks fell into two general classes: nonportable timekeepers driven by a falling weight, and portable timekeepers such as table clocks and crude watches, driven by a coiled spring. Gemma Frisius suggested the latter for use at sea, but with reservations. Knowing the unreliable temperament of spring-driven timekeepers, he admitted that sand and water clocks would have to be carried along to check the error of a

[42] R. T. Gould, *op. cit.*, p. 16. According to the Act of 1712 the Board was comprised of: "The Lord High Admiral or the First Lord of the Admiralty; The Speaker of the House of Commons; The First Commissioner of the Navy; The First Commissioner of Trade; The Admirals of the Red, White and Blue Squadrons; The Master of the Trinity House; The President of the Royal Society; The Astronomer-Royal; The Savilian, Lucasian, and Plumian Professors of Mathematics."

spring-driven machine. In Spain, during the reign of Philip II, clocks were solicited which would run exactly twenty-four hours a day, and many different kinds had been invented. According to Alonso de Santa Cruz there were "some with wheels, chains and weights of steel: some with chains of catgut and steel: others using sand, as in sandglasses: others with water in place of sand, and designed after many different fashions: others again with vases or large glasses filled with quicksilver: and, lastly, some, the most ingenious of all, driven by the force of the wind, which moves a weight and thereby the chain of the clock, or which are moved by the flame of a wick saturated with oil: and all of them adjusted to measure twenty-four hours exactly." [43]

Robert Hooke became interested in the development of portable timekeepers for use at sea about the time Huygens perfected the pendulum clock. One of the most versatile scientists and inventors of all time, Hooke was one of those rare mechanical geniuses who was equally clever with a pen. After studying the faults of current timekeepers and the possibility of building a more accurate one, he slyly wrote a summary of his investigations, intimating that he was completely baffled and discouraged. "All I could obtain," he said, "was a Catalogue of Difficulties, *first* in the doing of it, *secondly* in the bringing of it into publick use, *thirdly*, in making advantage of it. Difficulties were proposed from the alteration of *Climates, Airs, heats* and *colds*, temperature of *Springs*, the nature of *Vibrations*, the wearing of Materials, the motion of the Ship, and divers others." Even if a reliable timekeeper were possible, he concluded, "it would be difficult to bring it to use, for Sea-men know their way already to any Port. . . ." As for the rewards: "the Praemium for the Longitude," there never was any such thing, he retorted scornfully. "No King or State would pay a farthing for it."

In spite of his pretended despondency, Hooke nevertheless lectured in 1664 on the subject of applying springs to the balance of a watch in order to render its vibrations more uniform, and demonstrated, with models, twenty different ways of doing it. At the same time he confessed that he had one or two other methods up his sleeve which he hoped to cash in on at some future date. Like many scientists of the time, Hooke expressed the principle of his balance spring in a Latin anagram; roughly: *Ut tensio, sic vis,* "as the tension is, so is the force," or, "the force exerted by a spring is directly proportional to the extent to which it is tensioned." [44]

The first timekeeper designed specifically for use at sea was made by Christian Huygens in 1660. The escapement was controlled by a pendu-

[43] *Ibid.*, p. 20; this translation is from a paraphrase by Duro in his *Disquisiciones Nauticas.*

[44] *Ibid.*, p. 25. The anagram was a device commonly used in the best scientific circles of the time to establish priority of invention or discovery without actually disclosing anything that might be seized upon by a zealous colleague.

lum instead of a spring balance, and like many of the clocks that followed, it proved useless except in a flat calm. Its rate was unpredictable; when tossed around by the sea it either ran in jerks or stopped altogether. The length of the pendulum varied with changes of temperature, and the rate of going changed in different latitudes, for some mysterious reason not yet determined. But by 1715 every physical principal and mechanical part that would have to be incorporated in an accurate timekeeper was understood by watchmakers. All that remained was to bridge the gap between a good clock and one that was nearly perfect. It was that half degree of longitude, that two minutes of time, which meant the difference between conquest and failure, the difference between £20,000 and just another timekeeper.[45]

One of the biggest hurdles between watchmakers and the prize money was the weather: temperature and humidity. A few men included barometric pressure. Without a doubt, changes in the weather did things to clocks and watches, and many suggestions were forthcoming as to how this principal source of trouble could be overcome. Stephen Plank and William Palmer, watchmakers, proposed keeping a timekeeper close to a fire, thus obviating errors due to change in temperature. Plank suggested keeping a watch in a brass box over a stove which would always be hot. He claimed to have a secret process for keeping the temperature of the fire uniform. Jeremy Thacker, inventor and watchmaker, published a book on the subject of the longitude, in which he made some caustic remarks about the efforts of his contemporaries.[46] He suggested that one of his colleagues, who wanted to test his clock at sea, should first arrange to have two consecutive Junes equally hot at every hour of every day. Another colleague, referred to as Mr. Br . . . e, was dubbed the Corrector of the Moon's Motion. In a more serious vein, Thacker made several sage observations regarding the physical laws with which watchmakers were struggling. He verified experimentally that a coiled spring loses strength when heated and gains it when cooled. He kept his own clock under a kind of bell jar connected with an exhaust pump, so that it could be run in a partial vacuum. He also devised an auxiliary spring which kept the clock going while the mainspring was being wound. Both springs were wound outside the bell by means of rods passed through stuffing boxes, so that neither the vacuum nor the clock mechanism

[45] *Ibid.*, pp. 27–30; Huygens described his pendulum clock in his *Horologium Oscillatorium*, Paris, 1673.

[46] Ibid., pp. 32, 33; Jeremy Thacker wrote a clever piece entitled: *The Longitudes Examined, beginning with a short epistle to the Longitudinarians and ending with the description of a smart, pretty Machine of my Own which I am (almost) sure will do for the Longitude and procure me The Twenty Thousand Pounds.* By Jeremy Thacker, of Beverly in Yorkshire. ". . . *quid non mortalia pectora cogis Auri sacra Fames* . . ." London. Printed for J. Roberts at the Oxford Arms in Warwick Lane, 1714. Price Sixpence.

would have to be disturbed. In spite of these and other devices, watchmakers remained in the dark and their problems remained unsolved until John Harrison went to work on the physical laws behind them. After that they did not seem so difficult.[47]

Harrison was born at Foulby in the parish of Wragby, Yorkshire, in May, 1693. He was the son of a carpenter and joiner in the service of Sir Rowland Winn of Nostell Priory. John was the oldest son in a large family. When he was six years old he contracted smallpox, and while convalescing spent hours watching the mechanism and listening to the ticking of a watch laid on his pillow. When his family moved to Barrow in Lincolnshire, John was seven years old. There he learned his father's trade and worked with him for several years. Occasionally he earned a little extra by surveying and measuring land, but he was much more interested in mechanics, and spent his evenings studying Nicholas Saunderson's published lectures on mathematics and physics. These he copied out in longhand including all the diagrams. He also studied the mechanism of clocks and watches, how to repair them and how they might be improved. In 1715, when he was twenty-two, he built his first grandfather clock or "regulator." The only remarkable feature of the machine was that all the wheels except the escape wheel were made of oak, with the teeth, carved separately, set into a groove in the rim.[48]

Many of the mechanical faults in the clocks and watches that Harrison saw around him were caused by the expansion and contraction of the metals used in their construction. Pendulums, for example, were usually made of an iron or steel rod with a lead bob fastened at the end. In winter the rod contracted and the clock went fast, and in summer the rod expanded, making the clock lose time. Harrison made his first important contribution to clockmaking by developing the "gridiron" pendulum, so named because of its appearance. Brass and steel, he knew, expand for a given increase in temperature in the ratio of about three to two (100 to

[47] The invention of a "maintaining power" is erroneously attributed to Harrison. See R. T. Gould, *op. cit.*, p. 34, who says that Thacker antedates Harrison by twenty years on the invention of an auxiliary spring to keep a machine going while it was being wound. In spite of the magnitude of John Harrison's achievement, the inventive genius of Pierre Le Roy of Paris produced the prototype of the modern chronometer. As Rupert Gould points out, Harrison's Number Four was a remarkable piece of mechanism, "a satisfactory marine timekeeper, one, too, which was of permanent usefullness, and which could be duplicated as often as necessary. But No. 4, in spite of its fine performance and beautiful mechanism, cannot be compared, for efficiency and design, with Le Roy's wonderful machine. The Frenchman, who was but little indebted to his precessors, and not at all to his contemporaries, evolved, by sheer force of genius, a timekeeper which contains all the essential mechanism of the modern chronometer." (See R. T. Gould, *op. cit.*, p. 65.)

[48] For a biographical sketch of Harrison see R. T. Gould, *op. cit.*, pp. 40 ff. Saunderson (1682–1739) was Lucasian professor of mathematics at Cambridge. Harrison's first "regulator" is now in the museum of the Clockmakers' Co. of London. "The term 'regulator' is used to denote any high-class pendulum clock designed for use solely as an accurate time-measurer, without any additions such as striking mechanism, calendar work, &c." (Gould, p. 42 *n.*)

62). He therefore built a pendulum with nine alternating steel and brass rods, so pinned together that expansion or contraction caused by variation in the temperature was eliminated, the unlike rods counteracting each other.[49]

The accuracy of a clock is no greater than the efficiency of its escapement, the piece which releases for a second, more or less, the driving power, such as a suspended weight or a coiled mainspring. One day Harrison was called out to repair a steeple clock that refused to run. After looking it over he discovered that all it needed was some oil on the pallets of the escapement. He oiled the mechanism and soon after went to work on a design for an escapement that would not need oiling. The result was an ingenious "grasshopper" escapement that was very nearly frictionless and also noiseless. However, it was extremely delicate, unnecessarily so, and was easily upset by dust or unnecessary oil. These two improved parts alone were almost enough to revolutionize the clockmaking industry. One of the first two grandfather clocks he built that were equipped with his improved pendulum and grasshopper escapement did not gain or lose more than a second a month during a period of fourteen years.

Harrison was twenty-one years old when Parliament posted the £20,000 reward for a reliable method of determining longitude at sea. He had not finished his first clock, and it is doubtful whether he seriously aspired to winning such a fortune, but certainly no young inventor ever had such a fabulous goal to shoot at, or such limited competition. Yet Harrison never hurried his work, even after it must have been apparent to him that the prize was almost within his reach. On the contrary, his real goal was the perfection of his marine timekeeper as a precision instrument and a thing of beauty. The monetary reward, therefore, was a foregone conclusion.

His first two fine grandfather clocks were completed by 1726, when he was thirty-three years old, and in 1728 he went to London, carrying with him full-scale models of his gridiron pendulum and grasshopper escapement, and working drawings of a marine clock he hoped to build if he could get some financial assistance from the Board of Longitude. He called on Edmund Halley, Astronomer Royal, who was also a member of the Board. Halley advised him not to depend on the Board of Longitude, but to talk things over with George Graham, England's leading horologist.[50] Harrison called on Graham at ten o'clock one morning, and together they talked pendulums, escapements, remontoires and springs until eight o'clock in the evening, when Harrison departed a happy man. Graham had advised him to build his clock first and then apply to the

[49] *Ibid.*, pp. 40–41. Graham had experimented with a gridiron pendulum and in 1725 had produced a pendulum with a small jar of mercury attached to the bob which was supposed to counteract the expansion of the rod caused by a rise in temperature.

[50] *Ibid.*, pp. 42, 43; Graham and Tompion are the only two horologists buried in Westminster Abbey.

Board of Longitude. He had also offered to loan Harrison the money to build it with, and would not listen to any talk about interest or security of any kind. Harrison went home to Barrow and spent the next seven years building his first marine timekeeper, his "Number One," as it was later called.

In addition to heat and cold, the archenemies of all watchmakers, he concentrated on eliminating friction, or cutting it down to a bare minimum, on every moving part, and devised many ingenious ways of doing it; some of them radical departures from accepted watchmaking practice. Instead of using a pendulum, which would be impractical at sea, Harrison designed two huge balances weighing about five pounds each, that were connected by wires running over brass arcs so that their motions were always opposed. Thus any effect on one produced by the motion of the ship would be counteracted by the other. The "grasshopper" escapement was modified and simplified and two mainsprings on separate drums were installed. The clock was finished in 1735.

There was nothing beautiful or graceful about Harrison's Number One. It weighed seventy-two pounds and looked like nothing but an awkward, unwieldly piece of machinery. However, everyone who saw it and studied its mechanism declared it a masterpiece of ingenuity, and its performance certainly belied its appearance. Harrison mounted its case in gimbals and for a while tested it unofficially on a barge in the Humber River. Then he took it to London where he enjoyed his first brief triumph. Five members of the Royal Society examined the clock, studied its mechanism and then presented Harrison with a certificate stating that the principles of this timekeeper promised a sufficient degree of accuracy to meet the requirements set forth in the Act of Queen Anne. This historic document, which opened for Harrison the door to the Board of Longitude, was signed by Halley, Smith, Bradley, Machin and Graham.

On the strength of the certificate, Harrison applied to the Board of Longitude for a trial at sea, and in 1736 he was sent to Lisbon in H.M.S. *Centurion,* Captain Proctor. In his possession was a note from Sir Charles Wager, First Lord of the Admiralty, asking Proctor to see that every courtesy be given the bearer, who was said by those who knew him best to be "a very ingenious and sober man." Harrison was given the run of the ship, and his timekeeper was placed in the Captain's cabin where he could make observations and wind his clock without interruption. Proctor was courteous but skeptical. "The difficulty of measuring Time truly," he wrote, "where so many unequal Shocks and Motions stand in Opposition to it, gives me concern for the honest Man, and makes me feel he has attempted Impossibilities." [51]

No record of the clock's going on the outward voyage is known, but

[51] *Ibid.*, pp. 45–46.

after the return trip, made in H.M.S. *Orford*, Robert Man, Harrison was given a certificate signed by the master (that is, navigator) stating: "When we made the land, the said land, according to my reckoning (and others), ought to have been the Start; but before we knew what land it was, John Harrison declared to me and the rest of the ship's company, that according to his observations with his machine, it ought to be the Lizard—the which, indeed, it was found to be, his observation showing the ship to be more west than my reckoning, above one degree and twenty-six miles." It was an impressive report in spite of its simplicity, and yet the voyage to Lisbon and return was made in practically a north and south direction; one that would hardly demonstrate the best qualities of the clock in the most dramatic fashion. It should be noted, however, that even on this well-worn trade route it was not considered a scandal that the ship's navigator should make an error of 90 miles in his landfall.

On June 30, 1737, Harrison made his first bow to the mighty Board of Longitude. According to the official minutes, "Mr. John Harrison produced a new invented machine, in the nature of clockwork, whereby he proposes to keep time at sea with more exactness than by any other instrument or method hitherto contrived . . . and proposes to make another machine of smaller dimensions within the space of two years, whereby he will endeavour to correct some defects which he hath found in that already prepared, so as to render the same more perfect . . ." The Board voted him £500 to help defray expenses, one half to be paid at once and the other half when he completed the second clock and delivered same into the hands of one of His Majesty's ship's captains.[52]

Harrison's Number Two contained several minor mechanical improvements and this time all the wheels were made of brass instead of wood. In some respects it was even more cumbersome than Number One, and it weighed one hundred and three pounds. Its case and gimbal suspension weighed another sixty-two pounds. Number Two was finished in 1739, but instead of turning it over to a sea captain appointed by the Board to receive it, Harrison tested it for nearly two years under conditions of "great heat and motion." Number Two was never sent to sea because by the time it was ready, England was at war with Spain and the Admiralty had no desire to give the Spaniards an opportunity to capture it.

In January, 1741, Harrison wrote the Board that he had begun work on a third clock which promised to be far superior to the first two. They voted him another £500. Harrison struggled with it for several months, but seems to have miscalculated the "moment of inertia" of its balances. He thought he could get it going by the first of August, 1741, and have it ready for a sea trial two years later. But after five years the Board learned "that it does not go well, at present, as he expected it would, yet he plainly

[52] *Ibid.*, p. 47.

perceived the Cause of its present Imperfection to lye in a certain part [the balances] which, being of a different form from the corresponding part in the other machines, had never been tried before." Harrison had made a few improvements in the parts of Number Three and had incorporated in it the same antifriction devices he had used on Number Two, but the clock was still bulky and its parts were far from delicate; the machine weighed sixty-six pounds and its case and gimbals another thirty-five.[53]

Harrison was again feeling the pinch, even though the Board had given him several advances to keep him going, for in 1746, when he reported on Number Three, he laid before the Board an impressive testimonial signed by twelve members of the Royal Society including the President, Martin Folkes, Bradley, Graham, Halley and Cavendish, attesting the importance and practical value of his inventions in the solution of the longitude problem. Presumably this gesture was made to insure the financial support of the Board of Longitude. However, the Board needed no prodding. Three years later, acting on its own volition, the Royal Society awarded Harrison the Copley medal, the highest honor it could bestow. His modesty, perseverance and skill made them forget, at least for a time, the total lack of academic background which was so highly revered by that august body.[54]

Convinced that Number Three would never satisfy him, Harrison proposed to start work on two more timekeepers, even before Number Three was given a trial at sea. One would be pocketsize and the other slightly larger. The Board approved the project and Harrison went ahead. Abandoning the idea of a pocketsize chronometer, Harrison decided to concentrate his efforts on a slightly larger clock, which could be adapted to the intricate mechanism he had designed without sacrificing accuracy. In 1757 he began work on Number Four, a machine which "by reason alike of its beauty, its accuracy, and its historical interest, must take pride of place as the most famous chronometer that ever has been or ever will be made." It was finished in 1759.[55]

Number Four resembled an enormous "pair-case" watch about five inches in diameter, complete with pendant, as though it were to be worn. The dial was white enamel with an ornamental design in black. The hour and minute hands were of blued steel and the second hand was polished. Instead of a gimbal suspension, which Harrison had come to distrust, he used only a soft cushion in a plain box to support the clock. An adjustable

[53] *Ibid.*, pp. 47–49, for details of the technical improvements made in No. 2 and No. 3.

[54] *Ibid.*, p. 49. Some years later he was offered the honor of Fellow of the Royal Society, but he declined it in favor of his son William.

[55] *Ibid.*, pp. 50, 53; for a full description of No. 4 see H. M. Frodsham in the *Horological Journal* for May, 1878—with drawings of the escapement, train and remontoire taken from the duplicate of No. 4 made by Larcum Kendall.

outer box was fitted with a divided arc so that the timekeeper could be kept in the same position (with the pendant always slightly above the horizontal) regardless of the lie of the ship. When it was finished, Number Four was not adjusted for more than this one position, and on its first voyage it had to be carefully tended. The watch beat five to the second and ran for thirty hours without rewinding. The pivot holes were jeweled to the third wheel with rubies and the end stones were diamonds. Engraved in the top-plate were the words "John Harrison & Son, A.D. 1759." Cunningly concealed from prying eyes beneath the plate was a mechanism such as the world had never seen; every pinion and bearing, each spring and wheel was the end product of careful planning, precise measurement and exquisite craftsmanship. Into the mechanism had gone "fifty years of self-denial, unremitting toil, and ceaseless concentration." To Harrison, whose singleness of purpose had made it possible for him to achieve the impossible, Number Four was a satisfactory climax to a lifetime of effort. He was proud of this timekeeper, and in a rare burst of eloquence he wrote, "I think I may make bold to say, that there is neither any other Mechanical or Mathematical thing in the World that is more beautiful or curious in texture than this my watch or Time-keeper for the Longitude . . . and I heartily thank Almighty God that I have lived so long, as in some measure to complete it." [56]

After checking and adjusting Number Four with his pendulum clock for nearly two years, Harrison reported to the Board of Longitude, in March 1761, that Number Four was as good as Number Three and that its performance greatly exceeded his expectations. He asked for a trial at sea. His request was granted, and in April, 1761, William Harrison, his son and right-hand man, took Number Three to Portsmouth. The father arrived a short time later with Number Four. There were numerous delays at Portsmouth, and it was October before passage was finally arranged for young Harrison aboard H.M.S. *Deptford*, Dudley Digges, bound for Jamaica. John Harrison, who was then sixty-eight years old, decided not to attempt the long sea voyage himself; and he also decided to stake everything on the performance of Number Four, instead of sending both Three and Four along. The *Deptford* finally sailed from Spithead with a convoy, November 18, 1761, after first touching at Portland and Plymouth. The sea trial was on.

Number Four had been placed in a case with four locks, and the four keys were given to William Harrison, Governor Lyttleton of Jamaica, who was taking passage on the *Deptford*, Captain Digges, and his first lieutenant. All four had to be present in order to open the case, even for winding. The Board of Longitude had further arranged to have the longitude of Jamaica determined *de novo* before the trial, by a series of obser-

[56] *Ibid.*, p. 63.

vations of the satellites of Jupiter, but because of the lateness of the season it was decided to accept the best previously established reckoning. Local time at Portsmouth and at Jamaica was to be determined by taking equal altitudes of the sun, and the difference compared with the time indicated by Harrison's timekeeper.

As usual, the first scheduled port of call on the run to Jamaica was Madeira. On this particular voyage, all hands aboard the *Deptford* were anxious to make the island on the first approach. To William Harrison it meant the first crucial test of Number Four; to Captain Digges it meant a test of his dead reckoning against a mechanical device in which he had no confidence; but the ship's company had more than a scientific interest in the proceedings. They were afraid of missing Madeira altogether, "the consequence of which, would have been Inconvenient." To the horror of all hands, it was found that the beer had spoiled, over a thousand gallons of it, and the people had already been reduced to drinking water. Nine days out from Plymouth the ship's longitude, by dead reckoning, was 13° 50′ west of Greenwich, but according to Number Four and William Harrison it was 15° 19′ W. Captain Digges naturally favored his dead reckoning calculations, but Harrison stoutly maintained that Number Four was right and that if Madeira were properly marked on the chart they would sight it the next day. Although Digges offered to bet Harrison five to one that he was wrong, he held his course, and the following morning at 6 A.M. the lookout sighted Porto Santo, the northeastern island of the Madeira group, dead ahead.

The *Deptford's* officers were greatly impressed by Harrison's uncanny predictions throughout the voyage. They were even more impressed when they arrived at Jamaica three days before H.M.S. *Beaver*, which had sailed for Jamaica ten days before them. Number Four was promptly taken ashore and checked. After allowing for its rate of going (2⅔ seconds per day losing at Portsmouth), it was found to be 5 seconds slow, an error in longitude of 1¼′ only, or 1¼ nautical miles.[57]

The official trial ended at Jamaica. Arrangements were made for William Harrison to make the return voyage in the *Merlin*, sloop, and in a burst of enthusiasm Captain Digges placed his order for the first Harrison-built chronometer which should be offered for sale. The passage back to England was a severe test for Number Four. The weather was extremely rough and the timekeeper, still carefully tended by Harrison, had to be moved to the poop, the only dry place on the ship, where it was pounded unmercifully and "received a number of violent shocks." However, when it was again checked at Portsmouth, its total error for the five months' voyage, through heat and cold, fair weather and foul (after allowing for its rate of going), was only $1^{m}\ 53\frac{1}{2}^{s}$, or an error in longitude of 28½′

[57] *Ibid.*, pp. 55–56 and 56 *n*.

(28½ nautical miles). This was safely within the limit of half a degree specified in the Act of Queen Anne. John Harrison and son had won the fabulous reward of £20,000.

The sea trial had ended, but the trials of John Harrison had just begun. Now for the first time, at the age of sixty-nine, Harrison began to feel the lack of an academic background. He was a simple man; he did not know the language of diplomacy, the gentle art of innuendo and evasion. He had mastered the longitude but he did not know how to cope with the Royal Society or the Board of Longitude. He had won the reward and all he wanted now was his money. The money was not immediately forthcoming.

Neither the Board of Longitude nor the scientists who served it as consultants were at any time guilty of dishonesty in their dealings with Harrison; they were only human. £20,000 was a tremendous fortune, and it was one thing to dole out living expenses to a watchmaker in amounts not exceeding £500 so that he might contribute something or other to the general cause. But it was another thing to hand over £20,000 in a lump sum to one man, and a man of humble birth at that. It was most extraordinary. Moreover, there were men on the Board and members of the Royal Society who had designs on the reward themselves or at least a cut of it. James Bradley and Johann Tobias Mayer had both worked long and hard on the compilation of accurate lunar tables. Mayer's widow was paid £3000 for his contribution to the cause of longitude, and in 1761 Bradley told Harrison that he and Mayer would have shared £10,000 of the prize money between them if it had not been for his blasted watch. Halley had struggled long and manfully on the solution of the longitude by compass variation, and was not in a position to ignore any part of £20,000. The Reverend Nevil Maskelyne, Astronomer Royal, and compiler of the *Nautical Almanac*, was an obstinate and uncompromising apostle of "lunar distances" or "lunars" for finding the longitude, and had closed his mind to any other method whatsoever. He loved neither Harrison nor his watch. In view of these and other unnamed aspirants, it was inevitable that the Board should decide that the amazing performance of Harrison's timekeeper was a fluke. They had never been allowed to examine the mechanism, and they pointed out that if a gross of watches were carried to Jamaica under the same conditions, one out of the lot might perform equally well—at least for one trip. They accordingly refused to give Harrison a certificate stating that he had met the requirements of the Act until his timekeeper was given a further trial, or trials. Meanwhile, they did agree to give him the sum of £2500 as an interim reward, since his machine had proved to be a rather useful contraption, though mysterious beyond words. An Act of Parliament (February, 1763) enabling him to receive £5000 as soon as he disclosed the secret of his invention, was completely nullified by the absurdly rigid conditions set up by the Board. He was finally granted a new trial at sea.[58]

The rules laid down for the new trial were elaborate and exacting. The difference in longitude between Portsmouth and Jamaica was to be determined *de novo* by observations of Jupiter's satellites. Number Four was to be rated at Greenwich before sailing, but Harrison balked, saying "that he did not chuse to part with it out of his hands till he shall have reaped some advantage from it." However, he agreed to send his own rating, sealed, to the Secretary of the Admiralty before the trial began. After endless delays the trial was arranged to take place between Portsmouth and Barbados, instead of Jamaica, and William Harrison embarked on February 14, 1764, in H.M.S. *Tartar*, Sir John Lindsay, at the Nore. The *Tartar* proceeded to Portsmouth, where Harrison checked the rate of Number Four with a regulator installed there in a temporary observatory. On March 28, 1764, the *Tartar* sailed from Portsmouth and the second trial was on.

It was the same story all over again. On April 18, twenty-one days out, Harrison took two altitudes of the sun and announced to Sir John that they were forty-three miles east of Porto Santo. Sir John accordingly steered a direct course for it, and at one o'clock the next morning the island was sighted, "which exactly agreed with the Distance mentioned above." They arrived at Barbados May 13, "Mr. Harrison all along in the Voyage declaring how far he was distant from that Island, according to the best settled longitude thereof. The Day before they made it, he declared the Distance: and Sir John sailed in Consequence of this Declaration, till Eleven at Night, which proving dark he thought proper to lay by. Mr. Harrison then declaring they were no more than eight or nine Miles from the Land, which accordingly at Day Break they saw from that Distance."[59]

When Harrison went ashore with Number Four he discovered that none other than Maskelyne and an assistant, Green, had been sent ahead to check the longitude of Barbados by observing Jupiter's satellites. Moreover, Maskelyne had been orating loudly on the superiority of his own method of finding longitude, namely, by lunar distances. When Harrison heard what had been going on he objected strenuously, pointing out to Sir John that Maskelyne was not only an interested party but an active and avid competitor, and should not have anything to do with the trials. A compromise was arranged, but, as it turned out, Maskelyne was suddenly indisposed and unable to make the observations.

[58] *Ibid.*, pp. 57 ff. On August 17, 1762, the Board refused to give Harrison a certificate stating that he had complied with the terms set forth in the Act of Queen Anne.

[59] *Ibid.*, p. 59; see also *A narrative of the proceedings relative to the discovery of the longitude at sea; by Mr. John Harrison's time-keeper* . . . [By James Short] London: printed for the author, 1765, pp. 7, 8.

After comparing the data obtained by observation with Harrison's chronometer, Number Four showed an error of 38.4 seconds over a period of seven weeks, or 9.6 miles of longitude (at the equator) between Portsmouth and Barbados. And when the clock was again checked at Portsmouth, after 156 days, elapsed time, it showed, after allowing for its rate of going, a total gain of only 54 seconds of time. If further allowance were made for changes of rate caused by variations in temperature, information posted beforehand by Harrison, the rate of Number Four would have been reduced to an error of 15 seconds of loss in 5 months, or less than 1/10 of a second a day.[60]

The evidence in favor of Harrison's chronometer was overwhelming, and could no longer be ignored or set aside. But the Board of Longitude was not through. In a Resolution of February 9, 1765, they were unanimously of the opinion that "the said timekeeper has kept its time with sufficient correctness, without losing its longitude in the voyage from Portsmouth to Barbados beyond the nearest limit required by the Act 12th of Queen Anne, but even considerably within the same." Now, they said, all Harrison had to do was demonstrate the mechanism of his clock and explain the construction of it, "by Means whereof other such Timekeepers might be framed, of sufficient Correctness to find the Longitude at Sea. . . ." In order to get the first £10,000 Harrison had to submit, on oath, complete working drawings of Number Four; explain and demonstrate the operation of each part, including the process of tempering the springs; and finally, hand over to the Board his first three timekeepers as well as Number Four.[61]

Any foreigner would have acknowledged defeat at this juncture, but not Harrison, who was an Englishman and a Yorkshireman to boot. "I cannot help thinking," he wrote the Board, after hearing their harsh terms, "but I am extremely ill used by gentlemen who I might have expected different treatment from. . . . It must be owned that my case is very hard, but I hope I am the first, and for my country's sake, shall be the last that suffers by pinning my faith on an English Act of Parliament." The case of "Longitude Harrison" began to be aired publicly, and several of his friends launched an impromptu publicity campaign against the Board and against Parliament. The Board finally softened their terms and Harrison reluctantly took his clock apart at his home for the edification of a committee of six, nominated by the Board; three of them, Thomas Mudge, William Matthews and Larcum Kendall, were watchmakers. Harrison then received a certificate from the Board (October 28, 1765) entitling him to £7500, or the balance due him on the first half of the reward. The second half did not come so easily.

[60] R. T. Gould, *op. cit.*, pp. 59, 60.
[61] *Ibid.*, pp. 60 ff.; Act of Parliament 5 George III, Cap. 20; see also James Short. *op. cit.*, p. 15.

Number Four was now in the hands of the Board of Longitude, held in trust for the benefit of the people of England. As such, it was carefully guarded against prying eyes and tampering, even by members of the Board. However, that learned body did its humble best. First they set out to publicize its mechanism as widely as possible. Unable to take the thing apart themselves, they had to depend on Harrison's own drawings, and these were redrawn and carefully engraved. What was supposed to be a full textual description was written by the Reverend Nevil Maskelyne and printed in book form with illustrations appended: *The Principles of Mr. Harrison's Time-Keeper, with Plates of the Same.* London, 1767. Actually the book was harmless enough, because no human being could have even begun to reproduce the clock from Maskelyne's description. To Harrison it was just another bitter pill to swallow. "They have since published all my Drawings," he wrote, "without giving me the last Moiety of the Reward, or even paying me and my Son for our Time at a rate as common Mechanicks; an Instance of such Cruelty and Injustice as I believe never existed in a learned and civilised Nation before." Other galling experiences followed.[62]

With great pomp and ceremony Number Four was carried to the Royal Observatory at Greenwich. There it was scheduled to undergo a prolonged and exhaustive series of trials under the direction of the Astronomer Royal, the Reverend Nevil Maskelyne. It cannot be said that Maskelyne shirked his duty, although he was handicapped by the fact that the timekeeper was always kept locked in its case, and he could not even wind it except in the presence of an officer detailed by the Governor of Greenwich to witness the performance. Number Four, after all, was a £10,000 timekeeper. The tests went on for two months. Maskelyne tried the watch in various positions for which it was not adjusted, dial up and dial down. Then for ten months it was tested in a horizontal position, dial up. The Board published a full account of the results with a preface written by Maskelyne, in which he gave it as his studied opinion "That Mr. Harrison's Watch cannot be depended upon to keep the Longitude within a Degree, in a West-India Voyage of six weeks, nor to keep the Longitude within Half a Degree for more than a Fortnight, and then it must be kept in a Place where the Thermometer is always some Degrees above freezing." (There was still £10,000 prize money outstanding.)[63]

The Board of Longitude next commissioned Larcum Kendall, watchmaker, to make a duplicate of Number Four. They also advised Harrison that he must make Number Five and Number Six and have them tried at

[62] R. T. Gould, *op. cit.*, pp. 61–62; 62 *n*; the unauthorized publication of Harrison's drawings with a preface by the Reverend Maskelyne, appeared under the title: *The principles of Mr. Harrison's time-keeper, with plates of the same,* London, 1767.

[63] R. T. Gould, *op. cit.*, p. 63; see also Nevil Maskelyne's *An account of the going of Mr. John Harrison's watch . . .* London, 1767.

sea, intimating that otherwise he would not be entitled to the other half of the reward. When Harrison asked if he might use Number Four for a short time to help him build two copies of it, he was told that Kendall needed it to work from and that it would be impossible. Harrison did the best he could, while the Board laid plans for an exhaustive series of tests for Number Five and Number Six. They spoke of sending them to Hudson's Bay and of letting them toss and pitch in the Downs for a month or two as well as sending them out to the West Indies.

After three years (1767–1770) Number Five was finished. In 1771, just as the Harrisons were finishing the last adjustments on the clock, they heard that Captain Cook was preparing for a second exploring cruise, and that the Board was planning to send Kendall's duplicate of Number Four along with him. Harrison pleaded with them to send Number Four and Number Five instead, telling them he was willing to stake his claim to the balance of the reward on their performance, or to submit "to any mode of trial, by men not already proved partial, which shall be definite in its nature." The man was now more than ever anxious to settle the business once and for all. But it was not so to be. He was told that the Board did not see fit to send Number Four out of the kingdom, nor did they see any reason for departing from the manner of trial already decided upon.

John Harrison was now seventy-eight years old. His eyes were failing and his skilled hands were not as steady as they were, but his heart was strong and there was still a lot of fight left in him. Among his powerful friends and admirers was His Majesty King George the Third, who had granted Harrison and his son an audience after the historic voyage of the *Tartar*. Harrison now sought the protection of his king, and "Farmer George," after hearing the case from start to finish, lost his patience. "By God, Harrison, I'll see you righted," he roared. And he did. Number Five was tried at His Majesty's private observatory at Kew. The king attended the daily checking of the clock's performance, and had the pleasure of watching the operation of a timekeeper whose total error over a ten week's period was 4½ seconds.[64]

Harrison submitted a memorial to the Board of Longitude, November 28, 1772, describing in detail the circumstances and results of the trial at Kew. In return, the Board passed a resolution to the effect that they were not the slightest bit interested; that they saw no reason to alter the manner of trial they had already proposed and that no regard would be paid for a trial made under any other conditions. In desperation Harrison decided to play his last card—the king. Backed by His Majesty's personal interest in the proceedings, Harrison presented a petition to the House of Commons with weight behind it. It was heralded as follows: "The Lord North, by His Majesty's Command, acquainted the House that His Maj-

[64] R. T. Gould, *op. cit.*, pp. 64–65.

esty, having been informed of the Contents of the said Petition, recommended it to the Consideration of the House." Fox was present to give the petition his full support, and the king was willing, if necessary, to appear at the Bar of the House under an inferior title and testify in Harrison's behalf. At the same time, Harrison circulated a broadside, *The Case of Mr. John Harrison*, stating his claims to the second half of the reward.[65]

The Board of Longitude began to squirm. Public indignation was mounting rapidly and the Speaker of the House informed the Board that consideration of the petition would be deferred until they had an opportunity to revise their proceedings in regard to Mr. Harrison. Seven Admiralty clerks were put to work copying out all the Board's resolutions concerning Harrison. While they worked day and night to finish the job, the Board made one last desperate effort. They summoned William Harrison to appear before them; but the hour was late. They put him through a catechism and tried to make him consent to new trials and new conditions. Harrison stood fast, refusing to consent to anything they might propose. Meanwhile a money bill was drawn up by Parliament in record time; the king gave it the nod and it was passed. The Harrisons had won their fight.

[65] *Ibid.*, p. 66; see also the *Journal of the House of Commons*, 6. 5. 1772.

COMMENTARY ON

JOHN COUCH ADAMS

THE discovery in 1846 of the planet Neptune was a dramatic and spectacular achievement of mathematical astronomy. The very existence of this new member of the solar system, and its exact location, were demonstrated with pencil and paper; there was left to observers only the routine task of pointing their telescopes at the spot the mathematicians had marked. It is easy to understand the enthusiastic appraisal of Sir John Herschel, who, on the occasion of the presentation of gold medals of the Royal Astronomical Society to Leverrier and Adams, said that their codiscovery of Neptune "surpassed by intelligible and legitimate means, the wildest pretensions of clairvoyance." [1]

As early as about 1820 it had been recognized that irregularities in the motion of Uranus, deviations of its observed orbit from its calculated positions, could be accounted for only by an outside disturbing force. The German astronomer Bessel at that time remarked to Humboldt that sooner or later the "mystery of Uranus" would "be solved by the discovery of a new planet." [2] The problem was solved by two astronomers working entirely independently of each other: Urban Jean Leverrier, director of the Paris Observatory, and John Couch Adams, a twenty-six-year-old Fellow of St. John's College in Cambridge. The conquest of this intricate and incredibly laborious problem of "inverse perturbations"—i.e., given the perturbations, to find the planet—was an intellectual feat deservedly acclaimed as "sublime"; but the personal behavior of various participants in the event falls short of sublimity. The affair was marked by episodes of confusion and fecklessness, of donnish hairpulling and Gallic backbiting, of stuffiness, jealousy and general academic blight. Sir Harold Spencer Jones gives a balanced account; as the present Astronomer Royal he is understandably charitable to Sir George Airy, the Astronomer Royal in Adams' day, who almost succeeded—more or less innocently—in doing Adams out of proper recognition for his work.

I add a few biographical details about Adams not covered in Jones' lecture. While the discovery of Neptune was his most sensational achievement, it came at the beginning of a long and distinguished service to astronomy and mathematics. Adams succeeded George Peacock, the noted algebraist, as professor of astronomy and geometry at Cambridge. This post, as well as a fellowship in mathematics at Pembroke College, he held until his death. His researches are represented by important memoirs on

[1] *Nature*, Vol. XXXIV, p. 565.
[2] Giorgio Abetti, *The History of Astronomy*, 1952, New York, p. 214.

lunar motion, on the effect of planetary perturbations on the orbits and periods of certain meteors, on various problems of pure mathematics. Adams was a stylish craftsman in his mathematical work; even the examination questions he set in prize competitions were admired for their finish. Like Euler and Gauss, he found pleasure in undertaking immense numerical calculations, which he carried out with consummate accuracy. But he was not chained to his specialty. He read widely in history, biology, geology and general literature; he took a deep interest in political questions. During the Franco-Prussian war, he was so moved "that he could scarcely work or sleep." [3] Adams was a shy, gentle and unaffected man. He refused knighthood in 1847, just as he had refused to be drawn into the bitter controversy over the question of who was first to discover Neptune. The honor was tendered in a foolish attempt to settle a foolish question. The entire business was beneath Adams. "He uttered no complaint; he laid no claim to priority; Leverrier had no warmer admirer." [4] He died after a long illness on January 21, 1892.[5]

[3] J. W. L. Glaisher, biographical memoir in *The Scientific Papers of John Couch Adams*, ed. by William Grylls Adams, Cambridge, 1896, p. XLIV.

[4] *The Dictionary of National Biography*; Vol. XXII, *Supplement*, article on Adams, p. 16.

[5] For further information on the Adams-Airy affair see J. E. Littlewood, *A Mathematician's Miscellany*, London, 1953, pp. 116–134.

There are seven windows in the head, two nostrils, two eyes, two ears, and a mouth; so in the heavens there are two favorable stars, two unpropitious, two luminaries, and Mercury alone undecided and indifferent. From which and many other similar phenomena of nature, such as the seven metals, etc., which it were tedious to enumerate, we gather that the number of planets is necessarily seven.—FRANCESCO SIZZI (argument against Galileo's discovery of the satellites of Jupiter)

4 John Couch Adams and the Discovery of Neptune

By SIR HAROLD SPENCER JONES

ON the night of 13 March 1781 William Herschel, musician by profession but assiduous observer of the heavens in his leisure time, made a discovery that was to bring him fame. He had for some time been engaged upon a systematic and detailed survey of the whole heavens, using a 7 in. telescope of his own construction; he carefully noted everything that appeared in any way remarkable. On the night in question, in his own words:

'In examining the small stars in the neighbourhood of H Geminorum I perceived one that appeared visibly larger than the rest; being struck with its uncommon appearance I compared it to H Geminorum and the small star in the quartile between Auriga and Gemini, and finding it so much larger than either of them, I suspected it to be a comet.'

Most observers would have passed the object by without noticing anything unusual about it, for the minute disk was only about 4 sec. in diameter. The discovery was made possible by the excellent quality of Herschel's telescope, and by the great care with which his observations were made.

The discovery proved to be of greater importance than Herschel suspected, for the object he had found was not a comet, but a new planet, which revolved round the Sun in a nearly circular path at a mean distance almost exactly double that of Saturn; it was unique, because no planet had ever before been discovered; the known planets, easily visible to the naked eye, did not need to be discovered.

After the discovery of Uranus, as the new planet was called, it was ascertained that it had been observed as a star and its position recorded on a score of previous occasions. The earliest of these observations was made by Flamsteed at Greenwich in 1690. Lemonnier in 1769 had observed its transit six times in the course of 9 days and, had he compared the observations with one another, he could not have failed to anticipate Herschel in the discovery. As Uranus takes 84 years to make a complete revolution round the Sun, these earlier observations were of special value for the investigation of its orbit.

The positions of the planet computed from tables constructed by Delambre soon began to show discordances with observation, which became greater as time went on. As there might have been error or incompleteness in Delambre's theory and tables, the task of revision was undertaken by Bouvard, whose tables of the planet appeared in 1821. Bouvard found that, when every correction for the perturbations in the motion of Uranus by the other planets was taken into account, it was not possible to reconcile the old observations of Flamsteed, Lemonnier, Bradley, and Mayer with the observations made subsequently to the discovery of the planet in 1781.

'The construction of the tables, then,' said Bouvard, 'involves this alternative: if we combine the ancient observations with the modern, the former will be sufficiently well represented, but the latter will not be so, with all the precision which their superior accuracy demands; on the other hand, if we reject the ancient observations altogether, and retain only the modern, the resulting tables will faithfully conform to the modern observations, but will very inadequately represent the more ancient. As it was necessary to decide between these two courses, I have adopted the latter, on the ground that it unites the greatest number of probabilities in favour of the truth, and I leave to the future the task of discovering whether the difficulty of reconciling the two systems is connected with the ancient observations, or whether it depends on some foreign and unperceived cause which may have been acting upon the planet.'

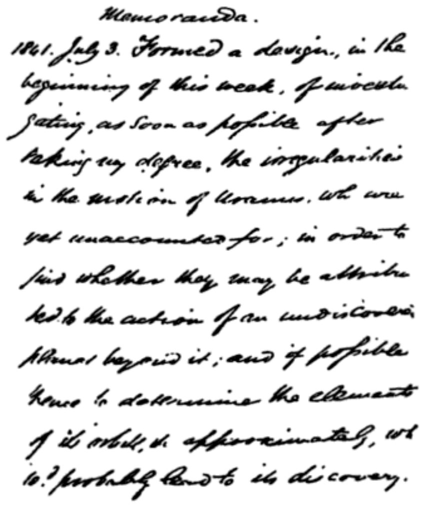
Further observations of Uranus were for a time found to be pretty well represented by Bouvard's Tables, but systematic discordances between observations and the tables gradually began to show up. As time went on, observations continued to deviate more and more from the tables. It began to be suspected that there might exist an unknown distant planet, whose gravitational attraction was disturbing the motion of Uranus. An alternative suggestion was that the inverse square law of gravitation might not be exact at distances as great as the distance of Uranus from the Sun.

The problem of computing the perturbations in the motion of one planet by another moving planet, when the undisturbed orbits and the masses of the planets are known is fairly straightforward, though of some mathematical complexity. The inverse problem, of analysing the perturbations in the motion of one planet in order to deduce the position, path and mass of the planet which is producing these perturbations, is of much greater complexity and difficulty. A little consideration will, I think, show that this must be so. If a planet were exposed solely to the attractive influence of the Sun, its orbit would be an ellipse. The attractions of the other planets perturb its motion and cause it to deviate now on the one side and now on the other side of this ellipse. To determine the elements of the elliptic orbit from the positions of the planet as assigned by observation, it is necessary first to compute the perturbations produced by the other planets and to subtract them from the observed positions.

The position of the planet in this orbit at any time, arising from its undisturbed motion, can be calculated; if the perturbations of the other planets are then computed and added, the true position of the planet is obtained. The whole procedure is, in practice, reduced to a set of tables. But if Uranus is perturbed by a distant *unknown* planet, the observed positions when corrected by the subtraction of the perturbations caused by the *known* planets are not the positions in the true elliptic orbit; the perturbations of the unknown planet have not been allowed for. Hence when the corrected positions are analysed in order to determine the elements of the elliptic orbit, the derived elements will be falsified. The positions of Uranus computed from tables such as Bouvard's would be in error for two reasons; in the first place, because they are based upon incorrect elements of the elliptic orbit; in the second place, because the perturbations produced by the unknown planet have not been applied. The two causes of error have a common origin and are inextricably entangled in each other, so that neither can be investigated independently of the other. Thus though many astronomers thought it probable that Uranus was perturbed by an undiscovered planet, they could not prove it. No occasion had arisen for the solution of the extremely complicated problem of what is termed inverse perturbations, starting with the perturbed positions and deducing from them the position and motion of the perturbing body.

The first solution of this intricate problem was made by a young Cambridge mathematician, John Couch Adams. As a boy at school Adams had shown conspicuous mathematical ability, an interest in astronomy, and skill and accuracy in numerical computation. At the age of 16 he had computed the circumstances of an annular eclipse of the Sun, as visible from Lidcot, near Launceston, where his brother lived. He entered St John's College in October, 1839, at the age of 20, and in 1843 graduated Senior Wrangler, being reputed to have obtained more than double the marks awarded to the Second Wrangler. In the same year he became first Smith's Prizeman and was elected Fellow of his College.

Whilst still an undergraduate his attention had been drawn to the irregularities in the motion of Uranus. After his death there was found among his papers this memorandum, written at the beginning of his second long vacation:


Memoranda.
1841. July 3. Formed a design, in the
beginning of this week, of investi-
gating, as soon as possible after
taking my degree, the irregularities
in the motion of Uranus, wh. are
yet unaccounted for; in order to
find whether they may be attribu-
ted to the action of an undiscovered
planet beyond it; and if possible
thence to determine the elements
of its orbit, &c. approximately, wh.
wd. probably lead to its discovery.

'1841, July 3. Formed a design in the beginning of this week, of investigating, as soon as possible after taking my degree, the irregularities in the motion of Uranus, which are yet unaccounted for; in order to find whether they may be attributed to the action of an undiscovered planet beyond it; and if possible thence to determine the elements of its orbit, etc. approximately, which would probably lead to its discovery.'

As soon as Adams had taken his degree he attempted a first rough solution of the problem, with the simplifying assumptions that the unknown planet moved in a circular orbit, in the plane of the orbit of Uranus, and that its distance from the Sun was twice the mean distance of Uranus, this being the distance to be expected according to the empirical law of Bode. This preliminary solution gave a sufficient improvement in the agreement between the corrected theory of Uranus and observation to encourage him to pursue the investigation further. In order to make the observational data more complete application was made in February 1844 by Challis, the Plumian Professor of Astronomy, to Airy, the Astronomer Royal, for the errors of longitude of Uranus for the years 1818–26. Challis explained that he required them for a young friend, Mr Adams of St John's College, who was working at the theory of Uranus. By return of post, Airy sent the Greenwich data not merely for the years 1818–26 but for the years 1754–1830.

Adams now undertook a new solution of the problem, still with the assumption that the mean distance of the unknown planet was twice that of Uranus but without assuming the orbit to be circular. During term-time he had little opportunity to pursue his investigations and most of the work was undertaken in the vacations. By September 1845, he had completed the solution of the problem, and gave to Challis a paper with the elements of the orbit of the planet, as well as its mass and its position for 1 October 1845. The position indicated by Adams was actually within 2° of the position of Neptune at that time. A careful search in the vicinity of this position should have led to the discovery of Neptune. The comparison between observation and theory was satisfactory and Adams, confident in the validity of the law of gravitation and in his own mathematics, referred to the 'new planet'.

Challis gave Adams a letter of introduction to Airy, in which he said that 'from his character as a mathematician, and his practice in calculation, I should consider the deductions from his premises to be made in a trustworthy manner'. But the Astronomer Royal was in France when Adams called at Greenwich. Airy, immediately on his return, wrote to Challis saying: 'would you mention to Mr Adams that I am very much interested with the subject of his investigations, and that I should be delighted to hear of them by letter from him?'

Towards the end of October Adams called at Greenwich, on his way from Devonshire to Cambridge, on the chance of seeing the Astronomer Royal. At about that time Airy was occupied almost every day with meetings of the Railway Gauge Commission and he was in London when Adams called. Adams left his card and said that he would call again. The card was taken to Mrs Airy, but she was not told of the intention of Adams to call later. When Adams made his second call, he was informed that the Astronomer Royal was at dinner; there was no message for him and he went away feeling mortified. Airy, unfortunately, did not know of this second visit at the time. Adams left a paper summarizing the results which he had obtained and giving a list of the residual errors of the mean longitude of Uranus, after taking account of the disturbing action of the new planet. These errors were satisfactorily small, except for the first observation by Flamsteed in 1690. A few days later Airy wrote to Adams acknowledging the paper and enquiring whether the perturbations would explain the errors of the radius vector of Uranus as well as the errors of longitude; in the reduction of the Greenwich observations, Airy had shown that not only the longitude of Uranus but also its distance from the Sun (called the radius vector) showed discordances from the tabular values. Airy said at a later date that he waited with much anxiety for the answer to this query, which he looked upon as an *experimentum crucis*, and that if Adams had replied in the affirmative, he would at once have exerted all his influence to procure the publication of Adams's theory. It should be emphasized that neither Challis nor Airy knew anything about the details of Adams's investigation. Adams had attacked this difficult problem entirely unaided and without guidance. Confident in his own mathematical ability he sought no help and he needed no help.

Adams never replied to the Astronomer Royal's query; but for this failure to reply, he would almost certainly have had the sole glory of the discovery of Neptune. Airy and Adams were looking at the same problem from different points of view; Adams was so convinced that the discordances between the theory of Uranus and observation were due to the perturbing action of an unknown planet that no alternative hypothesis was considered by him; Airy, on the other hand, did not exclude the possi-

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1845 October

According to my calculations, the obs^d irregularities in the motion of Uranus may be accounted for, by supposing the existence of an ext^r planet, the mass & orbit of which are as follows

Mean Dist. (assumed nearly in accordance with Bode's law)
38.4
Mean sid^l motion in 365.25 days
1°.30.9
Mean Long. 1^st Oct^r 1845
323°.34'
Long. Perih^n
315.55
Eccent^y
0.1610
Mass (that of Sun being unity)
0.0001656

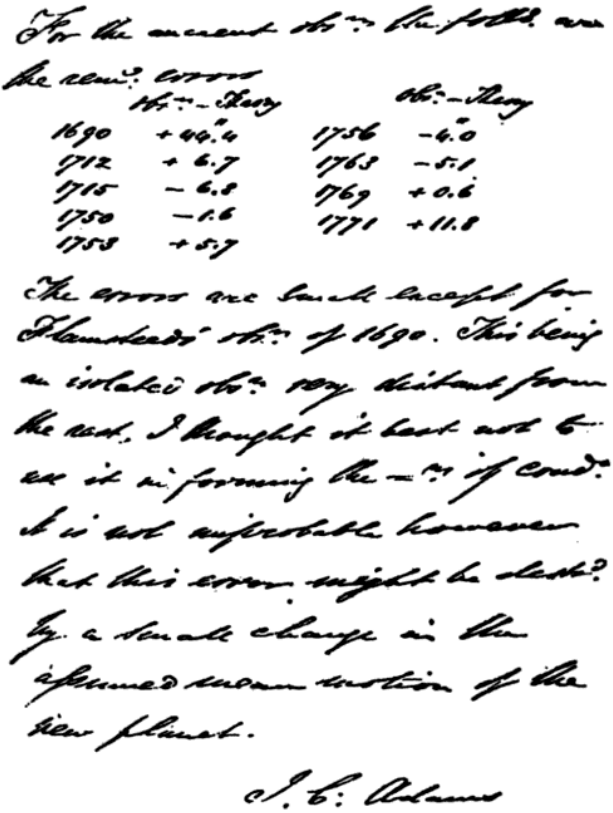
For the modern obs.ns I have used the method of Normal places; taking the mean of the Tabular errors as given by obs.ns near 3 consecutive opp.ns to correspond with the mean of the times & the Greenw. obs.ns have been used down to 1830. Since wh. the Cambridge & Greenwich obs.ns and those given in the Astron. Nachr. have been made use of. The follg. are the remg. errors of mean Longitude

	Obs.n − Theory		Obs.n − Theory		Obs.n − Theory
1780	+1″.27	1801	−0″.04	1822	+0″.30
1783	−0.23	1804	+1.76	1825	+1.92
1786	−0.96	1807	−0.21	1828	+2.25
1789	+1.82	1810	+0.56	1831	−1.06
1792	−0.91	1813	−0.94	1834	−1.44
1795	+0.09	1816	−0.31	1837	−1.62
1798	−0.99	1819	−2.00	1840	+1.73

The error for 1780 is concluded from that for 1781 given by obs.n compared with those of 4 or 5 following years & also with Lemonnier's obs.ns in 1769 & 1771.

bility that the law of gravitation might not apply exactly at great distances. The purpose of his query, to which he attached great importance, was to decide between the two possibilities. As he later wrote to Challis (21 December 1846):

'There were two things to be explained, which might have existed each independently of the other, and of which one could be ascertained independently of the other: viz. error of longitude and error of radius vector. And there is no *a priori* reason for thinking that a hypothesis which will



For the ancient obsns the follg are the remg errors

	Obs - Theory		Obs - Theory
1690	+44".4	1756	-4".0
1712	+6.7	1763	-5.1
1715	-6.8	1769	+0.6
1750	-1.6	1771	+11.8
1753	+5.7		

The errors are small except for Flamsteed's obsn of 1690. This being an isolated obsn very distant from the rest, I thought it best not to use it in forming the eqns of condn. It is not improbable however that this error might be destroyed by a small change in the assumed mean motion of the new planet.

J. C. Adams

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explain the error of longitude will also explain the error of radius vector. If, after Adams had satisfactorily explained the error of longitude he had (with the numerical values of the elements of the two planets so found) converted his formula for perturbation of radius vector into numbers, and if these numbers had been discordant with the *observed* numbers of discordances of radius vector, then *the theory would have been false,* NOT from any error of Adams's BUT from a failure in the law of gravitation. On this question therefore turned the continuance or fall of the law of gravitation.'

What were the reasons for Adams's failure to reply? There were several; he gave them himself at a later date (18 November 1846) in a letter to Airy. He wrote as follows:

'I need scarcely say how deeply I regret the neglect of which I was guilty in delaying to reply to the question respecting the Radius Vector of Uranus, in your note of November 5th, 1845. In palliation, though not in excuse of this neglect, I may say that I was not aware of the importance which you attached to my answer on this point and I had not the smallest notion that you felt any difficulty on it. . . . For several years past, the observed place of Uranus has been falling more and more rapidly behind its tabular place. In other words, the real angular motion of Uranus is considerably *slower* than that given by the Tables. This appeared to me to show clearly that the Tabular Radius Vector would be considerably increased by any Theory which represented the motion in Longitude. . . . Accordingly, I found that if I simply corrected the elliptic elements, so as to satisfy the modern observations as nearly as possible without taking into account any additional perturbations, the corresponding increase in the Radius Vector would not be very different from that given by my actual Theory. Hence it was that I waited to defer writing to you till I could find time to draw up an account of the method employed to obtain the results which I had communicated to you. More than once I commenced writing with this object, but unfortunately did not persevere. I was also much pained at not having been able to see you when I called at the Royal Observatory the second time, as I felt that the whole matter might be better explained by half an hour's conversation than by several letters, in writing which I have always experienced a strange difficulty. I entertained from the first the strongest conviction that the observed anomalies were due to the action of an exterior planet; no other hypothesis appeared to me to possess the slightest claims to attention. Of the accuracy of my calculations I was quite sure, from the care with which they were made and the number of times I had examined them. The only point which appeared to admit of any doubt was the assumption as to the mean distance and this I soon proceeded to correct. The work however went on very slowly throughout, as I had scarcely any time to give to these investigations, except during the vacations.

'I could not expect, however, that practical astronomers, who were already fully occupied with important labours, would feel as much confidence in the results of my investigation, as I myself did; and I therefore had our instruments put in order, with the express purpose, if no one else took up the subject, of undertaking the search for the planet myself, with the small means afforded by our observatory at St John's.'

Airy was a man with a precise and orderly mind, extremely methodical and prompt in answering letters. Another person might have followed the matter up, but not Airy. In a letter of later date to Challis, he said that 'Adams's silence . . . was so far unfortunate that it interposed an effectual barrier to all further communication. It was clearly impossible for me to write to him again.'

Meanwhile, another astronomer had turned his attention to the problem of accounting for the anomalies in the motion of Uranus. In the summer of 1845 Arago, Director of the Paris Observatory, drew the attention of his friend and protégé, Le Verrier, to the importance of investigating the theory of Uranus. Le Verrier was a young man, 8 years older than Adams, with an established reputation in the astronomical world, gained by a brilliant series of investigations in celestial mechanics. In contrast, Adams was unknown outside the circle of his Cambridge friends and he had not yet published anything.

Le Verrier decided to devote himself to the problem of Uranus and laid aside some researches on comets, on which he had been engaged. His investigations received full publicity, for the results were published, as the work proceeded, in a series of papers in the *Comptes Rendus* of the French Academy. In the first of these, communicated in November 1845 (a month after Adams had left his solution of the problem with the Astronomer Royal), Le Verrier recomputed the perturbations of Uranus by Jupiter and Saturn, derived new orbital elements for Uranus, and showed that these perturbations were not capable of explaining the observed irregularities of Uranus. In the next paper, presented in June 1846, Le Verrier discussed possible explanations of the irregularities and concluded that none was admissible, except that of a disturbing planet exterior to Uranus. Assuming, as Adams had done, that its distance was twice the distance of Uranus and that its orbit was in the plane of the ecliptic, he assigned its true longitude for the beginning of 1847; he did not obtain the elements of its orbit nor determine its mass.

The position assigned by Le Verrier differed by only 1° from the position which Adams had given seven months previously. Airy now felt no doubt about the accuracy of both calculations; he still required to be satisfied about the error of the radius vector, however, and he accordingly addressed to Le Verrier the query that he had addressed to Adams, but this time in a more explicit form. He asked whether the errors of the tabular radius vector were the consequence of the disturbance produced by an exterior planet, and explained why, by analogy with the moon's variation, this did not seem to him necessarily to be so. Le Verrier replied a few days later giving an explanation which Airy found completely satisfactory. The errors of the tabular radius vector, said Le Verrier, were not produced actually by the disturbing planet; Bouvard's orbit required correction, because it had been based on positions which were not true elliptic positions, including, as they did, the perturbations by the outer planet; the correction of the orbit, which was needed on this account, removed the discordance between the observed and tabular radius vector.

Airy was a man of quick and incisive action. He was now fully convinced that the true explanation of the irregularities in the motion of

Uranus had been provided and he felt confident that the new planet would soon be found. He had already, a few days before receiving the reply from Le Verrier, informed the Board of Visitors of the Royal Observatory, at their meeting in June, of the extreme probability of discovering a new planet in a very short time. It was in consequence of this strongly expressed opinion of Airy that Sir John Herschel (a member of the Board) in his address on 10 September to the British Association, at its meeting at Southampton, said: 'We see it [a probable new planet] as Columbus saw America from the shores of Spain. Its movements have been felt, trembling along the far-reaching line of our analysis, with a certainty hardly inferior to that of ocular demonstration.'

Airy considered that the most suitable telescope with which to make the search for the new planet was the Northumberland telescope of the Cambridge Observatory, which was larger than any telescope at Greenwich and more likely to detect a planet whose light might be feeble. Airy offered to lend Challis one of his assistants, if Challis was too busy to undertake the search himself. He pointed out that the most favourable time for the search (when the undiscovered planet would be at opposition) was near at hand. A few days later, Airy sent Challis detailed directions for carrying out the search and in a covering letter said that, in his opinion, the importance of the inquiry exceeded that of any current work, which was of such a nature as not to be totally lost by delay.

Challis decided to prosecute the search himself and began observing on 29 July 1846, three weeks before opposition. The method adopted was to make three sweeps over the area to be searched, mapping the positions of all the stars observed, and completing each sweep before beginning the next. If the planet was observed it would be revealed, when the different sweeps were compared, by its motion relative to the stars.

What followed was not very creditable to Challis. He started by observing in the region indicated by Adams: the first four nights on which observations were made were 29 July, 30 July, 4 August and 12 August. But no comparison was made, as the search proceeded, between the observations on different nights. He did indeed make a partial comparison between the nights of 30 July and 12 August, merely to assure himself that the method of observation was adequate. He stopped short at No. 39 of the stars observed on 12 August; as he found that all these had been observed on 30 July, he felt satisfied about the method of observation. If he had continued the comparison for another ten stars he would have found that a star of the 8th magnitude observed on 12 August was missing in the series of 30 July. This was the planet: it had wandered into the zone between the two dates. Its discovery was thus easily within his grasp. But 12 August was not the first time on which Challis had observed the planet; he had already observed it on 4 August and if he had compared the observations of 4 August with the observations of either 30 July or of 12 August, the planet would have been detected.

When we recall Airy's strong emphasis on carrying on the search in preference to any current work, Challis's subsequent excuses to justify his failure were pitiable. He had delayed the comparisons, he said, partly from being occupied with comet reductions (which could well have waited), and partly from a fixed impression that a long search was required to ensure success. He confessed that, in the whole of the undertaking, he had too little confidence in the indications of theory. Oh! man of little faith! If only he had shared Airy's conviction of the great importance of the search.

But we have anticipated somewhat. While Challis was laboriously continuing his search, Adams wrote on 2 September an important letter to Airy who, unknown to Adams, was then in Germany. He referred to the assumption in his first calculations that the mean distance of the supposed disturbing planet was twice that of Uranus. The investigation, he said, could scarcely be considered satisfactory while based on anything arbitrary. He had therefore repeated his calculations, assuming a somewhat smaller mean distance. The result was very satisfactory in that the agreement between theory and observations was somewhat improved and, at the same time, the eccentricity of the orbit, which in the first solution had an improbably large value, was reduced. He gave the residuals for the two solutions, and remarked that the comparison with recent Greenwich observations suggested that a still better agreement could be obtained by a further reduction in the mean distance. He asked for the results of the Greenwich observations for 1844 and 1845. He then gave the corrections to the tabular radius vector of Uranus and remarked that they were in close agreement with those required by the Greenwich observations.

Two days earlier, on 31 August, Le Verrier had communicated a third paper to the French Academy which was published in a number of the *Comptes Rendus* that reached England near the end of September. Challis received it on 29 September. Le Verrier gave the orbital elements of the hypothetical planet, its mass, and its position. From the mass and distance of the planet he inferred, on the reasonable assumption that its mean density was equal to the mean density of Uranus, that it should show a disk with an angular diameter of about 3·3 sec. Le Verrier went on to remark as follows:

'It should be possible to see the new planet in good telescopes and also to distinguish it by the size of its disk. This is a very important point. For if the planet could not be distinguished by its appearance from the stars it would be necessary, in order to discover it, to examine all the small stars in the region of the sky to be explored, and to detect a proper motion of one of them. This work would be long and wearisome. But if, on the contrary, the planet has a sensible disk which prevents it from being confused with a star, if a simple study of its physical appearance can replace the rigorous determination of the positions of all the stars, the search will proceed much more rapidly.'

After reading this memoir on 29 September, Challis searched the same night in the region indicated by Le Verrier (which was almost identical with that indicated by Adams, in the first instance, a year earlier), looking out particularly for a visible disk. Of 300 stars observed he noted one and one only as seeming to have a disk. This was, in actual fact, the planet. Its motion might have been detected in the course of a few hours, but Challis waited for confirmation until the next night, when no observation was possible because the Moon was in the way. On 1 October he learnt that the planet had been discovered at Berlin on 23 September. His last chance of making an independent discovery had gone.

For on 23 September Galle, Astronomer at the Berlin Observatory, had received a letter from Le Verrier suggesting that he should search for the unknown planet, which would probably be easily distinguished by a disk. D'Arrest, a keen young volunteer at the Observatory, asked to share in the search, and suggested to Galle that it might be worth looking among the star charts of the Berlin Academy, which were then in course of publication, to verify whether the chart for Hour 21 was amongst those that were finished. It was found that this chart had been printed at the beginning of 1846, but had not yet been distributed; it was therefore available only to the astronomers at the Berlin Observatory. Galle took his place at the telescope, describing the configurations of the stars he saw, while d'Arrest followed them on the map, until Galle said: 'And then there is a star of the 8th magnitude in such a such a position', whereupon d'Arrest exclaimed: 'That star is not on the map.' An observation the following night showed that the object had changed its position and proved that it was the planet. Had this chart been available to Challis, as it would have been but for the delay in distribution, he would undoubtedly have found the planet at the beginning of August, some weeks before Le Verrier's third memoir was presented to the French Academy.

On 1 October, Le Verrier wrote to Airy informing him of the discovery of the planet. He mentioned that the Bureau des Longitudes had adopted the name Neptune, the figure a trident, and that the name Janus (which had also been suggested) would have the inconvenience of making it appear that the planet was the last in the solar system, which there was no reason to believe.

The discovery of the planet, following the brilliant researches of Le Verrier, which were known to the scientific world through their publication by the French Academy, was received with admiration and delight, and was acclaimed as one of the greatest triumphs of the human intellect. The prior investigations of Adams, his prediction of the position of the planet, the long patient search by Challis were known to only a few people in England. Adams had published nothing; he had communicated his results to Challis and to Airy, but neither of them knew anything of the details of his investigations; his name was unknown in astronomical circles outside his own country. Adams had actually drawn up a paper to be read at the meeting of the British Association at Southampton early in September, but he did not arrive in sufficient time to present it, as Section A closed its meetings one day earlier than he had expected.

The first reference in print to the fact that Adams had independently reached conclusions similar to those of Le Verrier was made in a letter from Sir John Herschel, published in the *Athenaeum* of 3 October. It came as a complete surprise to the French astronomers and ungenerous aspersions were cast upon the work of Adams. It was assumed that his solution was a crude essay which would not stand the test of rigorous examination and that, as he had not published any account of his researches, he could not establish a claim to priority or even to a share in the discovery. Some justification seemed to be afforded by an unfortunate letter from Challis to Arago, of 5 October, stating that he had searched for the planet, in conformity with the suggestions of Le Verrier, and had observed an object on 29 September which appeared to have a disk and which later was proved to have been the planet. No reference was made in this letter to the investigations of Adams or to his own earlier searches during which the planet had twice been observed. Airy, moreover, wrote to Le Verrier on 14 October, mentioning that collateral researches, which had led to the same result as his own, had been made in England. He went on to say: 'If in this I give praise to others I beg that you will not consider it as at all interfering with my acknowledgment of your claims. You are to be recognized, without doubt, as the real predictor of the planet's place. I may add that the English investigations, as I believe, were not so extensive as yours. They were known to me earlier than yours.' It is difficult to understand why Airy wrote in these terms; he had expressed the highest admiration for the manner in which the problem had been solved by Le Verrier, but he was not in a position to express any opinion about the work of Adams, which he had not yet seen.

At the meeting of the French Academy on 12 October, Arago made a long and impassioned defence of his protégé, Le Verrier, and a violent attack on Adams, referring scornfully to what he described as his *clandestine* work. 'Le Verrier is to-day asked to share the glory, so loyally, so rightly earned, with a young man who has communicated nothing to the public and whose calculations, more or less incomplete, are, with two exceptions, totally unknown in the observatories of Europe! No! no! the friends of science will not allow such a crying injustice to be perpetrated. He concluded by saying that Adams had no claim to be mentioned, in the history of Le Verrier's planet, by a detailed citation nor even by the slightest allusion. National feeling ran very high in France. The paper *Le National* asserted that the three foremost British astronomers (Herschel, Airy and Challis) had organized a miserable plot to steal the discovery from M. Le Verrier and that the researches of Adams were merely a myth invented for this purpose.

In England opinion was divided; some English astronomers contended that because Adams's results had not been publicly announced he could claim no share in the discovery. But for the most part it was considered that the credit for the successful prediction of the position of the unknown planet should be shared equally between Adams and Le Verrier. Adams himself took no part in the heated discussions which went on for some time with regard to the credit for the discovery of the new planet; he never uttered a single word of criticism or blame in connexion with the matter.

The controversy was lifted to a higher plane by a letter from Sir John Herschel to *The Guardian* in which he said:

'The history of this grand discovery is that of *thought* in one of its highest manifestations, of science in one of its most refined applications. So viewed, it offers a deeper interest than any personal question. In proportion to the importance of the step, it is surely interesting to know that more than one mathematician has been found capable of taking it. The fact, thus stated, becomes, so to speak, a measure of the maturity of our science; nor can I conceive anything better calculated to impress the general mind with a respect for the mass of accumulated facts, laws, and methods, as they exist at present, and the reality and efficiency of the forms into which they have been moulded, than such a circumstance. We need some reminder of this kind in England, where a want of faith in the higher theories is still to a certain degree our besetting weakness.'

At the meeting of the Royal Astronomical Society on 13 November 1846, three important papers were read. The first, by the Astronomer Royal, was an 'Account of some Circumstances historically connected with the Discovery of the Planet Exterior to Uranus'. All the correspondence with Adams, Challis and Le Verrier was given, as well as the two memoranda from Adams, the whole being linked together by Airy's own comments. The account made it perfectly clear that Adams and Le Verrier had independently solved the same problem, that the positions which they had assigned to the new planet were in close agreement, and that Adams had been the first to solve the problem. The second was Challis's 'Account of Observations undertaken in search of the Planet discovered at Berlin on September 23, 1846', which showed that in the course of the search for the planet, he had twice observed it before its discovery at Berlin, and that he had observed it a third time before the news of this discovery reached England. The third paper was by Adams and was entitled 'An Explanation of the observed Irregularities in the Motion of Uranus, on the Hypothesis of Disturbances caused by a more Distant Planet; with a determination of the Mass, Orbit, and Position of the Disturbing Body'.

Adams's memoir was a masterpiece; it showed a thorough grasp of the problem; a mathematical maturity which was remarkable in one so young; and a facility in dealing with complex numerical computations. Lieut. Stratford, Superintendent of the Nautical Almanac, reprinted it as an Appendix to the *Nautical Almanac* for 1851, then in course of publication, and sent sufficient copies to Schumacher, editor of the *Astronomische Nachrichten*, for distribution with that periodical. Hansen, the foremost exponent of the lunar theory, wrote to Airy to say that, in his opinion, Adams's investigation showed more mathematical genius than Le Verrier's. Airy, a competent judge, gave his own opinion in a letter to Biot, who had sent to Airy a paper he had written about the new planet. He sent it, he said, with some diffidence because he had expressed a more favourable opinion of the work of Adams than Airy had given. In reply, Airy wrote: 'On the whole I think his [Adams's] mathematical investigation superior to M. Le Verrier's. However, both are so admirable that it is difficult to say.' He went on to state that 'I believe I have done more than any other person to place Adams in his proper position'.

With the independent investigations of both men published, there was no difficulty in agreeing that each was entitled to an equal share of the honour. The verdict of history agrees with that of Sir John Herschel who, in addressing the Royal Astronomical Society in 1848, said:

'As genius and destiny have joined the names of Le Verrier and Adams, I shall by no means put them asunder; nor will they ever be pronounced apart so long as language shall celebrate the triumphs of science in her sublimest walks. On the great discovery of Neptune, which may be said to have surpassed, by intelligible and legitimate means, the wildest pretensions of clairvoyance, it would now be quite superfluous for me to dilate. That glorious event and the steps which led to it, and the various lights in which it has been placed, are already familiar to everyone having the least tincture of science. I will only add that as there is not, nor henceforth ever can be, the slightest rivalry on the subject of these two illustrious men—as they have met as brothers, and as such will, I trust, ever regard each other—we have made, we could make, no distinction between them on this occasion. May they both long adorn and augment our science, and add to their own fame, already so high and pure, by fresh achievements.'

Although on 1 October, Le Verrier had informed Airy that the Bureau des Longitudes had assigned the name Neptune to the new planet, Arago announced to the French Academy on 5 October that Le Verrier had delegated to him the right of naming the planet and that he had decided, in the exercise of this right, to call it Le Verrier. 'I pledge myself', he said, 'never to call the new planet by any other name than Le Verrier's Planet.' As though to justify this name, Le Verrier's collected memoirs on the perturbations of Uranus, which were published in the *Connaissance des Temps* for 1849 were given the title 'Recherches sur les mouvements de la planète Herschel (dite Uranus)' with a footnote to say that Le Verrier considered it as a strict duty, in future publications, to ignore the name of Uranus entirely and to call the planet only by the name of Herschel!

The name Le Verrier for the planet was not welcomed outside France. It was not in accordance with the custom of naming planets after mythological deities, and it ignored entirely the claims of Adams. Moreover, it might set a precedent. As Smyth said to Airy: 'Mythology is neutral ground. Herschel is a good name enough. Le Verrier somehow or other suggests a Fabriquant and is therefore not so good. But just think how awkward it would be if the next planet should be discovered by a German, by a Bugge, a Funk, or your hirsute friend Boguslawski!'

The widespread feeling against the name of Le Verrier was shared in Germany by Encke, Gauss and Schumacher and in Russia by Struve. Airy therefore wrote to Le Verrier:

'From my conversation with lovers of astronomy in England and from my correspondence with astronomers in Germany, I find that the name assigned by M. Arago is not well received. They think, in the first place, that the character of the name is at variance with that of the names of all the other planets. They think in the next place that M. Arago, as your delegate, could do only what you could do, and that you would not have given the name which M. Arago has given. They are all desirous of receiving a mythological name selected by you. In these feelings I do myself share. It was believed at first that you approved of the name Neptune, and in that supposition we have used the name Neptune when it was necessary to give a name. Now if it was understood that you still approve of the name Neptune (or Oceanus as some English mythologists suggested—or any other of the same class), I am sure that all England and Germany would adopt it at once. I am not sure that they will adopt the name which M. Arago has given.'

Airy might have added, but did not, that there was a general feeling, not merely in England, but also in Germany and Russia, that the name Le Verrier by implication denied any credit to the work of Adams and that, for this reason also, it was inappropriate.

Le Verrier, in reply, said that since one spoke of Comet Halley, Comet Encke, etc., he saw nothing inappropriate in Planet Le Verrier; that the

Bureau des Longitudes had given the name Neptune without his consent and had now withdrawn it; [1] and that, since he had delegated the selection of the name to Arago, it was a matter that no longer concerned him. At a later date, when relations between Arago and Le Verrier had become strained, the true story was told by Arago. It appears that Arago had at first agreed to the name Neptune, but Le Verrier had implored him, in order to serve him as a friend and as a countryman, to adopt the name Le Verrier. Arago had in the end agreed, but on condition that Uranus should always be called Planet Herschel, a name which Arago himself had frequently used. The greatest men are liable to human weaknesses and failings; Le Verrier was described by his friends as a *mauvais coucheur,* an uncomfortable bedfellow. By the general consensus of astronomers the name Neptune was adopted for the new planet; the name Le Verrier did not long survive.

In the history of the discovery of Neptune so many chances were missed which might have changed completely the course of events, that it is perhaps not surprising to find that the planet might have been discovered 50 years earlier. When sufficient observations of Neptune had been obtained to enable a fairly accurate orbit to be computed, a search was made to find out whether the planet had been observed as a star before its discovery. It was discovered that a star recorded in the *Histoire Céleste* of Lalande as having been observed on 10 May 1795 was missing in the sky; its position was marked as uncertain but was in close agreement with the position to be expected for Neptune. The original manuscripts of Lalande at the Paris Observatory were consulted; it was found that Lalande had, in fact, observed Neptune not only on 10 May but also on 8 May. The two positions, being found discordant and thought to refer to one and the same star, Lalande rejected the observation of 8 May and printed in the *Histoire Céleste* only the observation of 10 May marking it as doubtful, although it was not so marked in the manuscript. The change in position in the two days agreed closely with the motion of Neptune in the interval. If Lalande had taken the trouble to make a further observation to check the other two, he could scarcely have failed to discover the planet. Airy's comment, when sending the information about the two observations to Adams, was 'Let no one after this blame Challis'.

[1] This statement by Le Verrier was not correct. The minutes of the Bureau des Longitudes show that the Bureau had not considered assigning a name by 1 October, when Le Verrier had written not only to Airy but also to various other astronomers in Germany and Russia informing them that the Bureau des Longitudes 'had adopted the name Neptune, the figure a trident'. The Bureau neither assigned the name Neptune nor subsequently withdrew it. The minutes of the Bureau des Longitudes show that Le Verrier's statements were repudiated by the Bureau at a subsequent meeting. It seems that the name Neptune was Le Verrier's own choice in the first instance but that he soon decided that he would like the planet to be named Le Verrier. There is no explanation of his reasons for stating that the name Neptune had been assigned by the Bureau des Longitudes; it was, in fact, outside the competence of the Bureau to assign a name to a newly discovered planet.

COMMENTARY ON
H. G. J. MOSELEY

ON August 10, 1915, the twenty-eight-year-old British physicist, Henry Gwyn-Jeffreys Moseley, died in the trenches of Gallipoli. This extraordinarily gifted young scientist had left the laboratory of Sir Ernest Rutherford in Manchester to join the army in 1914. He was the victim not only of a Turkish bullet but of an incomparably stupid recruitment program which permitted a scientist of such proven talent to become a soldier. For Moseley's brilliance was known. He had worked with Rutherford for some years, and at the age of twenty-seven had made discoveries in physics as important as any achieved in this century. Thus was squandered a life of the greatest promise to the history of science. It was from such spectacular instances of waste that both Great Britain and the U. S. learned to keep their best scientists out of the firing lines of the Second World War.

Moseley's researches on atomic structure are reported in two communications to the *Philosophical Magazine*, from which the excerpts below have been selected. Although comparatively easy to explain, they are written in the usual terse and difficult idiom of the physicist. Therefore, I had better summarize their content and say a few words about how Moseley's work fitted into the scene of contemporary physics.

After Roentgen discovered X-rays in 1895 a number of scientists devoted themselves to comparing these radiations with light waves. Attempts were made to reflect and refract X-rays, to determine whether they produced interference phenomena as does light. The attempts failed. If a parallel beam of light is allowed to fall on a grating, a surface on which many thousands of fine lines have been drawn to the inch, the transmitted or reflected light is broken up into its component colors, or wave lengths, and appears as a spectrum. When this procedure was applied to X-rays it also failed. These experiments indicated either that X-rays were not waves or that the waves were so short that the methods of study used for light were unsuitable. In 1912 Max von Laue suggested that, while a man-made grating was too crude for the job, a grating provided by nature in the form of crystals might catch waves less than a thousandth as long as the shortest light waves. Von Laue worked out the mathematics of his theory and Friedrich and Knipping confirmed it in a series of beautiful experiments. The array of regular lines of atoms in the crystal served as a supergrating ("of very minute dimensions") and yielded characteristic diffraction spectra for X-rays. These spectra could be photographed and their lines analyzed. Thus, the wave nature of X-rays was established and, incidentally, a way was opened into the great new field of the structure of crystals.[1]

Now we come to Moseley's experiments. X-rays are produced by permitting a stream of electrons emitted by the negative pole (cathode) in a vacuum tube to impinge on a solid target. Roentgen had used platinum for the target but it soon became clear that if other elements were substituted they would also respond to cathode bombardment by emitting X-rays. Moseley successively used forty-two different elements as targets. He passed each set of X-rays through a crystal, photographed the resulting diffraction patterns and analyzed the waves emanating from each sample. He noticed that his figures were falling into a remarkable order. As he moved element by element up the Periodic Table—in which the elements are arranged by atomic weight and bracketed in groups according to their chemical properties—he found a regular increase in the square roots of the frequency of vibration of the characteristic spectrum lines. If this square root is multiplied by an appropriate constant so as to convert the regular increase to unity, one gets what is known as the series of atomic numbers ranging from 1 for hydrogen to 98 for californium. (Moseley went only as far as 79, for gold, when he was called away on more urgent business. The remaining places in the table were gradually filled in by other physicists.)

What are these numbers which match so smoothly the order of the Periodic Table? (A few irregularities crop up but they have been explained and do not shake the theory.) The little arithmetic trick of converting the increase to unity should not mislead anyone to believe that the numbers merely represent a superimposed order. For the fact is that the atomic number of each element, representing the square root of frequency of an atom "suitably excited" to emit X radiation, assigns to the element its true place in the Table because it is the number of the unit positive charges in the nucleus of the atom. It is this charge which determines the element's chemical behavior. All subsequent advances in nuclear physics depend upon and are an outgrowth of the fundamental insight gained by Moseley. The beauty and simplicity of his theory, its revelation of the existence of an almost uncanny step-by-step order in the arrangement of the basic structures of matter, would have pleased the ancient Greek philosophers who held that number ruled the universe. "In our own day," says Dampier, "Aston with his integral atomic weights, Moseley with his atomic numbers, Planck with his quantum theory, and Einstein with his claim that physical facts such as gravitation are exhibitions of local space-time properties, are reviving ideas that, in older, cruder forms, appear in Pythagorean philosophy."

[1] For a further discussion of X-rays, von Laue's experiment, and the structure of crystals, see the selections by Bragg and Le Corbeiller, and the introduction preceding them, pp. 851–881.

The most important discoveries of the laws, methods and progress of Nature have nearly always sprung from the examination of the smallest objects which she contains. —J. B. LAMARCK

5 Atomic Numbers

By H. G. J. MOSELEY

THE HIGH FREQUENCY SPECTRA OF THE ELEMENTS

IN the absence of any available method of spectrum analysis, the characteristic types of X radiation, which an atom emits if suitably excited, have hitherto been described in terms of their absorption in aluminium. The interference phenomena exhibited by X-rays when scattered by a crystal have now, however, made possible the accurate determination of the frequencies of the various types of radiation. This was shown by W. H. and W. L. Bragg, who by this method analysed the line spectrum emitted by the platinum target of an X-ray tube. C. G. Darwin and the author extended this analysis and also examined the continuous spectrum, which in this case constitutes the greater part of the radiation. Recently Prof. Bragg has also determined the wave-lengths of the strongest lines in the spectra of nickel, tungsten, and rhodium. The electrical methods which have hitherto been employed are, however, only successful where a constant source of radiation is available. The present paper contains a description of a method of photographing these spectra, which makes the analysis of the X-rays as simple as any other branch of spectroscopy. The author intends first to make a general survey of the principal types of high-frequency radiation, and then to examine the spectra of a few elements in greater detail and with greater accuracy. The results already obtained show that such data have an important bearing on the question of the internal structure of the atom, and strongly support the views of Rutherford and of Bohr.

Kaye has shown that an element excited by a stream of sufficiently fast cathode rays emits its characteristic X radiation. He used as targets a number of substances mounted on a truck inside an exhausted tube. A magnetic device enabled each target to be brought in turn into the line of fire. This apparatus was modified to suit the present work. The cathode stream was concentrated on to a small area of the target, and a platinum plate furnished with a fine vertical slit placed immediately in front of the part bombarded. The tube was exhausted by a Gaede mercury pump, charcoal in liquid air being also sometimes used to remove water vapour. The X-rays, after passing through the slit marked S in Figure 1, emerged

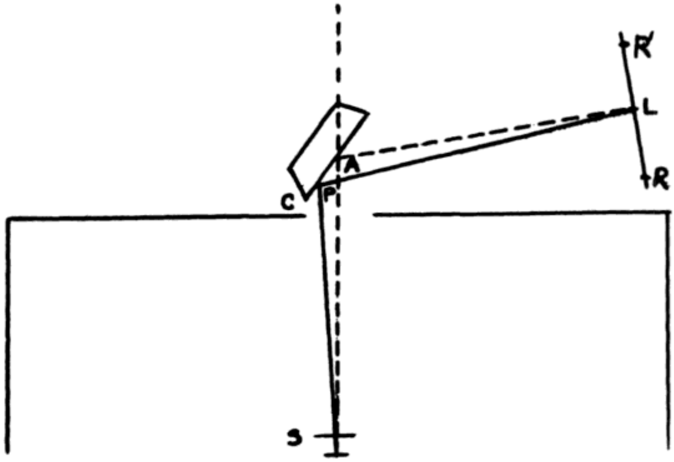


FIGURE 1

through an aluminum window of ·02 mm. thick. The rest of the radiation was shut off by a lead box which surrounded the tube. The rays fell on the cleavage face, *C*, of a crystal of potassium ferrocyanide which was mounted on the prism-table of a spectrometer. The surface of the crystal was vertical and contained the geometrical axis of the spectrometer.

Now it is known that X-rays consist in general of two types, the heterogeneous radiation and characteristic radiations of definite frequency. The former of these is reflected from such a surface at all angles of incidence, but at the large angles used in the present work the reflexion is of very little intensity. The radiations of definite frequency, on the other hand, are reflected only when they strike the surface at definite angles, the glancing angle of incidence θ, the wave-length λ, and the "grating constant" d of the crystal being connected by the relation

$$n\lambda = 2d \sin \theta, \qquad \ldots\ldots\ldots\ldots\ldots(1)$$

where n, an integer, may be called the "order" in which the reflexion occurs. The particular crystal used, which was a fine specimen with face 6 cm. square, was known to give strong reflexions in the first three orders, the third order being the most prominent.

If then a radiation of definite wave-length happens to strike any part *P* of the crystal at a suitable angle, a small part of it is reflected. Assuming for the moment that the source of the radiation is a point, the locus of *P* is obviously the arc of a circle, and the reflected rays will travel along the generating lines of a cone with apex at the image of the source. The effect on a photographic plate *L* will take the form of the arc of an hyperbola, curving away from the direction of the direct beam. With a fine slit at S, the arc becomes a fine line which is slightly curved in the direction indicated.

The photographic plate was mounted on the spectrometer arm, and both the plate and the slit were 17 cm. from the axis. The importance of this arrangement lies in a geometrical property, for when these two distances are equal the point L at which a beam reflected at a definite angle strikes the plate is independent of the position of P on the crystal surface. The angle at which the crystal is set is then immaterial so long as a ray can strike some part of the surface at the required angle. The angle θ can be obtained from the relation $2\theta = 180° - SPL = 180° - SAL$.

The following method was used for measuring the angle SAL. Before taking a photograph a reference line R was made at both ends of the plate by replacing the crystal by a lead screen furnished with a fine slit which coincided with the axis of the spectrometer. A few seconds' exposure to the X-rays then gave a line R on the plate, and so defined on it the line joining S and A. A second line RQ was made in the same way after turning the spectrometer arm through a definite angle. The arm was then turned to the position required to catch the reflected beam and the angles LAP for any lines which were subsequently found on the plate deduced from the known value of RAP and the position of the lines on the plate. The angle LAR was measured with an error of not more than $0°{\cdot}1$, by superposing on the negative a plate on which reference lines had been marked in the same way at intervals of $1°$. In finding from this the glancing angle of reflexion two small corrections were necessary in practice, since neither the face of the crystal nor the lead slit coincided accurately with the axis of the spectrometer. Wave-lengths varying over a range of about 30 per cent. could be reflected for a given position of the crystal.

In almost all cases the time of exposure was five minutes. Ilford X-ray plates were used and were developed with rodinal. The plates were mounted in a plate-holder, the front of which was covered with black paper. In order to determine the wave-length from the reflexion angle θ it is necessary to know both the order n in which the reflexion occurs and the grating constant d. n was determined by photographing every spectrum both in the second order and the third. This also gave a useful check on the accuracy of the measurements; d cannot be calculated directly for the complicated crystal potassium ferrocyanide. The grating constant of this particular crystal had, however, previously been accurately compared with d', the constant of a specimen of rock-salt. It was found that

$$d = 3d' \frac{{\cdot}1988}{{\cdot}1985}.$$

Now W. L. Bragg has shown that the atoms in a rock-salt crystal are in simple cubical array. Hence the number of atoms per c.c.

$$2\frac{N\sigma}{M}=\frac{1}{(d')^3}:$$

N, the number of molecules in a gram-mol., $= 6{\cdot}05 \times 10^{23}$ assuming the charge (e) on an electron to be $4{\cdot}89 \times 10^{-10}$; σ, the density of this crystal of rock-salt, was 2·167, and M the molecular weight = 58·46.

This gives $d' = 2{\cdot}814 \times 10^{-8}$ and $d = 8{\cdot}454 \times 10^{-8}$ cm. It is seen that the determination of wave-length depends on $e^{1/3}$ so that the effect of uncertainty in the value of this quantity will not be serious. Lack of homogeneity in the crystal is a more likely source of error, as minute inclusions of water would make the density greater than that found experimentally.

Twelve elements have so far been examined. . . .

The Plate shows the spectra in the third order placed approximately in register. Those parts of the photographs which represent the same angle of reflexion are in the same vertical line. . . . It is to be seen that the spectrum of each element consists of two lines. Of these the stronger has been called α in the table, and the weaker β. The lines found on any of the plates besides α and β were almost certainly all due to impurities. Thus in both the second and third order the cobalt spectrum shows Niα very strongly and Feα faintly. In the third order the nickel spectrum shows Mnα_2 faintly. The brass spectra naturally show α and β both of Cu and of Zn, but Znβ_2 has not yet been found. In the second order the ferro-vanadium and ferro-titanium spectra show very intense third-order Fe lines, and the former also shows Cuα_3 faintly. The Co contained Ni and 0·8 per cent. Fe, the Ni 2·2 per cent. Mn, and the V only a trace of Cu. No other lines have been found; but a search over a wide range of wave-lengths has been made only for one or two elements, and perhaps prolonged exposures, which have not yet been attempted, will show more complex spectra. The prevalence of lines due to impurities suggests that this may prove a powerful method of chemical analysis. Its advantage over ordinary spectroscopic methods lies in the simplicity of the spectra and the impossibility of one substance masking the radiation from another. It may even lead to the discovery of missing elements, as it will be possible to predict the position of their characteristic lines. . . .

Phil. Mag. (1914), p. 703.

The first part of this paper dealt with a method of photographing X-ray spectra, and included the spectra of a dozen elements. More than thirty other elements have now been investigated, and simple laws have been found which govern the results, and make it possible to predict with

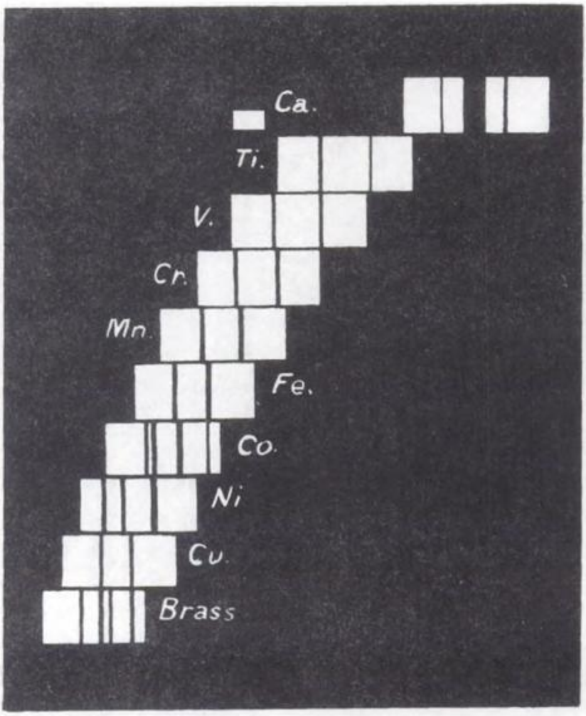


FIGURE 2—X-ray spectra.

confidence the position of the principal lines in the spectrum of any element from aluminium to gold. The present contribution is a general preliminary survey, which claims neither to be complete nor very accurate. . . .

The results obtained for radiations belonging to Barkla's K series are given in Table I, and for convenience the figures already given in Part I are included. The wave-length λ has been calculated from the glancing angle of reflexion θ by means of the relation $n\lambda = 2d \sin \theta$, where d has been taken to be $8{\cdot}454 \times 10^{-8}$ cm. As before, the strongest line is called α and the next line β. The square root of the frequency of each line is plotted in Figure 3, and the wave-lengths can be read off with the help of the scale at the top of the diagram.

The spectrum of Al was photographed in the first order only. The very light elements give several other fainter lines, which have not yet been fully investigated, while the results for Mg and Na are quite complicated, and apparently depart from the simple relations which connect the spectra of the other elements. In the spectra from yttrium onwards only the α line has so far been measured, and further results in these directions will be

TABLE I

	α line $\lambda \times 10^8$ cm.	Q_K	N Atomic Number	β line $\lambda \times 10^8$ cm.
Aluminium	8·364	12·05	13	7·912
Silicon	7·142	13·04	14	6·729
Chlorine	4·750	16·00	17	—
Potassium	3·759	17·98	19	3·463
Calcium	3·368	19·00	20	3·094
Titanium	2·758	20·99	22	2·524
Vanadium	2·519	21·96	23	2·297
Chromium	2·301	22·98	24	2·093
Manganese	2·111	23·99	25	1·818
Iron	1·946	24·99	26	1·765
Cobalt	1·798	26·00	27	1·629
Nickel	1·662	27·04	28	1·506
Copper	1·549	28·01	29	1·402
Zinc	1·445	29·01	30	1·306
Yttrium	0·838	38·1	39	—
Zirconium	0·794	39·1	40	—
Niobium	0·750	40·2	41	—
Molybdenum	0·721	41·2	42	—
Ruthenium	0·638	43·6	44	—
Palladium	0·584	45·6	46	—
Silver	0·560	46·6	47	—

TABLE II

	α line $\lambda \times 10^8$ cm.	Q_L	N Atomic Number	β line $\lambda \times 10^8$ cm.	φ line $\lambda \times 10^8$ cm.	γ line $\lambda \times 10^8$ cm.
Zirconium	6·091	32·8	40	—	—	—
Niobium	5·749	33·8	41	5·507	—	—
Molybdenum	5·423	34·8	42	5·187	—	—
Ruthenium	4·861	36·7	44	4·660	—	—
Rhodium	4·622	37·7	45	—	—	—
Palladium	4·385	38·7	46	4·168	—	3·928
Silver	4·170	39·6	47	—	—	—
Tin	3·619	42·6	50	—	—	—
Antimony	3·458	43·6	51	3·245	—	—
Lanthanum	2·676	49·5	57	2·471	2·424	2·313
Cerium	2·567	50·6	58	2·366	2·315	2·209
Praseodymium	(2·471)	51·5	59	2·265	—	—
Neodymium	2·382	52·5	60	2·175	—	—
Samarium	2·208	54·5	62	2·008	1·972	1·893
Europium	2·130	55·5	63	1·925	1·888	1·814
Gadolinium	2·057	56·5	64	1·853	1·818	—
Holmium	1·914	58·6	66	1·711	—	—
Erbium	1·790	60·6	68	1·591	1·563	—
Tantalum	1·525	65·6	73	1·330	—	1·287
Tungsten	1·486	66·5	74	—	—	—
Osmium	1·397	68·5	76	1·201	—	1·172
Iridium	1·354	69·6	77	1·155	—	1·138
Platinum	1·316	70·6	78	1·121	—	1·104
Gold	1·287	71·4	79	1·092	—	1·078

given in a later paper. The spectra both of K and Cl were obtained by means of a target of KCl, but it is very improbable that the observed lines have been attributed to the wrong elements. The α line for elements from Y onwards appeared to consist of a very close doublet, an effect previously observed by Bragg in the case of rhodium.

The results obtained for the spectra of the L series are given in Table II and plotted in Figure 3. These spectra contain five lines, α, β, γ, δ, ϵ, reckoned in order of decreasing wave-length and decreasing intensity. There is also always a faint companion α' on the long wave-length side of α, a rather faint line ϕ between β and γ for the rare earth elements at least, and a number of very faint lines of wave-length greater than α. Of these, α, β, ϕ, and γ have been systematically measured with the object of finding out how the spectrum alters from one element to another. The fact that often values are not given for all these lines merely indicates the incompleteness of the work. The spectra, so far as they have been examined, are so entirely similar that without doubt α, β, and γ at least always exist. Often γ was not included in the limited range of wave-lengths which can be photographed on one plate. Sometimes lines have not been measured, either on account of faintness or of the confusing proximity of lines due to impurities. . . .

CONCLUSIONS

In Figure 3 the spectra of the elements are arranged on horizontal lines spaced at equal distances. The order chosen for the elements is the order of the atomic weights, except in the cases of A, Co, and Te, where this clashes with the order of the chemical properties. Vacant lines have been left for an element between Mo and Ru, an element between Nd and Sa, and an element between W and Os, none of which are yet known, while Tm, which Welsbach has separated into two constituents, is given two lines. This is equivalent to assigning to successive elements a series of successive characteristic integers. On this principle the integer N for Al, the thirteenth element, has been taken to be 13, and the values of N then assumed by the other elements are given on the left-hand side of Figure 3. This proceeding is justified by the fact that it introduces perfect regularity into the X-ray spectra. Examination of Figure 3 shows that the values of $\nu^{1/2}$ for all the lines examined both in the K and the L series now fall on regular curves which approximate to straight lines. The same thing is shown more clearly by comparing the values of N in Table I with those of

$$Q_K = \sqrt{\frac{\nu}{\frac{3}{4}\nu_0}},$$

ν being the frequency of the line and ν_0 the fundamental Rydberg frequency. It is here plain that $Q_K = N - 1$ very approximately, except for

High-Frequency Spectra of the Elements.

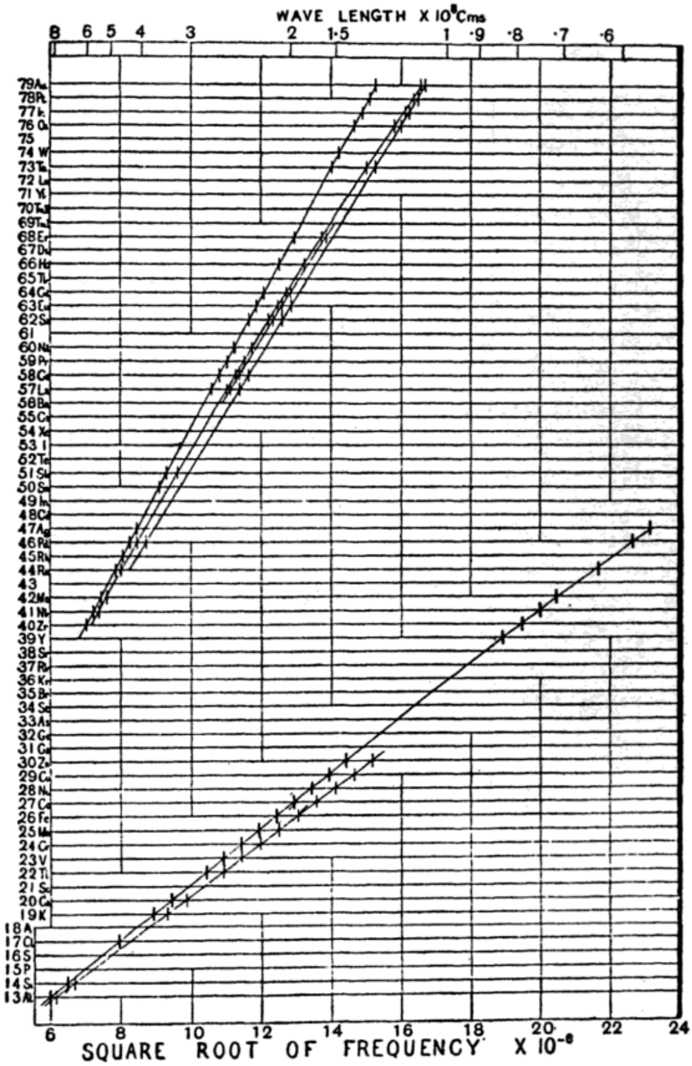


FIGURE 3

the radiations of very short wave-length which gradually diverge from this relation. Again, in Table II a comparison of N with

$$Q_L = \sqrt{\frac{\nu}{{}^5\!/_{36}\nu_0}},$$

where ν is the frequency of the Lα line, shows that $Q_L = N - 7{\cdot}4$ approximately, although a systematic deviation clearly shows that the relation is not accurately linear in this case.

Now if either the elements were not characterized by these integers, or any mistake had been made in the order chosen or in the number of places left for unknown elements, these regularities would at once disappear. We can therefore conclude from the evidence of the X-ray spectra alone, without using any theory of atomic structure, that these integers are really characteristic of the elements. Further, as it is improbable that two different stable elements should have the same integer, three, and only three, more elements are likely to exist between Al and Au. As the X-ray spectra of these elements can be confidently predicted, they should not be difficult to find. The examination of keltium would be of exceptional interest, as no place has been assigned to this element.

Now Rutherford has proved that the most important constituent of an atom is its central positively charged nucleus, and van den Broek has put forward the view that the charge carried by this nucleus is in all cases an integral multiple of the charge on the hydrogen nucleus. There is every reason to suppose that the integer which controls the X-ray spectrum is the same as the number of electrical units in the nucleus, and these experiments therefore give the strongest possible support to the hypothesis of van den Broek. Soddy has pointed out that the chemical properties of the radio-elements are strong evidence that this hypothesis is true for the elements from thaliium to uranium, so that its general validity would now seem to be established.

COMMENTARY ON

The Small Furniture of Earth

THE next two selections deal with the wave theory of X-rays and the atomic theory of crystals. The bringing together and verification of these theories in a single famous experiment was one of the major events of the twentieth-century renaissance in physics.

Crystals were the subject of much attention in the eighteenth and nineteenth centuries, their optical properties and geometric relations being carefully studied by mineralogists, crystallographers and mathematicians. As early as 1824 the hypothesis was advanced that crystals consist of layers of atoms distributed in regular patterns;[1] in the 1890s mathematicians had worked out fully the number of possible ways the atoms inside a crystal could be distributed. However, experimental proof of these fundamental ideas was still lacking in 1910. Attempts to confirm the theory of crystals by diffraction experiments with light were unsuccessful because the waves were too long for the job; it was as if one were to try to measure the inside of a thimble with a foot-rule.

Another important hypothesis, also in a doubtful status at that time, related to X-rays. The majority of physicists were convinced that X-rays were waves of a length much shorter than light rays, but again early attempts to prove the conjecture were unsuccessful.

The crucial experiment testing both hypotheses was proposed by the German physicist Von Laue, who was then assistant lecturer in Professor Sommerfeld's department at the University of Munich.[2] He reasoned that if X-rays were short waves and crystals three-dimensional lattices of atoms, a pencil of X-rays passed through a crystal would produce a characteristic pattern on a photographic plate. The X-rays, in other words, could be made to report the arrangement of the tiny furniture encountered inside the crystal. The experiment, performed in 1912, was an extraordinary triumph.[3] The X-rays portrayed the interior of the crystals, and the crystals repaid the favor by disclosing the form of the X-rays. Von Laue

[1] Max von Laue, *History of Physics*, New York, 1950, p. 119: "The first scientist to combine the newly created concept of the chemical atom with this idea [of the building-block structure of crystals] and to assume that space lattices are made up of chemical atoms was the physicist Ludwig August Seeber. . . . He published his ideas in 1824, i.e., thirty-two years prior to the entry of atomistics into modern physics in the form of the kinetic theory of gases."

[2] See also the introduction to the paper by H. G. J. Moseley, pp. 840–841.

[3] The experiment, though conceived by Laue, was actually performed by two young Munich research students, Friedrich and Knipping, who had just taken their doctorates under Röntgen. See Kathleen Lonsdale, *Crystals and X-Rays*, London, 1948, pp. 1–22, for an authoritative historical introduction to the subject; also the standard work for the advanced student: Sir Lawrence Bragg, *The Crystalline State, a General Survey*, London, 1949.

described this reciprocal disclosure as "one of those surprising events to which physics owes its power of conviction."

The first of the selections below, on Von Laue's experiment and its relation to the geometry of X-rays and crystals, is by the great physicist Sir William Bragg. The history of modern crystallography is in large part the history of his researches. Bragg was born in Cumberland in 1862 and after a brilliant school career accepted a professorship in physics at the University of Adelaide, Australia. His first research paper did not appear, remarkably enough, until 1904, when he was forty-two. It is unusual for a physicist so long to defer his original investigations, but the paper itself—it was concerned with alpha particles—was immediately recognized as a first-class achievement and marked the beginning of a prolific output of creative studies. In 1907 Bragg was elected a Fellow of the Royal Society, and a year later returned to England to take the Cavendish chair at Leeds. It was there that he became interested in X-rays, which he then regarded, contrary to prevailing opinion, as particles rather than waves. The Laue experiment of 1912 convinced him he was mistaken, and in the same year Bragg and his son took up the research on X-rays and crystals for which in 1915 they jointly received the Nobel prize in physics. Their work "laid the foundation of one of the most beautiful structures of modern science"; it was used for fundamental advances in both inorganic and organic chemistry, metallurgy is deeply indebted to it, as are other branches of pure and applied science.[4]

Bragg had a crowded and happy career during which he worked on many other subjects besides X-rays and raised a crop of distinguished pupils who enriched many parts of physics. In 1915 he held a chair at University College, London; during the First World War he directed acoustic research on submarine detection; in 1923 he became director of the Royal Institution and of the Davy-Faraday Research Laboratory. Not the least of his gifts was in popular exposition. He enjoyed nothing more than to give lectures and experimental demonstrations to youngsters and general audiences; his connection with the Royal Institution (he served as president from 1935 to 1940) fortunately facilitated his exercise of this art.

The article which follows is a chapter from Bragg's book, *The Universe of Light*, based on Christmas Lectures delivered in 1931.

The second selection is a simple and attractive account by Philippe Le Corbeiller of the mathematics of crystals. A suitable complement to Bragg's discussion, it emphasizes the experimental proof of the theory of space groups. In the development of crystallography the mathematics of group theory and of symmetry has, as I remarked earlier, played a remarkable part. Just as Adams and Leverrier, for example, decreed the motion

[4] E. N. daC. Andrade, "Sir William Bragg" (obituary), *Nature*, March 28, 1942.

and position of the planet Neptune before it was discovered, so mathematicians by an exhaustive logical analysis of certain properties of space and of the possible transformations (motions) within space, decreed the permissible variations of internal structure of crystals before observers were able to discover their actual structure. Mathematics, in other words, not only enunciated the applicable physical laws, but provided an invaluable syllabus of research to guide future experimenters. The history of the physical sciences contains many similar instances of mathematical prevision. Models, concepts, theories are initially expressed in mathematical form; later they are tested by observation and either confirmed, or disproved and discarded. Not uncommonly, of course, the model is overhauled to conform more closely to experimental data and the tests are then repeated. The theory of space groups and crystal classes is among the most successful and striking examples of mathematical model making.

Mr. Le Corbeiller is professor of general education and of applied physics at Harvard University. He was born and educated in France, has served as a member of the engineering staff of the *Département des Communications*, as an official of the French Government Broadcasting System and in other administrative and academic posts. His publications include papers on algebra, number theory, oscillating generators, electroacoustics and other scientific topics. The article on crystals, cast in the congenial framework of a mock-Socratic dialogue, first appeared in *Scientific American*, January 1953.

Why think? Why not try the experiment? —JOHN HUNTER (*Letter to Edward Jenner*)

The true worth of an experimenter consists in his pursuing not only what he seeks in his experiment, but also what he did not seek.
—CLAUDE BERNARD

6 The Röntgen Rays

By SIR WILLIAM BRAGG

. . . X-RAYS are generally produced as a consequence of the electric spark or discharge in a space where the pressure of the air or other gas is extremely low. The electric spark has for centuries been a subject of interested observation, but no great step forward was made until it was arranged that the discharge should take place in a glass tube or bulb from which the air had been pumped out more or less completely. The spark became longer, wider, and more highly coloured as the pressure

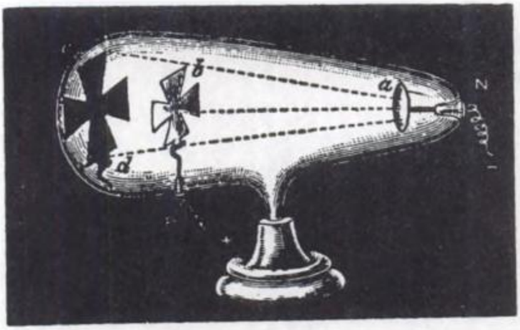


FIGURE 1—The cathode or negative terminal is on the right at *a*. The rays proceed in straight lines across the tube and excite fluorescence on the opposite wall. A metal cross *b* casts a sharp shadow.

diminished. When Crookes so improved the air pump that pressures of the order of the millionth part of atmospheric pressure became attainable a phenomenon appeared which had not been previously observed. The negative terminal became the source of a radiation which shot in a straight line across the bulb and had mechanical effects. It generated heat whenever it struck the opposite wall or some body placed to intercept it: it excited vivid fluorescence in glass and many minerals: it could turn a light mill wheel if it struck the vanes. And, a most important property, the stream could be deflected by bringing a magnet near it. This was an extremely important observation for it suggested that the stream consisted of electrified particles in flight. Such a stream would be equivalent to an electric current and would therefore be susceptible to the force of a magnet. Illustrations of Crookes's experiments are given in Figures 1, 2, 3, and 4. They are taken from the original blocks used in the published

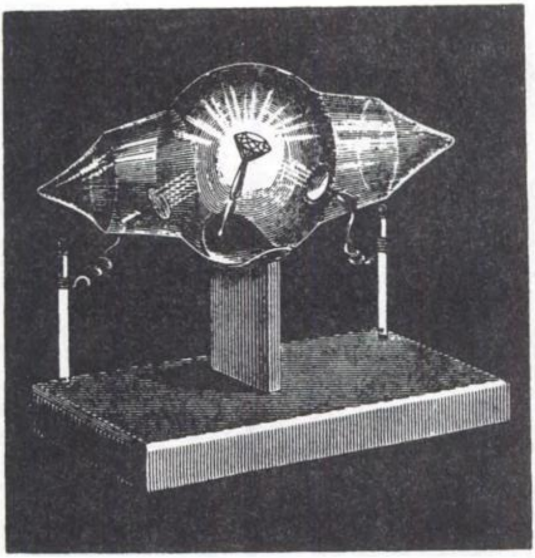


FIGURE 2—The rays excite a vivid fluorescence in the diamond mounted in the centre of the tube.

account of a discourse which he gave at the Royal Institution in April 1879. Crookes believed that the stream consisted of molecules of some kind. He argued that his air pump had attained such perfection that the comparatively few molecules left in the tube could move over distances comparable with the length of the tube without coming into collision with other molecules. Such a condition, he said, was as different from that of a gas as the latter from that of a liquid. At the end of a paper contributed to the Royal Society in the same year (1879) he wrote in a dim but interesting foreshadowing of the future which was partly to be verified:

'The phenomena in these exhausted tubes reveal to physical science a new world—a world where matter exists in a fourth state, where the corpuscular theory of light holds good, and where light does not always move in a straight line; but where we can never enter, and in which we must be content to observe and experiment from the outside.'

J. J. Thomson, Wiechert and others showed that the stream consisted of particles carrying negative charges of electricity, and that these carriers were far smaller than even the hydrogen atom. The name 'electron' was

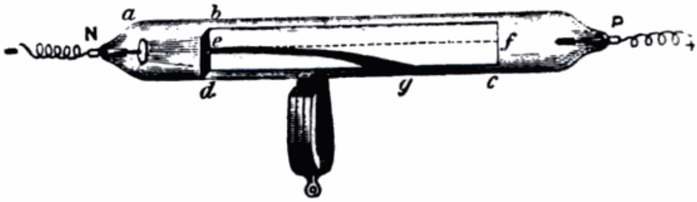


FIGURE 3—The cathode rays are limited to a narrow pencil by means of a slot placed in front of the cathode at *a*. The deflections of the stream by a horseshoe magnet are then observed easily.

given to them. It appeared that electrons could be torn away from any kind of atom, if sufficient electric force was supplied by the induction coil or other electric contrivance for producing the requisite power; and that the electrons from all sources were exactly alike. Evidently the electron was a fundamental constituent of matter. The stream of electrons was called the cathode ray because it issued from the negative or cathode terminal.

Röntgen was investigating the cathode ray phenomena when he found that photographic plates near by became fogged although no light could

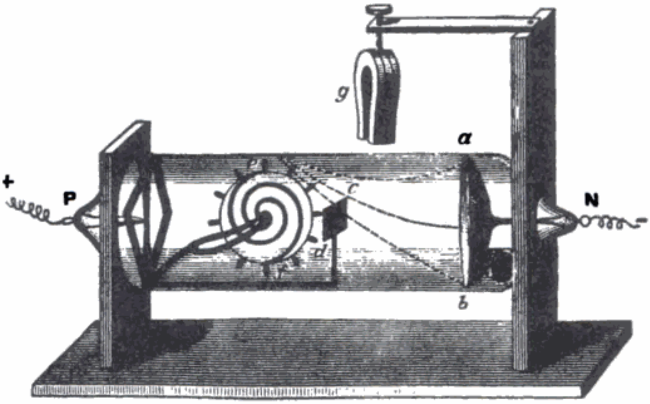


FIGURE 4—The cathode *a* is made in the shape of a saucer: this is found to have the effect of concentrating the rays to a point. Normally a screen *c* intercepts the rays, but the magnet *g* deflects them so that they get over the top of the screen and strike the vanes of the little wheel *ef*, which then spins rapidly. If the disposition of the magnet is reversed, the rays go under *c* and the wheel spins the opposite way.

have reached them. He traced the cause to a radiation issuing from his glass bulb, and in particular from the place where the cathode rays struck the wall: and proceeded to examine the general characteristics of the new rays upon which he had stumbled.

In many respects they resembled light. They moved in straight lines and cast sharp shadows, they traversed space without any obvious transference of matter, they acted on a photographic plate, they excited certain materials to fluorescence, and they could, like ultra-violet light, discharge electricity from conductors. In other ways the rays seemed to differ from light. The mirrors, prisms and lenses which deflected light had no such action on X-rays: gratings as ordinarily constructed did not diffract them: neither double refraction, nor polarisation was produced by the action of crystals. Moreover they had an extraordinary power of penetrating matter. Nothing seemed to hold them up entirely, though everything exerted some power of absorption: heavier atoms were more effective than lighter. Hence arose the quickly observed power of revealing the inner constitution of bodies opaque to light: bones cast shadows much deeper than those due to the surrounding flesh.

If the velocity of X-rays could have been shown without question to have been the same as that of light it would have established their identity: but the experiment though attempted was too difficult. Barkla showed that a pencil of X-rays could have 'sides' or be polarised if the circumstances of their origin were properly arranged, but the polarisation differed in some of its aspects from that which light could be made to exhibit. Laue's experiment brought the controversy to an end, by proving that a diffraction of X-rays could be produced which was in every way parallel to the diffraction of light: if the diffraction phenomena could be depended upon to prove the wave theory of light, exactly the same evidence existed in favour of a wave theory of X-rays.

LAUE'S EXPERIMENT

Let us now consider the details of Laue's famous experiment which has had such striking consequences. The plan of it was very simple. A fine pencil of X-rays was to be sent through a crystal and a photographic

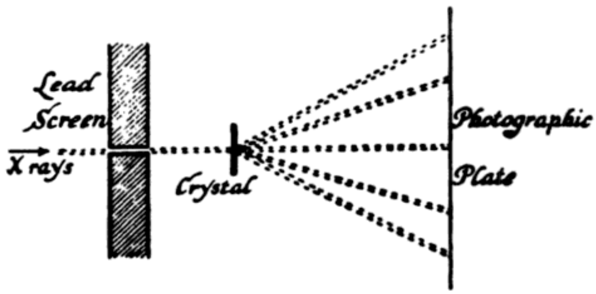


FIGURE 5—The X-rays pass through a fine opening in a lead screen and impinge upon the crystal as shown. Laue's diffraction pattern is formed upon the photographic plate.

plate was to be placed to receive the pencil after it had emerged on the other side, Figure 5. Laue surmised that beside the main image on the plate which would be caused by the incidence of the pencil there might be other subsidiary images. He based his forecast on a consideration of effects of this kind, which occur in the case of light. When a train of ether waves falls upon a plate on which parallel lines are ruled, or is transmitted through such a plate, or passes through an atmosphere in which fine particles of uniform size are suspended, there are regular deflections of the energy in various directions constituting 'diffracted' pencils. . . . In all cases of this kind it is a necessity that there should be no great disparity between the length of the wave on the one hand, and that of the regular spacing or particle diameter on the other. Laue thought that previous failures to find diffraction in the case of X-rays might be due to a want of observance of this condition. If the X-ray wave-lengths were thousands of times shorter than those of light, as he had reason for believing, it was useless to look for diffraction effects with ordinary gratings in the ordinary way. One ought to employ gratings in which the lines were drawn thousands of times closer together than in the usual practice. This is not practicable: no one can draw millions of parallel lines to the inch.

It was possible, however, that Nature had already provided the tool which could not be constructed in the workshop. The crystal might be the appropriate grating for the X-rays, because its atoms were supposed to be in regular array, and the distances that separated them were, so far as could be calculated, of the same order as the wave-length of the X-ray. Whether these anticipations were well or ill founded, they became of little consequence when the experiment was made in 1912 by Laue's colleagues, Friedrich and Knipping, and was completely successful. A complicated but symmetrical pattern of spots appeared upon the photographic plate, which, though unlike any diffraction pattern due to light, was clearly of the same nature. It was soon found that every crystal produced its own pattern and that the experiment opened up not only a new method for the investigation of the nature of X-rays, but also a new means of analysis of the structure of crystals. Examples of these patterns are given in Plate I A, B; they may be compared with Plate II C (pp. 866, 867).

In order that these points may be clear it is necessary to examine, at not too great length, the details of the experiment and its implications. We have already examined certain phenomena of crystalline structure in the case of Iceland spar; but it will be convenient to reconsider the subject and to discuss it more generally.

The most striking and characteristic features of a crystal are its regularity of form, the polished evenness of its faces and the sharpness of its edges. If we compare crystals of the same composition we find that the angles between faces are always exactly the same from crystal to crystal: while the relative values of the areas of the faces may vary considerably. In technical terms, the faces of different specimens may be unequally developed. It is natural to infer that there is an underlying regularity of structure, involving the repetition in space of a unit which is too small to be visible. As a simple analogy we might take a piece of material woven, as is customary, with warp and weft at right angles to each other. However it might be torn, it would form pieces with right angles at all the corners: but the pieces would not necessarily be square. There would be two principal directions at right angles to each other: and all tears would take place at right angles to one or other of them. If the two directions were exactly alike, in all the characteristics that could be examined, if for example, they tore with equal ease, and if the frayed edges were the same on all sides, the warp and weft must be identical: they must be composed of the same threads and have the same spacings. We could rightly say that the material is founded on a 'square' pattern. This would still be the case even if both warp and weft were not simple but complex: if for example, each of them contained coloured threads at various intervals. So long as the scheme of repetition was the same in both we should still say the pattern was square: as for example a tartan might be.

The analogy of a woven material is insufficient to represent all the complications of a crystal structure; because warp and weft cannot be inclined to each other at any angle except ninety degrees: but it illustrates the important point that in any structure built of repetitions in space, the angles of the whole must be always the same; while no such restriction applies to the areas of the faces. The unit that is repeated in every direction determines the angular form. If, for example, the unit of a composition in a plane had the form of the small unit (Figure 6), the whole might have various shapes such as are illustrated in the same figure. The edges need not always include the same angles as the edges of A but they would necessarily be inclined to one another at angles which from specimen to specimen would be invariable.

So also in the solid crystal various faces might be developed which would, in regard to the angles made with each other, display the same constancy of mutual inclination. As this is what we should expect if the crystal is composed of units repeated regularly in all directions, and as the facts agree with expectations, we assume that our preliminary conceptions of crystal structure are correct.

What will happen when a train of ether waves meets such a crystalline arrangement?

A crystal can be thought of as a series of layers spaced at regular intervals, just as in two dimensions a regular assemblage of points can be thought of as a set of rows equally spaced. Also, just as in the simpler case

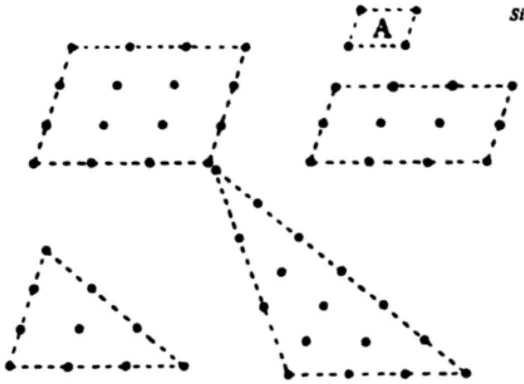


FIGURE 6—The unit of a planar design is included in the outline A. The multiplication of the unit may assume various forms, a few of which are shown. The mutual inclinations of the edges are limited to certain definite angles.

the rows might be made up in various ways as in Figure 7, so also any one crystal can be divided up in an infinite number of ways into parallel sheets.

It is convenient to consider the problem of the diffraction of X-rays in stages: first taking the scattering by a single unit, then by a sheet of units, then by the whole crystal which is made up of a succession of sheets.

The unit in a crystal is made up of a certain number of atoms arranged in a certain way: the composition and arrangement vary from crystal to crystal. When the train of waves meets the unit each atom in it scatters and can be regarded as the centre of a series of ripples spreading outwards in spherical form. At a little distance these melt into one another, and in the end there is but one spherical wave having its centre somewhere within the unit. There is however this peculiarity about the wave, that it is not equally strong in all directions. To take a simple case, imagine the unit to consist of two atoms A and B, separated by a distance

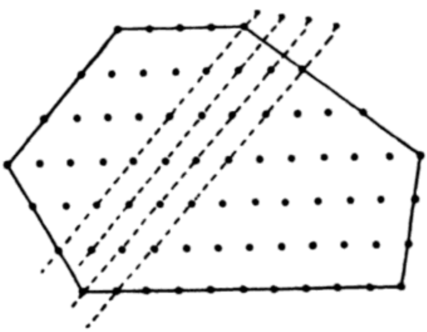


FIGURE 7—The points in the figure can be arranged in rows in various ways.

equal to half the length of a wave, as in Figure 8. The oncoming waves arrive simultaneously at the two. The waves scattered by A and B start off together. In the direction ABC the two systems are always in opposition: a crest of one set fits into a hollow in the other. In this direction they destroy each other's effects. The same happens in the reverse direction BAD. But in every other direction there is no such complete interference: in a direction such as that marked by the arrow P they support each other to some extent, and more so as the direction P is separated from C or D. In this case the combination of the scattered waves will have a spherical form, but it will not be equally intense all round. There will

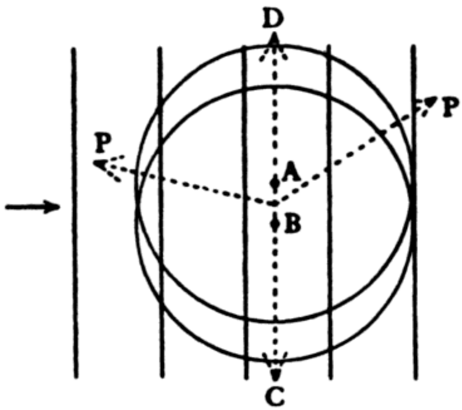


FIGURE 8—Waves represented by the vertical lines are moving upon the two atoms A and B which scatter a small fraction of their energy. The two are half a wave-length distant from one another. The spherical waves that spread away annul one another in the direction A B C or B A D, because the crest of one fits into the hollow of the other. But energy is scattered in all other directions such as are indicated by the arrows marked P.

be points C and D where the wave vanishes as the figure shows.

Other arrangements of the atoms lead to other distributions of intensity on the spherical surface: and the more complicated the arrangement the more complicated the distribution.

This complexity has however no effect on the development of our argument and is described only to make the picture more real. The one important point is that whatever the composition and arrangement of the unit, all the units behave alike. As far as we are concerned for the moment we may disregard the inequality of the distribution of the energy on the surface of the scattered wave, and remember only that the waves are in the end spherical, and that they may be regarded as originating from regularly arranged points which represent the positions of the units.

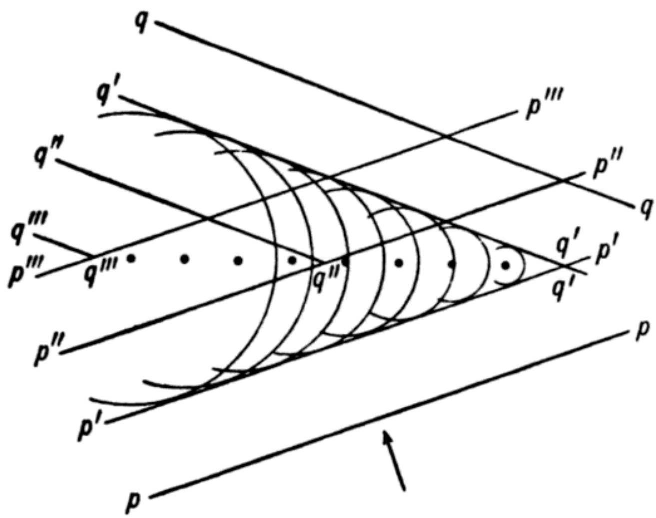


FIGURE 9—Waves, 'pp,' 'p′p″' . . . sweep over a row of points where some scattering takes place. The most of the energy goes on, but a fraction is reflected in the form of waves 'qq,' 'q′q′.' . . .

We now go on to consider the combined effect of the units in a sheet. Suppose that the dots in Figure 9 represent some of the units of pattern in a sheet which is at right angles to the paper. A set of waves is shown in section by the straight lines *pp*, *p′p′*, etc. As each wave sweeps over the points, spherical waves spread away from the point in turn, and a reflected wave is formed by their combination. This is in fact another instance of the application of the Huygens principle. We have a case of simple reflection, differing only from reflection by a mirror in the fact that only a portion of the original energy is carried away by the reflected wave. We know by experiment that in the case of a single sheet this portion is extremely small: the X-rays often sweep over millions of sheets before they are finally spent.

An analogous effect is frequently to be observed in the case of sound. A regular reflection can take place at a set of iron railings, the bulk of the energy going through. We hear such a reflection when we pass the railings in a car.

It is to be observed that the even spacing of the units, and of the dots which stand for them in Figure 9 is not necessary so far as the effect of a sheet is concerned. Nor need the railings be regularly arranged, in order to produce an echo: a reflection can even be observed from a hedge. Regularity is not important until we consider the possibility of reflection by many different sets of planes within the crystal.

In the diagram Figure 10 we represent a section of these sheets by the lines S_1 S_2 S_3, and so on; we draw them as full lines and not as rows of dots because it is of no importance where the units or the representative dots lie in each sheet. For convenience also we show by straight lines the directions in which the waves are travelling instead of the waves themselves. Thus aPa_1 represents a case of reflection in a single sheet which we have just been considering. Besides the set represented by aPq_1 there is another case of reflection represented by bQb_1, another by cRc_1, and so on. The dimensions of the figure are grossly exaggerated in certain directions so as to show the argument more clearly. The distances between the layers are in actuality minute compared to the width of the pencil. Each ray, like bQb_1 represents a train of waves moving on so broad a

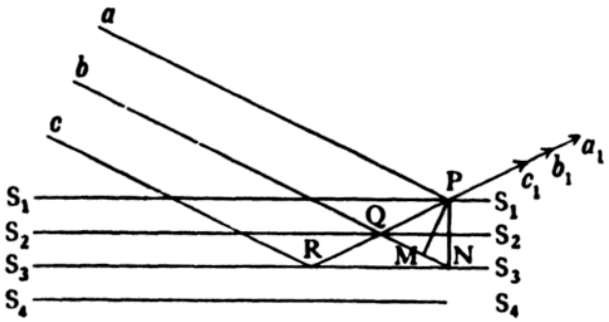


FIGURE 10—The diagram illustrates the reflection of a train of waves by a series of regular spaced sheets, each capable of reflecting a small fraction of the energy of the train.

front that the various reflected trains overlap one another sideways.

The reflected set represented by bQb_1 has had further to go than aPa_1, before it re-appears and joins the latter. If we draw perpendicular distances PM and PN the extra distance is MN.

And again the set cRc_1 lags behind bQb_1 just as much again as this lags behind aPa_1, because the sheets are spaced at even distances. Behind this other reflections follow at regular intervals. The wave reflected by the crystal is the sum of all these. We may represent the summation by Figure 11 in which curves representing the reflected sets of waves are put one below another: each lagging behind the one above it by the distance MN. These waves are to be added up; for instance, along the vertical line shown in Figure 11 we are to add together Oa, Ob, Oc, etc., giving positive signs to those that are above the horizontal line and negative to those that are below. Usually the sum of those quantities will be zero, because they are just as likely to be above as below the line, and in their millions every possible size up to a maximum is to be found. The meaning of this is that there is no reflected pencil: its constituents have

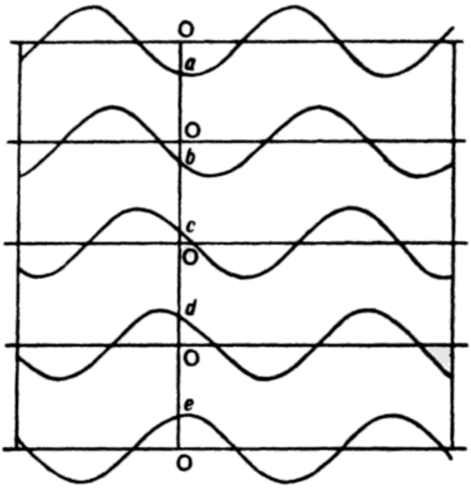


FIGURE 11—A diagram to show how the reflected waves of Figure 10 add up to nothing unless they are all exactly in phase with one another. The quantity $Oa + Ob + Oc + \ldots$ is zero because there are as many positive as negative terms in the millions that must be added together. The exception is when the reflections are all in phase, the crests of one set being exactly above or below (in this kind of figure) crests of all the other sets.

destroyed one another. There is one exception to this rule. If the lag is exactly one wave-length or two, or three, or any whole number of wave-lengths, so that the curves shown in Figure 11 lie exactly below one another, then the sum of them all is just a multiple of one of them, and as the multiplier is large the reflection is large also. The reflected energy cannot of course be greater than the incident, but calculation shows that over a very small range on either side of the reflecting angle the reflection is complete.

The amount of the lag depends on two things, the angle at which the rays strike the crystal and the spacing of the sheets. If they are nearly perpendicular to the sheets, the lag is twice the distance between two sheets that are neighbours: and this is its maximum value. The more oblique the incidence the smaller the lag; at glancing incidence it becomes very small. Thus provided the wave-length is not too great there must always be some particular angle of incidence at which the lag is exactly one wave-length or even more wave-lengths: and at these angles the reflection leaps out strongly.

If our primary rays are of one wave-length we must turn the crystal round until the angle is right. If the angle of incidence has a fixed value we may get a reflection by throwing a mixed beam upon the crystal, out of which rays of the right wave-length will be selected for reflection while the rest pass on. The late Lord Rayleigh once showed at a lecture in the Royal Institution an analogous experiment in acoustics. As its dimensions are on a scale so much larger than those of X-rays and crystals, it helps to an appreciation of the latter case. Sound waves are produced by a whistle of very high pitch, known as a bird-call. The waves are only an inch or so in length, much shorter than the waves of ordinary speech, but hundreds of millions of times longer than the ether waves of X-rays. The note is so high that many ears, especially those of older people cannot perceive it. A set of muslin screens, about a foot square are arranged in parallel sequence, upon a system of lazy-tongs which allows the common spacing to be varied. The screens may for our purpose be taken to correspond to the sheets of Figure 10: each can reflect a small fraction of an incident sound wave, but allows the bulk of it to go on.

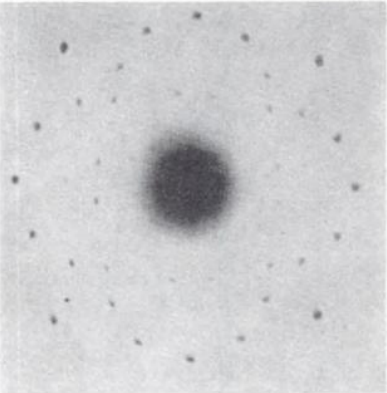
If now the whistle and the set of screens are arranged as in the photograph, Plate I C, the sound may be reflected by the screen. The reflected sound, if it exists, is very easily detected by means of the 'sensitive flame.' This is a luminous gas jet issuing under great pressure from a long narrow tube with a fine nozzle. The pressure is adjusted until the flame is on the point of flaring, under which circumstances a high pitched sound causes it to duck and flare in a most striking way. The rapid alternations of pressure in the sound wave are the direct cause of the effect. The sensitive flame is so placed that it can detect the reflected sound if there is any, but it is screened from any direct action of the bird-call.

It is then found that if, by means of the lazy-tongs, the common spacing of the screens is altered gradually and continuously the flame passes through successive phases of flaring and silence. The explanation is the same as that of the X-ray effect just described. If there is flaring it means that the reflections from the successive screens all conspire, and this happens if the spacing is so adjusted that the lag of one reflection behind another is a whole number of wave-lengths.

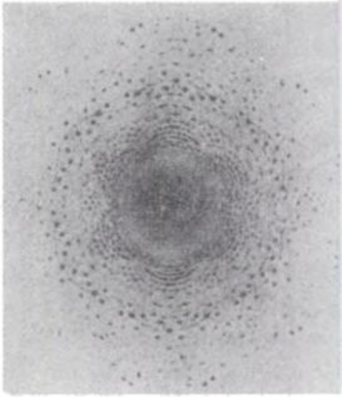
Rayleigh used the analogy for the purpose of explaining the brilliant colours of crystals of chlorate of potash. These crystals have a peculiar formation, being composed of alternating layers of the crystalline material differing only in the orientation of their crystalline axes. The thickness of the layer is thousands of times smaller than the spacing of the muslin screen, but again thousands of times larger than the spacings of the layers in which the crystalline units are disposed. It is of the order of the wave-length of visible light. The same argument holds in all three cases.

We have one more point to consider before we can appreciate Laue's experiment. We have to remember that there is not only one way, there are an infinite number of ways in which a crystal can be divided into parallel sheets. Suppose that Figure 12 represents for example the disposi-

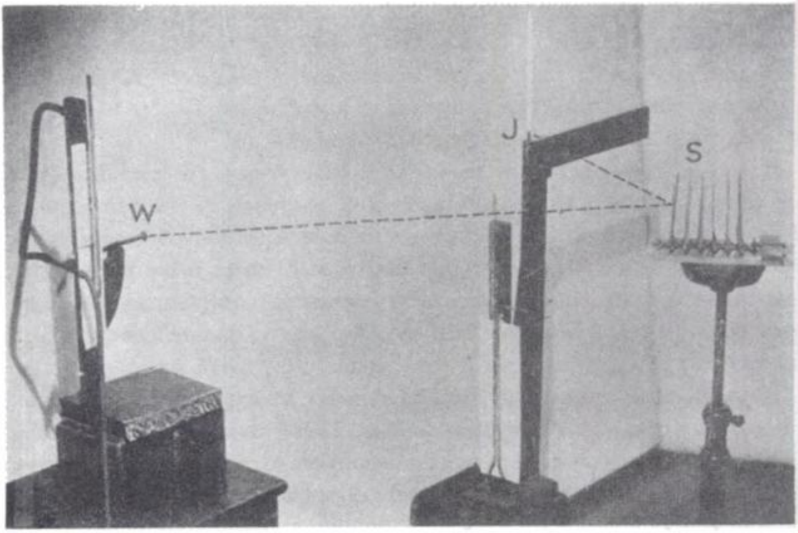
PLATE I



A. X-ray diffraction spectrum of rocksalt.



B. An X-ray diffraction spectrum of kaliophilite.
Bannister



C. A photograph of the apparatus used by Lord Rayleigh and described on pp. 864–865. The whistle is at W, the set of screens at S, and the luminous gas jet at J. The dotted lines show roughly the course of the sound waves that affect the jet. The photograph shows the appearance of the jet when there is no sound, or when the screens (seen edgeways in the picture) are not placed so that their reflections reinforce one another. Owing to a peculiarity in the form of the nozzle at J, the jet does not respond to sound proceeding directly from W to J. When the screens are properly spaced the jet broadens and ducks to a fraction of its normal height.

ourselves. Here, for instance, the rule is to use nothing but regular polygons to limit our solids. I see nothing surprising in the fact that there should be exactly five solids obeying that arbitrary rule. But in physics we don't make the rules; therefore we shall never be able to say that a chapter of physics is closed."

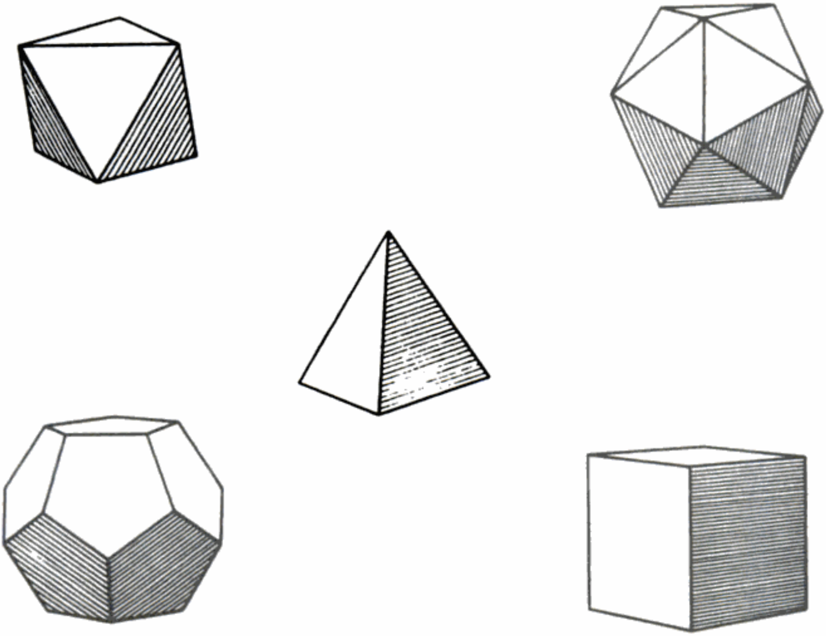


FIGURE 2—Regular solids, of which each face is regular, are five in number.

"Your argument," I answered, "is perfectly plausible. But it does not fit the case. There are 'regular solids' in physics also; we call them crystals. So here we have geometry again, and arithmetic, but this time it does not all take place in our heads. Here nature invents the rules of the mathematical game, and of course it finds itself bound by the consequences of the rules."

"Sounds like nonsense to me," said Empeiros.

"The ancients," I continued, disregarding the comment, "noticed several beautifully shaped crystals, such as quartz, growing, as it were, out of shapeless rocks. The first scientific crystallographer was a Danish bishop called Steno, who in 1669 published a dissertation *Concerning Solids Naturally Contained within Solids.* Other naturalists carried on his research, and by 1782 a Frenchman, the Abbé René Just Haüy, had found the basic rule of the game of crystals: he had found how to obtain the shape of any crystal from that of some standard simple form.

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Volume 2

EDITED BY JAMES NEWMAN

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