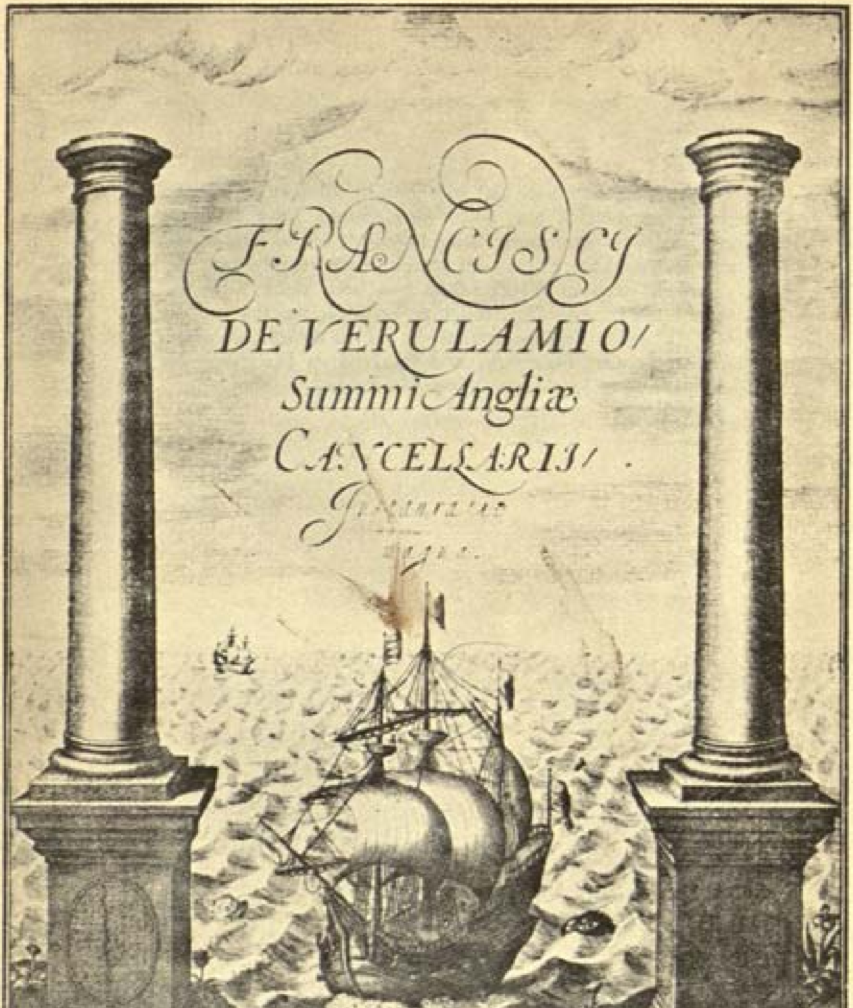
A HISTORY OF SCIENCE, TECHNOLOGY AND PHILOSOPHY

In the 16th & 17th Centuries

VOLUME ONE

"Come then, excellent Sir, and banish all fear of stirring up the pygmies of our time; long enough have sacrifices been made to ignorance and absurdity; let us spread the sails of true knowledge, and search more deeply into the innermost parts of Nature than has been done hitherto" (Henry Oldenburg, in a letter to Spinoza, written in July 1662, in which he reports that the Royal Society, of which he was the first Secretary, had received its Charter.—*The Correspondence of Spinoza*, tr. by A. Wolf, 1928, p. 100).



The Title-Page of Bacon's *Novum Organum*

A HISTORY OF SCIENCE, TECHNOLOGY AND PHILOSOPHY

In the 16th & 17th Centuries - Vol I

by A. Wolf

With the co-operation of
F. Dannemann and A. Armitage

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DEDICATED

TO

MY FATHER

LEWIS WOLF

WHOSE LOVE OF GOOD BOOKS
HAS NEVER WANED THROUGH
PREOCCUPATION WITH LEDGERS

Printed in the United States of America

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PREFACE

In the following pages the attempt is made to give a reasonably full account of the achievements of the sixteenth and seventeenth centuries in the whole field of "natural" knowledge. All the sciences, including several which have not hitherto been included in histories of science, receive due attention, and details are given of all the important work done in each of them during the first two centuries of the modern period. A considerable amount of space is also allotted to the principal branches of technology. The volume, moreover, includes a fairly full account of the philosophy of the period as an aid to the understanding of the general intellectual orientation of its scientists. It is hoped that the exposition is sufficiently clear, and the illustrations sufficiently illuminating to enable the general reader to profit greatly from this history. Its primary aim, however, is to meet the needs of the serious student. For this reason the work is fully documented. The plan of incorporating a select bibliography (giving precise references) in the text will probably be found much more helpful than is the usual formal bibliography, which makes it about as easy to find the authority for a particular view as it is to find a needle in a haystack. A more formal bibliography for the whole modern period will be included in the concluding volume.

The present book is complete in itself. It is, however, intended to be only an instalment of a complete history of science. The author proposes to deal with the eighteenth and nineteenth centuries next, and then with ancient and mediaeval times. But each volume will be as nearly as possible self-contained. Human history cannot, of course, be strictly correlated with exact centuries. In science, as in other fields of human activity, the events of one century have their antecedents in earlier centuries and their consequences in later ones. For the sake of greater intelligibility, and self-completeness of each volume, the author accordingly did not, and will not, hesitate to make occasional incursions into other centuries than those principally concerned.

An encyclopaedic enterprise like the present may appear to be an anachronism in an age of extreme specialization. It is widely recognized, however, that the tendency toward a narrow specialism has already gone too far. The contemporary close relationship of science and philosophy, and the growing interest in the history and development of science, may be regarded as evidence of a growing recognition of the need of a wider outlook. This work was undertaken, in the first instance, in order to meet the requirements of students pursuing courses in the History, Methods, and Principles of Science in the University of London. It is hoped, however, that its usefulness will be much more far-reaching.

Needless to say, this enterprise would have been impossible without the aid of other experts. The author has been very fortunate in receiving most valuable help from a number of colleagues. Their names are set down here in alphabetical order, with a bare indication of the general nature of the assistance rendered by each of them. Mr. A. Armitage has been unstinting in his services, not only in connection with his special subjects, astronomy and mathematics, but also in many other ways. Professor F. Dannemann has placed at the author's disposal the fruits of many years' work in this field, though conditions in Germany have unfortunately prevented the closer co-operation intended originally. Miss R. Dowling has checked the biological portions. Professor L. N. G. Filon has, in spite of the great burdens of his high office as Vice-Chancellor of the University of London, made time to go carefully through all the chapters on astronomy, and has given them the benefit of his expert knowledge of the subject. To Professor W. T. Gordon are due some very helpful suggestions concerning the geology of the period. Mr. S. B. Hamilton has been most helpful with some of the sections on technology. Professor L. Rodwell Jones has looked through the chapter on geography. Dr. D. McKie has rendered valuable help by his special knowledge of the history of chemistry. Professor L. C. Robbins has examined the section on economics. Mr. D. Orson Wood has helped with a searching criticism of the chapters on physics. The book has also benefited from Mr. T. L. Wren's expert knowledge of the history of mathematics. The author is deeply grateful to all these colleagues, and warmly appreciates their friendly interest. But he has no desire to shirk his responsibility for the whole book.

The preparation of this work has naturally involved frequent appeals to libraries for old and rare books. The librarians of the London School of Economics, of University College, London, and of the University of London have spared no pains in finding what was required; and they have laid the author under great obligation.

Special attention has been paid to the illustrations, and all possible sources have been ransacked for them. Many of the line-drawings have been copied and adapted by Miss D. Meyer, to whose skill and sympathy the author is greatly indebted. The authorities of the Science Museum, London, have kindly permitted the reproduction of some of their photographs of old engravings, etc. The proprietors of *The Mining Magazine* have given permission for the use of many of the illustrations from the Hoovers' edition of Agricola.

Messrs. John Lane have consented to the reproduction of the facsimiles of the Bills of Mortality from W. G. Bell's *The Great Plague in London*. Messrs. Methuen and Co. have allowed the use of the frontispiece to *The Divining Rod* by Sir Wm. Barrett and T. Besterman. The author is grateful for all this kindness.

The reader will not need to be told that the writing of this book has cost the author a great deal of hard toil. What has sustained him throughout the long and laborious enterprise, apart from the intrinsic interest of the subject, is his belief that the world has need of a new intellectual re-orientation, and that to this end a close study of the history of human thought in its most objective spheres would be the best beginning. It was in this spirit of faith and hope that the work was undertaken and has been carried thus far. The author hopes that it may be received in the same spirit of faith and hope—and charity!

A. W.

UNIVERSITY OF LONDON
December 1934

A HISTORY OF SCIENCE, TECHNOLOGY, AND PHILOSOPHY
IN THE SIXTEENTH AND SEVENTEENTH CENTURIES

CHAPTER I

MODERN SCIENCE

The Beginning of Modern Science

The divorce of science from philosophy, and the differentiation of science into a multiplicity of sciences, had not yet taken place at the beginning of the modern period. Knowledge was still regarded as a whole; and the term philosophy was widely used to denote any kind of inquiry, whether scientific or philosophical, in the subsequent and narrower meanings of these terms. These changes, however, were on the way. The mathematical and the experimental tendencies of the pioneers of modern science inevitably led to a divorce between exact or experimentally verified science on the one hand, and merely speculative philosophy on the other. Similarly, although all sorts of subjects were frequently investigated by the same person, and dealt with in one and the same volume, the rapid accumulation of scientific results inevitably led soon to a division of labour and a differentiation into the several sciences. The classification of the sciences adopted in the present volume may possibly appear to some people as something of an anachronism. But it is justifiable on grounds of simplicity and orderliness. An account of the first centuries of modern science would be a hopeless tangle in the absence of an orderly scheme of exposition. And, after all, it is the business of a history to make things clearer than they would be otherwise.

Historical epochs do not appear suddenly. They usually need some antecedent preparation. Hence the difficulty of determining their beginning. The modern period in science emerged rapidly out of the Renaissance, which revived certain ancient tendencies which were opposed to the mediaeval outlook, and, partly for that reason, appealed to those who were dissatisfied with the mediaeval attitude towards life and reality. The differences between pagan antiquity and mediaeval Christendom are fairly obvious. The tendency of mediaeval Christianity was towards self-repression and other-worldliness. The ideal Christian, conforming to the vows of the religious life, had his thoughts fixed on Heaven. Nature and natural phenomena had no intrinsic interest for him. Natural desires had to be transmuted into mystical ecstasy; spontaneous personal thought had to be subordinated to authority. Into this oppressive atmosphere the recovered classics of Greece and Rome came like a refreshing sea breeze. Poets and painters and others were inspired with a new interest in natural phenomena; and an impulse to self-assertion, intellectual and emotional, filled some of the bolder spirits. In these respects, modernism was essentially a revival of antiquity, brought about with the help of the literature of antiquity. And modern science, in its early stages, was helped more specifically by the astronomical, mathematical, and biological treatises transmitted from ancient times, and most of all perhaps by the mechanical treatises of Archimedes and the technological works of Hero of Alexandria and Vitruvius.

Mediaeval lack of interest in natural phenomena and disregard of individual judgment had their roots in the domination of a supernatural outlook, an other-worldly mentality. The Earth was of little interest in comparison with Heaven, the present life was at best but a preparation for the life hereafter. And the Church claimed absolute authority for truths revealed by the light of grace, in comparison with which the light of reason was of no consequence. True, Thomas Aquinas and his followers recognized the light of reason as a source of knowledge beside the light of grace; but even they left no doubt whatever about the subordination of natural knowledge to revelation. Attempts have been made to claim rationalism for Scholasticism; and Professor Whitehead has even gone so far as to describe modern science as "a recoil from the inflexible rationality of mediaeval thought" (*Science and the Modern World*, p. 11, ed. 1929). This is a somewhat misleading half-truth. No doubt the Scholastics were nimble intellectualists, and gave proof of great subtlety of thought. No doubt, also, they rendered valuable services in keeping the thought of Christendom alive during the lean Middle Ages. But their reasoning was always kept within the bounds of premises based on authority; they never attempted to exercise, nor permitted others to exercise, that wider rationality which seeks to embrace the whole of human experience without such arbitrary boundaries as dogmas prescribed by authority. Due regard for stubborn facts of observation is an essential part of any thoroughgoing rationality, not a recoil from it; and the kind of rationality that stops short of it is but imperfectly rational, however subtle and justifiable it may be in other respects. Now in this regard, too, modern science was a return to the implicit reliance on natural knowledge that was felt by the ancients. And the attention which stubborn facts of Nature received at the beginning of the modern era, the stress laid on experience and more particularly on experiment were largely prompted by the spirit of naturalism exemplified and encouraged by the recovered literature of pagan antiquity, as contrasted with the spirit of supernaturalism which pervaded the intellectual atmosphere of the Middle Ages. It was not the result of a recoil from rationality, but a big stride towards a freer and fuller rationality, unrestrained by arbitrary barriers. That is why science is universal, whereas the Churches are not. Science imposes no arbitrary restrictions on the reasoning by which it is cultivated; but the Churches usually confine the scope of reason within the arbitrary boundaries of their several creeds or dogmas.

The contrast just indicated may be described in a slightly different way. The naturalist outlook may be regarded as essentially secular and matter-of-fact; the supernaturalist outlook is apt to be rather mysterious. The former view expects regularity in Nature, the latter is prepared to find miracle and magic in natural phenomena. Even pagan antiquity was infected with superstitious credulity, but not to the same extent as was mediaeval Christendom. And it took a long time for the modern period to divest itself of its mediaeval superstitions. One has only to think of the persistence of the belief in witchcraft, and of the enormous number of victims who were condemned for it by intelligent judges and Church dignitaries during the early centuries of the modern period to realize what a strong hold the magical view of Nature had upon the intelligentsia, as well as upon the masses, of mediaeval and early modern times. It seems amazing to find great doctors like William Harvey and Sir Thomas Browne taking part in the examination of alleged witches. It was only by slow degrees that the growth of natural knowledge and the invention of mechanical contrivances for doing wonderful things by "natural" magic helped to rid the modern world of the mysterious powers of darkness which haunted the Middle Ages.

The secular attitude towards natural phenomena did not, of course, necessarily exclude a religious attitude towards the world. Kepler's is a particularly striking case in point. His attitude was not only religious but fervently mystical. His great astronomical discoveries were mainly prompted by religious aims. He set out to seek the ways of God, and found the courses of the planets. Descartes likewise had a tendency towards mysticism. This is clear from his account of the vision he had on the night of November 10, 1619, which will be recounted in due course. But his scientific books are secular in outlook. The pioneers of modern science were practically all of them deeply religious men, in fact loyal sons of the Church. Yet, fortunately for science, their attitude towards natural phenomena was mainly secular and matter-of-fact. Kepler's mystical effervescence was effectively restrained by the empiricism of Tycho Brahe who made a scientific astronomer of him even if he could not cure him of his tendency to Sun-worship. Galilei drew a definite line between the function of religion, to teach the way to Heaven, and the function of astronomical science, to discover the ways of the Heavens. And even Newton, who was more interested in the problems of conventional theology than were Galilei or Kepler or Descartes, was very careful to keep theological dogmas, and even philosophical hypotheses, out of science. The Scholastic or Thomistic view of two kinds or sources of knowledge may have been of service here, as it certainly was in the case of Descartes. Even the mystical experiences of men like Jacob Boehme, as well as of Kepler and Descartes, may have been of some value, no matter how they are explained psychologically. For they certainly helped to stiffen individual judgment against Church authority. Anyway modern science, like ancient thought, and unlike mediaeval thought, adopted a secular matter-of-fact attitude.

There are certain other differences distinguishing modern science from mediaeval thought. The differences in question, however, unlike those already indicated, do not correspond to any radical divergence between mediaeval mentality and Greek thought generally. They were rather due to the fact that mediaeval thinkers followed one set of Greek ideas, whereas the pioneers of modern science embraced another set of Greek views. Scholasticism had set up Aristotle as its authority on matters which did not involve religious dogma. Now Aristotle was primarily a biologist, and his science was mainly qualitative, not quantitative. He was concerned with the classification of things into classes and sub-classes, the enumeration of their qualities, and the distinction between essential and non-essential qualities. Mediaeval thought followed the Aristotelian tradition. But there was also another and earlier Greek tradition or school of thought, namely, the Pythagorean. This school laid supreme stress on number or quantity. And the founders of modern science were thoroughly imbued with the Pythagorean spirit. This is particularly true of Copernicus and Kepler, and almost as true of Galilei and Newton. So much was this the case that they tended to deny the objective reality of the so-called secondary qualities, because these could not be treated mathematically. It was mainly the non-mathematical men of science, like Boyle, Gilbert, and Harvey, who did not go to such extremes. At all events, modern science has continued to cling to the ideal of exact quantitative descriptions and laws wherever possible.

Another divergence in the choice of Greek traditions followed by mediaeval and modern thinkers is to be found in the kind of explanation in favour among them. The Scholastics were strongly addicted to the kind of explanation to which Socrates and Plato had given vogue. It consisted in the discovery of the ends or purposes which things served, the indication of what they were good for; and in Plato's cosmic scheme there was a hierarchy of ends or "goods" culminating in the supreme "Good" to which the whole creation moves. Mediaeval thought ran riot in the invention of fanciful ends which things were alleged to serve. The ends imagined were usually human ends. This kind of teleological explanation thus tended to encourage the homocentric prejudices of the Middle Ages. Everything was conceived as having been intended and designed to serve some human need. One might almost say that God Himself was regarded as mainly occupied with human affairs. When mankind was thus conceived as the focus of cosmic economy, the Earth, their stage, was naturally looked upon as the centre of the Universe. Hence the popularity of the geocentric theory, and one of the greatest obstacles to astronomical reform. Modern science started by rejecting and still rejects, as far as possible, teleological explanation. It embraced the method of explanation advocated by Democritus and the other atomists, namely explanation by reference to the causes or conditions which produce things, their efficient, not their final, causes. And this mode of explanation harmonized well with the mathematical tendency of modern science, for mathematics was the one department of knowledge in which teleology had obviously no place.

Such, in brief outline, are the general characteristics which distinguish modern science from mediaeval thought. Naturally the transformation was not complete at the outset. The number of scientific men was comparatively small to begin with, and even these showed compromises of all sorts, induced by fear or by habit. The martyrdom of Giordano Bruno and of Michael Servetus, the experiences of Galilei and others showed the need of caution in face of the powerful Churches. One can appreciate the wisdom of Leonardo da Vinci and kindred spirits in the fifteenth century in refraining from publishing their views. Judged by all the abovementioned criteria, Leonardo da Vinci was an eminently modern man of science. But the world was not yet ready for him, though it is probable enough that he and his intimate circle helped, in a personal and unobtrusive manner, to prepare the way for the coming advance. The first great move was made about the middle of the sixteenth century, with the publication of the heliocentric theory of Copernicus (1543). The scientific advance was not made simultaneously along the whole front, but in sections and at different times. Astronomy led the way. Physics followed in the seventeenth century. Chemistry moved forward in the eighteenth century. The biological sciences, in spite of the lead given by Vesalius (1543) and Harvey, lagged behind, and only made their advance in the nineteenth century.

The Heritage from the Past

Many of the tasks which the new age took in hand had already engaged the attention of the ancients, only they had been lost sight of during the Middle Ages, and the new era had to take them up again almost at the point where antiquity had left them. To these old tasks the modern period has, indeed, added new ones in ever-increasing numbers, and has become conscious of the endless possibilities in the way of new tasks, new discoveries, and new inventions. But this does not affect its indebtedness to antiquity, and our first endeavour must be to indicate summarily the extent of that legacy from the past.

The elements of mathematics had been developed by the Greeks in all essentials, and had been systematized most completely by Euclid. Archimedes and Apollonius made important additions to mathematical science, notably the theory of conic sections. Next, Ptolemy's *Almagest* gave the main outlines of plane and spherical trigonometry. Still later came the current system of numerals and the rudiments of algebra, largely through the help of India and Arabia.

The application of mathematics to the solution of problems in astronomy and mechanics was also taught strikingly by the ancients. The works of Ptolemy and of Archimedes contain numerous examples of such applications. Moreover, numerous observations of the courses of the stars had been made and recorded. A beginning had also been made with correct astronomical theories, which only needed fuller development. The astronomical methods and instruments of the Greeks were, in all essentials, the same as those used by the first astronomers of the modern period; and their problems, too, were essentially similar. The determination of the circumference of the Earth, its relation to the other celestial bodies, the topography of the region of the fixed stars, the exact measurements of space and time, the prediction of such astronomical events as eclipses—all these were problems familiar to antiquity, especially to the

Alexandrians, and the modern period obtained its first lessons in these things from the work of Ptolemy.

Statics and optics had likewise been developed scientifically in ancient times. These studies were, indeed, peculiarly suited to the deductive methods so dear to the Greeks. The results they obtained were taken over by the moderns. It was different with other branches of physics. Apart from a few sporadic observations, there was little of value that could be learned from Greek physics. This is especially true of magnetism and electricity. It also applies more or less to the study of air and of steam, though Hero of Alexandria had made some interesting contributions to the subject.

Chemistry, too, had been cultivated in Alexandria, where the empirical knowledge handed down from ancient Egypt came into contact with Greek thought, which helped to make it more scientific. But the influence of Neo-Platonism made Alexandrian chemists mystical searchers after such wonder-working substances as "the philosopher's stone" to transmute base metals into precious ones, or some "elixir" or "panacea" to cure all the ills of mortal life. The Middle Ages were specially interested in this kind of alchemy, though they also contributed some things of value to empirical chemistry. For a long time after the Copernican beginning of the modern period chemistry retained essentially its mediaeval character.

In the domain of natural history, and the descriptive sciences generally, the modern period likewise just carried on the work of antiquity. The first stimulus came from the renewed study of the classical writers, and in due course interest in independent observation increasingly displaced the customary reliance on books and authorities. As the more exact sciences developed they helped to stimulate the descriptive sciences so that their wealth of accumulated observations greatly exceeded what had been known to the ancients.

Moreover, the scientific knowledge acquired in antiquity had not been entirely lost during the Middle Ages. In the East, at all events, some measure of continuity with ancient science had been maintained with the help of Greek refugees or settlers. Attempts were even made to develop this knowledge by independent research. During the ninth and tenth centuries we find a number of Arabian writers showing some independence in science and medicine. And this movement reached its zenith in the eleventh century.

An important part in the preparation for the modern period was played by the development of technology. The technical arts are, of course, of ancient origin. But the development of mines, saltworks, foundries, glassworks, etc., in the eleventh century, and earlier, in Bohemia, Germany, Hungary, etc., had a special significance for the shaping of the modern era. The technicians employed in the various industries were not as a rule bookish. They inevitably learned their lessons from a direct study of the facts. No book-authority was of any use to them. Their practical knowledge was at first handed on by oral tradition, and so could not exercise much influence on pure science. But in course of time some of the technicians became vocal, or rather literary, and after the invention of printing their books played a part in the development of the objective attitude of modern science.

The Secularization of Knowledge

The chief obstacle in the path of science during the Middle Ages was the Christian Church. Concerned mainly with the lowly, disdainful of the world and the flesh, and believing itself in proud possession of divinely revealed truth concerning all that mattered, the Church was at first contemptuous and then hostile towards all those who sought knowledge of Nature by the independent light of reason. At times, indeed, the Church found it expedient to make use of scientific and philosophic arguments in self-defence against unbelievers or heretics. But any such secular thought had to be subordinated to Church dogma. Independent spirits like Roger Bacon (1214–92) and Leonardo da Vinci (1452–1519), who might have brought about a revival of science if they had been free to do so, were silenced in one way or another by the power of the Church. Even the Renaissance and the Reformation afforded no direct help to the advancement of science. The Renaissance, indeed, brought a refreshing breeze into Christendom through contact with naturalistic paganism. But it was more concerned with book-learning than with the first-hand study of nature, and at the Universities the study of classical literature proved inimical to the pursuit of science. As to the Reformers, they were at least as intolerant as was the Catholic Church. Indirectly, however, both these movements served the cause of science. The squabbles and the intolerant tyranny of the Churches alienated some of the best minds from them, and sent them in search of truth by the light of reason, and independently of the authority of revelation, which all the Churches claimed. Such people soon came under the influence of that spirit of naturalism which the Renaissance had reintroduced. And when the Universities showed themselves indifferent to science, new institutions or academies were founded for the advancement of experimental science. Notable among these new institutions were the Accademia del Cimento at Florence (founded in 1657), the Royal Society of London (founded in 1662), and the Paris Académie des Sciences (founded in 1666). These institutions were to some extent encouraged by the States in the hope that they would repay in useful discoveries—much in the same way, for instance, as Greenwich Observatory was founded (in 1675) in the interests of the British Navy. In this way the pursuit of knowledge was gradually secularized, passing from the mediaeval cloisters into the modern world, though the Church did not relinquish its strangle-hold without a struggle.

Moreover, there were certain political factors at work which tended in the same direction. At the beginning of the modern period in the history of science, England and Holland played important rôles in the scientific as well as in the economic and political spheres. Both these countries had been taught forbearance in the school of international trade; both had fought their fight with the Catholic Church; and their Governments were consequently more inclined to show a measure of tolerance, and to give considerable freedom to those who pursued natural knowledge. Holland, indeed, became the asylum of many learned Frenchmen who did not feel safe in their native country, where the Catholic Church exercised considerable control over the Universities. To Holland also the learned and scientific world is indebted for the numerous publications of the Elzevir and other presses which did so much for the advancement of knowledge in those critical days.

Scientific Instruments

One of the chief characteristics of modern science consists in its use of scientific instruments. The function of such instruments is various. They may enable the observer to observe much better what he can already observe with his unaided senses, though not so well. They may enable him to perceive something that would otherwise be entirely imperceptible. They facilitate the precise measurement of phenomena. Or they may make it possible to study a phenomenon under conditions so controlled as to justify reliable conclusions about it. In all these ways scientific instruments have been, and still are, a most important aid to modern science, and constitute one of its chief differences from preceding science, which had but the simplest kind of instruments at its service. Already the seventeenth century witnessed the invention and use of at least six very important scientific instruments, namely, the microscope, the telescope, the thermometer, the barometer, the air-pump, and the pendulum clock. An account of these and other instruments will be found in the following pages. But one or two general remarks may not be out of place here. Obviously the telescope enables astronomers to see distant celestial bodies far better than they could be seen with the naked eye, if indeed they could be seen at all without the aid of the telescope. Similarly with the microscope in the study of minute objects. The barometer and the thermometer likewise made it possible to observe and to measure variations in air-pressure and in temperature respectively, which would otherwise be either imperceptible or at least not measurable. The air-pump enabled physicists to study the properties of the air under conditions which settled once for all conflicting speculations about it. Lastly, the pendulum clock made it possible to measure small time intervals which either could not be measured at all or at least not so accurately before its invention. Moreover, the measurement and the quantitative correlation of phenomena plays so important a rôle in modern science that one can hardly imagine the very existence of modern science without the aid of the above-mentioned and similar scientific instruments.

CHAPTER II

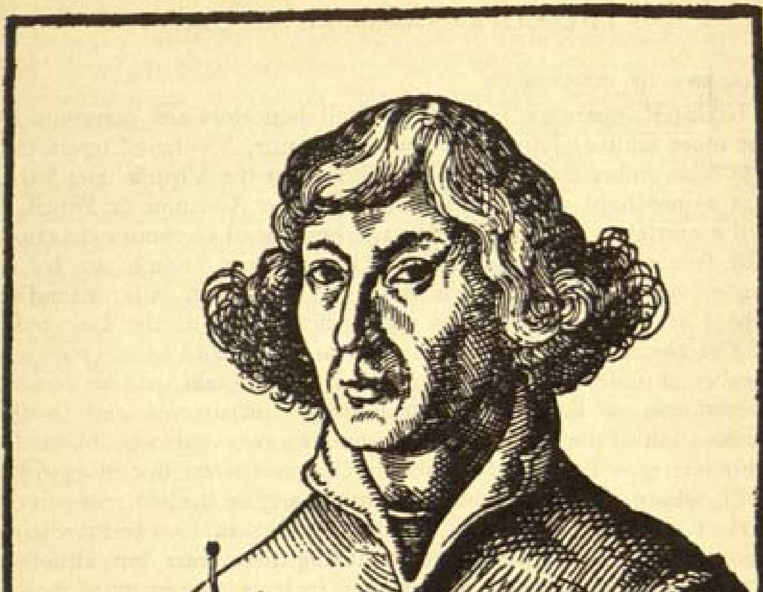
THE COPERNICAN REVOLUTION

THE LIFE OF COPERNICUS

Niklas Koppernigk (whom we shall henceforward designate by the more familiar latinized form of his name, Nicolaus Copernicus) was born February 19, 1473, at Thorn, on the Vistula. His father was a merchant whose nationality, whether German or Polish, is still a matter of controversy. His mother was of German extraction. The father died in 1483, and Copernicus was brought up by an uncle who destined him for a career in the Church. After attending school at Thorn, Copernicus spent three years at the University of Cracow. Here his interest in mathematics and astronomy was awakened under the teaching of Albert Brudzewski, and he became accustomed to the use of astronomical instruments and to the observation of the heavens. After spending two years with his uncle, who was now Bishop of Ermland, Copernicus set out in 1496 for Italy, where, during the following ten years, he studied successively at the Universities of Bologna, Padua, and Ferrara. Law and medicine were his professed subjects of study during these years, but, although not much is known about his activities in Italy, there is good reason for supposing that he devoted much of his time there to the pursuit of astronomy, both theoretical and practical.

While at Bologna Copernicus came into close personal touch with Domenico di Novara, the Professor of Astronomy there, a leader in that revival of Pythagorean ideals in natural philosophy which was awakening the Italian universities about that time. The two men observed together, and discussed, with a freedom unusual in the circles which Copernicus had hitherto frequented, the errors of Ptolemy's *Almagest*, and the possibility of improving upon the Ptolemaic system. There is little doubt that it was during his residence in Italy that Copernicus received the initial impulse towards that reform of astronomy which he achieved in his later, more secluded years.

During his absence in Italy Copernicus had been appointed a Canon of Frauenburg Cathedral, in his uncle's diocese, but after his return home he continued to live with his uncle at his palace at Heilsberg until the Bishop's death in 1512. He then took up his duties at Frauenburg, where he lived, with occasional interruptions, for the remaining thirty years of his life. These thirty years were, to outward appearance, the most uneventful in the life of Copernicus. He shared in the business of his Chapter, did a little political work, and gave free medical advice to the poor of the district. But it was during these years that Copernicus thought out the details of his



Illustr. 2.—Copernicus

planetary system, marshalled the intricate array of calculations by which that system was eventually reduced from speculation to numerical precision, and slowly perfected the manuscript in which the fruits of all his labours were at last given to the world.

Copernicus from the first was well aware of the opposition, based on both learned and doctrinal grounds, which would be aroused by the publication of his novel views on the constitution of the solar system. Hence, while he kept revising his manuscript year after year, he hesitated to publish it. Some inkling of his actual opinions, however, leaked out, awakening discussion and curiosity; and about 1529 he circulated among his friends a manuscript *Commentariolus*. This little tract gives a descriptive account of his system in very nearly its final form, but with all the calculations omitted. (A text of the *Commentariolus*, based on Curtze's collation of the two extant manuscripts, is printed in L. Prowe's *Nicolaus Coppernicus*, Berlin, 1883, 1884; Bd. II.) Again, about ten years later, Copernicus received a long visit from a young astronomer, Georg Joachim, better known by his Latin name Rheticus, who studied the still unpublished manuscript, and made its contents known to a wide circle by means of his printed *Narratio Prima* (1540).

It was to this Rheticus, three years later, that Copernicus, now old and ill, committed his manuscript when his friends at length prevailed upon him to publish it. The book was printed at Nürnberg and published in 1543; and the story goes that the first copy was given to Copernicus a few hours before his death on May 24, 1543.

The printed book bears the title *Nicolai Copernici Torinensis de revolutionibus orbium coelestium Libri VI*, and was dedicated to the reigning Pope, Paul III, whose interest and protection Copernicus claimed. The first edition, however, differs on almost every page from the original manuscript. The title itself is an addition, and there is reason to suppose that Copernicus would have preferred to call his work *De Revolutionibus* simply. The manuscript was lost for some two hundred years, but was rediscovered in time to serve as the basis for the *Säkular-Ausgabe* (Thorn, 1873), which is the authoritative edition of the text. (There is a German translation of the book by C. L. Menzzer, Thorn, 1879.)

For some years after the publication of Copernicus' book there was uncertainty as to whether his hypothesis was intended as a description of the actual motions of the Earth and planets, or merely as a computing device to facilitate the construction of planetary tables. This question was of all the more moment as, in the state of religious opinion at the time, the acceptance or rejection of the teachings of Copernicus depended to a considerable degree upon the sense in which they were to be understood. The uncertainty arose chiefly out of the circumstances in which the book was published. Rheticus, who at first superintended the printing, was called away before it was completed, and he entrusted the supervision of the work to Andreas Osiander, a local Lutheran clergyman, who was a mathematician and a friend of Copernicus. Osiander was afraid that the doctrine of the Earth's motion would offend philosophers and strict Lutherans, and so he inserted a little preface of his own which stated that the whole was to be regarded as a mere computing device without prejudice to scriptural or physical truth. The imposture was recognized by several friends of Copernicus from the first, and was finally exposed by Kepler (*Astronomia Nova*, on the *verso* of the title-page, ed. Frisch, Vol. III, p. 136). It was probably well-intentioned; Osiander had previously advised Copernicus to insert such a deprecatory preface, but Copernicus had refused to do so. The preface seemed strangely at variance with the text, but could not be finally proclaimed to be an addition until the original manuscript was recovered. To Copernicus, imbued with Pythagorean ideas, the most elegant and harmonious mathematical representation of the planetary motions doubtless appeared to be the only true planetary theory.

Copernican Astronomy

In the dedicatory preface to his *De Revolutionibus* Copernicus immediately brings his readers face to face with the long-standing problem which it was his life's work to solve. That problem was to ascertain what geometrical laws govern the motions of the planets in order to explain the apparent motions observed in the past, and to predict how the planets would move in the future. The successive attempts which had been made since ancient times to solve this problem had produced two main types of theory.

The theories of the first type all went back to the homocentric spheres of Eudoxus, the pupil of Plato. In that system each planet was supposed to be embedded in the equator of a uniformly rotating sphere having the Earth at its centre. The poles of this sphere were fixed in the surface of a second, exterior sphere, concentric with the first, and rotating uniformly about an axis constantly inclined to that of the first. This second sphere was similarly related to a third, and so on to the number of spheres required to account for the observed behaviour of the planet. This theory was in accordance with Aristotle's system of physics, of which, indeed, it formed the basis; and it had been revived by mediaeval natural philosophers on that account. But practical astronomers had lost patience with the system of homocentric spheres, not only because it was belied by several familiar celestial phenomena, but because the planetary motions turned out to be far too complicated to be represented in this way without an intolerably cumbersome combination of spheres. Thus no numerically determinate theory, fit to serve as the basis of planetary tables, had ever been arrived at with the aid of this hypothesis, and Copernicus realized that no progress was possible along these lines.

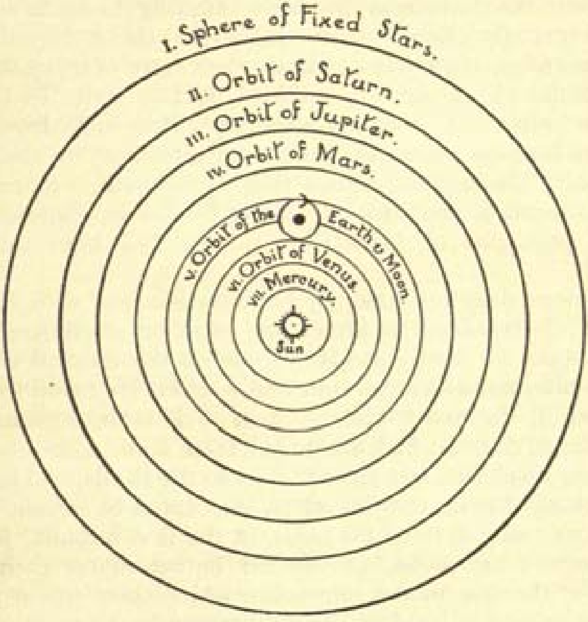
The planetary theories of the second type to which Copernicus refers in his preface were those which employed the eccentrics and epicycles of the Alexandrian astronomers. These theories set out from the simple conception of a planet uniformly describing a circle with the Earth at the centre, and then refined upon it by displacing the centre of the circle from the Earth, referring the uniform motion to an arbitrarily chosen point within the circle, regarding the moving point on the circle as merely the centre of a smaller circle in which the planet actually revolved, and so forth. In this way had been built up the complex planetary system of Ptolemy, which still, after fourteen centuries, dominated astronomy in the time of Copernicus. This system, unlike that of Eudoxus, was eminently suited to serve as the basis of tables; but in its elaboration the essential principles of Aristotelian physics had been thrown to the winds.

Copernicus describes how, in his dissatisfaction with this state of affairs, he resolved to attack the problem on different lines. Casting about for fresh ideas, he turned to the classical writers to see what alternative theories they had to offer. He found that quite a number of the early thinkers, such as Hicetas, Philolaus, and Heraclides of Pontus, had attributed some form of motion (axial rotation or revolution in a closed orbit) to the Earth; and he quotes several classical writers to this effect. We cannot be certain whether Copernicus really derived his ideas, in the first instance, from the writers whom he quotes, or whether he introduces their names merely for the sake of the impression which these would produce upon the readers of his day. We shall return to the question of the originality of Copernicus' conceptions later in this chapter. In any case he uses these classic passages as an excuse for introducing his own system, in which the Earth rotates on its axis and revolves about the Sun as one of the planets.

"Taking occasion thence," he writes, "I too began to reflect upon the Earth's capacity for motion. And though the idea appeared absurd, yet I knew that others before me had been allowed freedom to imagine what circles they pleased in order to represent the phenomena of the heavenly bodies. I therefore deemed that it would readily be granted to me also to try whether, by assuming the Earth to have a certain motion, demonstrations, more valid than those of others, could be found for the revolution of the heavenly spheres.

"And so, having assumed those motions which I attribute to the

Earth further on in the book, I found at length, by much long-continued observation, that, if the motions of the remaining planets be referred to the revolution of the Earth, and calculated according to the period of each planet, then not only would the planetary phenomena follow as a consequence, but the order of succession and the dimensions of the planets and of all the spheres, and the heaven itself, would be so bound together that in no part could anything



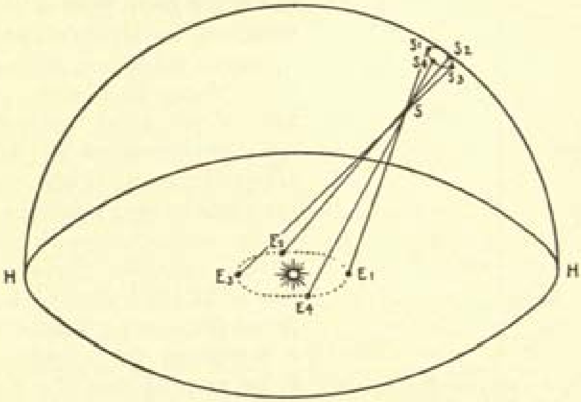
Illustr. 3.—The Universe according to Copernicus

be transposed without the disordering of the other parts and of the entire universe" (*Preface*).

The general arrangement of the solar system as conceived by Copernicus, suppressing the refinements which he subsequently introduced, is shown in his well-known diagram, where Mercury, Venus, the Earth, Mars, Jupiter, and Saturn describe concentric orbits about the Sun in the centre (see Illustr. 3).

"In the midst of all dwells the Sun. For who could set this luminary in another or better place in this most glorious temple than whence he could at one and the same time lighten the whole? . . . And so, as if seated upon a royal throne, the Sun rules the family of the planets as they circle round him. . . ." (I, 10).

Ever since the days of the Pythagoreans objection had been made to all planetary hypotheses which involved any motion of the Earth, on the ground that any such motion would give rise to a corresponding apparent motion in the stars (see Illustr. 4). Such an apparent motion, though sought, was never observed. Copernicus anticipates this sort of criticism by supposing that the stars are at a distance from us incomparably greater than the radius of the Earth's orbit, so that our annual motion makes no difference to their apparent direction. This objection, however, became ever more serious as observation improved in accuracy yet failed to reveal any annual stellar parallax. It was only finally removed

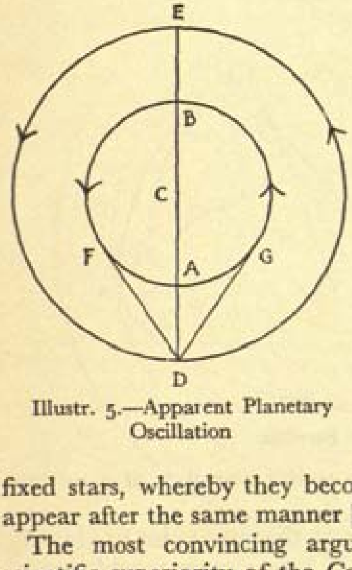


Illustr. 4.—Stellar Parallax

within the last hundred years, when stellar parallax of a minute order was observed in certain stars.

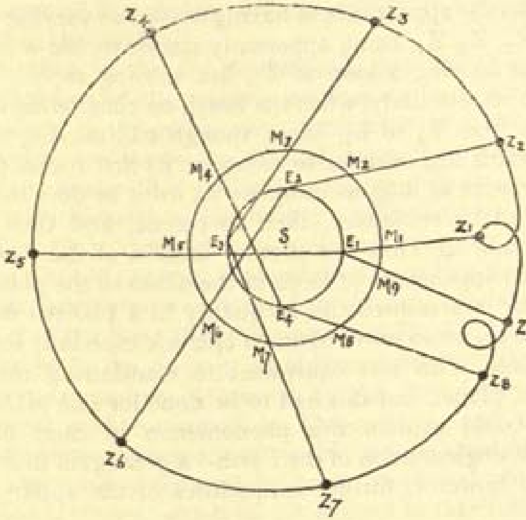
Copernicus was doubtless won over to the new point of view by its greater symmetry and coherence. These virtues would appeal to one imbued with Neo-Pythagorean ideas. For the essence of Pythagoreanism was its insistence that the universe is to be described in terms of mathematical relations; and that, of two geometrically equivalent planetary theories, the more harmonious and symmetrical was the more correct. But Copernicus had still to justify his point of view to the scholars of northern Europe, whose master was Aristotle rather than Pythagoras. He therefore devotes several of the early chapters of Book I to proving that the new system is just as much in accordance with Aristotelian physics as was the Ptolemaic system. His problem is to confute the arguments by which Aristotle had asserted that the Earth is at rest at the centre of the universe, while at the same time preserving Aristotle's principles intact and using them as the basis of his own arguments.

Copernicus reasons more soundly, however, from the principle that all motion is relative motion: "Every apparent change of position is due either to a motion of the object observed, or to a motion of the observer, or to unequal changes in the positions of both. . . . If then a certain motion be assigned to the Earth, it will appear as a similar but oppositely directed motion, affecting all things exterior to the Earth, as if we were passing them by" (I, 5). Copernicus applies this principle of the reciprocity of apparent motions in the first instance to account for the apparent diurnal rotation of the heavens: "If you will allow that the heavens have no part in this motion; but that the Earth turns from west to east, then, so far as pertains to the apparent rising and setting of the Sun, Moon, and stars, you will find, if you think carefully, that these things occur in this way" (I, 5). Later he brings this same principle to bear upon the phenomena connected with the apparent annual circuit of the Sun: "If [this circuit] be transposed from being a solar to being a terrestrial [phenomenon], and it be granted that the Sun is at rest, then the risings and settings of the constellations and the fixed stars, whereby they become morning and evening stars, will appear after the same manner [as before]" (I, 9).



Illustr. 5.—Apparent Planetary Oscillation

The most convincing argument, however, in favour of the scientific superiority of the Copernican hypothesis was the simple explanation which it could give of certain peculiarities in the apparent motions of the planets. If one of these bodies (say a superior planet) is observed night after night, it is found, in general, to be moving slowly across the southern sky from west to east relatively to the background of stars. From time to time, however, this eastward motion is arrested and reversed, and the planet travels a short distance from east to west before resuming its normal eastward direction. The physical significance of these *stations* and *retrogressions* of the planets had always been an enigma to astronomers; but Copernicus was able to show that these inequalities arise as necessary consequences of the annual motion of the Earth. Thus, suppose the respective orbits AB, DE of the Earth and of a superior planet (see Illustr. 5) to be coplanar circles concentric in C. Suppose at first that the Earth moves uniformly in its orbit with its mean motion, while the planet remains stationary at D. From D draw tangents DF, DG to the Earth's orbit. Then while the Earth is describing the arc FAG, the planet at D appears, to a terrestrial observer, to move in a retrograde direction through the angle FDG; but the planet appears to move direct through the same angle while the Earth is describing the remaining arc GBF of its orbit. That is, the planet appears to oscillate with an amplitude equal to the angle FDG. Now let the planet be supposed to move in its orbit



Illustr. 6.—Apparent Irregularities in Planetary Motion

with its mean motion, which is less than that of the Earth. Then the above-mentioned oscillation appears, to a terrestrial observer, to be superimposed upon the planet's steady eastward motion, and the peculiar movement characteristic of a planet is thus produced.

The way in which Copernicus thus explained the oscillation in the path of a planet as a mere appearance to a terrestrial observer in consequence of the orbital motion of the Earth may be made somewhat clearer by means of the above diagram (Illustr. 6).

Let S represent the position of the Sun at the centre of the universe. Let the smallest circle round S represent the orbit of the Earth, and E_1, E_2, E_3, E_4 four successive positions of the Earth at intervals of three months. Let the next larger circle represent the orbit of one of the planets, say Mars, and M_1, M_2, M_3, M_4, M_5, etc., successive positions of Mars at intervals of three months. Let the largest circle represent the place of the fixed stars, more especially of the various constellations, or the signs of the zodiac, among which the planets, as seen from the Earth, appear to move. Now, when the Earth is at E_1 then the planet Mars will be seen in the direction of the line E_1M_1, and will appear to be at Z_1; when the Earth is at E_2 and Mars at M_2 the latter will appear to be at Z_2; similarly when the Earth is at E_3, E_4, Mars will appear to be at Z_3, Z_4 respectively.*

It will be seen that in the course of the first year just described, although Mars really moved at a uniform velocity from M_1 to M_2, M_3, M_4, it has the appearance of having moved at varying velocities from Z_1 to Z_2, Z_3, Z_4, being apparently stationary for a time, then receding and forming a loop at Z_1, but moving rapidly from Z_2 to Z_3, and so on. Similarly, when the Earth on completing its second round passes from E_4 to E_1, Mars, though still moving uniformly from M_8 towards M_9, in order to complete its first round (for Mars takes nearly twice as long to complete its orbit as does the Earth), will appear to be stationary, then to recede, and then to loop between Z_8 and Z_9. Thus the circular motion of the Earth in its orbit gives the appearance of loops in the orbits of the planets.

Ptolemy had represented this inequality in a planet's motion by supposing the planet to move upon an epicycle especially introduced for the purpose. This was equivalent to transferring the Earth's motion to the planet. But this had to be done for *each* planet, while Copernicus could explain this phenomenon in each planet by reference to a single motion of the Earth—a great gain in simplicity.

There are, however, further inequalities in the apparent paths

* Although the distance from the Earth of even the nearest stars is of the order of a thousand to a million times that of the planets, yet the eye does not detect any difference in apparent distance between stars and planets. This is because our powers of judging distance are effective only within such a moderate range as that over which our vision normally extends on the Earth's surface. Within this range we instinctively judge the nearness of an object by the difference in the aspects which it presents to our two eyes, and also to some extent by the degree of effort necessary for accommodating the lenses of our eyes to the rays diverging from the object, and in bringing their two lines of gaze to converge upon it. The apparent size of a familiar object also assists us in localizing it, and so does the presence of an interposed series of other objects. But in viewing objects as distant as the planets, none of these aids to judgment is available. For the rays coming from these bodies are sensibly parallel, so that no accommodation or convergence of gaze is called forth. Moreover, even when (as with the Sun and Moon) celestial objects have an apparent size, we have no conception of their absolute size for comparison. And there is no series of objects extending from us to the heavens which would give us a scale of distance. Hence stars and planets alike appear projected on the background of a celestial sphere of indefinite radius.

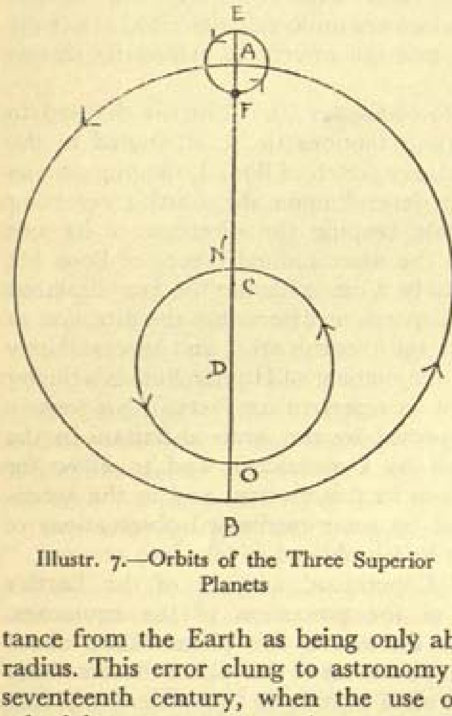
of the planets arising, as we know now, from the ellipticity of their orbits. Moreover, the Sun's rate of apparent motion in the ecliptic is not quite uniform from day to day. In order to account for these phenomena Copernicus was obliged to refine upon the simple scheme of Illustr. 3 (p. 16), where the Earth and planets all describe concentric circles with the Sun in the centre. In constructing his detailed planetary orbits, a task which occupies most of the *De Revolutionibus*, Copernicus employs eccentrics and epicycles like those of the ancients, but, unlike Ptolemy, he is always careful to ensure that his circular motions are uniformly described relatively to the centres of the circles, and not merely to arbitrarily chosen points within the circles.

A whole book of the *De Revolutionibus* (the third) is devoted to the consideration of the various motions to be attributed to the Earth. Already, in the preliminary sketch of Book I, the phenomena of the seasons are shown to depend upon the Earth's revolving annually about the Sun while keeping the direction of its axis approximately invariable. In the more refined theory of Book III the Earth's orbit is assumed to be a circle having the Sun displaced a little way from its centre. Copernicus determines the direction of the apse-line (see footnote on p. 136 f.) of this orbit, and its eccentricity with regard to the Sun. after the manner of Hipparchus. His theory is complicated by an attempt to represent an (actual) progressive motion of the apse-line (suspected by the Arab al-Battani in the ninth century and confirmed by Copernicus), and to allow for certain (imaginary) fluctuations in this motion, and in the eccentricity of the orbit, suggested by some mediaeval observations of questionable accuracy, which he felt obliged to take into account.

An important feature of Copernicus' account of the Earth's motion is his explanation of the precession of the equinoxes. Hipparchus of Rhodes, who discovered this phenomenon about 150 B.C., attributed it to a slow rotation of the sphere of stars about the axis of the ecliptic. Copernicus introduces the modern explanation of precession as due to an alteration in the plane of the Earth's equator causing the Earth's axis to describe a cone in space. Here again he needlessly complicates his theory in order to bring it into conformity with certain ancient and mediaeval observations. Throughout his work he adopts an entirely uncritical attitude to traditional data of this kind, and makes no allowance for the possibility of serious errors of observation, fraud, or textual corruption. This involves his theories in needless intricacies, while at the same time revealing his remarkable skill in geometry. Occasionally he relies upon twenty-seven observations of his own, which fill only one page of a modern edition, and are, on his own admission, crude.

"If only I can be correct to ten minutes of arc," he once said to Rheticus, "I shall be no less elated than Pythagoras is said to have been when he discovered the law of the right-angled triangle" (*Rhetici ephemerides novae*, 1550, p. 6).

Following upon this investigation of the Earth's motion, we have a book devoted to lunar theory. The Moon's relation to the Earth was unaffected by the change of outlook which Copernicus initiated, and he made no additions to the lunar inequalities in longitude known to Ptolemy. But his methods of representing these inequalities are more satisfactory than those of the *Almagest*. According to Ptolemy's theory, the angular diameter of the Moon should be twice as great at some times as it is at others; Copernicus found a means of representing the Moon's motion in longitude as correctly as Ptolemy had done, but without such a gross exaggeration of the slight fluctuations in the apparent size of its disc. Copernicus, however, adopts with only a slight alteration Ptolemy's gross underestimate of the Sun's distance from the Earth as being only about 1,200 times the Earth's radius. This error clung to astronomy until the latter half of the seventeenth century, when the use of the telescope for precise celestial measurements made an accurate determination possible.



Illustr. 7.—Orbits of the Three Superior Planets

The last two books of the *De Revolutionibus* (V and VI) treat of the motions of the planets in longitude and in latitude respectively.

Dealing first with the three superior planets, Copernicus assumes provisionally that, as regards motion in longitude, each has an orbit constituted as in Illustr. 7. The planet F describes an epicycle EF about a centre A, which revolves in a deferent circle AB whose centre is C. Epicycle and deferent are described in the same sense and the same period (viz. the sidereal period of the planet). If the Earth's orbit be represented by the circle NO, with centre D, then the radius AF is taken as one-third of CD. The elements of such an orbit (direction of apse-line ACDB and eccentricity CD/CB) can be determined, in theory, by three observations of the planet when it is in mean opposition, i.e. when it appears, to a terrestrial observer, to lie in the direction diametrically opposite to the centre D of the Earth's orbit. Copernicus confirms the adequacy of such a combination to represent approximately the motion of each of the superior planets by successively deriving the elements of the orbit from three of the observations in the *Almagest*, and then from three of his own, and by showing that the two sets of elements so obtained are in tolerable agreement with each other.

The combinations assigned to the inferior planets, Venus and Mercury, are considerably more complicated, but here too the elements of the hypothetical orbits are determined from suitably combined observations.

When once the elements of a planetary orbit had been thus fixed, Copernicus was able to consider an observation of the planet made when it was *not* in opposition. He could compare the position actually observed with the (calculated) position of the planet as it would appear from the centre of the Earth's orbit, and then find the difference between these two positions. From such data he was able to evaluate the radius of the planet's eccentric circle (deferent) in terms of the radius of the Earth's orbit. His results compare favourably with the modern "mean distances." We have here the earliest instance of an astronomer evaluating, in terms of the radius of the Earth's orbit, the dimensions of the planetary orbits without presupposing artificial or fanciful relations between these quantities, such as ancient astronomers who attempted the problem had been forced to assume.

In his account of the observed departures of the planets from the plane of the ecliptic—their motions in latitude—Copernicus introduces hypothetical periodic fluctuations in the inclinations of the planes of the several orbits. The necessity for doing this arose from what Kepler afterwards recognized as the fundamental mistake of referring the motions of the planets to the centre of the Earth's orbit instead of to the true Sun. Copernicus' treatment of the latitudes of the inferior planets is particularly complicated, and employs methods borrowed almost wholly from the *Almagest*.

It was an ambition of Copernicus to produce numerical planetary tables as accurate as any based on the geocentric hypothesis. From the theories set forth in the *De Revolutionibus* he constructed tables enabling the positions of the Sun, Moon, and planets at any given instant to be easily calculated. These tables, which form an essential feature of the book, were in fact an improvement upon those in current use, and this circumstance helped indirectly to make the new doctrine acceptable among astronomers. But the accuracy of the tables necessarily suffered through their being based upon a bare minimum of crude and often questionable observations embodied in a theory which was strained to conform to illusory physical laws. Such little improvement as could be made by a careful reconsideration of the data was effected a few years later by Copernicus' disciple Reinhold. Before the new cosmology could bear fruit in tables worthy of itself, however, there were needed the precise and systematic observations of Tycho Brahe, and the patient but adventurous genius of Kepler.

The Originality of Copernicus

We may now face the problem of assessing the originality of Copernicus' contribution to astronomy. It cannot be denied that his debts to Ptolemy were considerable. From the *Almagest* he derived many of his observational data and geometrical devices, as well as the material for his star catalogue.

In one sense, however, Copernicus' debt to Ptolemy was insignificant, for the ideas with which he revolutionized European astronomy were entirely alien to the Alexandrians. The rudiments of these ideas are to be found, if anywhere, in the speculations of a few men who stood apart from the main current of opinion, and whose recorded teachings are scattered through classical and mediaeval literature. A study of these passages, so far as they were probably known to Copernicus, seems to make it clear that the basic ideas underlying his system did not originate with him. For instance, the complete heliocentric system had been anticipated, in its broad outlines, by Aristarchus of Samos (*c.* 250 B.C.), who has been called on that account the "Copernicus of Antiquity"; though Copernicus' relation to the ideas of Aristarchus is unfortunately obscure. From whatever source he derived his fundamental ideas, however, Copernicus' great and unquestionable contribution to astronomy must be held to lie in his elaboration of those ideas into a coherent planetary theory capable of furnishing tables of an accuracy not before attained. It is true that we can no longer regard the Sun, or the centre of the Earth's orbit, or any other origin of reference, as being at rest in space except as a matter of temporary convenience in treating some special problem. Nevertheless, the scientific utility of the heliocentric point of view, and its contacts with observed fact, have increased enormously since it was formulated by Copernicus. To him it merely represented the most symmetrical arrangement of the planets, and the simplest manner of accounting for their observed motions. But to Kepler it was the necessary precondition for his discovery of the laws of planetary motion, and to Newton it opened the way to a rational explanation of those laws. Lastly, the cosmogonists, from Laplace to Jeans, have given to the heliocentric theory a new, genetic significance by recognizing in the Sun the central, parent body from which, under the action of centrifugal or tidal forces, the substance of the planets was originally ejected.

The Spread of Copernicanism

The Copernican system took the better part of a century to become firmly established in scientific thought. It was opposed from the beginning by Luther and the Reformers, and though at first it was tolerated by the Catholics, their resistance to it strengthened until, in 1616, they were prepared to forbid Galilei to teach Copernican astronomy. Nevertheless, the new doctrine spread widely, especially among practical astronomers. The heliocentric theory was favourably noticed by a number of English scientific writers, beginning with John Field in 1556 (*Ephemeris anni* 1557 *currentis juxta Copernici et Reinholdi canones*) and including William Gilbert, who sought to establish a relation between his speculations in magnetism and the Copernican theory (*De Magnete*, 1600, Bk. VI). Francis Bacon opposed the heliocentric hypothesis (*Novum Organum*, Bk. II, xlvi, etc.); and it owed its final acceptance in scientific circles chiefly to the authority of Galilei, Kepler, and Descartes, and, later, Newton.

Among the earliest adherents of the Copernican doctrine was Giordano Bruno (1548–1600), who started as a Dominican monk, and thereafter wandered through Europe teaching heretical opinions, for which he was finally burned at the stake by the Inquisition. In philosophy he was a pantheist, and one of the forerunners of Spinoza. Bruno's writings include an apology for Copernican astronomy, but he went further than his master and abandoned the belief that the stars are fixed to a crystal sphere having the Sun at its centre. He regarded them as Suns scattered through infinite space and forming the centres of innumerable planetary systems like ours. He intuitively anticipated many discoveries which were later established by observation, as, for example, that the Sun rotates on its axis and that the Earth is flattened at the poles. He regarded comets as a species of planets, and supposed that the solar system might possess more planets than those known at that period. Bruno foreshadowed to some extent the doctrine of the Conservation of Energy, teaching that the only thing that is eternal in this world of incessant change is the creative energy underlying all things.

After Copernicus, the next important astronomer was Tycho Brahe, and he would be dealt with next if chronology and astronomy were the main considerations. His work, however, is inseparable from that of Kepler, and the work of Kepler from that of Newton. Now, Galilei was a younger contemporary of Tycho Brahe and an older contemporary of Kepler. And Galilei holds a peculiarly important place as a pioneer of modern science. He not only made valuable astronomical discoveries, but he laid the foundations of dynamics, and thereby prepared the way for the Newtonian synthesis. Moreover, he made important contributions also to other sciences, inspired the foundation of the oldest scientific academy, and gave a great impetus to the invention of new scientific instruments. In these and other ways he exercised a paramount influence on the general progress of modern science, and not only on astronomy. It will therefore be convenient and appropriate to deal with him, with the first scientific academies, and with the new scientific instruments, before turning to Tycho Brahe and Kepler and the further progress of astronomical science.

(See J. L. E. Dreyer, *History of the Planetary Systems from Thales to Kepler*, Cambridge, 1906; A. Berry, *A Short History of Astronomy*, 1898; A. Armitage, *Copernicus, the Founder of Modern Astronomy*, London, 1938; E. Rosen, *Three Copernican Treatises*, New York, 1939; D. Stimson, *The Gradual Acceptance of the Copernican Theory of the Universe*, New York, 1917; R. Wolf, *Geschichte der Astronomie*, Munich, 1877; E. Zinner, *Die Geschichte der Sternkunde*, Berlin, 1931.)

CHAPTER III

GALILEO GALILEI

ITALY had been the scene of the revival of classical learning. It was in Italy likewise that the foundations of modern science were laid by Galilei and his disciples. When the darkness of the Middle Ages began to lift, Italy was divided into numerous republics and principalities which competed for the leadership sometimes by means of warfare and sometimes by means of more friendly forms of rivalry. These small States maintained themselves chiefly by means of commerce and industry. After the introduction of the mariner's compass and geographical charts Italian seamen had developed a considerable traffic with the Levant. One result of this was the rapid growth of Italian arts and crafts. The glassware of Venice, and the decorative enamelled pottery and the metal castings of Majolica and of other Italian cities were unrivalled. Of course Italy had also far greater achievements to her credit at a somewhat earlier period—the immortal poetry of Dante and Petrarch, the universal genius of Leonardo da Vinci, the supreme art of Raphael and Michael Angelo. But at the beginning of the modern period Italian art was beginning to decline, and the scientific spirit burst into vigorous growth. On the very day on which Michael Angelo died Galileo Galilei first saw the light, as if Italian science had been destined to take over the glory of Italian art.

EARLY LIFE OF GALILEI

Galileo Galilei was born on February 15, 1564, at Pisa, which was at that time under the Medicean government of Florence, though it had been a free city during the Middle Ages. Galileo's father was Vincenzio Galilei, an impoverished nobleman with a passion for music and mathematics, and the author of a Dialogue on Music, in which he protested against the customary appeal to authorities. The tastes and tendencies of the father reappeared with interest in the son.

At school Galileo showed much industry in his work, and a measure of independent thought which distinguished him from his schoolfellows. He next took up the study of medicine. In those days the study of medicine all over Europe was somewhat like the study of law in England at the present day—something to which sons might be put when their parents were not yet clear as to what they ought to do with them. The state of medicine at that time, however, was not such as to appeal to the interests of young Galilei. The exact sciences attracted him much more. The story is told that he used to listen at the door of the classroom in which lectures were given on mathematics, and that he tried to gather crumbs of information from the students as they left. When the lecturer on mathematics heard of it he took steps which enabled Galilei to change from the study of medicine to the study of mathematics and physics. His progress in these sciences was such that at the age of twenty-five he was appointed lecturer at the university of his native town.

His independent researches into physical phenomena had by this time convinced Galilei of the erroneousness of much that was taught as Aristotelian physics and was accepted as authoritative. He made no secret of his views. On the contrary, he openly attacked various Aristotelian views on physics with such persistence that he made himself unpopular with his colleagues, who regarded him as rather aggressive. A legend, now rejected, states that he dropped three bodies of different weights at the same time from the top of the Leaning Tower of Pisa and showed, from their simultaneous arrival on the ground, that Aristotle was wrong in teaching that the velocities of falling bodies varied with their weights. Stevin had made the experiment elsewhere before Galilei and there is no evidence that Galilei repeated it. Galilei's attitude displeased his Aristotelian colleagues and made them unfriendly towards him. When therefore the Senate of Venice offered him a post in the University of Padua in 1592, he readily accepted the invitation and began to lecture in Padua in December of that year.

Galilei was no respecter of books or of the mere shows of learning. Though well versed in Latin, the Esperanto of scholars of that time and long afterwards, he preferred to lecture and to write in Italian. And in his earliest treatise on motion, in which he controverts the above-mentioned Aristotelian doctrine about the velocity of falling bodies, he stated explicitly that he was quite indifferent whether his own view did or did not agree with that of others so long as it was in harmony with experience and reason. But Galilei did have a passion for close observation and reasoning. It is related that already as a youth he noticed, while sitting in the cathedral at Pisa, that as the lamp, suspended from the roof by a long chain, was swinging to and fro, each swing, whether short or long, seemed to take the same time, as ingeniously measured by his pulse-beat. With such an open and observant mind it was natural enough for him to take a sympathetic interest in the heliocentric theory of Copernicus, in spite of the hostility of the Church. Indeed, he seems to have embraced the Copernican view quite early in life, in fact "many years" before

1597, as appears from a letter which he wrote in that year to Kepler in acknowledgment of the latter's *Prodromus*. Part of this letter is worth quoting in this connection. "I consider myself fortunate," Galilei wrote, "to have found such a great confederate in the search for truth. It is really pitiable that there are so few who strive after truth and are ready to abandon perverse methods of philosophizing. However, this is not the place to lament the sorry plight of our age, but only to congratulate you on your splendid researches. . . . I do so all the more gladly because I have been for many years a follower of the Copernican theory. It explains to me the reasons of many phenomena which are quite incomprehensible according to the views commonly accepted. I have collected many arguments for the refutation of the latter, but I dare not publish them. . . . To be sure, I would dare do it, if there were many such men as you are. But as this is not the case, I must put them aside" (*Opere*, Edizione Nazionale, Vol. X, p. 68). Galilei had every reason to be cautious, as sad experience was to teach him in due course. Indeed, within three years of the writing of this letter, his countryman, Giordano Bruno, perished at the stake for his Copernican and other heresies. Galilei's first conflict with the anti-Copernicans took place in 1604, when the observation of a new star arrayed Galilei and Kepler in a united combat with the Aristotelians, who insisted on locating it within the lunar sphere, beyond which, according to the Aristotelians, no changes could occur and nothing new could appear.

The intellectual atmosphere at Padua, Galilei soon discovered, was not much more encouraging than it had been at Pisa. This is evident from another letter which he wrote to Kepler, and from which the following passage is worth citing as evidence of the amazing extent to which faith in authority may produce blindness to facts.

"I wish, my dear Kepler, that we could have a good laugh together at the extraordinary stupidity of the mob. What do you think of the foremost philosophers of this University? In spite of my oft-repeated efforts and invitations, they have refused, with the obstinacy of a glutted adder, to look at the planets or the Moon or my glass [telescope]! . . . Why must I wait so long before I can laugh with you? Kindest Kepler, what peals of laughter you would give forth if you heard with what arguments the foremost philosopher of the University opposed me, in the presence of the Grand Duke, at Pisa, labouring with his logic-chopping argumentations as though they were magical incantations wherewith to banish and spirit away the new planets [the satellites of Jupiter] out of the sky!" (*Opere*, Ed. Naz., Vol. X, p. 423).

Galilei's Astronomical Discoveries

The story of the telescope will be told in the chapter on scientific instruments. Here it need only be stated that in 1609 Galilei constructed a telescope on the Dutch pattern, and was the first to use it as a scientific instrument. The most important discovery which he made by means of it was that Jupiter had four satellites revolving round it. He first saw three of them on January 7, 1610, and all four a few days later. As a compliment to the ruling prince, he named them the "Medicean stars." The observation of Jupiter with its moons served Galilei as a convincing analogy of the solar system as conceived by Copernicus. Towards the end of 1610 Galilei discovered that Venus had phases like the Moon. Next he discovered the nature of the Milky Way, and very nearly discovered Saturn's Rings. What great significance he attached to his discoveries is clear from letters which he wrote to Belisario Vinta in January and July 1610. "I am quite beside myself with wonder," he wrote on January 30th, "and I am infinitely grateful to God that it has pleased Him to permit me to discover such great marvels as were unknown to all the preceding centuries. That the Moon is a body resembling the Earth, of this I felt certain already before. I have also observed a multitude of fixed stars that had never been seen before, and which are more than ten times as numerous as those which are visible to the naked eye. . . . And I know now what the Milky Way is" (*Opere*, Ed. Naz., Vol. X, p. 280). On July 30, 1610, he wrote: "I have discovered that Saturn consists of three spheres which almost touch each other, which never change their relative positions, and are arranged in a row along the zodiac so that the middle sphere is three times as large as the others" (*Idem*, p. 410). A word or two may be added here in elucidation of some of the points mentioned in this letter. The Moon, seen through the telescope, appeared to Galilei to have mountains and valleys, just like the Earth, and he even estimated the heights of the lunar mountains from the lengths of their shadows. With regard to the much larger number of fixed stars visible through the telescope as compared with the number seen with the unaided eye, Galilei counted, for instance, forty fixed stars in the constellation Pleiades, whereas he could only see six of them with the naked eye.

Another important astronomical discovery made by Galilei consisted of the Sun-spots, which he first observed in October 1610. But the honour of this discovery must be divided between him and two or three other astronomers of his time. Kepler, as we shall see, had somehow become aware of the existence of spots on the surface of the Sun, even without the aid of a telescope. And Fabricius had seen them through his telescope before Galilei did. In his *De maculis in sole observatis*, published in 1611, Fabricius reported as follows: "As I was carefully observing the edge of the Sun a black spot appeared unexpectedly. At first I thought it was a passing cloud. The next morning, however, the spot was visible again as soon as I looked, although it appeared to have changed its position slightly. The weather was dull during the three days following. When the sky was clear again the spot had shifted from east to west, and smaller spots occupied its previous position. Afterwards the large spot gradually moved towards the opposite edge and disappeared there. From their movements it was evident that the smaller spots would do likewise. A vague hope prompted me to expect their return. And the large spot did in fact reappear at the eastern edge ten days later." Another early observer of the Sun-spots was Scheiner, who observed them in April 1611. At first he suspected that the phenomenon was an optical illusion, or that it was due to some fault in his telescope. But after he and his friends had seen them through eight different telescopes, he could no longer doubt the reality of the spots. Even then he was uncertain whether the spots were on the Sun itself, or only near it. But he inferred from their movements, which he studied closely and with great perseverance, that the Sun must be rotating on its axis. Fabricius maintained from the first that the spots were on the Sun itself, and not due to the revolution of dark bodies near and round the Sun. Galilei confirmed this view by pointing out that the great diminution in the velocity with which the spots appeared to move when nearing the edge of the Sun, as compared with their velocity during the rest of their path across the Sun, could be best explained on that assumption. Eventually this view of the nature of the Sun-spots was generally accepted, and their movements afforded the data for determining the rotation-period of the Sun and the position of the solar equator.

Galilei also made various observations of nebulae. But he was not the first to do so. Simon Marius appears to have made the first observation of a nebula (the one in the constellation Andromeda) in 1612. Galilei regarded nebulae and the Milky Way as clusters of numerous stars.

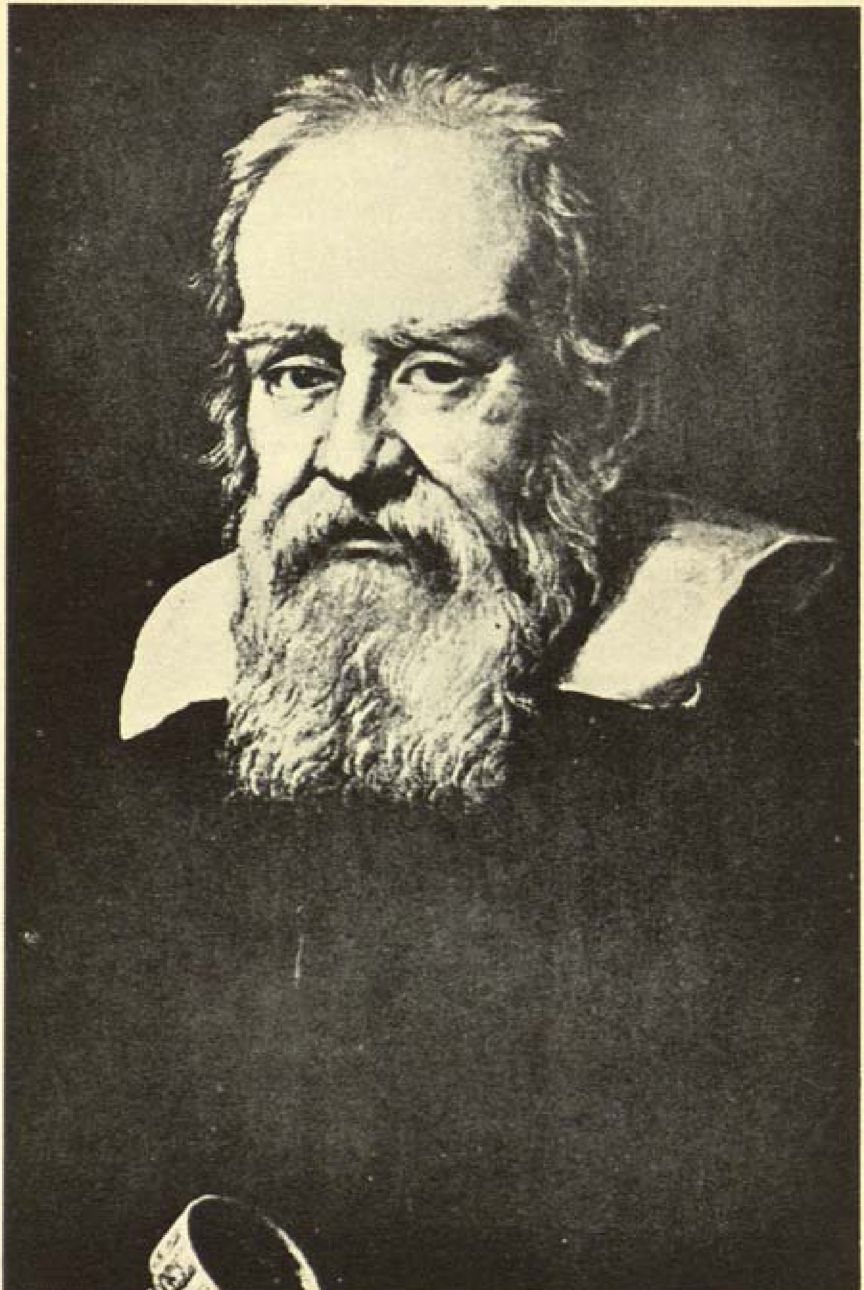
In order to complete this sketch of Galilei's contributions to astronomy, we must anticipate some of his later work. His last telescopic discoveries were made in 1637, just before he went blind—the daily and monthly librations of the Moon, the small alternations of visibility and invisibility of parts of the Moon's surface near the edges of her disc. Galilei next attacked the problem of measuring longitude on land and at sea—a very important matter for seafaring nations. He attempted to utilize for this purpose one of his early discoveries, namely, the satellites of Jupiter. In ancient and mediaeval times longitude was sometimes determined by reference to lunar eclipses, that is, by comparing the local times of the appearance of a lunar eclipse in different parts of the Earth. But, owing to the comparative rarity of the lunar eclipses, this method was not very helpful. The periods of revolution of Jupiter's satellites being so short that almost every night one or other of them is eclipsed by the central planet, Galilei thought it would be possible to use these eclipses for the purpose in question. He had actually constructed approximately accurate tables of the revolutions of these satellites. But for various reasons the ingenious idea did not come to fruition.

The Dialogue on the Ptolemaic and Copernican World-Systems

In 1632 Galilei published his *Dialogue concerning the two chief Systems of the World, the Ptolemaic and the Copernican* (English translation by T. Salusbury, 1661). The work consists of four comprehensive dialogues (or "Days"). Galilei's choice of the form of the dialogue as the medium of his thought may have been prompted by various literary and other reasons. The chief reason, however, was probably the desire to be cautious and not to commit himself too much. A discussion between several interlocutors always leaves the author the loophole of pleading, if necessary, that certain views were not really his own, but those of the imaginary character of the dialogue into whose mouth they have been put on literary or imaginative grounds. Of the characters in Galilei's *Dialogue* Salviati and Sagredo were friends and followers of Galilei, while Simplicio was named after the Aristotelian commentator, and plays the part of a fanatical defender of authority and tradition.

The *Dialogue* opens with an attack on the Aristotelian doctrine that the celestial bodies are totally different from the Earth in their nature and composition, and that the heavens are immutable. The appearance of new stars and the Sun-spots are cited as evidence to the contrary. The mountains visible in the Moon by means of the telescope disprove the Aristotelian view that the Moon is a perfect sphere. As to the indestructibleness of the heavenly bodies, it is contended that all matter, even terrestrial matter, is indestructible. "I never," says Salviati in the *Dialogue*, "was thorowly satisfied about this substantial transmutation (still keeping within pure natural bounds) whereby a matter becometh so transformed, that it should be necessarily said to be destroyed, so that nothing

Illustr. 8



Galileo Galilei

DIALOGO
DI
GALILEO GALILEI LINCEO
MATEMATICO SOPRAORDINARIO
DELLO STVDIO DI PISA.
E Filosofo, e Matematico primario del
SERENISSIMO
GR. DVCA DI TOSCANA.

Doue ne i congressi di quattro giornate si discorre
sopra i due

MASSIMI SISTEMI DEL MONDO
TOLEMAICO, E COPERNICANO;

Proponendo indeterminatamente le ragioni Filosofiche, e Naturali
tanto per l'vna, quanto per l'altra parte.

CON PRI VILEGI.

IN FIORENZA, Per Gio: Batista Landini MDCXXXII.

CON LICENZA DE' SVPERIORI.

Title page of Galilei's *Dialogo* (1632) remaineth of its first being, and that another body quite differing there-from should be thence produced; and if I fancy to my self a body under one aspect, and by and by under another very different, I cannot think it impossible but that it may happen by a simple transposition of parts, without corrupting or ingendring anything a-new" (Translation by Thomas Salusbury in his *Mathematical Collections and Translations*, London, 1661, Vol. I, pp. 27, 28).

Occasionally the *Dialogue* throws a shaft of ridicule at the Scholastics, reducing their arguments to absurdity. Thus, for example, when Simplicio contends that Aristotle could not have committed any mistakes in reasoning, seeing that he was the founder of Logic, the retort is made that a man might very well be a competent maker of musical instruments without being an expert musician.

Turning to the question whether all the heavenly bodies revolve round the Earth in twenty-four hours or whether the Earth really rotates on its axis in that period and thereby produces a merely apparent revolution of the starry heaven, the *Dialogue* allows that *prima facie* either hypothesis would account for the observed phenomena, but that, all considered, the hypothesis of the Earth's rotation is the more plausible one. When we consider the vastness of the starry heaven in comparison with the many million times smaller Earth, and think of the enormous velocity required for the starry heaven to complete its revolution round the Earth in one day, then it seems incredible that the heavens should be moving while the Earth stands still. Moreover, if the Earth is assumed to stand still, the fixed stars must be supposed to be moving in a direction contrary to that of the planets, all of which move from West to East, and move comparatively slowly. Again, the period of the revolution of the several planets increases with the size of their several orbits, the Moon completing its orbit in 28 days, Mars in 2 years, Jupiter in 12 years, and Saturn, the remotest planet, in 30 years. The same rule holds good of the satellites of Jupiter, which complete their orbits in 42 hours, 3½ days, 7 days, and 16 days respectively, according to their increasing distance from Jupiter. But if we assume the starry heaven to revolve round the Earth, then we should be faced with the paradox of an increasing periodicity from the Moon, with its thirty-day period, to Saturn, with its thirty-year period, followed by a sudden and enormous diminution in the periodicity of the vastly more distant stars, with a period of one day! Moreover, we should be compelled to suppose that even the fixed stars themselves move with vastly varying velocities according to their several distances from the celestial pole. To add to the difficulties of the Ptolemaic view, the positions of the fixed stars undergo slow changes. Some, which were at the equator thousands of years ago and followed the largest orbits, are now at a distance of several degrees from the equator, and must therefore move more slowly in smaller orbits. It may even happen that a fixed star which had always been moving may for a time remain stationary at the celestial pole, and then begin to move again.

Various arguments are directed not only against the Scholastic glorification of the heavenly bodies at the expense of the Earth, but also against the whole notion that immutability is a mark of perfection. "I cannot without great admiration, nay more, denial of my understanding," says Sagredo, one of the characters of the *Dialogue*, "hear it to be attributed to natural bodies, for a great honour and perfection, that they are impassible, immutable, inalterable, etc. And on the contrary, to hear it to be esteemed a great imperfection to be alterable, generable, mutable, etc. It is my opinion that the Earth is very noble and admirable, by reason of so many and so different alterations, mutations, generations, etc., which are incessantly made therein. . . . The like I say of the Moon, Jupiter, and all the other Globes of the World" (*op. cit.*, pp. 44, 45). Change is to be observed everywhere. New stars come into view (for instance, in 1572 and 1604), Sun-spots come and go, comets appear and disappear. The same kind of natural events happen throughout the universe, and even the heavens conform to natural laws. The Copernican or heliocentric hypothesis, it is contended in the *Dialogue*, also explains quite simply the halts and retrogressions of the planets as mere appearances due to the annual revolution of the Earth, whereas the Ptolemaic or geocentric hypothesis could not explain them at all without resort to the most extravagant suppositions. There are also certain terrestial phenomena which, according to the *Dialogue*, seem to support the Copernican hypothesis, namely, the tides and the trade-winds, which are best explained as due to the rotation of the Earth.

Among the greatest services which Galilei rendered to the Copernican theory must probably be included his treatment of the two chief objections to the heliocentric hypothesis, namely, the absence of stellar parallax and the vertical fall of terrestrial bodies. The first objection had been raised already in ancient times, by Aristotle, against every kind of non-geocentric view. It was contended, namely, that if the Earth revolves in an orbit round the Sun, then the fixed stars should show an apparent change of position (parallax) when the Earth passes from one position on its orbit to the extreme opposite position (see Illustr. 4, p. 17). The *Dialogue* met this objection by pointing out that the enormous distances of the fixed stars necessarily made such parallax imperceptible, for the fixed stars must be at least ten thousand times as far away from the Earth as is the Sun. (In fact it was not till 1838 that astronomical instruments and methods had developed sufficiently for the measurement of stellar parallax by F. W. Bessel.)

The other objection was equally old, having also been raised already by Aristotle, who argued that if the Earth rotated, then an object thrown up vertically should not return to the place from which it was thrown, but slightly to the west of that position, because during the time taken up by the rise and fall of the object the Earth must have rotated slightly towards the east; as a matter of fact, however, objects so thrown usually return to their original places. Moreover, it was argued that if the Earth rotated, then objects on its surface, at least not very near its poles, should be flung off the surface by the centrifugal force of the rotation. The *Dialogue* counters the former argument by reference to the law of inertia, one of Galilei's most important discoveries in the whole history of science. A stone dropped from a high tower will fall at its foot because the stone itself is moving eastward with the same velocity as the tower is moving eastward. A stone dropped from the mast-top of a stationary or of a moving ship will in either case fall at the foot of the mast. [It is noteworthy that Tycho Brahe had denied this, in his *Epist. Astr.*] If there is a slight deviation in the fall of the stone in the case of the moving ship, this deviation will be due to the resistance of the air. For, in relation to the moving ship, the air is at rest, whereas in the case of the stationary ship the mast, the stone, and the air share equally in the rotatory motion of the Earth, and therefore the air through which the stone falls will not in this case affect the direction of its fall. The second argument is countered by pointing out that in consequence of the comparative slowness of the Earth's rotation round its axis, the centrifugal force is much smaller than is the force of gravitation, and so objects remain on the Earth's surface, undisturbed by its rotation.

Galilei's *Dialogue* is one of the three greatest masterpieces of modern astronomical literature, the other two being the *Revolutions* of Copernicus, and the *Principia* of Newton. And it has the advantage of being the most readable of the three.

Galilei and the Church of Rome

At a comparatively early age Galilei, as has already been pointed out, had become a convinced Copernican. The work of Copernicus was on the Index of Prohibited Books, and Galilei had to be cautious. But in course of time his enthusiasm for the heliocentric theory ran away with him, and his dislike of Scholastic prejudice and intolerance must have betrayed him sometimes into utterances that were indiscreet in his time. In 1613 he published his *Letters on the Solar Spots*, which left no doubt about his Copernicanism. He was accused of heresy, and he defended himself with vigour, trying not only to explain away Biblical texts adverse to a heliocentric theory, but even to cite Biblical texts in support of it. He was consequently warned, in 1615, to abstain from theological argumentation. Early in 1616 the expert theologians of the Holy Office published the following edict: "The view that the Sun stands motionless at the centre of the universe is foolish, philosophically false, and utterly heretical, because contrary to Holy Scripture. The view that the Earth is not at the centre of the universe, and even has a daily rotation, is philosophically false, and at least an erroneous belief." All books teaching the doctrine of the Earth's motion were banned, and Galilei was warned by Pope Paul V not to "hold, teach, or defend" the Copernican theory.

Galilei had left Padua in 1610 and, except for occasional visits to Rome, was living in Florence under the patronage of the Duke of Tuscany. For several years following his admonition in 1616 Galilei kept more or less quiet, diligently pursuing his scientific researches. In 1623 he published his *Saggiatore*, in which he rather too ingeniously attempted to explain comets as atmospheric phenomena like halos and rainbows. It was dedicated to the new Pope, Urban VIII, who was interested in astronomy, had celebrated in verse Galilei's discovery of Jupiter's satellites, and now overlooked some passages in the *Saggiatore* containing a covert defence of Copernican views. Things looked so promising that Galilei appears to have attempted to convert the Pope to the heliocentric theory, or to persuade him at least to revoke the decree of 1616. But in vain. And when, in 1632, Galilei published the *Dialogue on the Two Chief World-Systems*, which stirred the whole learned world, the storm burst over his head. The *Dialogue* had been passed by the Censor before publication. But the Jesuit Scheiner, who had had a dispute with Galilei over the question of priority in the observation of the Sun-spots, succeeded in making mischief. It is alleged that he persuaded the Pope that it was he who was intended in the *Dialogue* by Simplicio, the clumsy defender of the geocentric theory. Anyway the book was banned, and the author was summoned to Rome by the Inquisition. At first he pleaded illness, but went to Rome in February 1633, and was put under detention. In June he was examined by the Inquisition and threatened with torture. He recanted, was sentenced to detention during the pleasure of the Inquisition, and was ordered to recite the seven penitential Psalms every week for three years. The recantation which Galilei was compelled to make, and to make in his bare shirt, is worth citing as a document in the history of the relation between religion and science. Here it is slightly abridged. "I bend my knee before the honourable Inquisitor-General, I touch the holy Gospel and give assurance that I believe, and always will believe, what the Church recognizes and teaches as true. I had been ordered by the holy Inquisition not to believe nor to teach the false theory of the motion of the Earth and the stationariness of the Sun because it is contrary to Holy Scripture. Nevertheless I wrote and published a book in which I expound this theory and advance strong grounds in its favour. I have consequently been pronounced to be suspect of heresy. Now, in order to remove every Catholic Christian's just suspicion of me, I abjure and curse the stated errors and heresies, and every other error and every opinion that is contrary to the teaching of the Church. I also swear that in future I will never, whether by written or spoken word, utter anything that may bring me again under suspicion. And I will immediately inform the holy tribunal if I see or suspect anything heretical anywhere." Apparently Galilei was intended, not only to change his own convictions, but also to turn spy on others, and to deliver them to the tender mercies of the holy Inquisition. Legend describes Galilei, after his enforced humiliating recantation, as muttering to himself the words, "but the Earth does move." The legend at least expresses the general belief as to what really passed in Galilei's soul, perhaps even the growing derision and condemnation of any attempt, whether of the Church or any other powerful organization, to stop the march of scientific thought.

The *Dialogue* and other Copernican works remained on the Index until 1822, when at long last the College of Cardinals declared it permissible to teach the Copernican theory in Catholic countries. So the infallible Church had to recant its earlier view. Scientific thought may move extremely slowly in some quarters; "but it does move."

After several months of detention or semi-detention Galilei was allowed to live in seclusion at Arcetri near Florence. His devotion to science remained unabated. But henceforth he confined himself to investigations which were not likely to bring him into conflict with the Church. His most important contributions to science, the *Discourses on Two New Sciences*, were published in 1638, by the Elzevirs, at Leiden, in Holland. It had been completed in 1636, but could not be published at once because his works were banned in Italy. In 1637 Galilei became totally blind, but he continued to do what he could, with the aid of his disciples, notably Viviani and Torricelli.

In 1638 Galilei was visited at Arcetri by John Milton, the great Puritan poet, whose *Samson Agonistes* (1671) might be regarded as embodying the tragedy of the blind Galilei as well as that of the poet. Six years afterwards Milton referred to this visit in his *Areopagitica* (1644), the majestic plea "For the Liberty of Unlicenc'd Printing," the opening pages of which express his preference of "the old and elegant humanity of Greece" over "the barbarick pride of a *Hunnish* and *Norwegian* stateliness." Milton's remarks are as significant to-day as ever, perhaps even more so, in view of the new wave of Hunnish barbarity, Fascist tyranny in the land of Galilei, and the growing contempt for liberty. In the course of his criticism of the Order of Parliament against printing unlicensed books, Milton remarks: "And lest som should perswade ye, Lords and Commons! that these arguments of lerned mens discouragement at this your Order, are meer flourishes, and not reall, I could recount what I have seen and heard in other Countries, where this Kind of Inquisition tyrannizes; when I have sat among their learned men, for that honor I had, and bin counted happy to be born in such a place of *Philosophic* Freedom, as they suppos'd England was while themselves did nothing but bemoan the servil condition into which Lerning amongst them was brought; that this was it which had dampt the glory of Italian wits; that nothing had bin there writt'n now these many years but flattery and fustian. There it was that I found and visited the famous *Galileo* grown old, a prisoner to the Inquisition, for thinking in Astronomy otherwise than the Franciscan and Dominican Licensers thought" (*Areopagitica*, ed. T. Holt White, 1819, pp. 116 f.).

Galilei died in 1642. In the same year a new star rose in the West—Newton was born.

Discourses on Two New Sciences

Galilei's astronomical discoveries were certainly very important, and made a great impression on thoughtful people even outside the ranks of men of science. From a purely scientific point of view, however, his contributions to mechanics were even more important. They were epoch-making. And the *Discourses* in which they were dealt with were rightly described by him as presenting two new sciences, or branches of science. Throughout his active life Galilei had been occupied, on and off, with problems of mechanics, but he concentrated on them with special intensity after his tragic experiences at the hands of the Church, and brought all his experiments and results together in the *Discourses* (English translation by H. Crew and A. de Salvio, New York, 1914). This work also is in the form of dialogues, and the characters are the same as in the *Dialogue* of 1632, namely, Sagredo, Salviati, and Simplicio, of whom the first two represent Galilei's views, while the last defends the Aristotelian or Scholastic views.

Galilei's epoch-making contributions to mechanics consisted mainly in building up dynamics, that is, the science of moving bodies. Except for some minor contributions made by Archimedes, Leonardo da Vinci, and a few others, next to nothing had been done in this branch of mechanics. Galilei's investigations into the laws of falling bodies, the movement of pendulums, and of projectiles, set an example of the scientific combination of quantitative experiment with mathematical demonstration, which has remained the ideal method of the exact sciences.

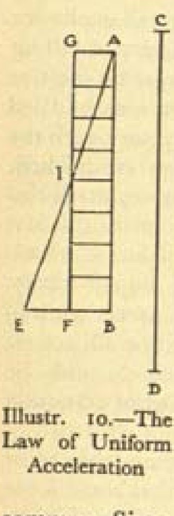
THE LAWS OF FALLING BODIES

Reference has been made to Galilei's criticism of Aristotelian physics and his rejection of the Aristotelian view that the velocity of falling bodies varies with their weight. But this, of course, threw no positive light on the law of their fall. Even superficial observations had led to the suggestion that the velocity of falling bodies may vary with the duration of their fall, but nothing definite had been established. Galilei first introduced the idea of uniform acceleration, as distinguished from uniform velocity, and solved the problem of the law of falling bodies in terms of such acceleration. By uniform acceleration Galilei meant equal increases in velocity in equal times. Another requisite for the study of dynamics was a correct idea of inertia, and this likewise Galilei was the first to introduce. Of course it was known long before his time that a body at rest can only be set in motion by the action of some force upon it. But the extension of the principle of inertia to bodies in motion was not dreamed of before Galilei. It was commonly supposed that a moving body must come to rest, even without any kind of resistance, unless some force continues to keep it moving. As against this assumption Galilei formulated it as part of the principle of inertia that a body once in motion continues to move with the same velocity and in the same direction unless some force acts upon it. Moreover, he held that when a force does act upon a body, the effect is just the same whether the body is at rest or in motion. These conceptions made it possible to describe correctly what happens when a body falls freely. In this case there is a force (*gravity*) which is acting continuously on the body with cumulative effect, since the effect produced at every moment continues, according to the law of inertia. The result is a uniform acceleration in the velocity of the falling body. Thus if a body at rest is allowed to fall, and falls for a time t, and attains at the end to a velocity v, then its velocity will have increased uniformly from 0 at the beginning (when it started from rest) to v

at the end; and the distance, *s*, covered during the fall will therefore be the same as if it had fallen all the time with the uniform velocity $v/2$, namely, $vt/2$. Galilei's graphic or geometrical method of dealing with this problem is typical of his mathematical method, and may be quoted as a simple example of its application.

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 [Illustr. 10] 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 [*momenta*] assumed by the moving body may also be 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.



Illustr. 10.—The Law of Uniform Acceleration

"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."

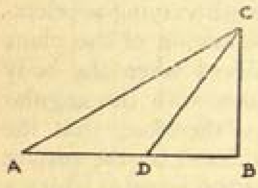
From the equation $s = vt/2$ Galilei derives various other laws. The most important of these is the law that the distances covered by a falling body, starting from rest, vary with the square of the time of the fall. For it has already been explained that the velocity of a falling body varies with the time, say $v = gt$, where g stands for some constant. Therefore $s = t^2 \times g/2$.

Galilei next tried to ascertain the actual acceleration of falling bodies. The direct measurement of it was impossible with the apparatus available at that time. So he resorted to the device of measuring the slower accelerations of bodies rolling down an inclined plane. It was known that the same object descends with varying accelerations according to the varying degrees of inclination of the plane of descent. The greatest acceleration is achieved when the body falls vertically, and this acceleration diminishes with the angular deviation from the vertical. It would seem, therefore, that the impetus, energy, or the tendency to fall is affected by the surface along which the body falls. He discovered that this impetus which a body receives during its fall varies with the proportion which the height of the inclined plane bears to its length. His experiments with inclined planes were made in this way. A board about twelve yards long was grooved. The groove, about half an inch wide, was made straight and smooth, and was covered with very smooth parchment. The board was then raised at one end to various heights. A smooth ball, made of polished brass, was then allowed to roll down the whole length of the groove, and the time taken to cover the whole distance was noted. The same ball was next allowed to run down a quarter of the whole distance, and the time taken was similarly noted. It was found that the time taken to cover a quarter of the distance was half of that taken for the whole distance, and the general result of numerous repetitions of the experiment was that the distances were proportional, for any given value of the inclination, to the squares of the times required to traverse them. The consistency of the results could only have been moderately good, since Galilei was unaware of the part played by the rotational inertia of the rolling ball in checking its acceleration down the slope.

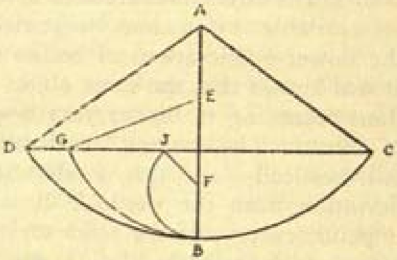
Galilei's experiments were also handicapped by the lack of a suitable instrument for measuring short intervals of time. And it is interesting to note how he surmounted this difficulty. Briefly, he used the time-honoured water-clock supplemented by the balance.

Water contained in a larger vessel was allowed to flow through a small opening at the bottom into a smaller vessel during the time of the fall under observation. The water collected in the smaller vessel was then carefully weighed, and the relative weights of water obtained during the different experiments yielded the relative times taken by the falling body for the various distances or angles of inclination. If the level of the water in the large vessel was maintained constant, then the resulting measure of time would be accurate.

Another important fact discovered by Galilei from his experiments with inclined planes was this, namely, that the *final* speed of a falling body varies only with the vertical height, not with the angular inclination of the plane. Thus (see Illustr. 11) a body will acquire



Illustr. 11.—Motion down an Inclined Plane



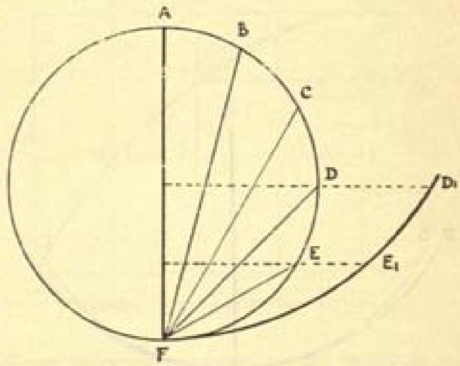
Illustr. 12.—Pendular Oscillations

exactly the same final speed whether it falls from C to A or to D or to B.

This law was further confirmed by experiments with the pendulum, where the effects of rotational inertia are negligible (see Illustr. 12). A pendulum, AB, was set swinging near a wall, so as to describe the arc CBD. By means of a nail in the wall the thread of the pendulum was then intercepted at E, and the arc described was changed to BG. When a nail was so placed as to intercept the thread at F, the arc described was changed to BJ. Thus in all cases (allowing for the resistance of the air and of the thread) the pendulum rose to the level CD, though the actual path was varied. Similarly, on the return swing the pendulum always rose to C approximately, whether it began its return journey from D or G. The one thing that mattered appeared to be the height from which the pendulum fell (C, D, or G), not the nature of the arc, etc.

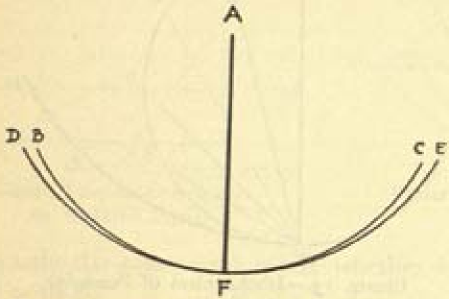
PENDULAR OSCILLATIONS

Another difficulty which Galilei encountered in his dynamic researches was that of eliminating the resistance which the air offered to the moving bodies with which he experimented. The air-pump had not yet been invented, so the influence of the air could not be eliminated. But Galilei felt sure that the difference in the velocity of a falling cork and of a falling piece of lead was due to the greater retardation suffered by the light cork than by the heavy lead through the same amount of air-resistance. By experimenting with inclined planes, down which bodies moved with slower velocities than when falling vertically, the influence of air resistance, it is true, was considerably reduced, but then the contact of the moving bodies with the surface of the plane introduced a new kind of resistance. Galilei, however, discovered a way of getting more or less rid of this difficulty by experimenting with a pair of pendulums, one of which consisted of a bob of cork attached to about four or five yards of fine thread, while the other consisted of a bob of lead attached to a similar length of fine thread. When the two pendulums were set in motion in the same way and at the same time they moved along arcs having the same radius. Even after swinging to and fro a great many times, no appreciable difference between their motions was observable. It appeared, therefore, that the resistance of the medium played no appreciable rôle in the case of pendular oscillation. This fact led Galilei to pay special attention to pendulum experiments.

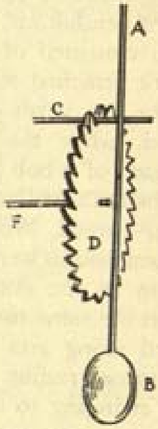


Illustr. 13.—Isochronism of Pendular Oscillations

One of the results of these experiments was the confirmation of his early observation in the Cathedral at Pisa, namely, that a swing of the same pendulum takes the same time, to all appearance, no matter how wide or how narrow the sweep of the swing may be. And this result bore a close resemblance to one of the results of Galilei's experiments with inclined planes. A ball rolling down inclined planes representing the chords of different arcs of a vertical circle, and each terminated by the lowest point of the circle, takes the same time to describe each plane. Thus (see Illustr. 13) it takes the same time for the ball to roll down from B, C, D, or E to F, or indeed to fall vertically from A to F. Similarly a pendulum suspended from A takes the same time to swing from D_1 to F as from E_1 to F. Pendulums having some of them bobs of lead, others bobs of cork, but the same length of fine thread, were set swinging at an angle of 50° to the vertical. At first they swept an arc extending 50° on either side of the vertical (AF in Illustr. 13) or 100° altogether. Gradually the arcs diminished to 40°, 30°, 20°, etc., until the pendulums stopped. But all the swings took equally long. Galilei appears to have confined his experiments to smaller angles. For larger angles the law just mentioned does not hold good. Huygens subsequently showed that the isochronism of pendular oscillations holds good only of the movements along the arcs of a cycloid, not of a circle. But in the case of small angles the difference is negligible



Illustr. 14.—Circular and Cycloidal Paths of Oscillation



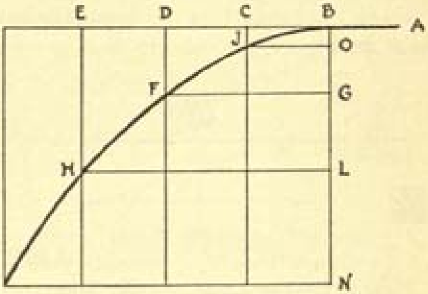
Illustr. 15.—Pendulum and Toothed Wheel

(see Illustr. 14, in which BFC is an arc of a cycloid, and DFE is that of a circle).

The discovery of the isochronism of the oscillations of the pendulum suggested to Galilei the possibility of constructing pendulum clocks. He actually gave his son, and his disciple Viviani, instructions to that end. His idea of a pendulum clock is represented in Illustr. 15. A strong bristle, C, was fixed to the pendulum, AB, in such a way that at each swing to and fro it should turn the toothed wheel, D, on its axis, F, a distance equal to the breadth of one of its teeth. The necessary calculations presented no difficulty. The problem was to invent some means to keep the pendulum going long enough to be of use. This was first achieved by Huygens.

PROJECTILES

Having succeeded in assimilating the oscillations of pendulums to the movements of falling bodies, Galilei next attempted to do the same for the motions of projectiles. His researches were guided by two principles, namely, the extended principle of inertia (which has already been referred to), and the principle that every force acting upon a body produces its independent effect—a principle which he was the first to recognize explicitly, although it had already been applied by ancient and mediaeval astronomers to explain the movements of the heavenly bodies. The application of these principles naturally leads to the use of the parallelogram of motions or velocities, which had already been anticipated in some measure in the Aristotelian *Mechanics*. Owing to the analogy between the laws of composition of displacements, and of forces, Newton has described Galilei as the discoverer of the principle of the parallelogram of forces.



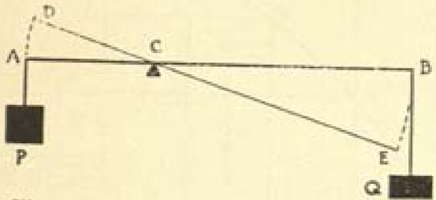
Illustr. 16.—Curvilinear Trajectory

We may now consider Galilei's application of the above principle to a particular instance. Suppose a body moving along a horizontal surface. According to the principle of inertia the body will tend to move in the same direction and with a uniform velocity so long as no other force acts upon it. If, however, the surface on which the body moves terminates abruptly, then gravity will come into play and introduce a new motion. The body will now move along a curvilinear path. Let AB (Illustr. 16) represent the horizontal path terminating at B. On reaching B the body ceases to be supported and owing to its weight a new motion is introduced, namely, the vertical fall along BN. But the uniform motion on the horizontal path has not been annihilated. The two motions combine, and the body moves neither along BCDE merely, nor along BOGLN merely, but along the curvilinear path BJFH, where DF = 4.CJ, because BD = 2.BC, and the distance through which a body falls varies as the square of the time. Similarly EH = 9.CJ. The curve is therefore a semi-parabola. Galilei then proceeds to show that when an object is thrown obliquely upwards its path will be just a parabolic path. He supposed that a rope fixed at its two ends and hanging freely under gravity between them would also tend towards a parabolic shape (which in fact forms a sufficiently close approximation to the catenary).

Galilei knew that the actual motions of falling bodies, pendulums, and projectiles were not precisely as he described them. There were various disturbing factors which he had to abstract from in order to get any results at all. The resistance of the air, the convergence of gravitational motion towards the centre of the Earth, and other circumstances had to be ignored by him, because mathematical analysis was not yet sufficiently developed for the simultaneous treatment of so many variables. A more refined study of ballistic problems was made by Johann Bernoulli and other mathematicians of the eighteenth century, but the complete theory of this branch of mechanics has yet to be established.

THE PRINCIPLE OF VIRTUAL VELOCITIES

Galilei not only laid the foundations of dynamics, as distinguished from statics, he also taught that peculiar combination of static and dynamic principles known as the Principle of Virtual Velocities or Displacements. By these are meant the resolutes of the velocities or displacements of a system of particles, in the directions of the respective forces acting upon the particles, during any hypothetical motion of the system under those forces, and compatible with the connections of the system. The principle in question seems to have been explicitly recognized first by Johann Bernoulli in a letter to Varignon, in 1717. It asserts that when the system is passing through a position of equilibrium, the sum of the products of the several forces into the resolved velocities of their respective points of application adds up to zero. Formulated at the beginning of the nineteenth century by Coriolis, as the Principle of Virtual Work, the proposition asserts that when the forces acting upon a system are in equilibrium, the total work which they do in any arbitrarily prescribed infinitesimal displacement of the system is zero. Thus take, for example, the case of a lever in equilibrium (see Illustr. 17). Two forces, P and Q, are acting at right angles on the arms of the lever, ACB, so that its equilibrium is disturbed, and the arms of the lever suffer respectively the displacements AD and BE. For small angles these lines, AD and BE, may be regarded as straight lines at right angles to ACB. We may say, then, that when equilibrium is still maintained the forces P and Q are related to each other inversely as their displacements, that is to say, P : Q :: BE : AD. In this way merely implicit static relations are made explicit. In the form of the

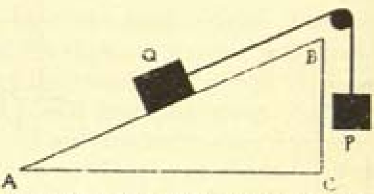


Illustr. 17.—The Lever and the Principle of Virtual Velocities

maxim "whatever is gained in power is lost in speed," the principle was implicitly recognized in early times in connection with the lever; and there are anticipations of it in the works of Aristotle.

Galilei also applied the principle to pulleys and inclined planes. Take, for instance, the case of equilibrium of weights, P and Q, on an inclined plane which is twice as long as it is high (see Illustr. 18). In this case $P = Q/2$. As Galilei pointed out, the equilibrium of bodies may be determined, according to this principle, by bringing them nearer to, or farther from, the centre of the Earth. For if the weight P sinks the distance h, then the weight Q will rise the distance $h/2$. Since $P = Q/2$, $Ph = Qh/2$.

By means of the notion of virtual displacements, Galilei also determined the relation between force and load in the case of pulleys. Assuming that the paths, s and w, of the force and of the load are proportionate to the number of the ropes over which the load is distributed, Galilei obtained the equation $Ps = Qw$. The work done by the force (Ps) equals the work done by the load (Qw).



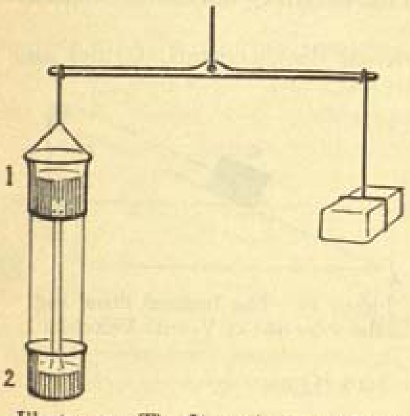
Illustr. 18.—The Inclined Plane and the Principle of Virtual Velocities

THE DYNAMICS OF IMPACT

Galilei's researches were fruitful so long as he confined himself to the action of forces upon a single body or mass. But he was less successful in dealing with reactions between bodies, and did not succeed in unravelling their mathematical laws.

Galilei saw and stated clearly that the force of impact depends on two factors, namely, the mass of the impinging body and its velocity at the time of impact. Hence his contention that the force of an impact is infinitely greater than that of mere pressure, because one of the two factors determining the energy of an impact, namely velocity, is equal to zero in the case of mere pressure. Hence also his application of the term "dead weight" for the mere pressure of a body at rest.

One of his experiments on impact led to important developments later on, and it may be of interest to describe it here. He suspended from one arm of a beam balance two buckets, one of which was arranged above the other (see Illustr. 19). The upper bucket contained water, the lower one was empty. A weight was suspended from the other arm of the balance so that the system was in equilibrium. The water from the upper bucket was then allowed to flow (through an opening in the bottom of this bucket) into the lower bucket. Galilei looked out for the effect of the impact of the water on reaching the lower bucket; but there appeared to be none. For a moment at first the arm carrying the buckets actually rose slightly, as if the system of buckets had become lighter. But as soon as the water reached the lower bucket equilibrium was re-established, and no effect seemed to be produced by the impact of the flowing water on the lower bucket. Galilei was perplexed and could not understand it. But the explanation is really as follows. Once a steady state of flow has been established (assuming the amount of water in the upper bucket so large that there is, for an appreciable time, sensible constancy of head) no extra force can be acting on the system consisting of buckets, fluid, and hangers because the part of the fluid which moves being in steady motion, the total vertical resolute of momentum of the system is constant. Hence the total vertical external force is zero. And since the total weight is clearly constant, the reaction on the hanger must be equal and opposite to this total weight, and therefore the same as it was initially.



Illustr. 19.—The Interrelation of Forces in a System of Bodies

Other Researching in Physics

HYDROSTATICS

Research into the mechanics of fluids had been neglected since the time of Archimedes until Galilei took them up again. First of all he carried out a series of experiments in verification of the hydrostatic laws formulated by Archimedes, which he found to be correct. Contrary to the current view that the floating of a solid depends on its shape, Galilei, like Archimedes before him, showed that it depends on its specific gravity, and that a body will float in a fluid if the specific gravity of the body is less than that of the fluid. The Aristotelian view which associated floating with form or shape had been based on the familiar phenomenon that very thin plates of metal float on the surface of water. Galilei showed that they really rest in a hollow on the surface of the water, and that as soon as they are properly immersed in the water they sink and do not rise again. The actual explanation of the floating of thin metal plates and needles was not forthcoming until the eighteenth century, after the discovery of the surface tension of liquids. The same discovery also explained another phenomenon which Galilei could not account for, namely, the cohesion of drops of water on leaves.

One of the experiments by means of which Galilei associated the floating of bodies in liquids with their specific gravities was the following. He immersed a ball of wax in pure water. The ball sank to the bottom. He then dissolved varying quantities of salt in the water, thereby increasing gradually its specific gravity. When the solution reached a certain degree of concentration the wax ball rose to the surface.

Galilei also gave vogue to the idea that fluids consist of isolated particles which are so mobile that the slightest pressure sets them in motion. In this way every pressure is transmitted through the whole mass of a fluid. This conception is still generally accepted, and is indeed at the base of all hydrostatic and hydrodynamic researches.

Galilei attempted to link up the mechanics of fluids with the general principles of the mechanics of solid bodies. For this purpose he first applied the Principle of Virtual Work (or Virtual Velocities) to hydrostatic relations. The full significance of this new method in hydrostatics was first appreciated by Pascal, who made full use of it.

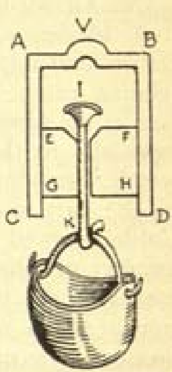
In his investigation of static relations Archimedes had introduced the concept of "static moment," and when explaining simple machines he had concentrated attention mainly on the weights involved and their distances from the fulcrum. Galilei, on the other hand, regarded static relations from a dynamic point of view, and considered the weights and the distances of their virtual fall (that is of their fall in the event of a displacement of the system) or virtual displacements, as the decisive factors for the determination of the conditions of equilibrium. This principle of virtual velocities or displacements amounts in the last resort to the statement that equilibrium is maintained when the work done by the force equals the work done by the load, the work done being calculated by multiplying the weight by the vertical displacement.

The simplest instance of the application to Hydrostatics of the Principle of Virtual Velocities with which Galilei deals is that of the immersion of a prismatic body in a similar prismatic vessel filled with some fluid. Galilei compares the displacement of the prism, or its equivalent velocity, with the displacement which the surface of the fluid undergoes in the opposite direction. The displacements, or the velocities, of the prism and of the surface are related in inverse proportion to the corresponding surfaces, namely, the base of the prism and the surface of the fluid. When the prism is drawn out again there is a corresponding fall in the level of the fluid. The product of the weight multiplied by the velocity of the immersed body must, if equilibrium is to be maintained, be equal to the product of the weight and velocity of the raised mass of fluid, and in this way the Principle of Virtual Velocities finds an application in this case. Galilei also extended the application of this principle to the relations of fluids in intercommunicating pipes, which seemed to him to be analogous to the preceding case. For the fall of the fluid in the narrower pipe and its rise in the wider pipe are analogous to the immersion of the prism and the consequent rise in the level of the water, and the rise and fall are likewise inversely proportional to the squares of the diameters of the pipes.

PNEUMATICS

Air, like fire, had been credited from ancient times with the attribute of "levity," that is, an absolute tendency to rise, while water and earth were credited with absolute "gravity" or the tendency to fall. Gaiilei proved the falsity of this view about air by experiment. Taking a glass bulb he forced air into it by means of a syringe. The bulb full of compressed air was then weighed carefully. When the scales were in exact equilibrium the bulb was opened so that some of the air that had been forced into it escaped. It was then observed that the bulb had lost appreciably in weight. This showed that air has "gravity" or weight. For if air had levity, then by forcing extra air into the bulb the bulb should have become lighter, and the escape of part of the air from it should have made the bulb heavier. Having shown that air has weight, Galilei proceeded to determine its specific gravity. Taking a bulb full of air he filled it three-quarters full of water without allowing the air to escape. The bulb with its contents was then weighed exactly. The air was then allowed to escape, and as much air as had previously filled three-quarters of the bulb got away. The bulb with its residual contents was weighed again, and so the weight of the escaped air was determined in relation to that of an equal volume of water. Galilei's estimate was that water is four hundred times as heavy as air. Actually it is 773 times as heavy, but, of course, allowance must be made for the inadequacy of the scales he used to respond correctly to the difference produced by the escape of a comparatively small volume of air.

In view of his determination of the weight of air, it seems remarkable that Galilei should have failed to clear up the mystery of the water-pump and kindred phenomena. The rise of water in a pump, as also such phenomena as suction, and the adhesion of smooth plates, used to be explained by the Scholastics as due to Nature's alleged abhorrence of a vacuum. This kind of quasi-psychical explanation could not have satisfied Galilei, yet he could not escape from it altogether. So he tried at least to determine experimentally the quantitative aspect of the phenomena by attempting to measure the amount of resistance there is to the formation of a vacuum. The experiment is described in the *Discourses* as follows: "I will tell you how to separate the force of the vacuum from the others, and afterwards how to measure it. For this purpose let us consider a continuous substance whose parts lack all resistance to separation except that derived from a vacuum, such as is the case with water. . . . Whenever a cylinder of water is subjected to a pull and offers a resistance to the separation of its parts this can be attributed to no other cause than the resistance of the vacuum. In order to try such an experiment I have invented a device which I can better explain by means of a sketch than by mere words. Let CABD (Illustr. 20) represent the cross section of a cylinder either of metal or, preferably, of glass, hollow inside and accurately turned. Into this is introduced a perfectly fitting cylinder of wood, represented in cross section by EGHF, and capable of up-and-down motion. Through the middle of this cylinder is bored a hole to receive an iron wire, carrying a hook at the end K, while the upper end of the wire, I, is provided with a conical head. The wooden cylinder is countersunk at the top so as to receive with a perfect fit the conical head of the wire, IK, when pulled down by the end K. Now insert the wooden cylinder EH in the hollow cylinder AD, so as not to touch the upper end of the latter but to leave free a space of two or three finger-breadths; this space is to be filled with water by holding the vessel with the mouth CD upwards, pushing down the stopper EH, and at the same time keeping the conical head of the wire, I, away from the hollow portion of the wooden cylinder. The air is thus allowed to escape alongside the iron wire (which does not make a close fit) as soon as one presses down the wooden stopper. The air having been allowed to escape and the iron wire having been drawn back so that it fits snugly against the conical depression in the wood, invert the vessel, bringing its mouth downwards, and hang on the hook K a vessel which can be filled with sand or any heavy material in quantity sufficient to



Illustr. 20.—The Force of a Vacuum

finally separate the upper surface of the stopper, EF, from the lower surface of the water to which it was attached only by the resistance of the vacuum. Now weigh the stopper and wire together with the attached vessel and its contents; we shall then have the force of the vacuum" (*Discourses concerning Two New Sciences*, p. 62, Vol. VIII, of the National Edition; p. 14 f. of the English translation by Crew and de Salvio).

SOUND

Our knowledge of Galilei's work on acoustics is derived mainly from Mersenne, who continued the work under the influence of Galilei and apparently under his guidance. It was his discoveries relating to the laws of the oscillations of the pendulum that led Galilei to direct his attention to the vibrations of strings, and especially to the phenomenon of so-called sympathetic vibration, which was popularly explained as due to some kind of sympathy on the part of other strings with the vibrating string. First of all, Galilei showed the dependence of the pitch of a note upon the number of vibrations occurring in a given time. He did this by means of the following experiment. He moved a sharp piece of iron across a plate of brass. Whenever a distinct note was thus produced, he noticed a number of fine lines on the plate at equal distances from one another. When, by means of a quicker movement, he produced a higher note, then the lines were closer together; when the note was lower the lines were farther apart. Evidently the closeness and number of the lines corresponded to the greater or smaller number of vibrations of the iron. Galilei next utilized the number of lines which appeared in a unit of time whenever a certain note was produced, in order to study the phenomena of sound quantitatively. He produced, for instance, two notes by successively stroking the brass plate more rapidly and less rapidly, and when he obtained two consonant notes which in music are said to constitute the "fifth," he counted the lines of the brass plate and measured their mutual distances, and discovered that there were forty-five lines (and therefore vibrations) for the higher note to thirty lines (and therefore vibrations) for the lower note. Experiments on the relation of notes to the strings producing them were, of course, very old. Pythagoras (sixth century B.C.) had already instituted such experiments. But the relation hitherto studied had been solely that between the pitch of a note and the *length* of the string. Galilei first drew attention to the rate of *vibrations* (or frequency) as the really important factor in determining the pitch of a note produced by any sounding body. By simple experiments like the above Galilei discovered that the rates of vibration for the key-note, the fourth, the fifth, and the octave above it are in the proportion of 1 : 4/3 : 3/2 : 2, that is, as 6 : 8 : 9 : 12 (*Discourses concerning Two New Sciences*, First Day, near the end). These experiments were important, but the account of them is unfortunately obscure in some respects.

Galilei also considered the physiological problem of the consonance and dissonance of notes. He suggested that notes are felt to be consonant when the vibrations which produce them stimulate the drum of the ear with a certain rhythmic regularity. On the other hand, notes are felt to be dissonant when they are produced by vibrations that are not rhythmic, and therefore act in an irregular and disturbing manner upon the drum of the ear.

LIGHT AND MAGNETISM

Apart from his share in the construction of the telescope, Galilei did not devote much attention to the study of light. It is noteworthy that he assumed that light travels with a finite velocity, and that he actually carried out some experiments with light-signals in order to determine this. But he was not successful.

Under the influence of Gilbert's work on magnetism Galilei attempted to apply magnetic concepts to the explanation of astronomical phenomena. These attempts occupy a certain amount of space in his *Dialogue*. Thus, for example, he attributed to magnetic influences such phenomena as the Earth's rotation round its axis, the constancy in the direction of the Earth's axis, and the fact that the Moon always has the same side turned towards the Earth. He also tried various experiments on magnets showing how the power of a magnet may be greatly intensified by means of a polished armature. In this way he claims to have increased the power of a magnet eightyfold, and to have made a magnet carry a load twenty-six times its own weight.

THE THERMOSCOPE, ETC.

Galilei's contributions to the construction and use of the thermoscope (or thermometer), as also of the microscope, and the telescope, will be described in the chapter on scientific instruments; his researches on the strength of beams will be dealt with in Chapter XXI.

(See J. J. Fahie, *Galileo*, 1903, and *Memorials of Galileo*, 1929; P. Duhem, *Les Origines de la Statique*, 2 vols., Paris, 1905–6, *Études sur Léonard de Vinci*, 3 vols., Paris, 1906–13, and papers in *Annales de Philosophie Chrétienne*, 1908; E. Wohlwill, *Galilei und sein Kampf für die Copernicanische Lehre*, 2 vols., Hamburg, 1909–26; W. W. Bryant, *Galileo*, 1925; Lane Cooper, *Aristotle, Galileo, and the Tower of Pisa*, New York and London, 1935; F. Sherwood Taylor, *Galileo and the Freedom of Thought*, London, 1938.)

CHAPTER IV

SCIENTIFIC ACADEMIES OF THE SEVENTEENTH CENTURY

THE Church of Rome might arrest the body of Galilei, but his spirit went marching on. Not only his disciples, Viviani and Torricelli, but many others were infected with his enthusiasm for experimental science; and within a comparatively short time influential institutions were organized for the express purpose of advancing experimental science by the co-operative work of their members, many of whom were also stimulated thereby to pursue important scientific researches of their own. Of the new institutions the most important were the Accademia del Cimento of Florence, the Royal Society of London, and the Académie des Sciences of Paris.

The formation of the scientific academies at that time was no mere accident; it was a significant expression of the spirit of the age. It was the spirit that prompted Francis Bacon to put on the title-page of his *Novum Organum* the picture of a ship in full sail boldly venturing to pass beyond the Pillars of Hercules, the limits of the old world (see *Frontispiece*).

It was the golden age of pioneers. The spirit of man had been long fettered by tradition and authority. The craving for knowledge had had to seek satisfaction within the covers of a few books sanctioned by authority. The need for intellectual activity could only find vent in comparing and harmonizing what other people had already said. Everything else had been more or less out of bounds. Gradually, however, the force of revolt was growing; and now, in spite of powerful resistance on the part of established authority, some of the bolder spirits broke the shackles of Scholasticism and, venturing out on uncharted seas, sought to see the world with their own eyes, and to interpret it with their own intelligence. The Universities might have been expected to lead, or at least to share, in this movement for intellectual emancipation. But they did nothing of the kind. For they were controlled by the Church. Philosophy was only tolerated as the handmaid of Theology, and the University as the Cinderella of the Church. It was, indeed, highly significant of the times, that the vast majority of the pioneers of modern thought were either entirely detached from the Universities, or were but loosely associated with them. New organizations, and indeed essentially secular organizations, were necessary to foster the new spirit, and to enable it to express itself. Francis Bacon dreamed of such institutions in his *New Atlantis*. His successors, partly under the stimulus of his visions, saw his dreams come true. The scientific academies came into being in response to the new needs of the new age. It was in these societies that modern science found the opportunities and the encouragement which were denied to it at the Universities, not only in the seventeenth century, but for a long time afterwards.

The Accademia del Cimento

The Academy of Experiments was founded in Florence in 1657. Its moving spirits were two of Galilei's most distinguished disciples, Viviani and Torricelli. The necessary financial support came from the two Medicis, the Grand Duke Ferdinand II of Tuscany and his brother Leopold, who had both of them studied under Galilei. More than a decade before the formal institution of the Academy the two Medici brothers had started a laboratory well equipped with such scientific apparatus as was then obtainable. And during the years 1651 to 1657 various men of science met more or less regularly in this laboratory for experimentation and discussion. The Accademia del Cimento was merely a more formal reorganization of this informal society. The two Medicis continued to be its financial patrons. They were really interested and active patrons. This was especially the case with Prince Leopold. It is remarkable that the year in which he was created a Cardinal (1667) was also the year in which the Academy was discontinued. Small wonder that some people suspect a sinister bargain, and see in the closing of the Academy the price which the Pope exacted from the would-be Cardinal.

The roll of members of the Florentine Academy of Experiments included, besides Viviani and Torricelli, the anatomist Borelli, who applied the principles of mechanics to physiology, the Danish anatomist and mineralogist Steno, the embryologist Redi, and the astronomer Domenico Cassini, who was subsequently virtual director of the newly erected observatory in Paris. These men and others jointly carried out numerous experiments in physics during the years 1657 to 1667. When, in 1667, the Society was disbanded, one member of the Academy, Antonio Oliva, fell into the hands of the Inquisition while at Rome, and to escape torture he committed suicide by jumping from a high window of the prison. Fortunately a record of the most important researches has been transmitted to posterity.

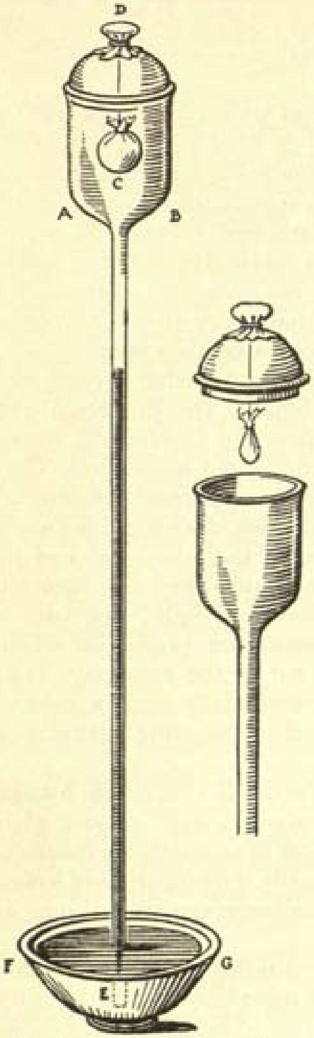
The members of the Accademia del Cimento published an account of their experiments and discoveries jointly in the *Saggi di naturali esperienze fatte nell' Accademia del Cimento*, Florence, 1666 (English translation by Richard Waller: *Essayes of Natural Experiments made in the Academie del Cimento*, London, 1684). The most important sections of this work are those which deal with the measurement of temperature and of atmospheric pressure.

The most detailed section of the *Saggi* is that devoted to experiments on the natural pressure of the air. The Academicians repeated the barometric investigations of Torricelli (see Chapter V), and performed a considerable number of interesting pneumatic experiments. In one of these a small bladder containing only a little air was suspended from the cover of a bell-shaped chamber at the top of a barometer-tube (see Illustr. 21). The tube was filled with mercury, and the cover was put on, with the bladder inside the chamber. The mercury was allowed to subside, and a Torricellian vacuum was thus formed around the bladder, which immediately swelled to its full size under the pressure of the contained air.

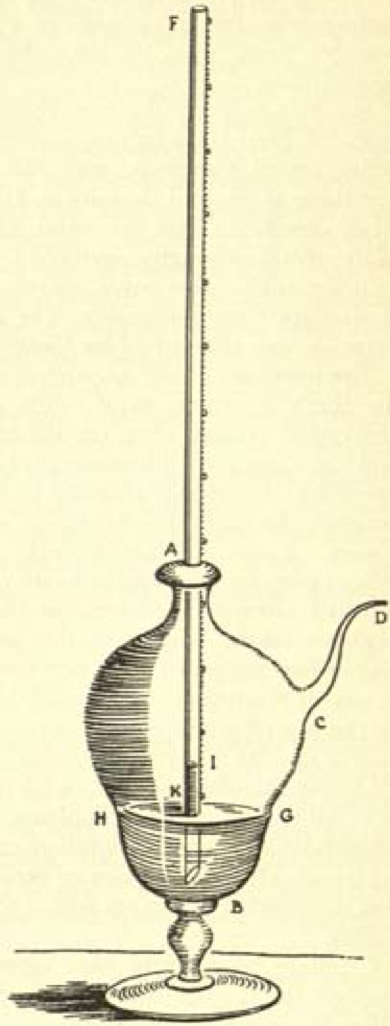
With similar apparatus it was shown that, in a Torricellian vacuum, the familiar rise of liquids in fine tubes still occurred, that drops of liquid retained their globular form, and that a needle was attracted by a magnet, these phenomena being therefore independent of the pressure of the air. But attempts to ascertain whether excited amber would attract straws, and whether a bell was audible, in the vacuum, were inconclusive. The Academicians repeated several of Boyle's experiments, including the boiling of warm water; and they observed the behaviour of animals deprived of air. They also constructed an air-pump, but this proved rather a failure.

Several instruments were devised for demonstrating how the atmospheric pressure falls off with increasing height above the level of the ground. One of these, shown in Illustr. 22, consisted of a graduated glass tube, open at each end, and inserted in a glass vessel having an opening at the side but otherwise air-tight. Enough mercury was poured into the vessel to cover the lower end of the tube, and the opening in the side was then sealed up. If the instrument was now taken to the top of some tower or other high place, the mercury was found to rise in the tube, since the pressure in the sealed vessel now exceeded the pressure on the mercury surface in the tube.

Numerous experiments were performed on the freezing of water and of other liquids, a freezing-mixture of ice and salt, such as Descartes had described in his *Météores*, being employed in some of these. The ratio in which water expands upon freezing was correctly estimated as about 9 : 8; and in the course of these experiments the immense force of this expansion was demonstrated. Metal vessels were completely filled with water, securely closed down, and then surrounded with ice. They were found to be invariably cracked open by the pressure of the freezing water within. The Academicians used a pendulum to compare the different periods of



Illustr. 21.—Experiment in a Vacuum



Illustr. 22.—Barometer of the Accademia del Cimento

time required by a freezing-mixture to freeze samples of different liquids. In order to increase the accuracy of their measurement of time, they kept the bob of the pendulum always in the same plane by means of a bifilar suspension (see Illustr. 71). They also tried the significant experiment of placing a mass of ice at some distance in front of a concave mirror and noting the behaviour of a sensitive thermometer placed at its focus. The thermometer showed a fall of temperature, but the possibility of direct cooling by the ice could not be ruled out, so that the experiment was regarded as inconclusive.

In order to study the compressibility of water the Academicians repeated Francis Bacon's experiment. They filled a silver vessel with water, sealed it securely, and then hammered it out of shape so as to reduce its internal capacity and thus compress the enclosed liquid. They concluded that the water had transpired through the pores of the metal. Though convinced by this result that water is highly incompressible, the investigators did not venture to pronounce it absolutely incompressible. The fact that water is actually compressible was established by Canton about a century later.

The members of the Accademia del Cimento further investigated the thermal expansion of solids and liquids, the liberation and absorption of heat when certain substances are dissolved in water, and the elementary phenomena of electricity and magnetism. They calculated the velocity of sound by noting the apparent time-interval between the flash and the report of a cannon discharged a known distance away; but they wrongly supposed that the wind had no effect upon the apparent velocity of the sound. They also repeated Galilei's attempt to determine the velocity of light, but with a negative result. Several of the experiments on projectiles which Galilei had suggested were first carried out by the Academy. Thus it was demonstrated that a ball fired horizontally from a cannon on the top of a tower reached the ground at the same instant as a similar ball let fall simultaneously.

Of the members of the Academy Torricelli concerned himself especially with optical problems. He showed how minute glass spheres could be used as simple microscopes of considerable magnifying power. He also investigated geometrically the properties of lenses, and constructed telescopes which were an improvement on those of Galilei.

Capillary phenomena were especially studied by Borelli, whose work on the subject, however, appeared separately from that of his fellow members of the Academy. The rise of liquids in fine tubes had already been described by Leonardo da Vinci (1490), but had been ignored by Pascal. Borelli discovered how the rise depends on the nature of the tube. He noticed that the rise was more pronounced when the interior of the tube was wet than when it was dry; and he found that the height to which the liquid rose was inversely proportional to the diameter of the tube ($h : h' = d' : d$). He discovered also that objects floating on a liquid (such as boards floating on water) attracted each other when within a certain distance, provided both had previously been wetted with the liquid. He found, however, that repulsion occurred if only one of the objects had been wetted. A satisfactory explanation of these capillary phenomena was first provided by Clairaut about the middle of the eighteenth century.

The researches of the Accademia del Cimento, as may have been noted, were severely scientific, in the sense that they were conducted on careful experimental lines, and that the conclusions drawn were strictly limited to the necessities of the observed evidence, instead of attempting speculative flights. This kind of self-restraint may have been due mainly to the mutual criticism which naturally resulted from the co-operation of the members in their joint researches. For, as was subsequently remarked by Laplace, "whereas the individual man of science may be easily tempted to dogmatize, a scientific society would very soon come to grief through the clash of dogmatic views. Moreover, the desire to convince others leads to a mutual agreement not to assume anything beyond the results of observation and of calculation" (*Précis de l'histoire de l'astronomie*, 1821, p. 99). It is not unlikely that the speculative restraint exercised by many members of the Royal Society of London, and especially Newton's dislike of speculative hypotheses in science, was due to similar causes, though the individualism of the members of the Royal Society was rather more pronounced than that of the members of its Italian prototype.

The Royal Society

The Royal Society appears to have developed from an informal association of adherents of Francis Bacon's experimental philosophy. These men began to meet weekly in London about 1645 to discuss natural problems. Among their number were John Wallis (1616–1703), the eminent mathematician and divine; John Wilkins (1614–72), afterwards Bishop of Chester, whose interests ran to mechanical inventions and astronomical speculations; a group of physicians including Jonathan Goddard, George Ent, and Christopher Merret; Samuel Foster, the Professor of Astronomy at Gresham College; and Theodore Haak, a German from whom the idea of these weekly meetings seems to have originated. The association represented a wide range of interests and opinions, but its members agreed to exclude theology and politics from their discussions.

With the removal of Wallis, Wilkins, and Goddard to Oxford, about 1649, the association was divided into two parts, and a small society grew up at Oxford which included Seth Ward (1617–89), the Savilian Professor of Astronomy there, who attempted in his writings to improve on the planetary theories of the time; and William Petty (1623–87) who was one of the earliest writers to make a systematic study of the statistics of population and mortality. For a time the Oxford society met at the lodgings of Robert Boyle (1627–91), whose name has already been mentioned and whom we shall meet again in the history of chemistry. This society, however, soon lost many of its most active members by removal, and it finally came to an end in 1690. Meanwhile the London branch was flourishing, and numbered among its members Christopher Wren (1632–1723), who was versed in many branches of science though principally remembered now as an architect; Laurence Rooke (1622–62), who had been chemical assistant to Boyle before becoming Professor of Astronomy at Gresham College; Sir Robert Moray, an old partisan of Charles I and the society's president until its incorporation; Lord Brouncker (1620–84), a distinguished mathematician who was elected to the presidency after the incorporation; and John Evelyn, the diarist. These and many others were in the habit of holding their meetings at Gresham College after the weekly lectures of Wren and Rooke. In 1658 these meetings were interrupted for a time in consequence of the political upheavals of the period, and the College was turned into a barracks.

Shortly after the Restoration of Charles II, however, the men who were soon to form the nucleus of the Royal Society resumed their weekly meetings at Gresham College. At the same time they worked out a plan for the establishment of a definite society devoted to the pursuit of experimental knowledge. This scheme was ultimately realized with the foundation of the Royal Society by charter on July 15, 1662. A second charter, extending the privileges of the Society, was granted in the following year.

At the meetings of the Royal Society it was the custom from the first to allot special inquiries or pieces of research to individual members or groups of members, who were required to report their results to the Society in due course. Thus we find Lord Brouncker charged with the prosecution of experiments on the recoil of guns; Boyle was invited to demonstrate the working of his air-pump; and the preparation of a report on the anatomy of trees was entrusted to Evelyn. At the same time members were urged to come forward with any new experiments which they thought would advance the purposes of the Society. Among the earliest such experiments to be tried were the production of colours by chemical combinations, the calcination of antimony to see whether its weight would increase in the process, the measurement of the density of air, the quantitative comparison of the breaking loads of wires of various metals, and several ineffectual attempts to compress water. The early meetings were thus taken up with the reports and discourses of members, the demonstration of experiments, the exhibition of rarities of every description, and with the lively discussions and speculations to which all these gave rise. As time went on committees were set up to direct the various branches of the Society's activities. One of these, the Committee for Histories of Trades, concerned itself with the principles of industrial technology, and reports were made to the Society from time to time on the processes involved in such industries as shipping, mining, brewing, refining, the manufacture of cloth, and so forth. There was a committee for collecting reports of natural phenomena, and another for improving mechanical inventions. There were further committees for astronomy, anatomy, chemistry, etc. The privileges of the Society, however, did not include any endowment, and some years elapsed before the members could enjoy the use of proper laboratory accommodation.

In 1662 Robert Hooke was appointed Curator to the Society with the duty of preparing for each meeting three or four experiments of his own, and any others that the Society might require from time to time. Hooke was the most able experimenter and the most ingenious and fertile inventor that the Society had at that time, and some of the investigations which he carried out in connection with the Society are worth noting. In order to ascertain whether the force of gravity falls off appreciably with increase of distance from the Earth's centre, Hooke took an accurate balance to the top of Westminster Abbey and there weighed a piece of iron and a long piece of packthread. He then suspended the iron from one of the scale-pans by means of the packthread and weighed the iron and thread again. The iron being now much nearer the ground should have appeared to weigh more, were the falling off in gravity appreciable; but Hooke could detect no sensible difference in weight under the two different conditions. He later repeated this experiment in the steeple of old St. Paul's, where he also had the opportunity of studying the behaviour of a 200-ft. pendulum. One of Hooke's earliest communications to the Society dealt with a method of verifying the physical relation known as "Boyle's Law," with the original establishment of which he had himself been closely associated. Hooke also carried out a series of measurements of refractive indices of transparent liquids with an instrument of his own design. With his microscope the members of the Society eagerly examined the cellular structure of cork, the "eels" in vinegar, the anatomy of insects, and the various other minute objects which were subsequently described and depicted in his *Micrographia* (1665).

Besides their researches in the physico-chemical sciences, the early members of the Royal Society, especially the medical men, gave much attention to biological problems, and performed many dissections and experiments on animals. One of the privileges of the Society was the right to claim the bodies of executed persons for dissection, and in 1664 a committee was formed to undertake dissections upon every execution-day. Samuel Pepys, after his admission to the Society (of which he ultimately became the President), showed especial interest in this department of its work. Reports were received from physicians in all parts of the country describing clinical cases of exceptional interest. The medical members also practised vivisection extensively, though generally without any useful or conclusive results. Injections of liquids (such as mercury, tobacco-oil, etc.) were made into the veins of animals, or organs were removed and nerves severed, the results being noted. Many experiments were made on the transfusion of blood between similar or dissimilar animals, including dogs, sheep, foxes, and pigeons—a piece of research to which the Royal Society was stimulated upon hearing of the success of Lower's transfusions at Oxford. Later the experiment of introducing sheep's blood into the veins of a man was tried without untoward consequences.

The rôle of air both in breathing and in combustion was studied, chiefly by Boyle and Hooke, with the aid of the air-pump. Small animals or lighted lamps, or sometimes both together, were placed in the receiver, and their behaviour when the air was evacuated was noted. Hooke showed that the heart of a dissected dog could be kept beating for over an hour by injecting air into its lungs through an opening in the windpipe. Several members made personal trial of the number of respirations for which the air enclosed in a bladder of given size would suffice. The question of the possibility of spontaneous generation came up for discussion at the meetings of the Society when dead bodies of animals, though sealed so as to exclude air, were found to breed maggots.

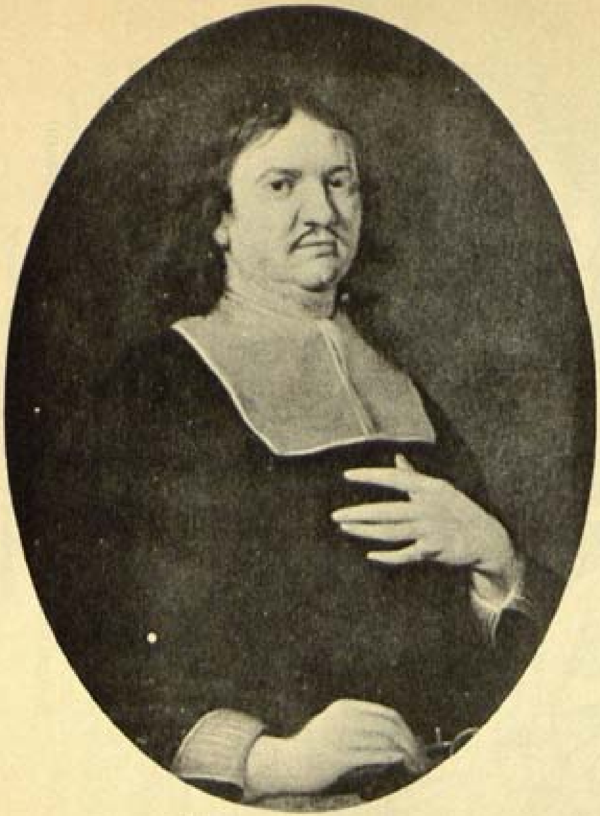
In order to accommodate the growing number of natural specimens (zoological, botanical, geological, etc.) acquired by the Society, a repository was opened in 1663, Hooke being made Keeper. Here also were preserved many instruments and mechanical devices manufactured or invented by the members, as well as many curiosities of no scientific value. Many of these objects had been brought by travellers from abroad. The Royal Society, indeed, set on foot many inquiries as to the conditions, natural productions, etc., of foreign lands, and welcomed reports from explorers, sea-captains, and others, together with specimens of any valuable minerals, fruits, etc., which they might come across. An elaborate

Illustr. 23

Brouncker Charles II Bacon

(*Reproduced by courtesy of the Royal Society*)

Illustr. 24

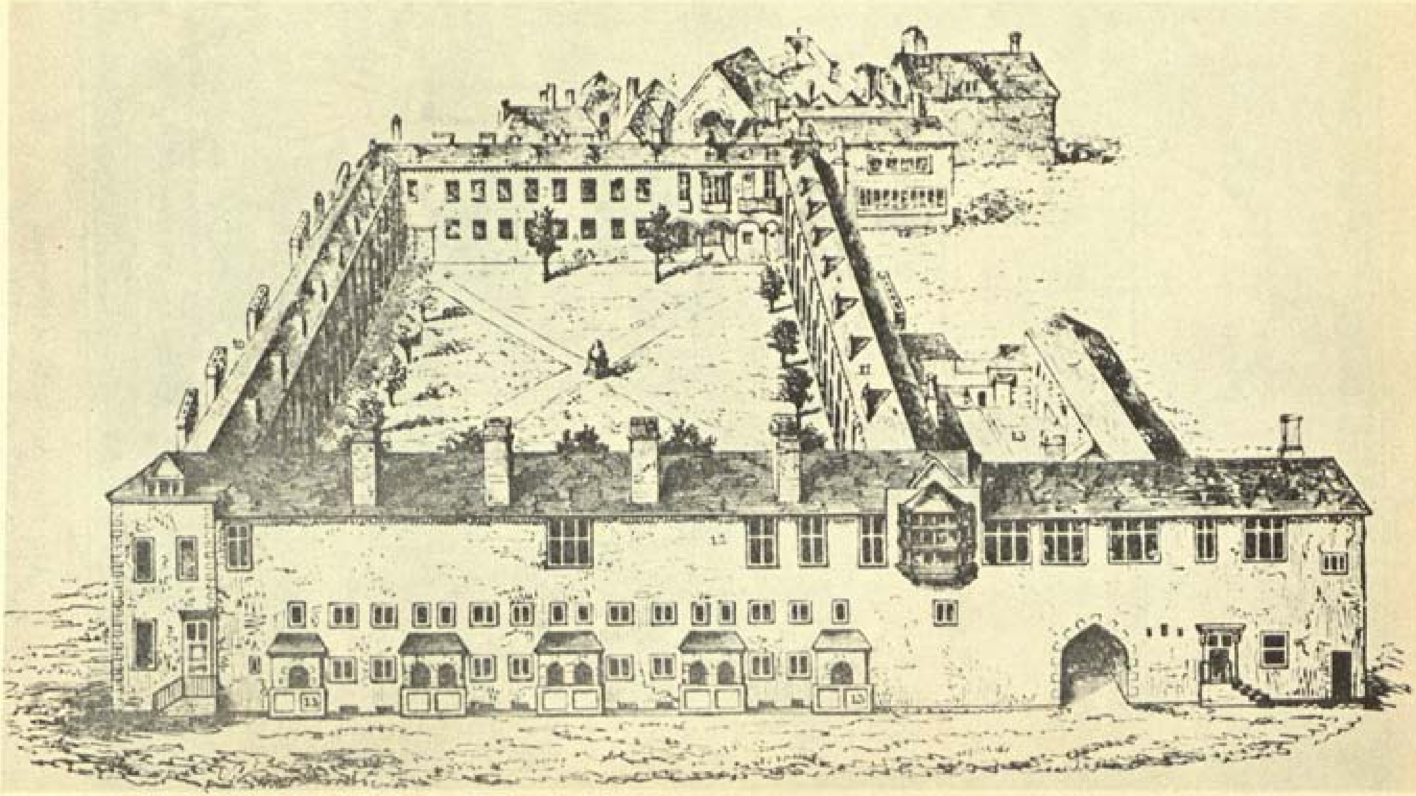


Henry Oldenburg

Illustr. 25

John Wilkins

Illustr. 26



Gresham College

The Paris Academy of Sciences
(A Visit by Louis XIV)

programme of physical experiments with the barometer, thermometer, hygrometer, pendulum, etc., was drawn up as early as 1660, and sent out to Teneriffe to be tried at various altitudes ranging from sea-level to the summit of the Peak.

The Royal Society also frequently undertook to investigate popular beliefs of the time, from which the members themselves were not entirely free. Sir Christopher Wren related an alleged instance of "sympathy" between a wound and a bandage lately removed from it; attempts were made to produce vipers from the powdered lungs and livers of these reptiles; and several magnetic cures were reported. The miraculous properties of the salamander were discussed, and an experiment was arranged to ascertain whether a spider could escape when surrounded by powder made from the horn of a "unicorn," which had apparently been provided by the Duke of Buckingham.

The publication of the *Philosophical Transactions of the Royal Society* was begun in March 1665 by Henry Oldenburg, the Secretary of the Society, on his own account. The contents of the *Philosophical Transactions* consisted mainly of papers and abstracts contributed by the members, observations of remarkable phenomena reported from various parts, learned correspondence and controversy with foreign investigators, and notices of newly published scientific books.

The universal curiosity manifested by the early members of the Royal Society towards unfamiliar natural phenomena of every description proved a source of weakness. They cast their net too widely, and forfeited the advantages of united and prolonged concentration upon a limited set of problems. Hence the true significance of the young Society in the development of science must be measured, not so much by its corporate contribution to the stock of scientific knowledge, as by its vitalizing influence upon the master-spirits whom it bound together and whom we shall meet again, each at work in his own special field of inquiry.

The Académie des Sciences

The French Académie des Sciences had its origin in certain informal gatherings of a group of philosophers and mathematicians in Paris towards the middle of the seventeenth century. This group, which included such men as Descartes, Pascal, Gassendi, and Fermat, used to meet at the cell of Mersenne to discuss current scientific problems and to suggest fresh mathematical and experimental researches. Later, more regular meetings took place at the houses of Montmort, the Master of Requests, and of Thevenot, a widely read and much travelled man. Foreign scholars, including Hobbes, Huygens, and Steno, were much attracted, and finally, at the suggestion of Charles Perrault, Colbert proposed to Louis XIV the establishment of a regular Academy. This body was originally intended to concern itself with history and literature, as well as with scientific matters, but this scheme was not realized, and when the new Academy held its first meeting on December 22, 1666, it was as a gathering devoted entirely to scientific studies. Its members received pensions from the King as well as financial assistance with their researches. These researches fell into the two groups of *mathematics* (including mechanics and astronomy), and *physics*, which at that time was held also to include chemistry, botany, anatomy, and physiology. The Academicians met in a room in the Royal Library with a laboratory adjoining, and carried out their researches in common. They met twice a week, devoting their sessions to physics and mathematics alternately.

In pure physics the Academy repeated a number of the experiments of the Accademia del Cimento and of the Royal Society. They studied the expansive force of freezing water as shown by its capacity to crack the metal vessels in which it was confined. They also performed several experiments with the air-pump. In one of these a vessel of water containing a fish was placed in the receiver. When the air was evacuated no change was observed, but when air was re-admitted the fish sank to the bottom of the vessel and remained there owing to its sound (i.e. swimming bladder) having been evacuated during the previous exhaustion of the container of the air-pump. In order to ascertain whether heat could be transmitted through a vacuum, some butter was placed in the receiver and, after exhaustion, a piece of hot iron was held close at hand. It was found that the butter melted when the iron was brought sufficiently near. The growth of a plant kept for some days in an exhausted container was found to be arrested. Experiments were also conducted to ascertain whether the boiling of water made any difference to the facility with which it subsequently froze. No such difference could be discerned, but it was noticed that boiled water, owing to the absence of dissolved air, formed harder and more transparent ice. From such ice Mariotte, who was one of the early members of the Academy, was able to make burning-glasses. In these physical researches Huygens took a leading part, and it was while in Paris as a member of the Academy that he wrote his *Traité de la Lumière*, subsequently published in 1690.

Among the earliest chemical studies of the Academy was an investigation of the increase in weight shown by certain metals on calcination. S. C. Duclos exposed a pound of powdered antimony to the action of a burning mirror for an hour, and found that it increased by one-tenth of its original weight. He supposed the increase to be due to the addition of sulphureous particles from the air. It was suggested, however, that the antimony might have gained in weight at the expense of the containing vessel. Mineral waters from several localities were analysed and the several results compared.

In their biological researches it was the aim of the Academicians to study both the structure and the functions of the organs of animals and plants, and to use both their eyes and their reason, but especially their eyes. Their *Natural History of Animals* (1666 onwards; English translation by Alexander Pitfield, London, 1702) was based on the examination and dissection of a considerable number of animals, including a panther and an elephant, whose bodies they obtained from the Versailles menagerie. These dissections, however, were not carried out in accordance with any prearranged plan, and they were intended to illustrate the peculiarities of the animals studied rather than their points of similarity. They served, however, to remove certain vulgar errors in natural history. Following the example of the Royal Society the Academicians tried experiments on the transfusion of blood with dogs and other animals, but with little success. A prolonged study was made of coagulation in blood, milk, and other such fluids, with special reference to the conditions under which it occurs. Human bodies were also occasionally dissected at the sessions of the Academy. The structure of the human eye and ear was carefully described, and in this department Mariotte made his important discovery of the blind spot in the eye.

The methods adopted in the Academy for studying the constitution of plants were too crude and misinformed to give results of much value. A frequent procedure was to make a decoction or extract of the given plant, to mix this with certain solutions of iron or lead salts, and, from the resulting coloration or precipitate, if any, to pronounce which plants contained the more "terrestrial" sulphureous salts. Finding that "vulnerary" herbs were able to precipitate lead from solution in vinegar, they supposed that the extract absorbed the "points" which give vinegar its peculiar effect upon the tongue (a Cartesian notion), and that such herbs act in a similar manner upon the acids which ulcerate wounds; hence their healing virtue. Another mode of studying plants was to squeeze out their juices, allow these to evaporate, and then examine the essential salts which crystallized out. Much time, however, was wasted upon the fractional distillation of plants in retorts. The issuing vapours were condensed and tested for acid reactions and "sulphureous" properties with corrosive sublimate and other reagents, and the *caput mortuum* in the retort was thrown away. This process was applied to four hundred and fifty different plants, and on one occasion forty toads were subjected to fractional distillation. It was not until 1679 that Mariotte pointed out the futility of the procedure, which naturally destroys the very substances which it is desired to examine.

The pure mathematical researches of the Academy ran chiefly to discussions on problems arising out of Descartes' work in this field, and out of the use of vanishingly small quantities in geometry. A number of separate treatises by members of the Academy were produced; a joint treatise on mechanics was also compiled, but was of little scientific value. In hydrostatics the Academicians investigated the relation between the velocity of efflux of a fluid from a vessel and the head of pressure behind it, on lines already laid down by Torricelli.

In the sphere of applied mechanics the Academy appointed several of its members to study the tools and machines in common industrial use, with a view to elucidating their working principles, and improving or simplifying their construction. In addition, many ingenious mechanical devices were designed by the Academicians and were published in an illustrated catalogue. Especial attention was given to frictionless pulley combinations, pumps, and automatic saws. Foremost among the inventors was Perrault. He designed a movable mirror for directing the rays of a star or other heavenly body into a large fixed telescope. This device, which partly anticipated the modern siderostat, enabled an observer to follow the course of a star without moving the telescope. Perrault also suggested a form of clock in which the pendulum was to be kept in motion by water which flows into vessels on either side of the pendulum and depresses them alternately.

The work of the astronomical members of the Academy, especially of Picard and Auzout, represented a distinct advance, for it was they who introduced the practice of systematically using telescopes in conjunction with graduated circles for the precise measurement of angles. The line of collimation of the telescope was accurately defined by means of intersecting wires in the focal plane of the object glass. Systematic use was also made of micrometers for measuring the small angular separations of objects simultaneously visible in the field of view of the telescope. Picard conceived the idea of using the times of meridian transit of stars to determine their differences in right ascension, using Huygens' lately developed pendulum clock for this purpose. A special study was made at Paris of the somewhat neglected factor of astronomical refraction. The earliest astronomical observations of the Academicians were made in a garden at the back of their accustomed meeting place. But this was too much hemmed in by houses, and an appeal was made to the King to found a proper observatory. This was built, to Claude Perrault's design, in the Faubourg S. Jacques, and was practically finished by 1672. From 1669 the astronomical work of the Academy was carried on under G. D. Cassini, an Italian astronomer invited to Paris by Colbert.

Several foreign expeditions were organized by the Academy. Of these, two are especially noteworthy. In 1671 Picard went to Denmark in order to determine accurately the position of Uraniborg, Tycho Brahe's former observatory, already in ruins. He brought back with him to Paris Olaus Römer, who became a member of the Academy, and who, while in France, discovered that light was propagated gradually, not instantaneously. Another expedition, under Jean Richer, was sent to Cayenne in 1672 to observe an opposition of Mars. From a comparison of Richer's observations with those made simultaneously by Cassini in Paris, values of the parallax of the planet and of the Sun were deduced far exceeding in accuracy any previously obtained. Richer also made the important discovery that a pendulum, in order to beat seconds, must be made shorter at Cayenne than at Paris—a discovery that marked the beginning of speculations as to the exact shape of the Earth.

Upon the death of Colbert, in 1683, Louvois was appointed Protector of the Académie Royale des Sciences. He had little sympathy with purely theoretical researches, and the activities of the Academy waned until its complete reorganization and enlargement by Bignon in 1699.

The Berlin Academy

During the seventeenth century a number of scientific societies were founded in Germany. Among the earliest institutions of this kind was the *Societas Ereunetica*, founded at Rostock in 1622 by Joachim Jungius, the biologist and educational reformer, with the object of fostering and propagating natural science, and of placing it upon an experimental basis. This society, however, seems to have lasted only for about two years. Thirty years later there was founded the *Collegium Naturae Curiosorum*. This was essentially a brotherhood of physicians, and its chief activity was to be the publication of a journal describing the specialized medical researches of its members. There was also the *Collegium Curiosum sive Experimentale*, established in 1672, and recruited from among the students of its founder, Christopher Sturm, of Altdorf, whose fine collection of physical instruments served for the specifically experimental work of his academy. The only German scientific society, however, to attain the importance of the Royal Society, or of the *Académie des Sciences*, was the Berlin Academy. As an embodiment of the ideals of its founder, Leibniz, it must be regarded as a product of the seventeenth century, though, as it was not founded until 1700, the story of its later fortunes does not concern us here.

The Berlin Academy was the outcome of many years of careful planning and persistent advocacy on the part of Leibniz, though representing only a part of his ambitious schemes. He had early fallen foul of the current methods of education, with their emphasis upon abstractions and merely verbal scholarship. He thought the teaching of the young should rather be centred upon objective realities, and he stressed the importance of proper instruction in mathematics, physics, biology, geography, and history. He was anxious that German should take the place of Latin as the medium of instruction. If that step were taken, knowledge would be brought within reach of the whole nation, and linguistic associations with obsolete forms of thought would be broken in Germany, as they had already been broken in England and France through the influence of the vernacular writings of Bacon and Descartes. Leibniz thought that he could best propagate his views and achieve his reforms through the medium of a society of men like-minded with himself. From the time of his earliest manhood, his ideas on the constitution and functions of such a society underwent a gradual development, and they issued from time to time in concrete proposals. As first conceived by him, the society was to consist of a limited number of scholars whose duty it would be to record experiments, to correspond and co-operate with other scholars and academies abroad, to form a universal library, and to advise on matters concerning commerce and the arts. The society was to have authority, in Germany, to license the publication of only those books which satisfied their standards. Further details were embodied in two memoranda written by Leibniz about 1670 in which the proposed institution was designated as an *Academie oder Societät in Teutschland zum aufnehmen der Künste und Wissenschaften* (Foucher de Careil, *Œuvres de Leibniz*, Vol. VII, Paris, 1875, pp. 27 ff. and 64 ff.). The interests of the society were to be very extensive, and were to include history, commerce, records, art, education, etc., besides science and technology. Extensive research was to be carried on in anatomy and physiology, and new methods in social science were to be tested in connection with the treatment of the sick poor, the technical education of orphans, the supervision of prisons, etc. The society was to send out travelling teachers, and to publish a journal through which useful inventions, by whomsoever made, could be widely circulated. In these memoranda Leibniz complains that in Germany important inventions are not applied to practical life for the good of mankind, as they might be. They are often lost, or else they go abroad, and are subsequently re-introduced into Germany as novelties. This might be remedied, he thought, if there were a society to preserve and develop such inventions. Shortly afterwards Leibniz was able, during visits to Paris and London, to study the working of the *Académie des Sciences* and of the Royal Society at close quarters. He was thereby inspired to put forward a fresh scheme for establishing a society of picked men, adequately maintained and provided with instruments. Each member was to devote himself to experiment on some chosen problem, and to report his results in the German language. The knowledge so accumulated was to be applied systematically in the service of humanity, and ultimately to be embodied in a vast encyclopaedia of all the sciences. In 1676 Leibniz became Librarian to the Duke of Hanover, and, when a daughter of that house married the Elector Frederick I of Prussia, it occurred to Leibniz that a society such as he had in mind might be founded in Berlin under the protection of the Elector. He learned that meetings of scientists were already being held there at the house of Spanheim, the diplomat, whom he accordingly approached. He also seems to have tried to persuade the Electress to extend her scheme for founding an observatory at Berlin to include an academy of the sort he desired. Again, in 1699, Germany decided to adopt the Gregorian Calendar, and Leibniz suggested that the Elector should retain the monopoly of the calendars, and should apply the proceeds to endow an observatory and an academy. This was agreed to, and the new Academy received its charter on July 11, 1700.

The plans for organizing the Academy were mainly drawn up by Leibniz, in consultation with Jablonski, the Court preacher, while the Elector stipulated that history and the development of the German language should be among the Academy's interests. Leibniz was to be president, and, as in the Royal Society, there was to be a Council charged with the government of the Academy and the election of new members. There were to be three classes of meetings, concerned respectively with *Res physico-mathematicae*, *Lingua Germanica*, and *Res literariae*. To get the Academy properly running, with a meeting-place of its own and definite statutes, required ten years of effort in the face of obstacles and disappointments of all sorts. The Academy published the first volume of its *Miscellanea Berolinensia* in 1710, in Latin, after all. It contained fifty-eight articles, mainly on mathematics and science, of which twelve were by Leibniz himself. Thereafter, however, Leibniz became alienated from the other leaders of the Academy, which now fell for a time into decline, especially under the adverse rule of Frederick William I, but only to revive brilliantly when more favourable circumstances prevailed. In the original scheme of Leibniz the Berlin Academy was to be the centre of a network of related societies extending all over Germany and eventually all over the civilized world. Although this plan was not realized, yet the establishment of the St. Petersburg Academy (1724) seems traceable to a conversation which Leibniz had with Peter the Great.

(See M. Ornstein, *The Rôle of Scientific Societies in the Seventeenth Century*, Chicago, 1928; T. Birch, *The History of the Royal Society of London*, 1756–7; Sir Henry Lyons, *The Royal Society, 1660–1940*, Cambridge, 1944; R. T. Gunther, *Early Science in Oxford*, vol. iv, Oxford, 1925; J. L. F. Bertrand, *L'Académie des Sciences et les Académiciens de 1666 à 1793*, Paris, 1869; H. Brown, *Scientific Organizations in Seventeenth-Century France*, Baltimore, 1934.)

CHAPTER V

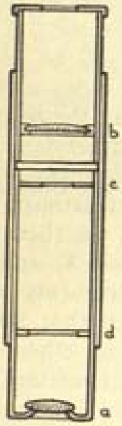
SCIENTIFIC INSTRUMENTS OF THE SEVENTEENTH CENTURY

Reference has already been made to the important function of scientific instruments in modern science. The merest beginnings of the history of modern science cannot be told without reference to some scientific instruments. And in the foregoing pages explicit mention has already been made of some of them, while the use of others has been implied in the results narrated. It will be convenient at this stage to relate as briefly as possible the story of the most important of the scientific instruments invented in the seventeenth century, and some anticipations of subsequent developments will have to be included in the story in order to make it reasonably self-contained. The instruments selected for historical treatment in the present chapter are the microscope, the telescope, the thermometer, the barometer, the air-pump, the pendulum-clock, and a few marine instruments. Various other scientific instruments will be dealt with in subsequent chapters. It will be seen that these instruments made their appearance, in some form or other, at the very beginning of the modern period. This is highly characteristic of an age which set out with a resolute determination to find things out for itself.

The Microscope

The simple microscope, or single converging lens of short focus, has a long history. Such magnifying glasses, as well as burning glasses, were well known to the ancient Greeks and to the mediaeval Arabs. The different kinds of images formed by various sorts of mirrors were also a favourite theme of the early mathematicians, who explained them on geometrical principles. The compound microscope, however, does not seem to have been discovered until about 1590, or even a little later. The compound microscope consists of a combination of several convergent lenses, one of which has a short focal length. The history of its invention is uncertain. But the credit for the invention most probably belongs to the Netherlands, where already in the Middle Ages the art of polishing glass and precious stones flourished, and where, by the end of the sixteenth century, the making of lenses for spectacles was a well-established industry. The earliest compound microscopes were so imperfect that some men of science, including Leeuwenhoek, one of the greatest microscopic biologists of the seventeenth century, preferred to use the single microscope.

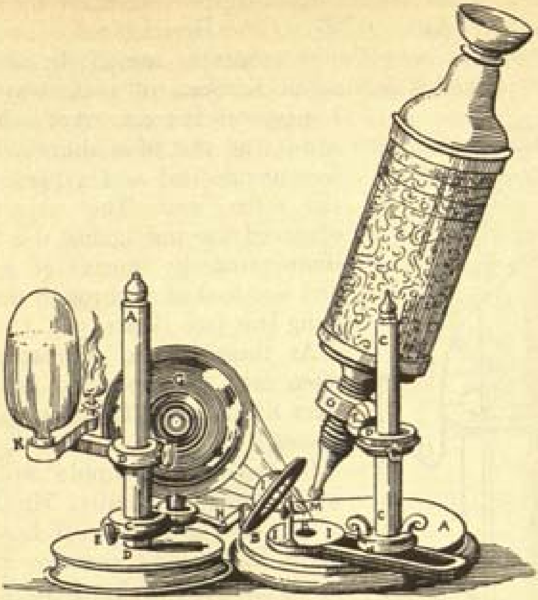
The credit for the invention of the compound microscope probably belongs to Zacharias Jansen. He was a spectacle-maker of Middelburg, Holland, and is said to have chanced upon the invention by a happy accident about the year 1590. His microscope consisted of a combination of a double convex lens and a double concave lens, the former serving as object-glass, the latter as eyepiece. One of the oldest examples of this type of compound microscope has been described by Borelius. The tube was about 18 inches long, and had a diameter of about 2 inches. Small objects placed on the pedestal of the microscope appeared much larger when seen through the tube. The Science Society of Middelburg still owns such a compound microscope reputed to have been constructed by Jansen.



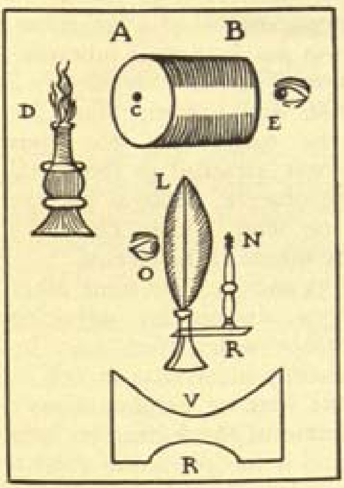
Illustr. 28.—Microscope with Two Convergent Lenses

The compound microscopes used nowadays are, of course, constructed differently. They consist of two convergent lenses, or of two lens-systems, each of which functions as a single lens. The lens nearest to the object (lens *a* in Illustr. 28) produces a real image which can be looked at through the second lens (*b*) as through a magnifying glass. This kind of microscope, however, was not constructed till the second decade of the seventeenth century.

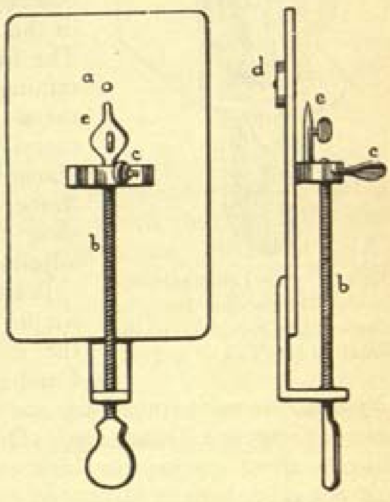
Galilei, it would appear, was the first to make use of the compound microscope for scientific purposes. In 1610, or even earlier, he studied by means of it the organs of motion and of sense in insects, and observed among other things the compound eyes of insects. Special credit for making microscopy popular is due to Hooke. He constructed one of the most famous of early compound microscopes, and his *Micrographia* (1665), the earliest treatise devoted entirely to an account of microscopical observations, showed abundantly how effectively the microscope could be used. Hooke's compound microscope (see Illustr. 29) had a single plano-convex lens for object-glass, and a plano-convex lens for eyepiece. The tube was 6 inches long, but could be lengthened by means of extra draw tubes. It was screwed on to a movable ring attached to a stand. The object to be observed was fixed on a pin rising from the base, and was illuminated by means of a lamp to which a spherical condenser was attached.



Illustr. 29.—Hooke's Compound Microscope



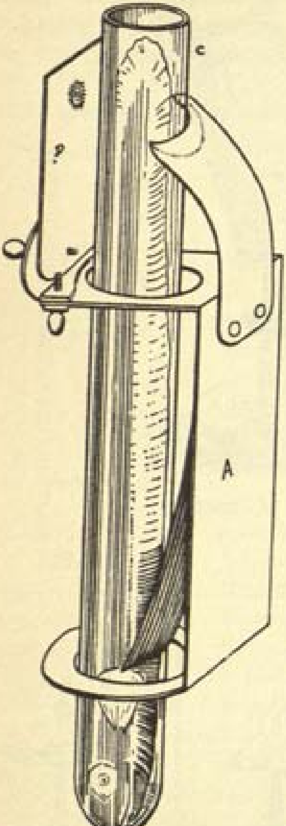
Illustr. 30.—Kircher's Microscope



Illustr. 31.—Leeuwenhoek's Simple Microscope

Other types of compound microscopes were made by Chérubin d'Orleans (1671), Kircher (1691), and Hertel (1716).

Of the single or simple microscopes in use in the seventeenth century that used by Athanasius Kircher, in 1646, was more or less typical. It consisted of a short tube, about the size of a thumb, having a lens at one end and a plane glass at the other end. The object to be observed was put against the flat glass, illuminated by means of a candle, and was looked at through the magnifying lens (see Illustr. 30).



Illustr. 32.—Leeuwenhoek's Simple Microscope used to Observe the Circulation of the Blood in the Tail of a Fish

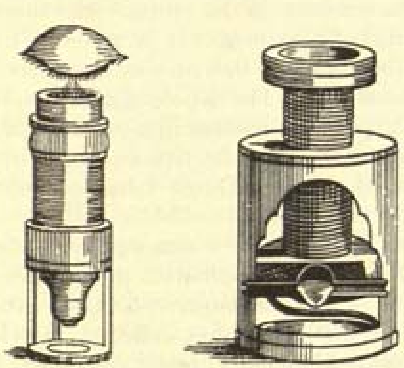
As these simple microscopes were often used for observing insects they were nicknamed "flea-glasses" or "fly-glasses."

Leeuwenhoek's simple microscopes were made differently. He mounted a lens in a flat plate of brass, or of silver, and used a concave mirror so as to focus the light on the object to be examined (see Illustr. 31).

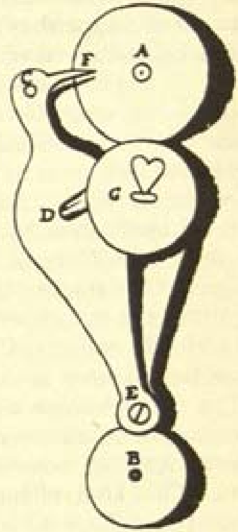
Illustr. 32 shows Leeuwenhoek's use of the simple microscope in order to observe the circulation of the blood in the transparent tail of a small fish. The fish was put in a glass tube containing water. The tube was fixed in a metal frame. And a metal plate (D) carrying the magnifying lens (just above D) was attached to the metal frame. The observer brought his eye close to the lens, which could be adjusted by means of the screws.

Illustrs. 33 and 34 show some other simple screw devices for adjusting the microscope which were used by Campani (1686) and Wilson (1700).

Finally, mention must also be made here of Stephen Gray's water microscope (Illustr. 35). The frame of the instrument was of brass about $\frac{1}{16}$ inch in thickness, and it was pierced at A by a hole about $\frac{1}{30}$ inch in diameter, surrounded on each face of the metal by a spherical depression in the surface. In using the microscope, the hole and the depression were filled with water so as to constitute a double convex lens. This was used for examining small objects, placed on the point F, or drops of water, placed in the hole C. The position of the object in relation to the lens could be adjusted by turning the support CDE about E, and by turning the screw G, which acted upon the support at D so as to bend it towards, or away from, the frame AB. In this way the object could be brought into focus. At B the metal was rather thicker, and contained a hole



Illustrs. 33 and 34.—Compani's and Wilson's Screw Microscopes



Illustr. 35.—Gray's Water Microscope

about $\frac{1}{10}$ inch in diameter. In this a drop of water could be formed, and the contained animalculae could be examined in light reflected from the opposite surface of the drop, which thus, according to Gray, served as its own microscope (*Phil. Trans.*, 1696, Vol. XIX, No. 223).

The Telescope

The history of the invention of the telescope is not very clear. The claims made on behalf of Roger Bacon may be dismissed. A better case might be made out for Leonard Digges, an Oxford mathematician, who died in 1571. He would appear to have constructed some kind of telescope. For his son, Thomas, has left a fairly circumstantial account of its use. But that is all the evidence there is. To all intents and purposes the telescope may be said to have been invented in 1608 by Hans Lippershey, a Dutch spectacle-maker, of Middelburg. A rival claim has been set up on behalf of another spectacle-maker of Middelburg, namely, the above-mentioned Zacharias Jansen, whose son is reported to have stated that his father made a telescope in 1604 on the model of an Italian telescope which was dated 1590. Descartes attributed the invention to James Metius. Official documents at the Hague are in favour of Lippershey. They go to show that on October 2, 1608, the States General considered Lippershey's application for a patent for a telescope which he had invented. He was awarded a sum of money and was asked to improve his instrument so that it might be used with both eyes at once. On December 15th he accordingly submitted a binocular, and received another money award, but he was refused the exclusive licence to sell the instrument on the ground that there were others who could make such instruments. It is noteworthy that an application by Metius for a similar patent was considered by the States General on October 17th. The whole dispute is of no great importance. The Dutch spectacle makers in any case only regarded the telescope as a curious toy. Its effective scientific use is so closely connected with Galilei that the Dutch telescope soon came to be known as the Galilean telescope.

The first telescope constructed by Lippershey was very like the first compound microscope, consisting of a combination of a double-convex lens, as object-glass, and a double-concave lens, as eye-piece. This kind of instrument is still sometimes called a Dutch telescope; and opera glasses and other binoculars are still constructed in that kind of way. Like the invention of the compound microscope, that of the telescope seems to have been the result of a happy accident. It is related that quite by chance Lippershey one day turned such a combination of lenses towards the weather-vane on a neighbouring church steeple and was pleasantly surprised to find the weather-vane considerably magnified.

The news of the wonderful invention spread rapidly. In Germany telescopes are said to have been on sale already by the end of 1608. In Italy Galilei heard of the invention in 1609. In France the telescope was used in 1610 for the observation of Jupiter's satellites.

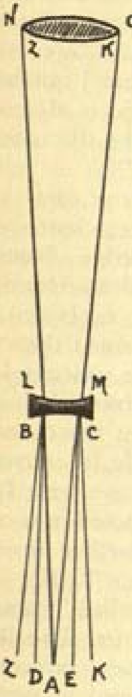
For the fullest appreciation of the scientific possibilities of the new invention we must turn to Galilei. He was at the height of his powers when news of the invention reached him. It filled him with such eager enthusiasm that he set to work at once to construct a telescope and to use it for making astronomical observations. In his *Sidereus Nuntius*, published in 1610 (translated into English as *The Sidereal Messenger* by E. S. Carlos in 1880), Galilei wrote as follows:—

"About ten months ago news reached my ears that a Dutchman had invented an instrument by means of which distant objects could be seen as clearly as near ones. This made me ponder how I could construct such an instrument. Guided by the laws of optics I hit upon the idea of fixing two lenses to the ends of a tube, one lens being plane-convex and the other plane-concave. When I brought my eye near the latter lens objects appeared to me only about a third of their actual distance away and nine times as large. As I stinted neither pains nor pence I was so successful that I obtained an excellent instrument which enabled me to see objects almost a thousand times as large and only one-thirtieth of the distance in comparison with their appearance to the naked eye."

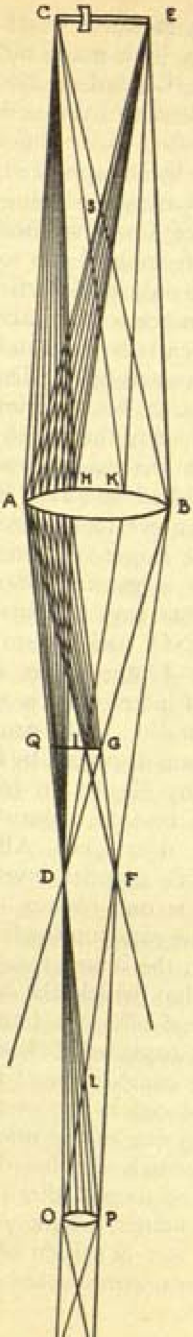
Galilei's telescope was essentially a Dutch telescope, but was much better than those constructed by the Dutch spectacle-makers, as might have been expected from his superior knowledge of optics.

The optical principles involved in the Dutch or Galilean telescope, as well as those involved in the microscope, were explained by Kepler in his *Dioptrics*, published in 1611. He explained that the blurred images seen when the eye looks through the concave lens LM (see Illustr. 36) become larger and more distinct when the convex lens NO is placed at a certain distance from the concave lens. He further explained that rays which are made to converge by means of a convex lens, NO, and fall on a concave lens, LM, before they reach their point of intersection, are refracted in such a way that either their point of intersection is moved farther forward (to A) or the rays become parallel (D, E) or divergent (Z, K).

The Dutch telescope was soon displaced by the so-called "astronomical telescope" suggested by Kepler in his *Dioptrics*. The later telescope, like the later microscope, consists of two convergent lenses (see Illustr. 37). The object-glass, AB, is placed at such a distance from the object, CE, that its inverted image would be indistinct, but by placing a second convex lens, OP, between the eye and this blurred image the rays coming from D and F are made convergent and distinct. And the image thus produced by the eye-piece appears larger than that which the lens OP receives from the lens AB. The astronomical telescope had two advantages over the Dutch telescope which it displaced. It had a wider field of view, and it rendered possible the comparison of the image of a distant object with a small object placed in the common focus of the two lenses, which led to the invention of the micrometer by Gascoigne (about 1638). Curiously enough Kepler did not construct the telescope described by him and named after him. The first telescope of the kind was made by Scheiner, whom we have already met in the chapter on Galilei. It was Scheiner also who, following yet another suggestion of Kepler's, constructed a telescope containing

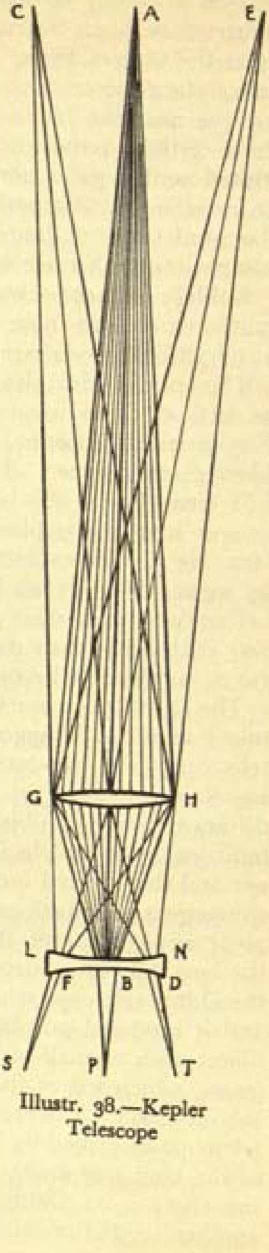


Illustr. 36.—Dutch Telescope



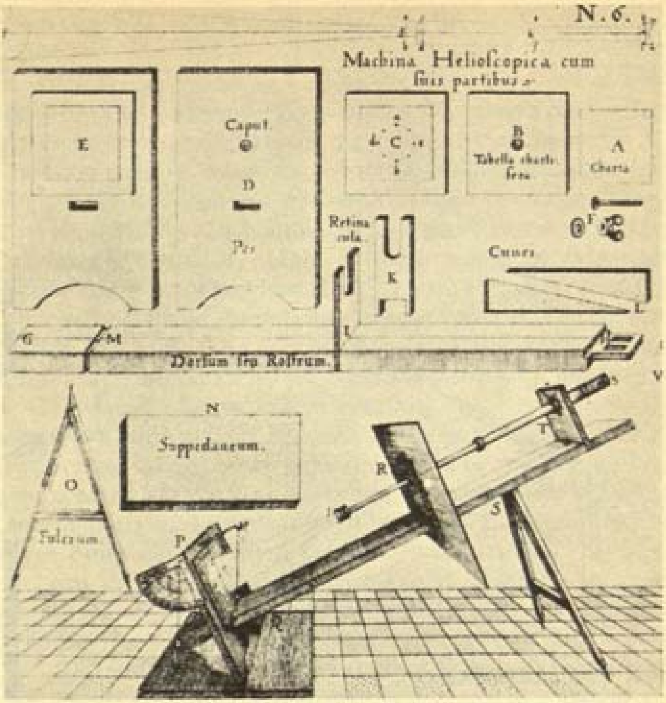
Centre:

Illustr. 37.—"Astronomical" Telescope



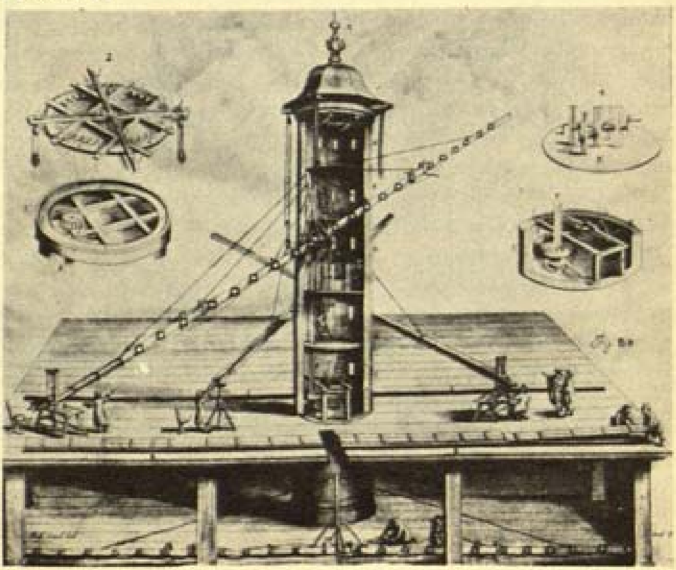
Illustr. 38.—Kepler Telescope

Illustr. 39



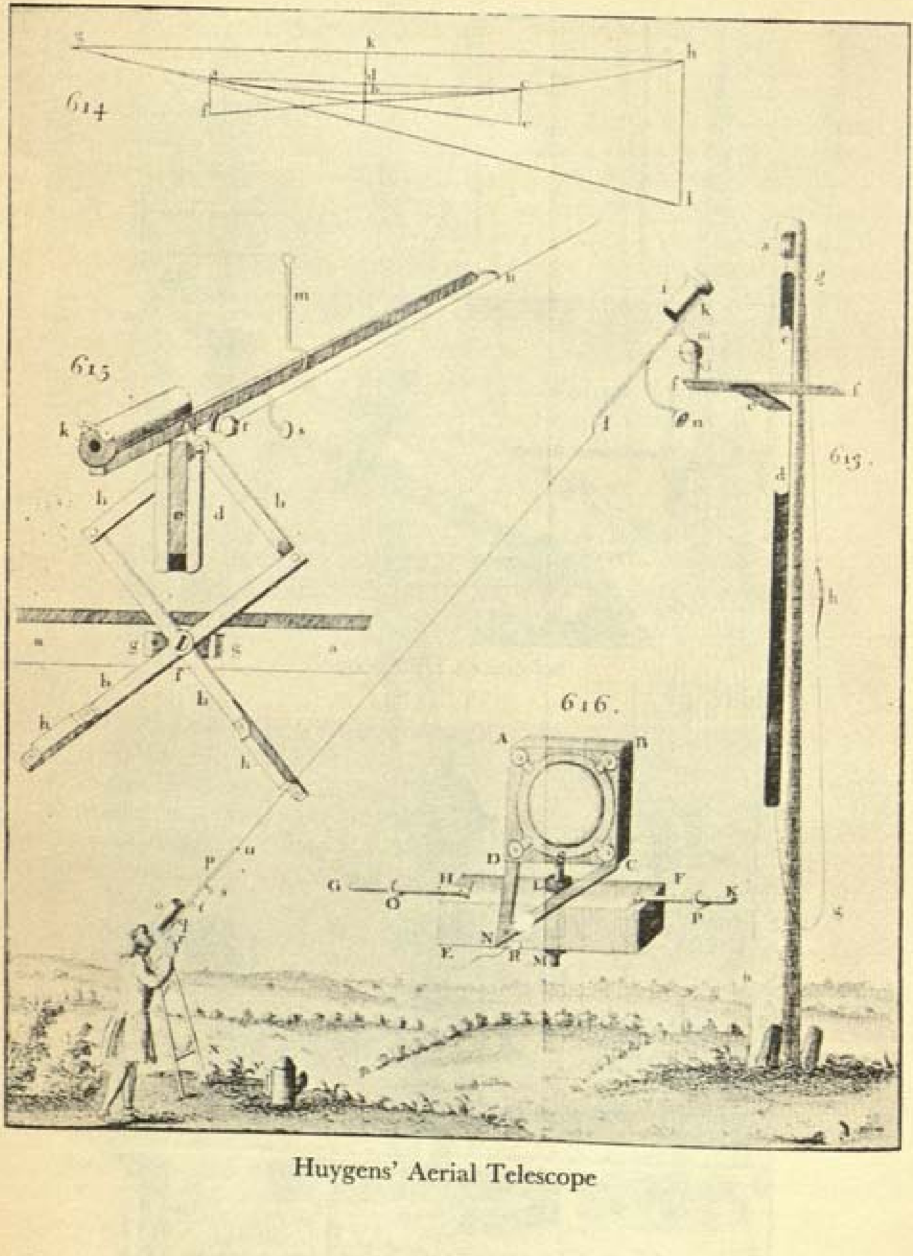
Scheiner's Helioscope

Illustr. 40



Hevelius' Long Telescopes

Illustr. 41



Huygens' Aerial Telescope

a third convex lens, which transformed the inverted image into an erect image.

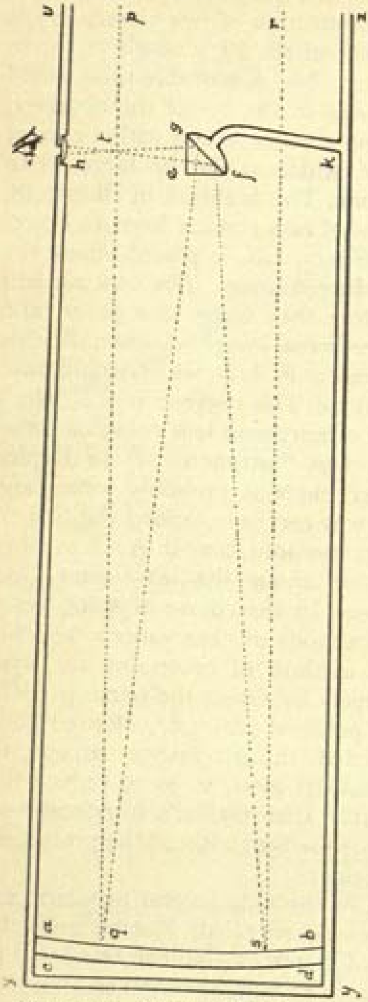
Other suggestions made by Kepler for the improvement of the telescope were the following. The substitution of two convex lenses (one placed immediately behind the other) for a single eyepiece would make it possible to use shorter tubes. A movable tube would make it possible to adapt the telescope to the eye of the observer. Lastly, he showed how, by combining a concave lens with a convex lens real images could be obtained which would be larger than those produced by a convex lens alone. This is shown in Illustr. 38, which shows the path of three beams of rays coming from an object at the points C, A, E. The concave lens, LN, is placed where the convex lens, GH, would project a blurred image. The concave lens catches the pencils of rays just before they come to a point, and brings them to a point at S, P, T, where a real image is formed which is more distinct and larger than the image at F, B, D, which would have been produced by the convex lens alone. This suggestion of Kepler's led in recent times to the invention of telephoto lens combinations.

It is not quite certain when the first "astronomical" or Kepler telescope was constructed. Scheiner made it probably some time between 1613 and 1617. Scheiner was certainly among the first to use a telescope for astronomical observations, and in April or May 1611 he observed the Sun-spots at about the same time that Fabricius and Galilei observed them. In the course of many years of astronomical work he made thousands of observations, and his experiences led him to devise a method of protecting the eyes during telescopic observations, namely by fitting the telescope with special darkening glass. He fixed polished plates of coloured glass in front of the lenses, and even tried, though without success, to make the lenses themselves of coloured glass, so as to reduce the intensity of the light. It is possible that Galilei's blindness was brought about through his observing the Sun without the protection of some such device as that of Scheiner.

Scheiner also devised a method of enabling several people to see at the same time what the telescope revealed. Placing what he called a helioscope (really a kind of Dutch or Galilean telescope) in a dark room, and directing it toward the Sun, he obtained an image of the Sun's disc with the Sun-spots on a white surface arranged at the rear of the telescope, so that all who were present in the room could see it. (See Illustr. 39.)

Scheiner gave an account of his astronomical work in a book called *Rosa Ursina*, which he published in 1630. ("Rosa" was the symbolic designation of the Sun; "Ursina" was intended as a compliment to his patron, the Duke of Orsini.)

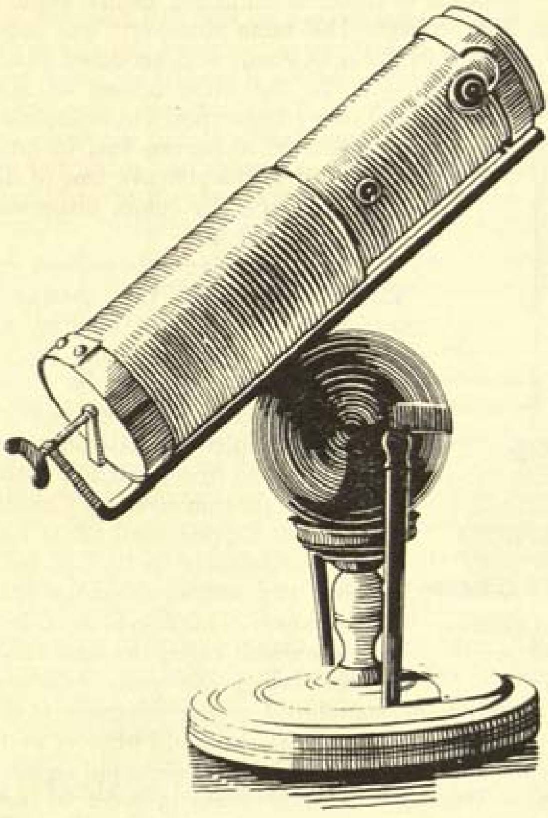
The lenses in use then were soon felt to be unsatisfactory. Their real defect was not understood until Newton discovered it. In the meantime Kepler, Descartes, and others attributed the aberration of the lenses to their spherical surface, and sought to surmount the trouble by the use of lenses with an hyperbolic surface. But such lenses were difficult to construct. Another way of getting over the difficulty consisted in the use of very long telescopes. Hevelius (or Hevel) of Danzig had a telescope which was 150 feet long, and he designed a tower to support it. To avoid the difficulties of constructing and mounting such long telescopes Huygens, following a suggestion of Auzout, introduced the "aerial telescope," which dispensed with the usual tube by arranging the object-glass and the eyepiece in the way shown in the accompanying illustration (Illustr. 41).



Illustr. 42.—Newton's Reflecting Telescope (Schema)

Newton's discovery of the composite nature of white light early led him to the conclusion that the principal defect of the refracting telescopes then in use was due, not to the spherical aberration of their object-glasses, but to their chromatic aberration, whereby the images formed have coloured edges. Moreover, he considered this defect of refracting telescopes incurable. He therefore considered the possibility of constructing some other type of telescope. Now in 1663 James Gregory had proposed the construction of a reflecting telescope as a cure for spherical aberration. Newton took up the suggestion in his own way, and in 1668 constructed the first reflecting telescope. In this instrument the rays coming from a distant object were concentrated by reflection from a concave mirror, the convergent beam being intercepted just before reaching its focus by a small plane mirror which directed it into an eyepiece placed at the side of the tube (see Illustr. 42).

Newton ground his own speculum and succeeded in producing a diminutive instrument of about 6 inches in length and 1 inch in



Illustr. 43.—Newton's Little Reflector

aperture, with which, however, he was able to observe the satellites of Jupiter and the phases of Venus. He subsequently constructed a similar telescope of larger dimensions. This he presented to the Royal Society, in whose Library it is now preserved (Illustr. 43).

Newton's assumption that the chromatic aberration of refracting telescopes was irremediable was subsequently shown to have been too pessimistic. In 1733 Chester More Hall succeeded in constructing achromatic lenses which produced images devoid of colour. He seems to have been led to his discovery by considering the (false) analogy of the human eye, an analogy which had been conceived also by David Gregory (1695), and was revived again later by Euler (1747). By mistake he thought that the different humours in the eye, by their different refractions of rays of light, produced colourless images on the retina, and so he concluded (fortunately) that lenses composed of different refracting media might produce images free from colour. The same discovery was made independently, in 1758, by John Dollond, who rendered valuable services in the construction of achromatic refracting telescopes. His achromatic lenses consisted of a convex lens of crown glass combined with a concave lens of flint glass, which corrects the colour dispersion caused by the crown lens.

Illustr. 44.—Galilei's Thermoscope

Soon after 1630 astronomers began to use telescopes for the measurement of angles. The telescopes used at first were of the Dutch (or Galilean) type. As will be more fully explained in a subsequent chapter, better results were obtained later when the Kepler (or "astronomical") telescope was used in conjunction with a micrometer, like that invented by Gascoigne. But this did not happen until about 1660. Some of the astronomers, in fact, preferred to do without any sort of telescope for angular measurement. This is evidenced by the controversy which raged between Hevelius and Hooke during the years 1668–79. Hooke strongly advocated the superiority of the telescope over diopters (open sights). Hevelius insisted that he could observe as accurately with open sights as Hooke could with telescopic sights. In 1679 Halley went to Dantzig for several weeks in order to compare the accuracy of his own observations, made by means of Hooke's telescopic sights, with Hevelius' observations by means of open sights. Halley declared that Hevelius had made out his case. Nevertheless Hooke was certainly right in his vindication of telescopic sights.

The Thermometer

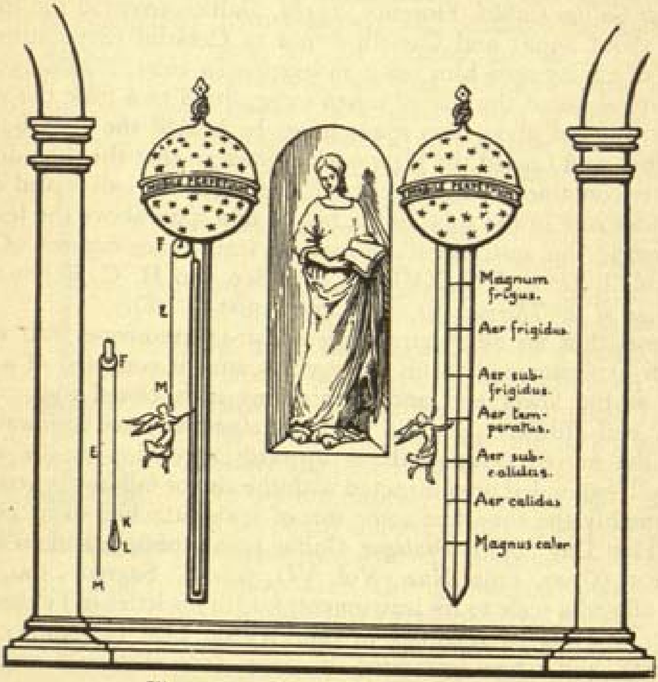
Galilei is usually credited with the invention of the first thermometer in modern times. His claim rests mainly on the testimony of his friends and pupils, as his own writings, so far as they have survived, seem to contain only one incidental reference to the principle of the instrument. Writing to Galilei on May 9, 1613, his friend Sagredo attributes the invention to him, though in a later letter (February 7, 1615) he claims himself to have improved upon the primitive form of the instrument, of which he made extensive use (*Le Opere di Galileo Galilei*, Edizione Nazionale, 1890–1909, Vol. XI, p. 506, and Vol. XII, p. 139). Again, according to Viviani (*Vita di Galileo Galilei*, Florence, 1718), Galilei invented the instrument about 1592; and Castelli writes to Cesarini (September 20, 1638) of having seen him use it in lectures in 1603: "Galilei took a glass vessel about the size of a hen's egg, fitted to a tube the width of a straw and about two spans long; he heated the glass bulb in his hands and turned the glass upside down so that the tube dipped in water contained in another vessel; as soon as the ball cooled down the water rose in the tube to the height of a span above the level in the vessel; this instrument he used to investigate degrees of heat and cold" (*Opere*, Vol. XVII, p. 377. See also H. C. Bolton: *The Evolution of the Thermometer, 1592–1743*, 1900, p. 18).

It was thus an air-thermometer or air-thermoscope with which Galilei experimented in his early years, and it consisted of a tube open at the lower end and terminating in a closed bulb at the upper end (Illustr. 44). The bulb contained air, and the water in the tube moved downwards or upwards according as the air in the bulb expanded or contracted with the rise or fall in temperature. Presumably the tube had some sort of scale attached to it. For, in the "First Day" of his *Dialogue*, Galilei speaks of 6, 9, and 10 *degrees* of heat (*Opere*, Ediz. Naz., Vol. VII, p. 55). Sagredo, too, must have affixed a scale to his instrument, for (in his letter of February 7, 1615) he gives its readings in the greatest heat of summer (360 degrees), and when immersed in snow (100 degrees), and in a mixture of snow and salt (zero).

The suggestion for the construction of a thermoscope may well have come to Galilei while reading the works of Hero of Alexandria. The ancients already knew that air expands when it becomes warmer. Some of Hero's mechanical toys were constructed and operated on this basis, and Philo of Byzantium (first century B.C. or A.D.) had actually made a thermoscope. Robert Fludd, who died in 1637, gave an account of a thermoscope which he said he had seen described in a manuscript about five hundred years old (*Philosophia Moysaica*, Goudae, 1638; *Mosaicall Philosophy*, London, 1659). Be that as it may, Galilei was the first modern man of science to think of utilizing the expansion of air for the measurement of temperature.

Sagredo describes the thermometer as an "instrument for measuring heat and cold." The word *thermomètre* first appears in *La récréation mathématique* (1624) by J. Leurechon.

While Galilei and Sagredo appear to have compared the temperature of various places and seasons, and to have experimented with freezing mixtures, Sanctorius, a medical friend of Galilei, and Professor of Medicine at Padua, used a special form of the thermoscope to indicate fluctuations in the heat of the human body, as



Illustr. 45.—Guericke's Thermoscope

he describes in his *Commentaria in artem medicinalem Galeni*, Venice, 1612 (written 1611). This peculiar thermoscope may be described as the first clinical thermometer. An account and illustration of it will be found in Chapter XVIII (see page 432). Here it need only be added that Sanctorius also used his thermoscope in an attempt to compare the heat of the Sun with that of the Moon.

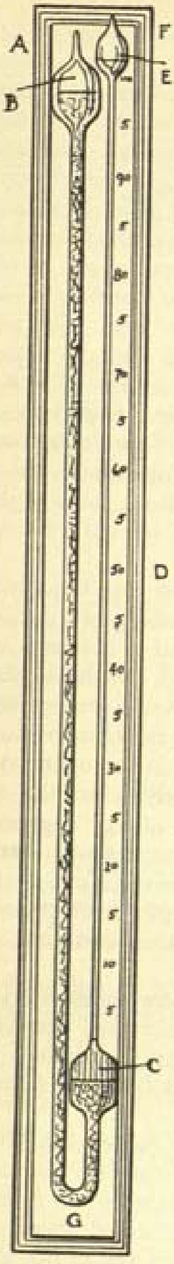
In his *Novum Organum* (1620) Francis Bacon describes an instrument very like Galilei's thermoscope with a paper scale attached to it (Book II, xiii, 38). Nothing, however, is known about this scale. In any case, such a scale could not have been reliable, if only because variations in atmospheric pressure, as well as variations in temperature, affected the position of the fluid in the tube, so that it could only be used for comparing temperatures within short intervals of time.

This characteristic fault of air-thermometers was clearly recognized by Boyle, following the discovery of his pneumatic law, and in the light of the known variability of the atmospheric pressure. "These Instruments," he writes, "being subject to be wrought upon by the differing weights of the Atmosphere, as well as by Heat and Cold, may . . . easily misinform us in several cases, unless in such Cases we observe by other Instruments the present weight of the Atmosphaere" (*New Experiments and Observations touching Cold*, London, 1665, p. 71; *Works*, ed. 1772, Vol. II, p. 498). The air-thermometer, however, continued to be employed and developed throughout the first half of the seventeenth century. Kircher (*Magnes, sive de arte magnetica*, Rome, 1641) describes a thermoscope in which a glass tube open at each end dipped into liquid contained in an otherwise closed bulb. Heating the air imprisoned above the liquid in the bulb caused it to expand and to force some of the liquid up the tube. Kircher mentions the use of mercury in this connection.

An improved air-thermometer was constructed by Otto von Guericke (*Experimenta Nova*, 1672, Book III). It consisted of a copper sphere containing air. To the sphere was attached a U-tube containing alcohol, which sealed the vessel (Illustr. 45). On the alcohol there was a float from which a thread passed over a pulley and supported a figure of a little angel which indicated the temperature. When the air in the sphere expanded, the alcohol in the open limb of the U-tube rose, and the angel fell; conversely when the air contracted the angel rose. Guericke used a scale of six "degrees," from "great heat" to "great cold." As an experimenter with barometers he knew, of course, of the varying pressure of the atmosphere, and he put a valve in the sphere of his thermoscope to compensate for variations of atmospheric pressure by correlated variations in the volume of air enclosed.

An air-thermometer in which temperatures were measured not by the expansion of the enclosed air, but by its increased pressure. and whose readings were regularly corrected for fluctuations in atmospheric pressure, was produced by Amontons (*Mém. de l'Acad. des Sciences*, Paris, 1688).

This instrument consisted of a mercury siphon barometer ABC, the tube of which was enlarged at the lower mercury surface into a bulb C, and was then continued vertically upwards so as to end in another bulb F. The portion CD of the tube contained potassium carbonate solution, while above that was a column DE of oil, and the tube ended in a sealed bulb FE of air. In constructing the



Illustr. 46.—Amontons' Thermometer

instrument, the tube, initially open at A and F, was held upright and filled with mercury, by means of a funnel attached with sealing-wax at F, until the level rose to within half an inch of A. The opening at A was then hermetically sealed by means of a candle-flame and blow-pipe. The tube was next inverted so as to remove the air from the bulb B, and the mercury from the limb FG. When the tube was set upright once again, the mercury rose to C, and a Torricellian vacuum was formed above B. The empty tube above C was now filled up to D with coloured potash solution, and the other half, up to E, with oil. The tube was allowed to stand for about a week for the liquids to find their true levels. The end F was then hermetically sealed, and the tube was fastened to a board upon which graduations were marked along CE. The temperature was shown by the position of the junction of the oil and the salt solution against the scale.

What Amontons' instrument, and others of the period, chiefly needed was an accurate standard scale. In a later paper on this problem (*Histoire de l'Académie des Sciences*, 1703), Amontons described another air-thermometer in the form of a U-tube in which the volume of the air was kept constant while its pressure (represented by the height of the confining mercury column) was varied and measured at the various temperatures to be compared. In this way Amontons hoped to avoid errors due to want of uniformity in the bore of the thermometer tube.

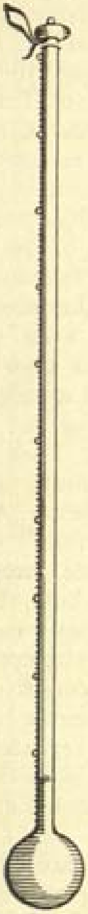
The first suggestion of a liquid thermometer seems to have been made by Jean Rey, a French doctor, in a letter to Mersenne, dated January 1, 1632 (Rey's *Essays*, 1777, p. 136). He reversed the arrangement in Galilei's thermoscope, filling the bulb with water and the stem with air, and using the expansion of the water as an index of temperature. He writes: "To make use of it, I put it in the sun, and sometimes in the hands of a fever patient, having filled it quite full of water except the neck; the heat expanding the water makes it ascend by a greater or less amount according to the great or little heat" (Bolton, *op. cit.*, p. 30). He does not appear to have sealed the end of the stem, and in that event the evaporation of the water must have made his instrument very unreliable.

A great improvement in the construction of liquid thermometers is attributed to the Grand Duke Ferdinand II of Tuscany, one of the founders of the Florentine Accademia del Cimento. He substituted coloured alcohol for water as the thermometric liquid, and the tube was hermetically sealed. This improvement may have been introduced as early as 1641, certainly by 1654. Such thermometers were in regular use among the members of the Florentine Academy, and were consequently known as Florentine thermometers (see Illustr. 47). The divisions on these thermometers were made directly on the glass, not on a separate scale attached to it; but as the divisions were marked by minute glass beads, instead of fine lines, some of the advantage of this arrangement was lost. Four kinds of thermometers were used by the Academy, according to the degree of accuracy required. They had from fifty to three hundred divisions respectively. Illustr. 48 shows a Florentine thermometer with three hundred divisions. As the tube was much too long to be left straight, it was ingeniously shaped into spiral form. The various instruments were made comparable by securing the same relation between the size of the bulb, the diameter of the stem, and the quantity of alcohol. Curiously enough these thermometers had no fixed points. The only attempt made by the Florentine Academy to secure fixed points consisted in taking the lowest position of the alcohol in the thermometer registered in Tuscany in mid-winter and the highest point registered in midsummer. These points coincided approximately with the sixteenth and the eightieth division respectively on the hundred-division thermometer.

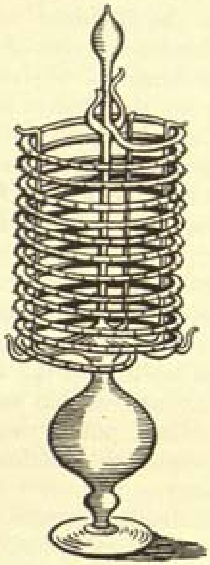
The Academicians used also to estimate temperature by means of a graduated series of hollow glass balls floating on the surface of alcohol in a vessel. Each of the balls would sink when the liquid rose above a corresponding degree of temperature and thus fell below a certain density. Thus as the alcohol was warmed up, the balls sank one after another in regular succession, and so registered the rise of temperature.

Florentine thermometers soon spread over Europe. Among the first Englishmen to occupy themselves with thermometric experiments were Boyle and Hooke. Boyle deplored the lack of an absolute thermometric standard (*New Experiments and Observations touching*

Cold, London, 1665; Discourse II; *Works*, ed. 1772, Vol. II, pp. 489 f.). He suggested the freezing point of oil of aniseed as a fixed point, but did not see the need for *two* fixed points. Hooke, in his *Micrographia* (1665, pp. 38, 39), writes of sealed thermometers



Illustr. 47.—Florentine Thermometer



Illustr. 48.—Florentine Spiral Thermometer

'which I have . . . brought to a great certainty and tenderness." They were filled with spirit of wine coloured with cochineal, which stood near the top of the tube in summer and near the bottom in winter, and was not easily frozen. A fixed point was obtained by marking on the stem where the spirit stood when the bulb was immersed in distilled water just freezing: "the rest of my divisions . . .

I place according to the Degrees of Expansion, or Contraction of the Liquor in proportion to the bulk it had when it indur'd the newly mention'd freezing cold."

Huygens, writing to Robert Moray on January 2, 1665, suggested standardizing thermometers by agreeing upon a definite proportion between the capacity of the bulb and the bore of the tube, and taking the freezing point *or* the boiling point of water as a fixed point from which degrees could be measured (*Schriften der naturforschende Gesellschaft*, Danzig; N.F. VII). The desirability of experimentally defining *two* fixed points and of dividing the intermediate range of temperature into an arbitrary number of equal degrees was recognized by H. Fabri (*Physica*, Leyden, 1669), who used snow and the greatest summer heat to give the extreme temperatures; by Dalencé (*Traittez des baromètres, thermomètres*, etc., Amsterdam, 1688), who suggested the freezing point of water and the melting point of butter as fixed points; and by C. Renaldini (*Naturalis Philosophia*, Padua, 1693, 1694), who had belonged to the Accademia del Cimento, and who suggested using *both* the melting point of ice and the boiling point of water, and dividing the interval into twelve equal parts.

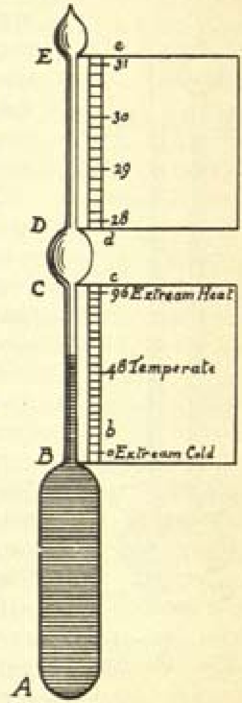
It is not known when or by whom the expansion of mercury was first thought of or employed in thermometry. The Florentine Academicians experimented with the metal, and found it less expansive than water, though quicker to respond to changes of temperature. Musgrave described mercury clinical thermometers. Halley (*Phil. Trans.*, 1693, Vol. XVII, p. 650) experimented on the thermal expansion of water, mercury, and alcohol, to see which would serve best for thermometry. He found water slow to respond to heating and cooling, though it ultimately showed appreciable changes in bulk. But the high freezing point of water ruled it out as unfit for use in our climate. Mercury scored a point by responding immediately to heating, but the proportional expansion of mercury was less than that of water, spirit of wine expanded very considerably, but boiled off violently before the water in the surrounding bath had reached boiling point. Reflecting on the results of his experiments, Halley concluded that no thermometric medium could rival the claims of air, and he seems to have conceived the idea of reviving the air-thermometer, with suitable safeguards against its defects.

Of more consequence were Newton's researches in thermometry, undertaken about the same time as Halley's, but not published until later (*Phil. Trans.*, 1701, Vol. XXII, p. 824). Newton drew up a scale of degrees of heat covering the range from the freezing point of water to the heat of a coal fire, and affording such intermediate data as the degrees of heat required to boil water, to melt wax, lead, and various combinations of easily fusible metals, and to raise bodies to red heat. In constructing this scale, Newton used, for the lower temperatures, a linseed-oil thermometer having its zero at the freezing point of water, and giving twelve degrees for the heat of the human body. The degree of heat was taken as proportional to the expansion of the oil. For the higher temperatures Newton employed a thick plate of iron which was heated red-hot and then cooled in a steady draught. The degree of heat of the plate at any instant was estimated by observing the time subsequently required for the iron to cool down to the degree of heat of the human body. For this purpose Newton assumed the Law of Cooling which bears his name: "the heat which hot iron, in a determinate time, communicates to cold bodies near it, that is, the heat which the iron loses in a certain time, is as the whole heat of the iron; and therefore, if equal times of cooling be taken, the degrees of heat will be in geometrical proportion" (*Phil. Trans.*, 1701, Vol. XXII, p. 828). This is now generally expressed by saying that the rate of cooling of a body at any moment is proportional to the excess of its temperature above that of its surroundings. The law holds only for small excesses of temperature; but Newton employed it to compare the degrees of heat at which various specimens of metals placed on the heated iron solidified as it cooled. He incidentally discovered that solidification occurred at definite degrees for the metals investigated. The series of determinations with the thermometer and the plate overlapped sufficiently to allow all the degrees of heat to be expressed in terms of the divisions of the thermometer.

In 1714 or thereabouts D. G. Fahrenheit (1686–1736) introduced the familiar type of thermometer still known by his name. Fahrenheit was the son of a wealthy Danzig merchant, but he spent most of his life in Amsterdam, where he devoted himself to scientific pursuits. He visited England, and was elected a member of the Royal Society. An interest in meteorology led him to the construction and improvement of thermometers. After working with alcohol for some time, Fahrenheit adopted mercury as the thermometric liquid, taking for his fixed points the temperature of melting ice and blood-temperature. He took the zero of his scale below the temperature of melting ice by half the interval between the two fixed points, the upper of which he first marked $22\frac{1}{2}°$, whereby the melting point of ice was $7\frac{1}{2}°$. For convenience, he subdivided each degree into four parts, giving the figures 30 and 90; later he replaced 90 by 96, and then the melting point of ice became 32° and the boiling point of water 212°, as on the scale now in use. Allowing for the fact that the boiling point of water varies with atmospheric pressure, Fahrenheit constructed a thermometer with which there was combined a barometer to be used, not for measuring heights, but for meteorological purposes only (a thermo-barometer, as it came to be called subsequently when improved by Cavallo, in 1781, and by W. H. Wollaston in 1817).

Only a few other facts relating to the thermometer need be mentioned here. The scale of 80 degrees between the freezing and boiling points of water was introduced by Reaumur in 1730 (*Mém. de l'Acad. des Sciences*, Paris, 1730, p. 452; 1731, p. 250). He was led to introduce this scale by noting that alcohol of standard concentration expanded from 1,000 parts by volume to 1,080 parts upon being warmed from the freezing point to the boiling point of water. Each degree in his scale thus represented a rise of temperature corresponding to an average expansion of one-thousandth of the initial volume of the alcohol. The scale of 100 degrees was invented in 1742 by A. Celsius, though the present centigrade scale is due to Christin of Lyons (1743). Celsius denoted the temperature of melting ice by 100°, and put zero at the boiling point of water (*Vetenskaps Akademiens Handlingar*, Stockholm, 1742). The first efficient maximum and minimum thermometer, with steel indices adjustable by a magnet, was invented by James Six (*Phil. Trans.*, 1782, Vol. LXXII, p. 72).

Before closing this account of thermometers, reference may be made to an allied instrument, namely, Fahrenheit's Hypsometer (Illustr. 49).

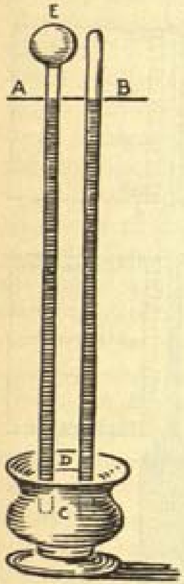


Illustr. 49.—Fahrenheit's Hypsometer

This instrument, for measuring the boiling points of liquids, was suggested by the discovery that the boiling point of a liquid depends upon the atmospheric pressure. It consisted of a cylinder AB out of which rose a tube BC leading, through a small bulb CD, to a tube DE of very fine bore, terminating in another bulb. The cylinder was filled with a liquid of good conducting properties, which, when exposed to normal air temperatures, rose to a point somewhere in the tube BC, thus measuring the temperature on the scale *bc*. When, however, the instrument was placed in boiling water, the liquid by its expansion filled the bulb CD and entered the tube DE, where its height served to measure, on the scale *de*, the temperature at which the water boiled under the existing pressure (*Phil. Trans.*, 1723–4, XXXIII, No. 385).

The Barometer

Down to the middle of the seventeenth century suction phenomena, such as the rise of water in the shaft of a pump, were generally attributed to Nature's alleged abhorrence of a vacuum. Galilei, however, in 1638, drew attention to the curious fact, known in his time, that water will not rise in the shaft of a common pump more than about 32 feet above its external level.



Illustr. 50.—Torricelli's Barometer

This observation led Galilei's pupil, Torricelli, to inquire to what height the alleged *horror vacui* was capable of raising mercury, which is about fourteen times as dense as water. He suspected that this height would be only about one-fourteenth of the height to which water could be raised, and when, on his suggestion, Viviani made the experiment, Torricelli's surmise proved correct. The apparatus used by the two investigators in joint experiments, in 1643, is shown in Illustr. 50 (*Esperienza dell' Argento Vivo*, Hellmann's *Neudrucke*, No. 7). A glass tube, about two yards in length and sealed at one end, was filled with mercury. The open end was stopped with the finger and the tube was then inverted and placed with the stopped end dipping into an open vessel of mercury. When the finger was removed the mercury surface in the tube sank to a height of about 30 inches above the mercury surface in the vessel, and remained at that level, leaving at the top of the tube an empty space which subsequently received the name of the "Torricellian Vacuum." Torricelli suspected that the column of mercury was counterpoised by the pressure of the atmosphere upon the free mercury surface; and he attributed small fluctuations in the height of the mercury column from day to day to changes in the atmospheric pressure. Torricelli's early death in 1647 prevented him from establishing his hypothesis and pressing it upon others; and the doctrine of the *horror vacui* was so deeply rooted that the convincing experiments of Pascal and Guericke were necessary before it could be banished from physics.

Pascal learned, through Mersenne, of Torricelli's experiments, and he repeated them for himself, both with mercury and with water. He was at first inclined to attribute the results to *horror vacui*, but he was won over to Torricelli's hypothesis, and definitely confirmed it by means of a crucial experiment, the idea of which he may have owed to Descartes. The experiment was carried out under Pascal's instructions by his brother-in-law Périer, in September 1648 (*Récit de la Grande Expérience de l'Equilibre des Liqueurs*, Paris, 1648, Hellmann's *Neudrucke*, No. 2). A Torricellian barometer was set up, each time with the same tube and the same mercury, at successive stations on the way up to the summit of the Puy-de-Dôme, in Auvergne. The height of the mercury was measured at each station, and it showed a progressive fall with increase of altitude. Meanwhile a second barometer, set up at the foot of the mountain, was read from time to time by another observer, and showed little change. The concomitant variation of barometric height with atmospheric pressure thus established pointed to an intimate connection between them. Upon the following day Périer repeated his observations, with a positive though less appreciable result, at the foot and on the pinnacle of the highest tower in Clermont; and later Pascal performed the experiment for himself upon lofty buildings in Paris. Later on such experiments became a favourite exercise among the members of the scientific societies of the period.

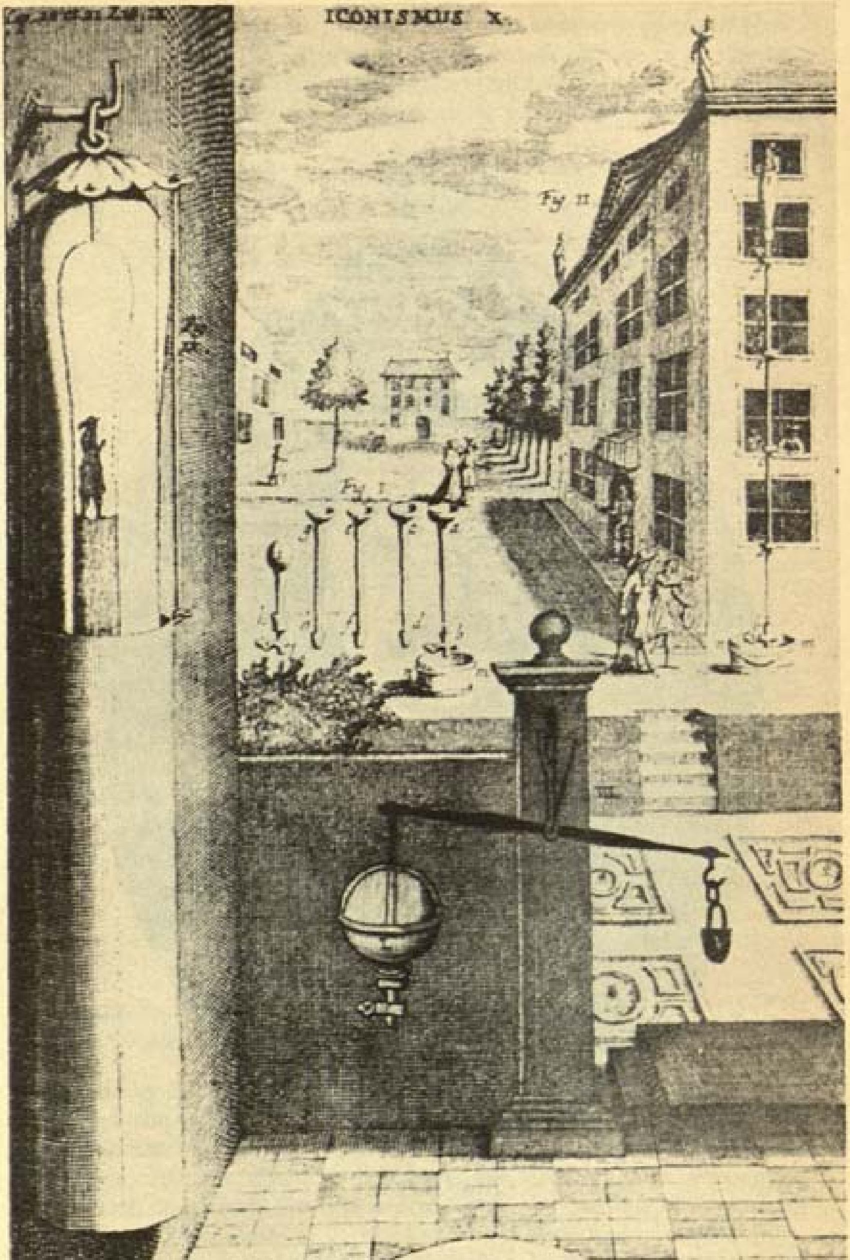
In his report to Pascal, Périer suggested that the height of the barometer might be tabulated numerically against the altitude of the place of observation, and the table used for ascertaining the height to which the atmosphere extends above the Earth. Pascal proposed the barometer as an instrument for the measurement of heights. He also estimated the weight of the entire atmosphere at eight trillion pounds. Halley was later able to tabulate pressure against altitude on the theoretical basis of Boyle's Law; he thus arrived at an estimate of the extent of the atmosphere, and showed how the table could be used with a barometer for measuring the heights of mountains, but this was not done until the beginning of the eighteenth century.

Robert Boyle about 1659 proved experimentally that the height of the fluid in a barometer depends upon the external pressure (*New Experiments Physico-Mechanicall*, 1660). He set up a barometer in the receiver of his air-pump, and noted that the column fell as the air was withdrawn, and rose as it was re-admitted.

A water-barometer was constructed by Otto von Guericke, but whether independently or in imitation of Torricelli is uncertain (*Experimenta Nova*, etc., 1672). He found that it was possible, by means of an exhausted receiver, to raise water by suction from the ground level to the third storey of his house, but not to the fourth storey. In order to ascertain the exact height to which the water would rise, Guericke contrived the apparatus shown in Illustr. 51. It consisted of a series of four brass tubes, *ab*, *cd*, *ef*, *gh* (I) joined end to end to form one long vertical tube (II), terminating above in a glass receiver *ik* (shown on a large scale in IV), and below in a tap which was immersed in a vessel of water *mn*. This tap was closed at first and the whole length *bi* of the tube and receiver was filled with water. The tap being then opened, the water in the tube sank to a certain level which could be observed through the side of the glass receiver, and which was indicated on a graduated scale by the outstretched arm of a wooden mannikin floating on the surface. The difference in the levels in the tube and in the vessel could then be ascertained with the aid of a plumb-line.

Guericke attributed the rise of the water to the pressure of the atmosphere, and the fluctuations in the level from day to day to variations in that pressure. He made a long-continued study of these fluctuations, and sought to correlate them with changes in the weather. A sudden drop in the pressure enabled him to foretell the onset of a severe storm in 1660. The connection between barometric height and weather was extensively investigated, and gave rise to much speculation in the seventeenth century and later. More or less crude mechanical explanations of the relation of pressure to rainfall, etc., were advanced by Boyle, Mariotte, and Halley among others.

Subsequent modifications of Torricelli's original form of barometer aimed at making the instrument more compact and portable, or more precise in its indications. An early improvement was the siphon-barometer in which the mercury trough was dispensed with, and the open end of the tube bent through two right angles, the atmospheric pressure being measured by the difference in the levels in the closed and open limbs respectively. Amontons, in 1665 (*Remarques et expériences physiques*, p. 121), suggested a form of barometer narrowing towards the closed end and suited for use at sea (see Illustr. 53), and later, in 1688 (*Acta Eruditorum*, p. 374), another type in which the pressure of the air was balanced by several successive columns of mercury, the height of the instrument being correspondingly reduced (Illustr. 54). In Morland's form (which was based on that of Ramazzini) the tube rose obliquely, so that a slight change in atmospheric pressure produced a considerable displacement of the mercury in the tube. In other forms based on the same principle the tube ascended in the form of a spiral. Huygens, following some suggestions of Descartes, sought to increase the sensitiveness of the barometer to changes of pressure by employing, in conjunction with mercury, liquids of lower specific gravity, such as water or spirit of wine.



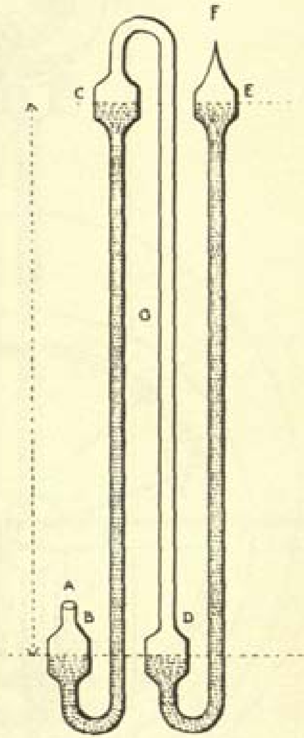
Guericke's Water-Barometer

Illustr. 52

Otto Guericke

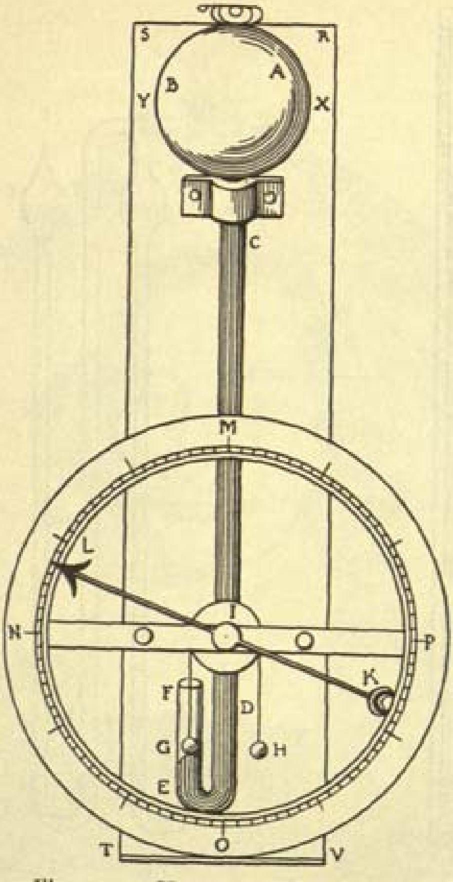
One of the most famous barometers is that known as *Hooke's Wheel Barometer* (*Micrographia*, *The Preface*, and Sprat, *H.R.S.*, p. 173). Hooke took a bulb AB (see Illustr. 55) with a stem CD $2\frac{1}{2}$ feet long, to the end of which was cemented an inverted siphon-tube DEF having

Illustr. 53.—Amontons' Sea-Barometer



Illustr. 54.—Amontons' Compound Barometer

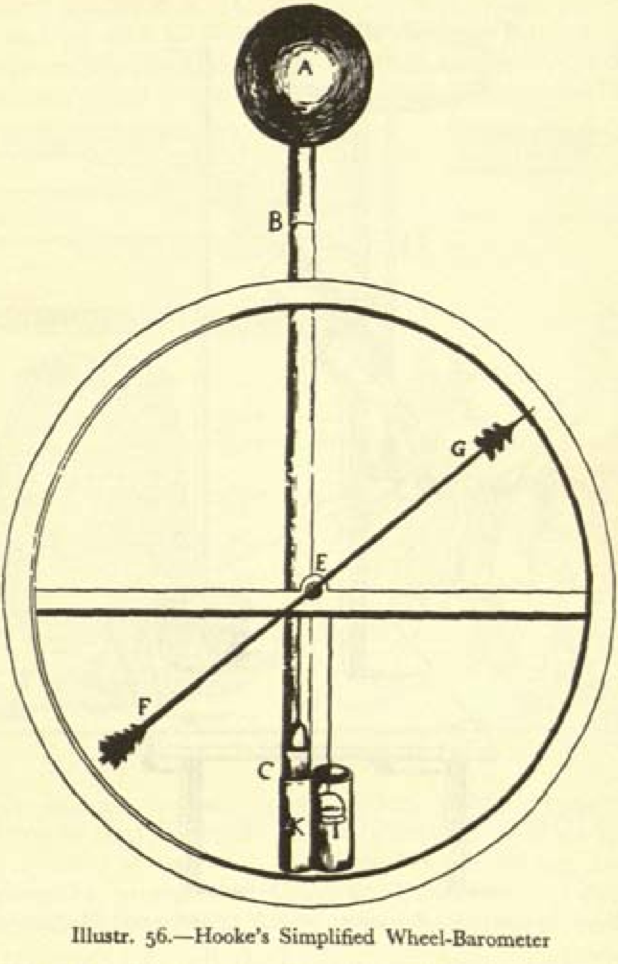
an opening at E and rising about 8 inches above that point. He attached the whole firmly to a board whose length he graduated in inches and tenths of an inch, starting from the line XY which was level with the centre of the bulb. He then sealed F with wax or cement, inverted the apparatus, and, by means of a funnel inserted in the opening at E, filled the bulb and tube with mercury, occasionally shaking the apparatus to detach air bubbles. He then sealed the opening at E, placed the instrument in an upright position, opened the end F of the tube, and, by means of a siphon, withdrew sufficient mercury from the open limb to make the level in the closed limb sink to XY. He then graduated the tube EF, or the adjoining wood, into divisions each corresponding to a change



Illustr. 55.—Hooke's Wheel-Barometer

of 1 inch in the difference of level of the mercury in the two limbs of the tube. He next fixed to the frame a graduated circle MNOP, at the centre of which was mounted a cylinder I capable of turning easily about its axis and carrying a light pointer KL over the graduated circle. Over this cylinder, whose circumference was twice the length of one of the divisions of the tube EF, was wound a silken thread with small steel weights at each end, the heavier of which rested on the mercury surface in the tube EF, while the other hung freely, "by means of which contrivance, every the least variation of the height of the Mercury will be made exceeding visible by the motion to and fro of the small Index KL."

Hooke later devised a method (described and depicted in *Phil.*

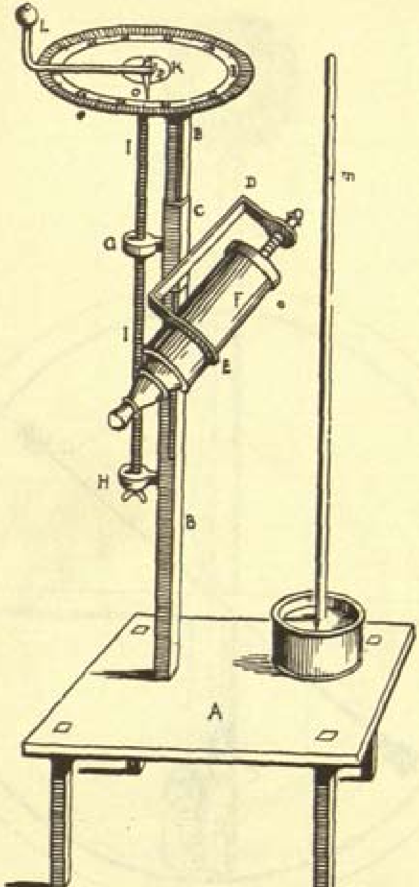


Illustr. 56.—Hooke's Simplified Wheel-Barometer

Trans., Vol. I, No. 13) for applying a pointer and scale to an ordinary barometer in which the tube stood in a trough of mercury (Illustr. 56). The pointer was worked as before by the rise and fall of a weight which, in this case, rested on the free surface of the mercury in the trough.

Stephen Gray in 1698 (*Phil. Trans.*, No. 237, p. 45) proposed to use a travelling microscope and micrometer screw for reading the level of the mercury with the utmost precision (Illustr. 57).

These and other more fantastic forms of barometer, however, have mostly gone out of use for scientific purposes, accuracy now



Illustr. 57.—Gray's Barometer with Microscope and Micrometer

being sought by refinement in methods of reading the instrument and in allowance for its various errors, such as those due to the thermal expansion of the mercury.

The earliest suggestion of the principle of the aneroid barometer, in which a fluid column is dispensed with altogether, appears to have been made by Leibniz in letters written to his friends about 1700.

The Air-Pump

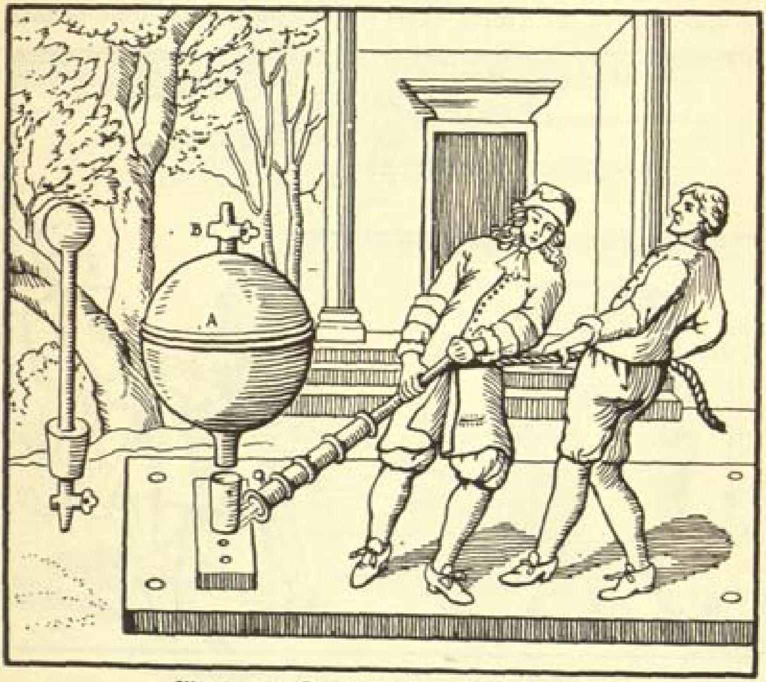
Of fundamental importance for the study of the physical properties of gases was the invention of the air-pump by Otto von Guericke, about the middle of the seventeenth century.

Guericke was born at Magdeburg in 1602, of an aristocratic family, and he died at Hamburg in 1686. After early studies in jurisprudence, he turned to mathematics and mechanics; and amid the upheavals of the Thirty Years War he spent much time helping

Illustr. 58.—Guericke's First Air-Pump

to fortify various German towns. When Tilly's troops sacked Magdeburg in 1631, Guericke narrowly escaped with his life, but later he returned to his native city, helped to rebuild and fortify it, and became its burgomaster. Guericke shared many of the usual philosophical notions of his time, and was moved to undertake his researches in pneumatics by controversies about the vacuum; but his work was remarkable for an emphasis on experiment, which was something new in Germany; and he was among those who prepared the way for the rise of experimental science in northern Europe.

The exact date of Guericke's invention of the air-pump is uncertain, but it cannot have been later than 1654, when he gave a public demonstration of its capabilities. There is some reason for supposing that the bulk of his researches may have been carried out between 1635 and 1645. The form of the instrument underwent a gradual evolution in his hands. The earlier types were of very simple design (see Illustr. 58). The first consisted of a cask well caulked with pitch and filled with water, which was evacuated by means of a brass pump having two valves. As the water was pumped

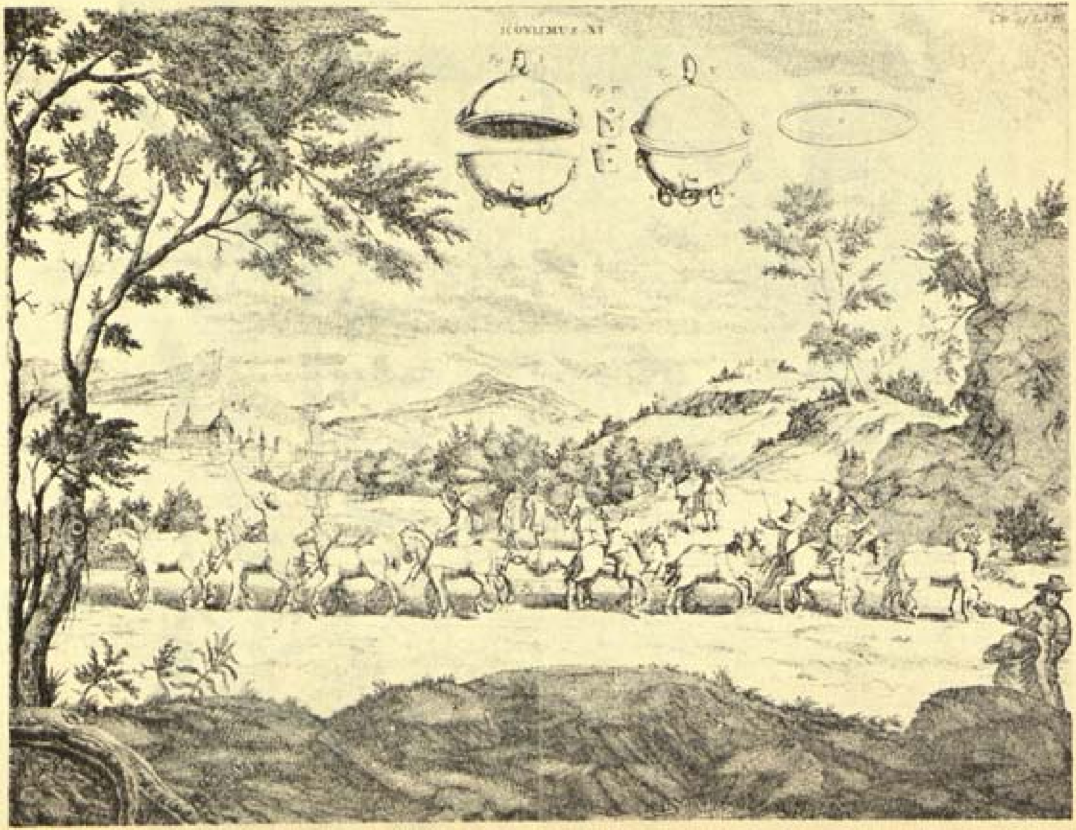


Illustr. 59.—Guericke's Second Air-Pump

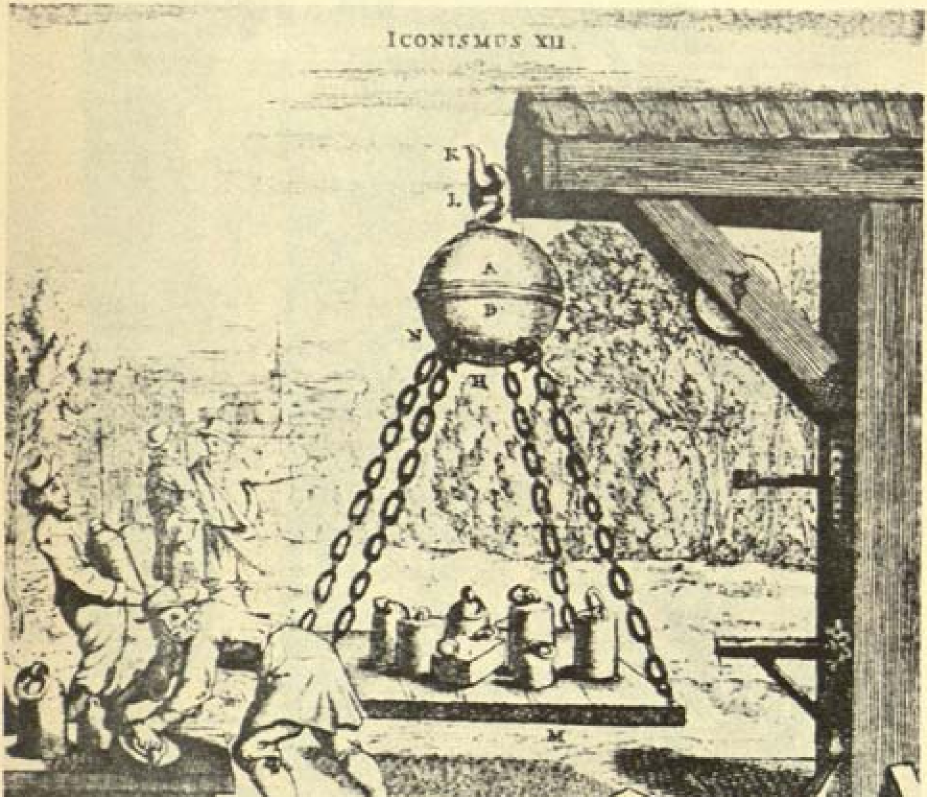
out, however, the air was heard rushing in through the pores of the wooden cask. A similar result was obtained when the cask was completely enclosed in a larger one, also containing water. Guericke accordingly gave up using wooden vessels, and attempted instead to evacuate a copper sphere from which he pumped out the air directly without previously filling it with water (see Illustr. 59).

The labour was heavy, and the sphere collapsed when a certain degree of exhaustion had been reached, owing, as Guericke realized, to the pressure of the external air, the vessel not having been made perfectly spherical. Guericke, however, had another copper sphere constructed free from this defect, and succeeded in obtaining fairly

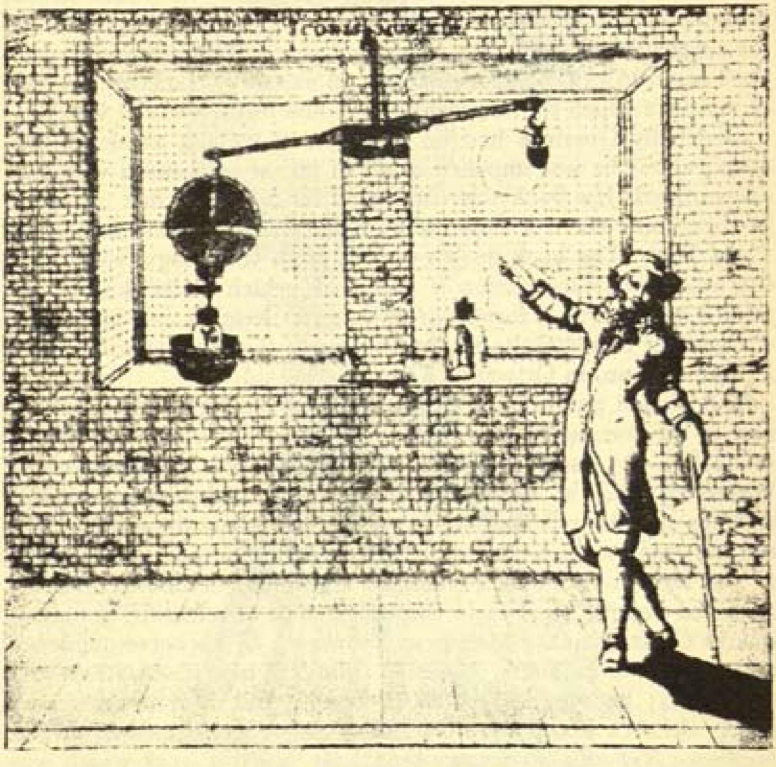
Illustr. 60



Guericke's Experiment with Magdeburg Hemispheres (1)



Guericke's Experiments with Magdeburg Hemispheres (2) high vacua. This must have been before 1654, for in that year he performed some striking pneumatic experiments before the Imperial Diet assembled at Ratisbon. The most impressive of these was that of the celebrated "Magdeburg Hemispheres." Two hollow bronze hemispheres were fitted carefully edge to edge, and the interior was evacuated through a stop-cock in one of them which was then



Illustr. 62.—Weight of Air

closed. A team of eight horses was harnessed to each hemisphere and the two teams were driven in opposite directions, but they were unable to pull the hemispheres asunder so long as the stop-cock was kept closed. (Illustr. 60.)

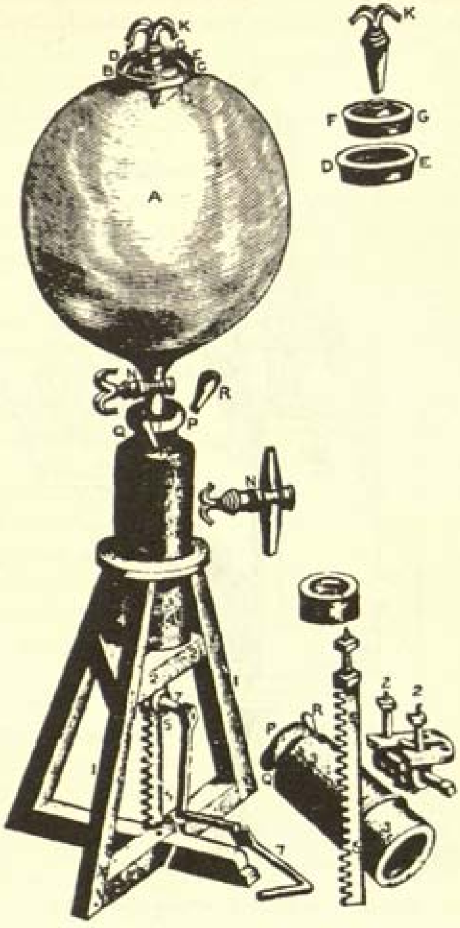
In another experiment it was shown, by means of weights, how much force was required to separate the two halves of such an evacuated sphere. (Illustr. 61.)

With his pumps Guericke performed many other interesting experiments in connection with problems which were later more thoroughly explored by Robert Boyle. By weighing a receiver on a balance before and after evacuation (see Illustr. 62) and showing that it weighed less when exhausted than when full, Guericke demonstrated that air has weight, and he arrived at a rough numerical estimate of the density of air. The fluctuations which he observed from day to day in the apparent weight of the exhausted receiver Guericke correctly attributed to small variations in the pressure of the atmosphere and in its Archimedean upthrust on the suspended receiver. He also noted that upon allowing air in a glass vessel to expand suddenly into an exhausted receiver, a cloud of moisture which showed rainbow colours formed in the vessel.

Originally Guericke had no intention of writing about his discoveries, but he was impelled to do so by the opposition which he encountered. His book was completed in 1663, but first appeared in 1672 under the title *Experimenta Nova (ut vocantur) Magdeburgica de Vacuo Spatio*. The book deals generally with cosmology, but by far the most important section is Book III, which bears the title *De propriis experimentis*. It forms one of the most weighty and instructive of the older monographs on physical topics (German translation by F. Dannemann, in Ostwald's *Klassiker*, No. 59).

The earliest published account, however, of Guericke's air-pump and of his pneumatic experiments was the work of Kaspar Schott (1608–66), a Jesuit Professor of Physics and Mathematics at Würzburg who, at Guericke's request, repeated his experiments with his air-pump. Schott was largely out of sympathy with the new experimental science, and never freed himself from the doctrine of the *horror vacui*, which Guericke vigorously combated. Yet he rendered definite services to the quickening of scientific investigation in Germany. Like Mersenne, he helped, by his correspondence with numerous inquirers, to spread tidings of new observations and discoveries; he suggested fresh problems, and kept controversies going. Schott's account of Guericke's researches appeared in his *Mechanica Hydraulico-Pneumatica* (1657). It was this work which was the means of stimulating Robert Boyle to have an air-pump constructed, as he had already long purposed to do.

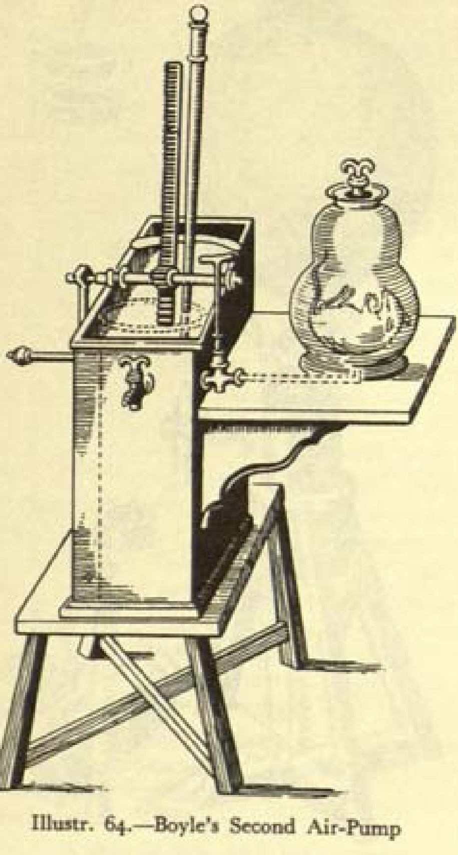
Boyle described this instrument, and his experiments with it, in his book *New Experiments Physico-Mechanicall touching the Spring of the Air* (Oxford, 1660). This air-pump was actually contrived and constructed, after several attempts, in 1658 or 1659, by Robert Hooke, as Boyle duly acknowledges. It marked an improvement on Guericke's model in several respects, e.g. the receiver could be evacuated with less labour, and contained an opening at the top through which objects could be let down, and which could then be closed with an air-tight stopper. The machine, shown in Illustr. 63, consisted essentially of a glass receiver, and a pump with which to exhaust it, the whole being supported on a wooden framework. The receiver had a stop-cock opening into the barrel of the pump.



Illustr. 63.—Boyle's First Air-Pump

The latter consisted of a brass cylinder, a piston in the form of a leather pad which fitted the cylinder closely and which was raised and lowered by means of a rack and pinion operated by a crank, and a valve consisting of a hole in the cylinder which could be stopped with a brass peg, or unstopped, at will. At the downward stroke of the pump the stop-cock was opened and the valve was closed, so that air was withdrawn from the receiver; at the upward stroke the stop-cock was closed and the valve opened, so that this air was expelled from the apparatus, and so on for successive strokes.

Boyle's second air-pump resembled the first, except that the cylinder was immersed in water and the glass receiver resting on a shelf at the side of the apparatus was evacuated by a pipe cemented

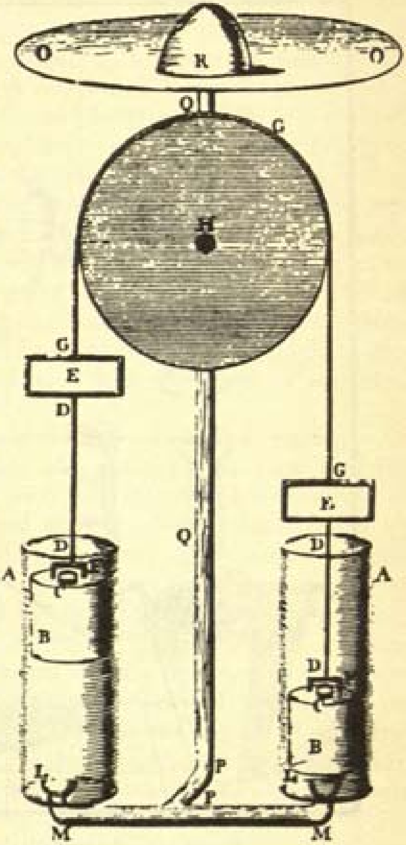


Illustr. 64.—Boyle's Second Air-Pump

into a groove in the shelf and with its orifice projecting upwards into the receiver to be evacuated. A stop-cock was arranged between the cylinder and the receiver.

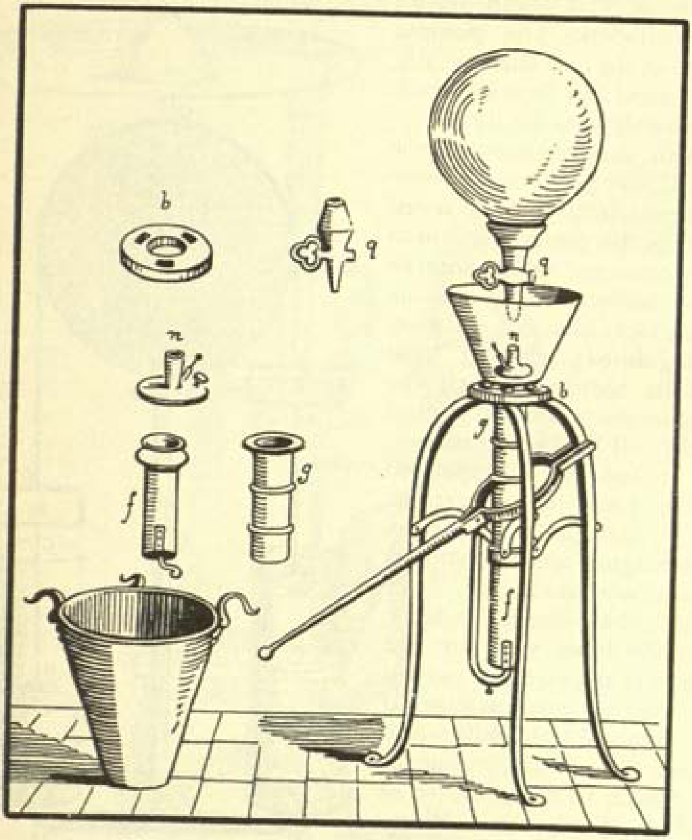
Boyle's third air-pump was double barrelled. BB are two hollow pistons, with two valves CC opening outwards to allow air to escape and prevent its re-entry. DDDD are connecting rods. GGG is a cord connected to the stirrups and passing over pulley H. LL, two valves at the bottom of the cylinders, opening inwardly to admit air from tube MM, which reaches via PPQQ to Plate O, on which the receivers, such as R, are placed, the plate O being bored in the middle. The engine was supported on a wooden frame; and water was poured through the orifice of Q in the plate O in a quantity just more than sufficient to fill the cylinders. The pumper stood in the iron stirrups, EE, and raised and depressed each alternately with his feet.

With this apparatus Boyle and Hooke performed numerous experiments. They noted that, in the partial vacuum of the receiver, small animals were suffocated: a candle flame went blue and was soon extinguished; glowing coal lost its redness, though the gunpowder priming of a pistol could still be discharged, and the sound of a suspended watch could no longer be heard, though the attraction of a magnet upon a compass-needle was unaffected. They found that closed bladders, partially filled with air and placed in the receiver, swelled and finally burst as the air was pumped out, while warm liquids broke into spontaneous ebullition. In the presence of Wallis, Ward, and Wren, Boyle proved experimentally that the column of mercury in the barometer is supported by atmospheric pressure. He did so by setting up a barometer in the receiver with its top projecting through the stopper, and noting the gradual fall of the column as the air was pumped out, and its re-ascent when air was once more admitted. By weighing in the exhausted receiver the air contained in a bladder, Boyle also arrived at a rough estimate of the density of the air. He noticed what appears to have been a luminous electrical discharge in the exhausted receiver, but was unable to account for it.



Illustr. 65.—Boyle's Third Air-Pump

Stimulated by Boyle's work, Guericke constructed an improved air-pump, shown in Illustr. 66. The instrument was mounted on a tripod screwed to the floor. The barrel *fg* of the pump was fixed at a convenient height between the legs of the tripod, and the

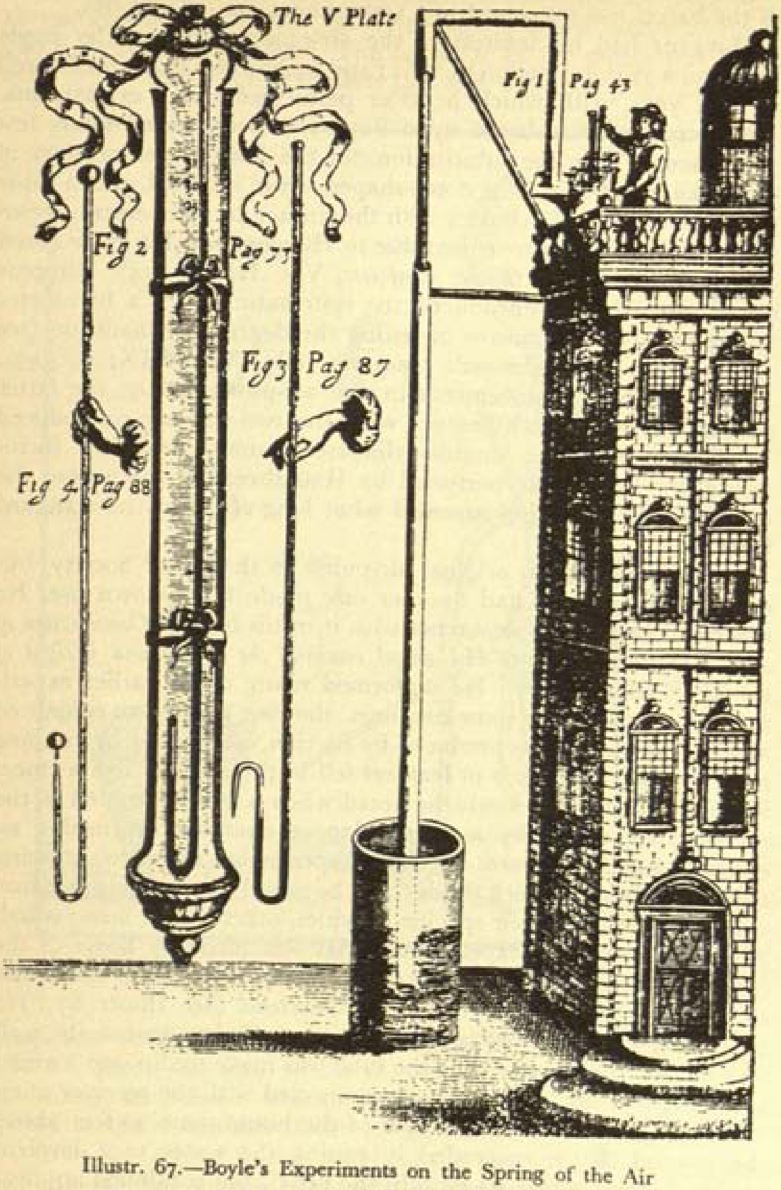


Illustr. 66.—Guericke's Improved Air-Pump

piston was worked by means of the lever. The barrel ended in a tube *n* (Illustr. 66) into which the conical end of the receiver was thrust, and under which was a leather valve. This valve opened at the downward stroke of the pump, and allowed air to pass from the receiver to the barrel; at the upward stroke it closed, and the air was expelled through the external valve. When the apparatus had been connected up and caulked, the funnel-shaped receptacle was filled with water to prevent as far as possible the re-entry of air into the receiver. With the same object the lower end of the barrel was immersed in a water-container (Illustr. 66).

Huygens had his interest in the air-pump awakened by Boyle while on a visit to London in 1661; and he had one made for himself in that year, with which he later performed many experiments. His instrument was based upon Boyle's, but contained many improvements. Thus the substitution, for the flask-shaped receiver of Guericke and Boyle, of a dome-shaped vessel inverted over a table and kept in air-tight contact with the surface by soft cement, seems to have been an improvement due to Huygens (see his letter dated December 21, 1661, *Œuvres complètes*, Vol. III, p. 414). Huygens seems also to have introduced the systematic use of a barometer in the receiver as a means of testing the degree of exhaustion (see E. Gerland in *Wiedemann's Annalen*, 1883, Vol. XIX, p. 549). Among further improvements in the air-pump during the latter part of the seventeenth century were the two-way tap, introduced by Papin; and the double-cylindered pump, probably introduced by Papin and perfected by Hauksbee, through whom the design of the air-pump assumed what long remained its standard form.

Boyle presented his original air-pump to the Royal Society, but some years later he had another one made for his own use. He described his further researches with it in his book *A Continuation of New Experiments Physico-Mechanical touching the Spring and Weight of the Air* (Oxford, 1669). He performed many of his earlier experiments again, and also some new ones, showing that, in an exhausted receiver heat could be produced by friction, and sparks by rubbing steel on sugar. A bunch of feathers fell in the receiver like a stone, while barely a sound could be heard when a bell suspended in the receiver was struck by a spring clapper operated by turning an external handle. Several of these experiments aimed at proving that the heights to which fluids could be raised by suction or pressure varied inversely as their specific gravities, other things being equal. Arising out of these experiments, trial was made by Boyle of the height to which water could be raised, with an apparatus somewhat similar to the water-barometer of Guericke. (See Illustr. 67.) He supported a tube whose upper section was of glass against the wall of a house. The lower end of the tube was made to dip into a vessel of water, and the upper end was connected with the receiver of an air-pump placed on the flat roof of the house some 30 feet above the ground. Boyle succeeded in raising the water to a level of 33 feet 6 inches above its level in the vessel, but continued application of the pump produced no further effect. By comparing the simultaneous heights of the columns in the water and mercury

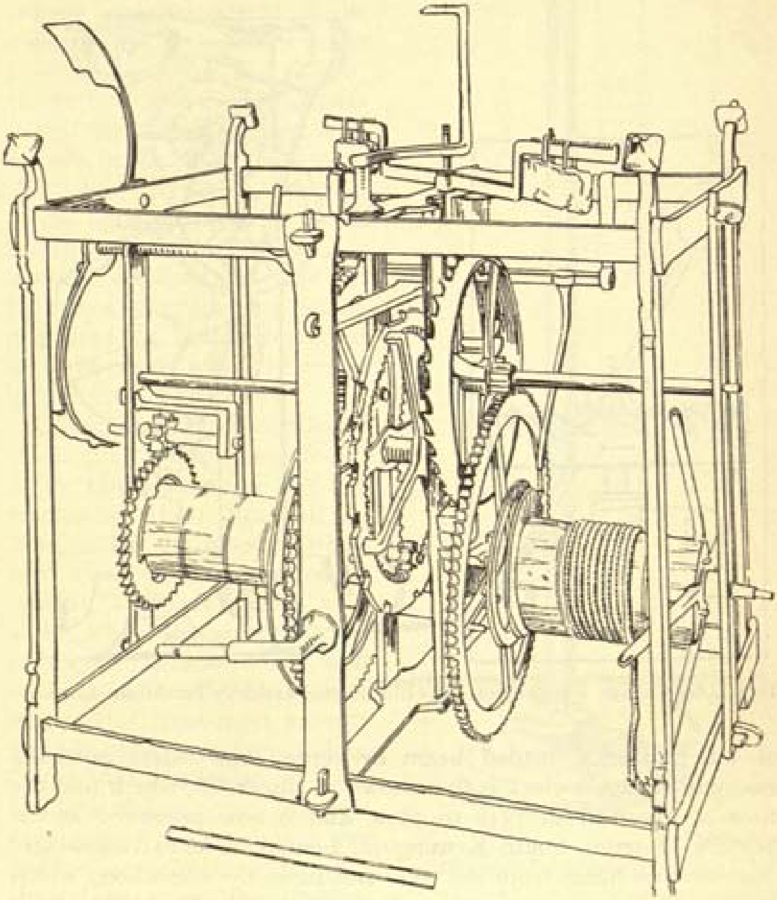


Illustr. 67.—Boyle's Experiments on the Spring of the Air

barometers respectively, Boyle obtained an improved value for the relative density of these two fluids.

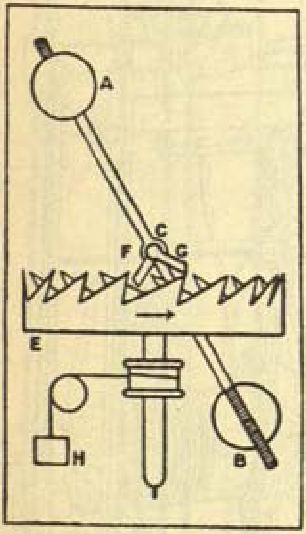
The Pendulum Clock

Various instruments for measuring time were used in antiquity and during the Middle Ages, and some of them have survived, if

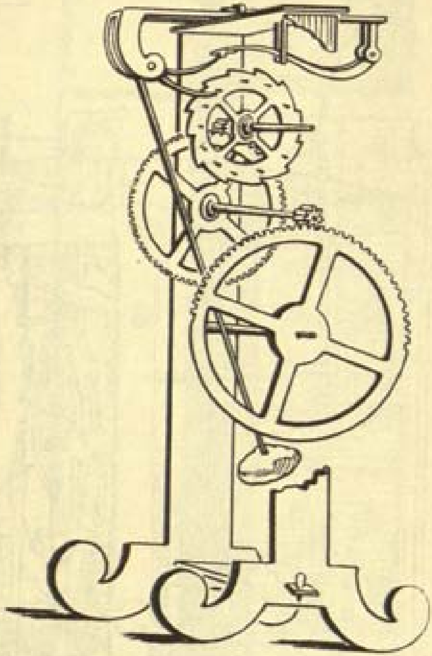


Illustr. 68.—The Dover Clock (1348)

only as ornaments or toys, to the present day. People are still more or less familiar with sun-dials (or shadow-clocks), clepsydras (or water-clocks), and sand-glasses. Burning wax candles, or oil-lamps, with scales attached, were likewise used as measures of fleeting time. During the later Middle Ages crude wheel-clocks came into use. Such clocks, driven by weights, appear to have been used in certain monasteries as early as the eleventh century. In the course of the thirteenth century the practice arose of placing such clocks in the steeples of important churches; and the practice became fairly common in the course of the fourteenth century. These clocks were regulated either by means of wind-vanes or by means

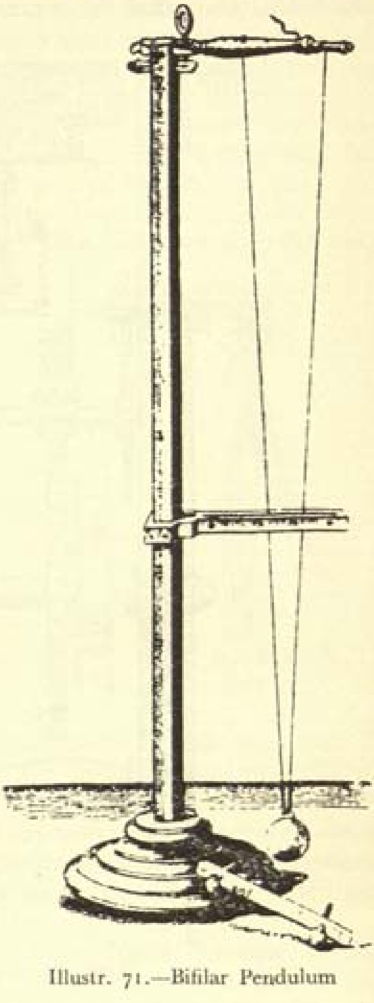


Illustr. 69.—Verge Escapement



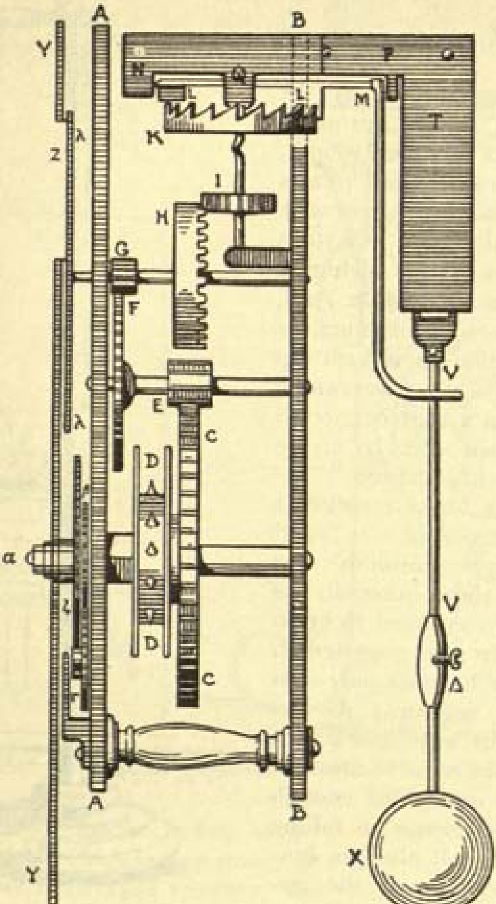
Illustr. 70.—Galilei's Pendulum Clock

of an horizontal loaded beam or verge. The oldest surviving example of such a clock is that shown in Illustr. 68, which told the time at Dover from 1348 to 1872, and is now preserved at the Science Museum, South Kensington, London. The driving-weight (not shown) hangs from the rope and turns the cog-wheel, which engages and sets in motion the adjoining cog-wheel, whose teeth in turn engage the vertical axis of a horizontal pendulum. The latter is set in motion by impulses communicated through two plates on its axis, which engage the cogs of the second wheel at diametrically opposite points. The frequency of the oscillations is controlled by means of sliding weights. The next diagram (Illustr. 69) gives a clearer idea of the mechanism of such clocks with a verge escapement. AB is a rod (called the "verge") having a weight attached to each end. It is fixed at right angles to a horizontal axis C, which is mounted on pivots close to a horizontal "crown" wheel, E. On the axis C are mounted two "pallets," F and G, so as to engage opposite teeth of the crown wheel, E, which is made to rotate by a cord and weight, H. As the crown wheel rotates one of the pallets engages with a tooth in the wheel. This stops the verge and sets it swinging in the opposite direction. And, as the process continues, a periodic oscillation is kept up by means of which time can be measured on a dial connected with the crown wheel by means of "wheels within wheels."



Illustr. 71.—Bifilar Pendulum

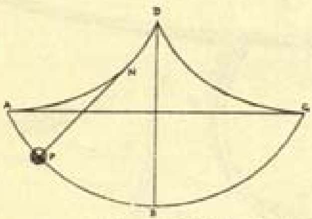
Of all the above-mentioned clocks the clepsydra was found to be the least unsuitable for measuring short intervals of time, and it continued to be so used even in the seventeenth century. We have already seen with what ingenuity Galilei combined the water-clock with the balance in order to measure intervals of time short enough for his experiments on falling bodies. We shall also see how Galilei's discovery of the isochronism of the pendulum was applied to the construction of a clinical instrument (the pulsilogy or pulsimeter) for measuring the rate of a patient's pulse (see page 433). It consisted of a pendulum suspended by a thread which could be shortened or lengthened until the frequency of the vibration of the pendulum was the same as the rate of the pulse; and an index of arbitrary units made it possible to make useful comparisons for medical purposes. Moreover, as has already been explained, Galilei near the close of his life devised a means of measuring time by means of a pendulum which was to be kept in motion by impulses automatically administered, and was to record the number of oscillations on a dial, by means of clock-work. He explained his



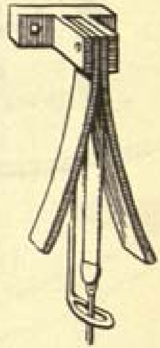
Illustr. 72.—Huygens' Clock

project to his son, Vincenzio, and his disciple, Viviani, and they made a drawing upon which Illustr. 70 is based. Vincenzio, however, died before he could complete his father's plan, and the task of inventing the pendulum clock fell to the lot of Christian Huygens, though the Accademia del Cimento made an important contribution by the invention of the bifilar pendulum (Illustr. 71).

Huygens patented his clock in 1657. A full description of it is given in his *Horologium Oscillatorium* (Paris, 1673), which deals besides with numerous mechanical problems which arose out of his investigations of pendular motion. Huygens' clock is shown in section in Illustr. 72, which is taken from his book. Like the earlier clocks it is driven by a descending weight supported by a cord which is wound on the drum D. The pull of the weight drives the clock and keeps the pendulum in motion by administering to it periodic, momentary impulses through an escapement. The pendulum, on its part, regulates the descent of the weight and the motion of the hands. The essential part of the instrument is the horizontal escapement-wheel, K, whose teeth alternately act upon the two pallets, L, L, of a horizontal axis connected with the pen-



Illustr. 73.—The Cycloidal Oscillation of a Pendulum



Illustr. 74.—Cycloidal Jaws

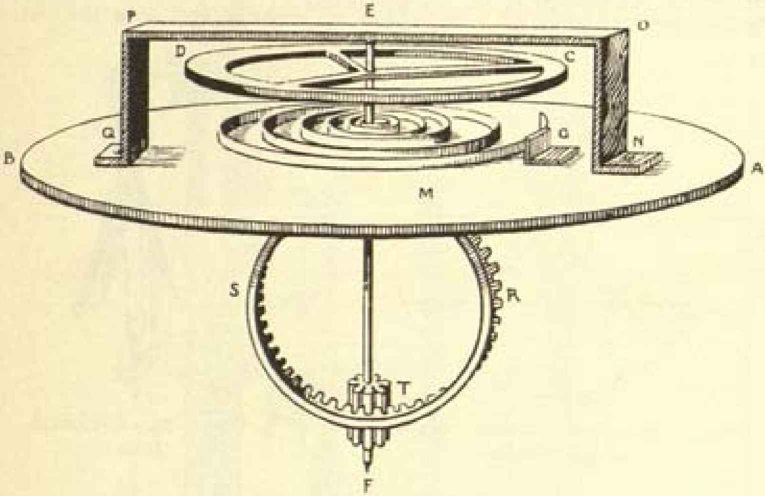
dulum. One of Huygens' pendulum clocks is still preserved in Leiden University, but it is not his first one, as is sometimes alleged to be the case.

The anchor-escapement now in common use was invented somewhat later. It was introduced into the art of clockmaking by Clement, a London horologist, in 1680, but had previously been described, and perhaps invented, by Robert Hooke.

Among the numerous mechanical problems discussed in the *Horologium Oscillatorium* is that of constructing an accurately isochronous simple pendulum, which Huygens solved by making the suspending thread wrap itself alternately about two cycloidal jaws (Illustr. 74). Under these conditions the bob itself describes a cycloid, which Huygens showed to be an isochronous curve, i.e. the bob always reaches the lowest point B of its arc in the same time, from whatever point, P, between A and B it starts. Huygens' application of this principle to his clock is shown in Illustr. 72;

but the device was soon rendered unnecessary by the introduction of the anchor-escapement and by the use of small impulses. Huygens was also an independent inventor of the balance-spring for watches (Illustr. 75). He published his book on the pendulum clock at Paris, and we shall shortly see the important use which Picard made of his colleague's invention at the Paris Observatory.

At one time there raged a controversy between the followers of Galilei and the friends of Huygens concerning the priority in the invention of the pendulum clock. There is no doubt, however, that Huygens' invention was made independently of Galilei's, from

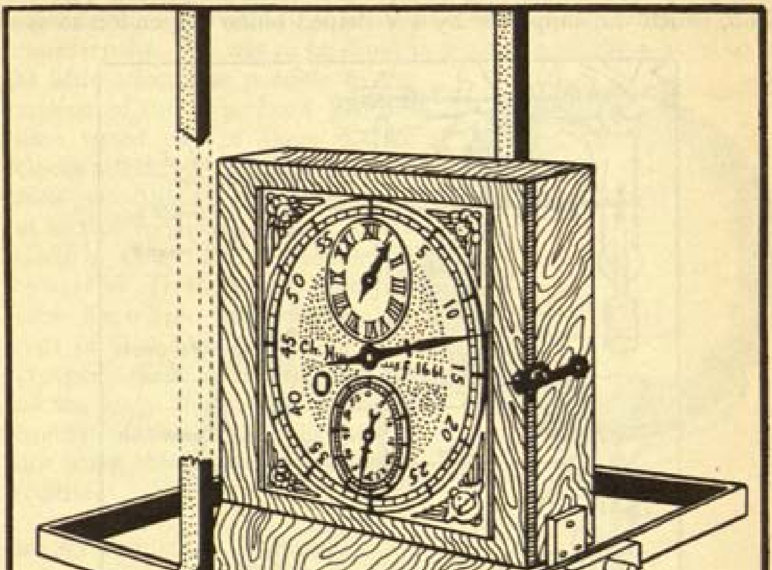


Illustr. 75.—Huygens' Balance Spring

which it differs essentially in principle. While Galilei had attempted to apply clockwork to the pendulum, Huygens applied the pendulum to the existing form of the clock, in place of the old balance.

Marine Instruments

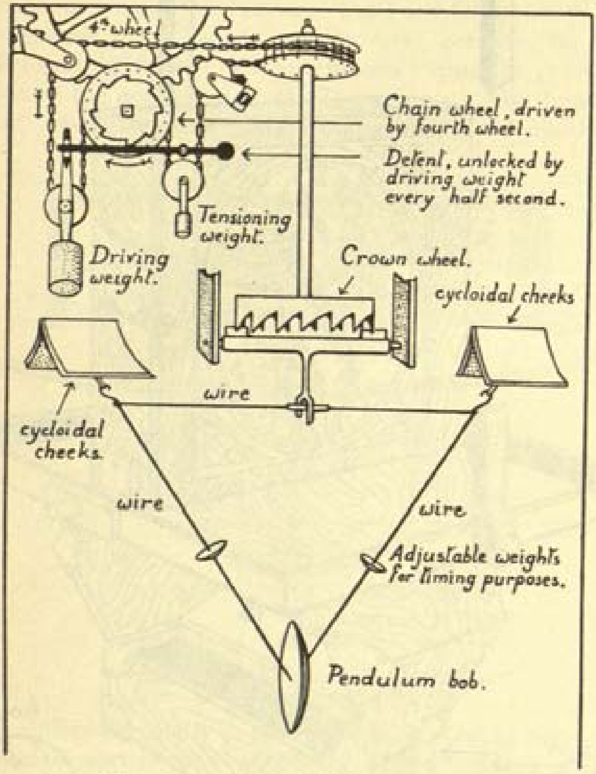
The seventeenth century witnessed the invention or the introduction of a number of scientific instruments for special use at sea. The most important of these were Huygens' marine clock, that is, his pendulum clock especially adapted for use on ships; a new kind of sounding instrument, invented by Hooke, for ascertaining the depth of the sea without the use of a line; and another instrument, also invented by Hooke, for procuring samples of sea-water from any desired depth. Other scientific instruments, such as magnetic dipping needles, wind gauges, and hydrometers, also came gradually to be regarded as part of the regular equipment of ships going on distant sea voyages.



Illustr. 76.—Huygens' Marine Clock (1)

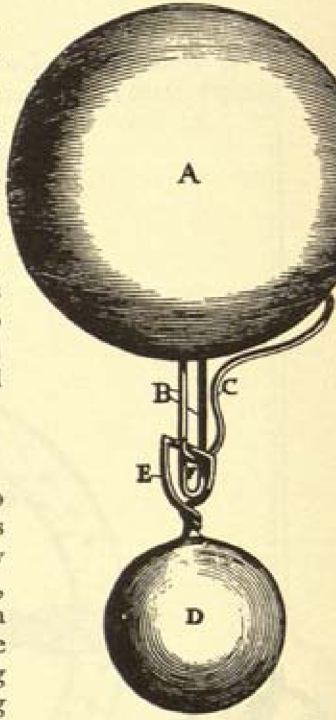
HUYGENS' MARINE CLOCK

About 1659 Huygens designed a marine clock, i.e. a clock intended to show standard time at sea for the purpose of determining longitude, and within a year or two several of these instruments had been constructed. In this work Huygens had the assistance of Alexander Bruce, Earl of Kincardine, who had taken refuge in Holland for political reasons. The marine clock was regulated by a short pendulum beating half seconds (Illustrs. 76 and 77). The bob, which was supported by a V-shaped bifilar suspension so as to



Illustr. 77.—Huygens' Marine Clock (2)

swing in one plane, can be seen in the figure above the base of the instrument, together with the movable weight on one of the suspending cords, which could be raised or lowered so as to regulate the rate of the clock. The cords wrapped themselves at each swing on cycloidal cheeks so that the bob performed strictly isochronous vibrations. The clock was kept in motion by a coiled spring which was regulated by the pendulum through a verge escapement. The crown-wheel, which was horizontal, was not moved by the direct action of the spring, but by impulses from a periodically falling weight which was wound up by the spring between successive impulses. This so-called *remontoire*, forms of which seem to have been known from the beginning of the seventeenth century, received manifold developments in the subsequent history of clockmaking. Its function was to maintain the driving force at a practically constant value. The instrument was weighted below by a heavy leaden counterpoise, and was to be slung in gimbals amidships, so as to be as little affected as possible by the motion of the ship. Lord Kincardine tested two of these marine clocks at sea, and was satisfied with their performance. They also proved of service to an expedition to the Guinea Coast under Holmes in 1664 (*Phil. Trans.*, Vol. I). But they must have been unsatisfactory except in very calm weather. Later, Huygens made some experiments on the use of the balance spring to control marine clocks, but he did not bring this scheme to practical fruition.



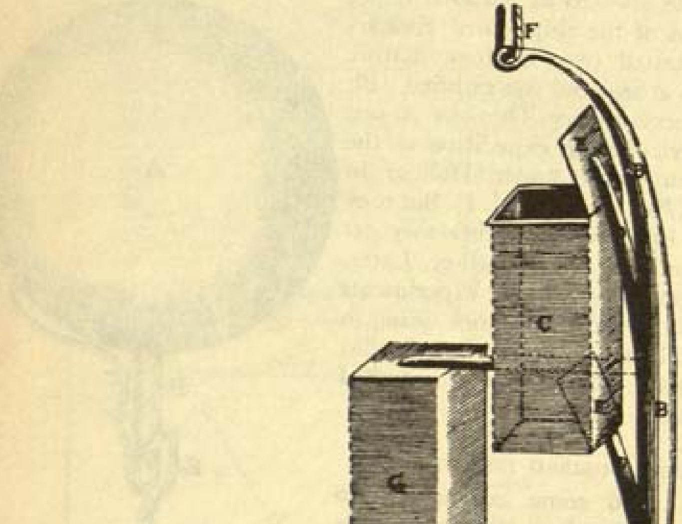
Illustr. 78.—Hooke's Sounding Instrument

HOOKE'S SOUNDING INSTRUMENT

In 1666 some instructions to seamen bound on distant voyages were drawn up by the Royal Society (*Phil. Trans.*, 1666, No. 9, and 1667, No. 24). These instructions contain a description of a contrivance invented by Hooke for estimating the depth of the sea without using a line. It consisted of a ball of light wood A, and a mass of lead or stone, D, sufficiently heavy to sink the first in water (Illustr. 78). The ball D was hung from A on the end, F, of a spring which was kept bent by interposing the ring E between the end of the spring and the staple B. The whole was to be allowed to sink in the sea where the depth was to be measured, and the shock of D upon the bottom would release the spring C, so that the ball A could rise to the surface. The time between the immersion of the apparatus and the reappearance of the wooden ball at the surface was to be measured by a watch, minute-glass, or seconds pendulum, and would enable the depth of the sea to be estimated when tables had been constructed on the basis of observations with the instrument at known depths. To this end trial was made of the apparatus in the Thames.

HOOKE'S SEA-WATER SAMPLER

Another instrument was proposed by Hooke for bringing up samples of water from any desired depth of the sea (Illustr. 79).

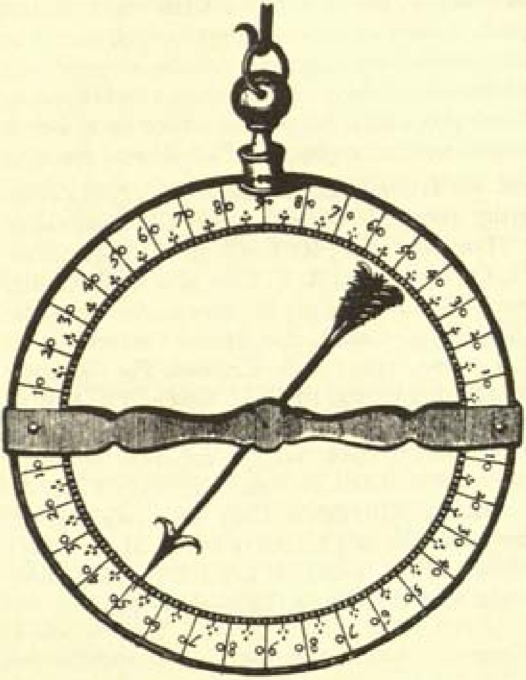


Illustr. 79.—Hooke's Water-Sampler

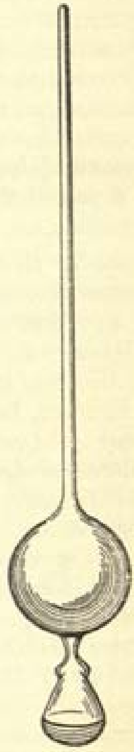
As the apparatus sank in the sea, the resistance of the water kept open the ends, E, E, of the box C, but when the apparatus was pulled up by the line F the box sank to the position G, where it was supported by the handles D, D, to the ends of which the doors E, E, were fixed. In this position the box was closed, and water could neither enter nor leave it. Thus it was possible to procure a specimen of the water at the greatest depth to which the apparatus was lowered. Various forms of sinking weights were to be employed for different depths.

DIPPING NEEDLES, ETC.

In the course of further instructions to sailors (*Phil. Trans.*, Vol. II, No. 24), they were recommended to carry dipping needles



Illustr. 80.—Dipping Needle



Illustr. 81.—Boyle's Hydrometer

(Illustr. 80) for measuring the angle of dip of a magnetic needle suspended at its centre of gravity, and free to turn in the magnetic meridian. They were to keep a register of the direction and strength of the wind at all places where they might happen to be. For obtaining numerical measurements of the strength of the wind, Hooke's apparatus, shown in Illustr. 176 (p. 309), was recommended. It worked somewhat like the drogue in a modern aerodrome. A flat board swung freely by an arm which travelled over a graduated dial as the board was blown outward by the wind, whose strength was thus measured in arbitrary units. The dial acted as a wind-vane to keep the board facing the wind.

Measured volumes of the sea-water at various places were to be weighed and the specific gravities compared, account being taken of temperature. The water could then be evaporated to dryness so that the salt could be weighed and the salinity estimated. A hydrometer was also recommended for comparing the specific gravities of various samples of sea-water. As improved by Boyle (Illustr. 81), this instrument consisted of a sealed, bulbous tube containing so much mercury as just to float in fresh water. When immersed in other liquids it sank to various depths which could be compared by means of graduations engraved on the stem with a diamond. (Hooke's instruments, above described, for ascertaining the depth of the sea, and procuring samples of water from below the surface, are again explained in this paper in *Phil. Trans.*, No. 24.)

(See E. Gerland and F. Traumüller, *Geschichte der physikalischen Experimentierkunst*, Leipzig, 1899; C. Dobell, *Antony van Leeuwenhoek and his "Little Animals,"* London, 1932; R. T. Gunther, *Early Science in Oxford*, vols. I and II, Oxford, 1923; R. S. Clay and T. H. Court, *The History of the Microscope*, London, 1932; H. Servus, *Die Geschichte des Fernrohrs*, Berlin, 1886; R. T. Gould, *The Marine Chronometer, its History and Development*, London, 1923; J. A. Repsold, *Zur Geschichte der astronomischen Messwerkzeuge*, Leipzig, 1908; E. Rosen, *The Naming of the Telescope*, New York, 1947; F. S. Taylor, "The Origin of the Thermometer," *Annals of Science*, 1942, vol. 5, 129–156. Some of Hooke's works have been republished in vols. VI, VII, VIII, X and XIII (1930–38) of R. T. Gunther's *Early Science in Oxford*, together with an account of his life and his diary from 1688 to 1693: see also *The Diary of Robert Hooke, M.A., M.D., F.R.S., 1672–1680*, edited by H. W. Robinson and W. Adams, London, 1935.)

CHAPTER VI

THE PROGRESS OF ASTRONOMY: TYCHO BRAHE AND KEPLER

The Copernican theory of the solar system chiefly commended itself to astronomers by the improved planetary tables which accompanied it. The original tables which Copernicus himself had computed were revised and enlarged some years after his death by Erasmus Reinhold, who called his edition *Tabulae Prutenicae* (1551) in honour of his patron the Duke of Prussia. But the observational data at the disposal of Copernicus and Reinhold were few and questionable, and the tables based upon them were far from giving an accurate representation of the actual motions of the planets. It was clear that little progress could be made towards securing correct tables until a stock of accurate and systematic observations of the planets had been obtained. Accordingly the history of astronomy in the latter half of the sixteenth century is concerned chiefly with the attempts that were made to meet this need. The outstanding figure of the period was the Danish astronomer Tycho Brahe, who recognized the need of the time most clearly and did most to supply it.

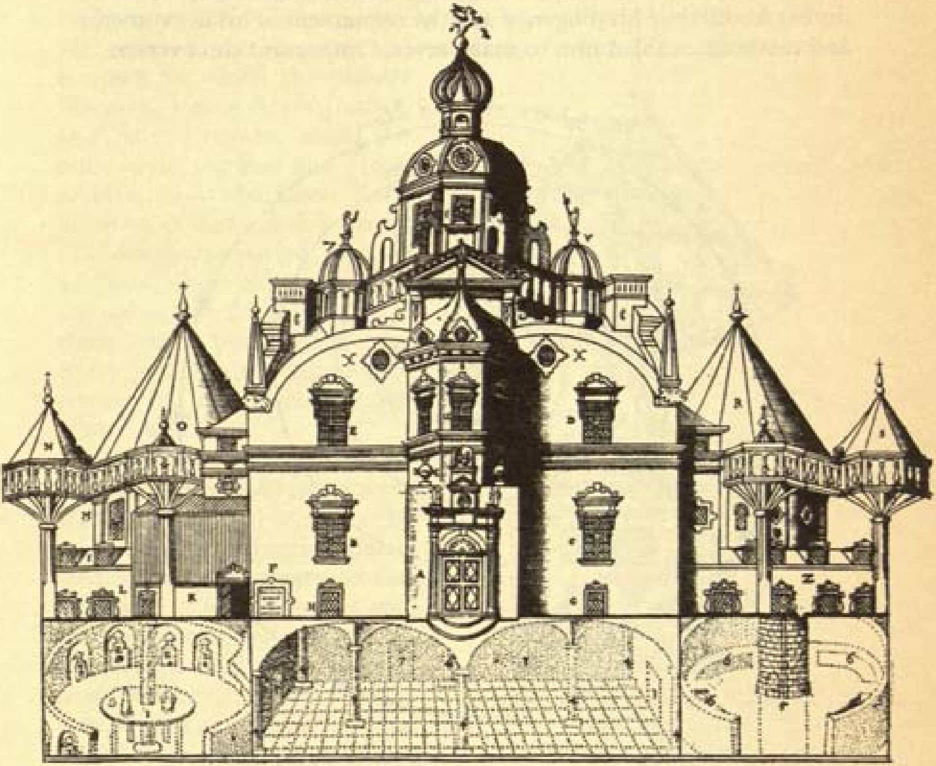
The Life of Tycho Brahe

Tycho Brahe was born on December 14, 1546, at Knudstrup in Scania (now in southern Sweden, but then a part of Denmark). He was the son of a Danish nobleman, and went as a boy to Copenhagen University. While he was there the occurrence of a solar eclipse at its predicted time awakened his curiosity, and turned his attention towards astronomy. He neglected his regular studies, secured and read the works of Ptolemy, and made his first recorded astronomical observation in 1563, on the occasion of a conjunction of the planets Jupiter and Saturn. Even the crude, home-made instruments at his disposal sufficed to reveal to Tycho the serious discrepancies between the positions of the planets as calculated from the Prussian or other tables and the positions actually observed. He seems already to have recognized the necessity of basing planetary tables upon a prolonged series of systematic and accurate observations.

After leaving Copenhagen, Tycho studied successively at the Universities of Leipzig, Wittenberg, Rostock, and Basle, everywhere seeking out the leading teachers of mathematics and astronomy, making observations from time to time, and occasionally producing astrological prognostications. In 1570 Tycho returned to Denmark, where he seems to have devoted himself for some time to chemical researches. But his interests were soon recalled to astronomy by the appearance of a remarkable new star, in the constellation Cassiopeia, in November 1572. This phenomenon remained visible for some eighteen months, during which Tycho repeatedly measured its angular distances from neighbouring stars by means of a home-made sextant. From these data he was able to draw an important conclusion, as will be related in due course. He traced the changes in the brightness and colour of the star throughout its period of visibility, and published an account of it, *De Nova Stella*, in 1573.

Shortly afterwards, in the course of a tour through Europe, Tycho visited the Landgrave Wilhelm IV of Hesse, a keen astronomer who had built an observatory with a movable roof at Cassel, and was already employing a crude type of clock. At the request of the Landgrave Wilhelm, the King of Denmark, Frederick II, decided in 1576 to extend his patronage to Tycho, who was offered a pension and the island of Hveen, in the Sound between Copenhagen and Elsinore, as a site for an observatory. Tycho accepted the offer, and built at Hveen a castle and observatory which he called Uraniborg (the Tower of Heaven). Surrounded by gardens, and sumptuously furnished, it comprised, besides observing apartments, a workshop in which nearly all the instruments were constructed, a library, a chemical laboratory, printing offices, etc. Besides his annual pension from the King, Tycho received the incomes from a number of farms and estates, and from a prebend of Roskilde Cathedral, and occasional lump sums to pay the debts into which he ran from time to time through his lavish expenditure. Aided by his children and by a band of assistants, Tycho continued observing at Hveen from 1576 to 1597.

In 1588, however, the King of Denmark died, and Protectors were elected to govern the country during the minority of the young Prince. Soon afterwards Tycho began to lose favour with the Court, which had never taken much interest in his work. He appears to have been tactless in his dealings with the nobles, extravagant with money, and oppressive towards his tenants, and he neglected certain obligations attached to his prebend of Roskilde. In consequence he was gradually deprived of his emoluments, and in 1597 he left Uraniborg with his family. After a short stay in Copenhagen he went to visit a German nobleman near Hamburg, where he wrote an account of his life, and of his instruments and methods, *Astronomiae instauratae Mechanica*, which appeared in 1598. In the same year Tycho was invited to Prague by the German Emperor Rudolph II, who in 1599 granted him a pension and installed him in a castle near the city, which Tycho fitted up as an observatory. While waiting for his instruments and books to be brought to him from Hveen, Tycho looked about for assistants to aid him in his

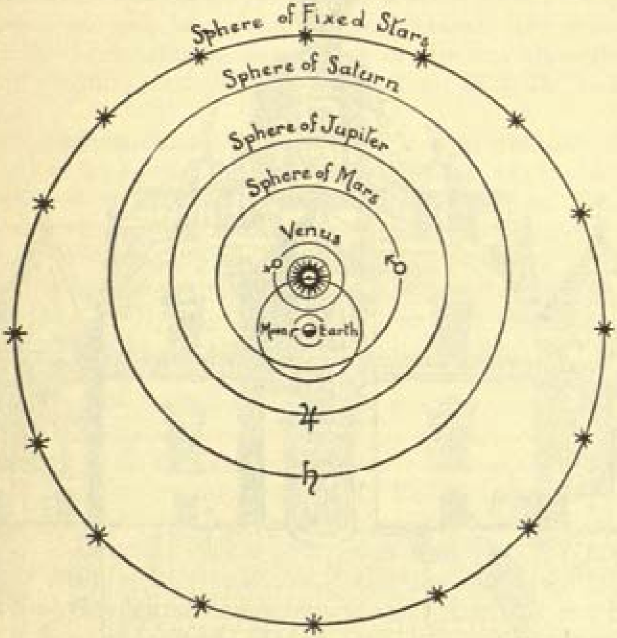


Illustr. 82.—Tycho Brahe's Observatory, Uraniborg

future researches. It was at this stage that he was joined by Johann Kepler, a young German astronomer. Kepler had attracted Tycho's notice by sending him a copy of his book, *Mysterium Cosmographicum*, which had been published in 1596. Early in 1600 Kepler arrived on a visit to Tycho, who engaged him as an assistant. Soon after this Tycho moved into Prague so as to be near the Emperor, and for a brief space resumed his observations. But before he could settle down to systematic work he was overtaken by sudden illness, and died on October 24, 1601.

Tycho Brahe's Contributions to Astronomy

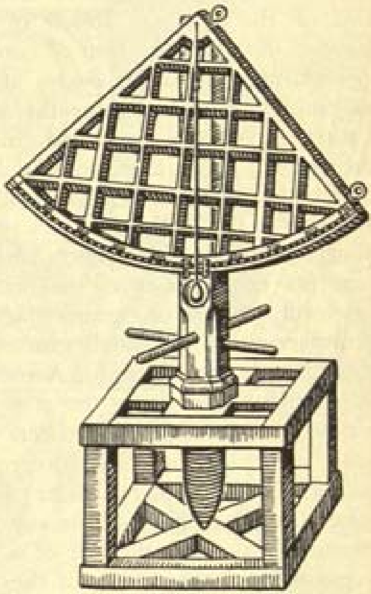
There were not many problems in precise astronomy which Tycho Brahe did not attack, and few important astronomical constants which he did not determine with an accuracy not before attained. His work was destined to bear its richest fruit after his death, but even during his lifetime his diligence and the refinement of his instruments and methods enabled him to make several important discoveries.



Illustr. 83.—The Universe according to Tycho Brahe

The earliest of his discoveries arose out of his observations of the new star in Cassiopeia, already alluded to. He was able to show that the new star showed no appreciable daily shift (parallax) relatively to the surrounding stars, as it would have done had it been as near to us as the Moon. Nor had it any proper motion such as a planet possesses. He concluded that the new star must belong to the region of the fixed stars—a region in which, according to the accepted Aristotelian cosmology of the time, no physical changes could occur. Tycho was later led to a somewhat similar conclusion concerning comets. He observed a series of these celestial bodies, beginning with the great comet of 1577, and was able to prove that they showed no sensible daily parallax, and hence must be considerably more remote than the Moon. His account of the comet of 1577 appeared in 1588 as *De Mundi aetherii recentioribus phaenomenis*. This was intended to be a part of a larger work, *Astronomiae instauratae Progymnasmata*, which was never completed.

It was in this volume of 1588 (Chap. VIII) that Tycho also outlined his theory of the solar system, hoping later to develop it in detail. He rejected the Copernican system in favour of a scheme of his own in which the planets Mercury, Venus, Mars, Jupiter, and Saturn revolve about the Sun, while the Sun and Moon revolve about the Earth. Each of these bodies revolves in its own distinctive period, and, in addition, they all share with the sphere of fixed stars in a daily revolution about the Earth, which is the immovable centre of the universe (see Illustr. 83).



Illustr. 84.—Tycho Brahe's Giant Quadrant

Tycho's reasons for abandoning the Copernican system were that the alleged motion of the heavy and sluggish Earth seemed to be contrary to the principles of sound physics and to the words of Scripture. Further, it had been known from antiquity that a revolution of the Earth about the Sun would necessarily produce an annual parallactic shift in the apparent positions of the stars. No one had ever observed such a shift; Tycho himself could not detect it; and hence, if it existed, it must be so small as to remove the stars to an incredible distance from the Earth. It was not, however, as a theorist but primarily as an observer that Tycho Brahe rendered the greatest services to astronomy; and to his observations we must now turn.

Tycho's earliest observations were made with such crude and portable instruments as were then in use among navigators. A considerable advance on these, however, was shown by a giant quadrant which he designed for the burgomaster of Augsburg, while on a visit to the city in 1569. This instrument, shown in Illustr. 84, was about 19 feet in radius, the framework being of wood, and the graduated limb of brass. The quadrant could be turned by levers about a vertical axis, and also, in its own plane, about its centre, so that the two sights (shown on the right-hand radius) could be set upon any celestial object above the horizon. The altitude of the object was read, to fractions of a minute, off the graduated scale with the aid of a plumb-line.

The instruments later constructed and employed by Tycho Brahe at Uraniborg were of several different types. Some were based on the armillary sphere of the ancient astronomers, which consisted of a combination of concentric, graduated metal circles representing the various circles of the celestial sphere, and which was employed to determine the celestial longitudes and latitudes of stars, etc. Tycho modified this type of instrument so that it gave instead right ascensions and declinations, and in so doing he reduced the number of circles required, and increased the symmetry of the instrument.

There were also in Tycho's observatory a number of other instruments each consisting essentially of some sector of a circle (quadrant, sextant, or octant) made of wood or metal, having at its circumference an accurately graduated metal rim and, at its centre, an accurately located sight. A movable sight was provided which either slid up and down the graduated metal arc, or was carried over it by a radial arm or *alidade* turning about the centre of the sector. These instruments were mounted on ball-and-socket supports, and so could be set in the plane determined by the observer's eye, and any two stars whose angular separation was required. To determine this angle the line of sights was directed to each star in succession, and the setting of the movable sight was in each case read off the graduated limb, when the difference of the two readings would give the angular separation of the stars. Sometimes two movable sights or *alidades* were available, when the two settings could be made simultaneously by two observers, and errors due to the diurnal motion of the stars could be thus avoided. Such a sector could be set up in the plane of the meridian with one end of the graduated arc vertically below or above the central sight, and could then be used to measure the altitudes at which celestial objects crossed the meridian. This latter type of instrument was exemplified in Tycho's celebrated Mural Quadrant (Illustr. 85).

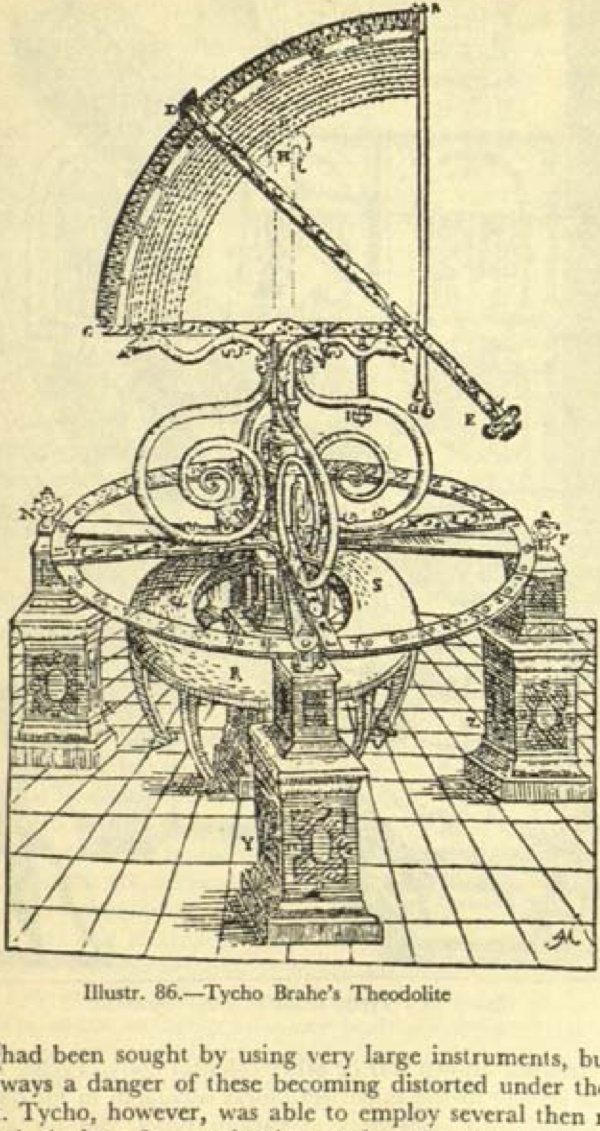
Tycho also constructed quadrants which could be rotated into any vertical plane so as to measure both the azimuth and the altitude of any celestial object in that plane. One of these instruments, to which the modern theodolite is analogous, is shown in Illustr. 86.

Such observations were the more important as, in the absence of clocks, the time had frequently to be deduced from the observed

Illustr. 85.—Tycho Brahe's Mural Quadrant

altitude of some known star or other celestial body. Tycho, indeed, possessed several of the imperfect clocks available in his day, and experimented with clepsydras of various types. But for accurate work he relied almost exclusively on the measurement of angular magnitude by such instruments as we have mentioned.

Previously to Tycho, increased refinement in the graduation of



Illustr. 86.—Tycho Brahe's Theodolite

scales had been sought by using very large instruments, but there was always a danger of these becoming distorted under their own weight. Tycho, however, was able to employ several then recently invented devices for graduating scales. Among these was the

Nonius, so called after its inventor, Nunez, in which the metal limb of the sector was engraved with a number of concentric arcs each divided into a different number of equal parts. The setting of the *alidade* could, in theory, be read to minutes and seconds by noting the division with which the fiducial line coincided, and the particular circle upon which that division lay. But in practice the graduation of instruments in this manner presented great difficulties, and Tycho ultimately adopted the "method of transversals" which is familiar to us as the basis of the diagonal scale. The *vernier* now used so extensively in physical and astronomical instruments was not described until 1631. Tycho also made important improvements in the construction of the sights by means of which the *alidade* of an instrument was directed towards a star.

Aided by such devices, by the excellent workmanship of his instruments, and by his own acuity of vision, Tycho developed extraordinary accuracy of observation. He reduced the margin of error by the repetition and combination of observations, so that the co-ordinates of the standard stars in his catalogue were defined with probable errors of the order of only 25 seconds. Moreover, he adopted an essentially modern attitude towards instrumental errors. Recognizing that these must exist, however careful the construction of any given instrument, he sought to detect them from suitable combinations of observations, and thenceforward applied them as corrections to all observations with that instrument.

Tycho's observations were thus sufficiently refined to be appreciably affected by atmospheric refraction. He endeavoured to correct this factor by investigating how the Sun's apparent declination (or distance from the celestial equator) varied with its altitude above the horizon, using the results of these and kindred observations for the construction of the earliest empirical refraction tables. Since, however, these involved a grossly exaggerated estimate of the Sun's parallax, they were misleading.

A knowledge of the topography of the fixed stars which form the background to the movements of the planets is of fundamental importance in precise astronomy. Tycho accordingly devoted much time at Hveen to the determination of star-places, and produced a catalogue which, upon its publication in 1602, superseded the antiquated catalogue in the *Almagest* of Ptolemy. Tycho's usual procedure was to determine the *declinations* of stars directly by measuring their meridian altitudes, and to determine their *right ascensions* (or celestial longitudes) indirectly by means of a chain of comparisons in which the Sun, the planet Venus, and suitably selected standard stars, formed the connecting links. Venus was chosen to serve as intermediary between the Sun and the stars because, when suitably placed, this planet is visible both by day and by night. Hence the difference in right ascension of Venus and the Sun could be obtained during the day (by measuring their respective declinations and their angular separation), while the difference in right ascension of Venus and a selected star could be similarly obtained the following night, when both Venus and the star were above the horizon. By combining the two results the difference in right ascension of the Sun and the star could be deduced, allowance being made for the travel of Venus between the observations. All that was now needed to define the star's place was the absolute right ascension of the Sun at the time of observation; and this was known from tables which were ultimately based upon Tycho's absolute determinations of the constants of the Sun's apparent orbit. Having thus determined the co-ordinates of a few selected stars in various parts of the sky, Tycho was able to dispense with Venus and the Sun and to use these stars as a standard scale of reference for the determination of further star-places. In this manner 777 star-places were accurately defined, but this number was later, and somewhat hurriedly, brought up to 1,000. Comparison of his right ascensions with those recorded by ancient and mediaeval observers led Tycho to an accurate estimate of the rate of precession of the equinoxes; and he gave up the long-established notion that this rate is subject to serious fluctuations in value.

Tycho made systematic observations of the Sun's meridian altitude throughout the year, and was thus able to improve upon the accepted values of the chief constants of the Sun's apparent orbit (eccentricity, longitude of apogee, length of the year, etc.). He could thus compute the accurate solar tables which, as we have seen, were required for the determination of the absolute right ascensions of stars. His instruments, with their open sights, were not sufficiently refined to enable him to correct the gross underestimate of the Sun's distance, which had come down from Ptolemy, and which had been modified only slightly by Copernicus.

During his years of work at Hveen, Tycho made regular determinations of the Moon's position at all points of her orbit, and it fell to him to make the first important advances in lunar theory since Ptolemy. He was in all probability the earliest to discover the inequality called the *variation*, which is now known to arise from the fact that the Earth and the Moon, when at different distances from the Sun, are attracted by the Sun with different intensities, this difference operating as a disturbing force, alternately accelerating and retarding the Moon in its orbit. He also recognized and allowed for another inequality, the *annual equation*, which is due to a yearly fluctuation in this disturbing force. This latter effect was independently discovered by Kepler. Tycho further detected fluctuations in the inclination of the Moon's orbit to the ecliptic (the Earth's orbit or the Sun's apparent orbit), and in the rate at which the nodes of the orbit travel round the ecliptic.

Of the greatest importance for the future progress of astronomy were Tycho Brahe's observations of the planets. He commenced these at an early age, his procedure being to measure the angular distances of a planet from adjacent stars with such crude instruments as were available. He continued this task with his mural quadrant and armillary spheres throughout his years of work at Hveen, but his early death deprived him of the opportunity of developing a numerical planetary theory on the basis of the observations so obtained. On his death-bed Tycho committed this task to Kepler; and the story goes that he exhorted him to fashion the new theory in accordance with the Tychonic, and not the Copernican, planetary system. To learn how the task was fulfilled we must follow the career of Johann Kepler.

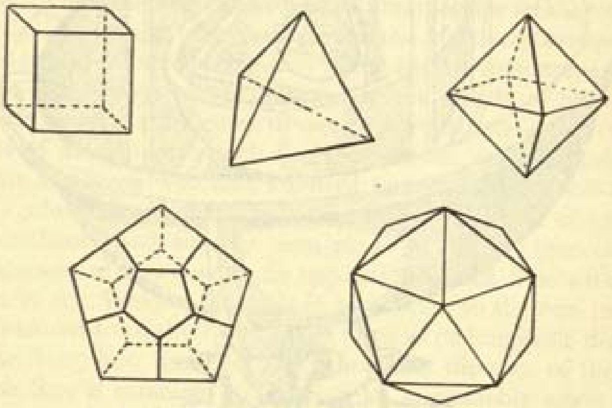
The Life of Kepler

Johann Kepler was born December 27, 1571, at Weil, near Stuttgart, the son of a Protestant officer in the service of the Duke of Würtemberg. From his earliest years Kepler was dogged by ill-health, and his parents were crippled by poverty. The Duke, however, had him educated at the monastic school of Maulbronn, and he passed from there to the great Protestant University of Tübingen, where he took his M.A. degree in 1591. Kepler's early studies were mainly theological, but at Tübingen he made the acquaintance of Michael Mästlin, the Professor of Mathematics and Astronomy there. Mästlin awakened his interest in these subjects and converted him to the Copernican doctrine as, according to some accounts, he had already converted Galilei. The growing freedom of Kepler's opinions made him ineligible for a career in the Church, where a rigorous orthodoxy then prevailed. He was glad, for the time being, to obtain a post as lecturer in astronomy at Graz, in Styria. Here, in his spare time, he began those researches on planetary problems which he pursued throughout his life, and which ultimately led him, after many failures, to his great discoveries. The fruits of his earliest speculations in this direction appeared in 1596 in his work *Prodromus Dissertationum Cosmographicarum continens Mysterium Cosmographicum.* He sent a copy of this book to Tycho Brahe, and we have seen how this led to correspondence between the two astronomers, and to an invitation from Tycho for Kepler to visit him at Prague. Very soon afterwards the growing persecution of Protestants in Styria forced Kepler to withdraw hurriedly into Hungary. He returned to Graz for a short space, but, in view of the changed conditions there, resolved to visit Tycho, and arrived at Prague early in 1600. He was cordially received by Tycho, who set him to work on the reduction of his planetary observations. There were a few initial difficulties in the relations of the two astronomers, due to their embracing different cosmological theories, and also to uncertainty as to Kepler's status. But during the last year of Tycho's life they worked amicably together, and we have seen how Kepler received from Tycho on his death-bed the treasure of his accumulated observations. Shortly before Tycho's death Kepler had received from the Emperor Rudolph the title of Imperial Mathematician; and he now succeeded to Tycho's position. The retrenchment of his salary, however, and the irregularity with which it was paid, kept him in constant financial difficulties and forced him to turn to teaching and the practice of astrology for additional income. "Mother Astronomy (he is reported to have said) would certainly starve if daughter Astrology did not earn the bread of both." In 1612 he left Prague for Linz, where he taught mathematics and supervised surveying operations. But despite these interruptions Kepler pressed on with his twofold task, first of working out a planetary theory in harmony with the doctrines of Copernicus, and secondly of constructing tables of the planetary motions, on the basis of Tycho's observations, which should supersede the inaccurate tables then in use. How Kepler succeeded in this task we shall relate in due course. The completion of the tables was delayed by shortage of money, by religious hostility, and by other interruptions. On one occasion Kepler had to hasten to the assistance of his mother, who had been brought to trial on a charge of witchcraft. He was at length forced to leave Linz for Ulm, and it was here that, in 1627, he published his *Tabulae Rudolphinae*, so called in honour of his old patron, the Emperor Rudolph, who had died fifteen years before. With the publication of his tables Kepler's work was practically at an end, and only a few more years of wandering life remained to him. For a time he associated himself with Wallenstein, who was then at the height of his power. The Imperialist leader valued Kepler for the sake of his services as an astrologer, and he made him Professor of Astronomy at Rostock. After the fall of Wallenstein, Kepler journeyed to Ratisbon to claim the arrears of his salary from the Diet, but was attacked by fever upon his arrival and died November 15, 1630. He was buried outside the gates of the city, but all trace of his grave was swept away during the Thirty Years War.

Kepler's Contributions to Astronomy

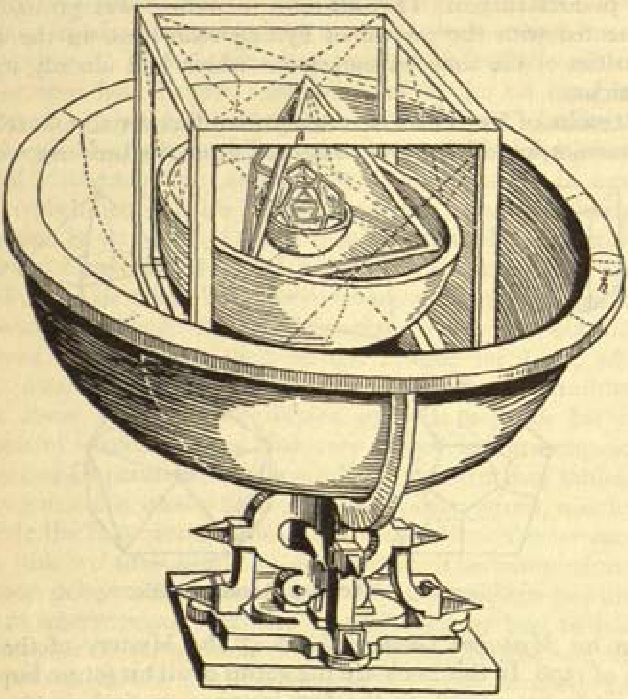
From the time of his earliest researches Kepler was inspired by the belief that God had created the world in accordance with some pre-existent harmony, certain manifestations of which might be traced in the number and sizes of the planetary orbits, and in the motions of the planets therein. This attitude to nature was probably not unconnected with the revival of Pythagorean ideas in the Italian Universities of the time—a movement which had already inspired Copernicus.

The results of Kepler's first attempts to discover simple relations or harmonies underlying the construction of the universe were set



Illustr. 87.—The Five Regular Solids

forth in his *Mysterium Cosmographicum* ("The Mystery of the Universe") of 1596. In this book are the germs of all his future inquiries. He seems to have tried, in the first instance, to find simple arithmetical proportions between the distances of the several planets from the Sun. He supposed that certain of these distances might be simple multiples of others; but no such rule emerged. Kepler next tried simple geometrical relations. He constructed a series of regular polygons of such sizes that a circle could be inscribed in each and at the same time circumscribed to the succeeding member of the series. He thought that the radii of successive circles might be proportional to the distances of successive planets; but here again he was disappointed. This last attempt, however, led him on to calculate the radii of the pairs of spheres which can be severally inscribed and circumscribed to the five regular solids (Illustr. 87) to see whether there might be any cosmic significance in these. The result satisfied him that he had discovered one of the fundamental secrets of the universe. The radii of the inscribed and circumscribed spheres of an octahedron were tolerably proportional to the greatest distance of Mercury and the least distance of Venus, respectively, from the Sun. Similarly the radii of the inscribed and circumscribed spheres of an icosahedron were found to represent the greatest distance of Venus and the least distance of the Earth. The dodecahedron,



Illustr. 88.—Kepler's Conception of the Planetary Spheres

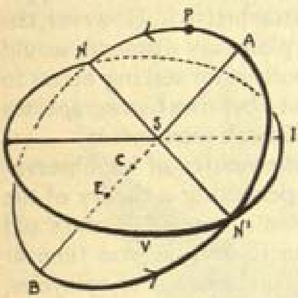
tetrahedron, and cube could similarly be interpolated between the successive orbits of the Earth, Mars, Jupiter, and Saturn (Illustr. 88).

The existence of only six planets (as then known) seemed thus to be connected with the existence of only five regular solids. The numerical agreement of the planetary distances, calculated from this scheme, with those deduced from observation was, indeed, imperfect; but Kepler could, at that time, reasonably attribute these discrepancies to faulty observations.

The *Mysterium Cosmographicum* contains also a valuable defence of the Copernican planetary system as against the Ptolemaic. Attention is drawn to the fact that the motions of the superior planets in their principal (Ptolemaic) epicycles are simply replicas of the Earth's motion in its annual (Copernican) orbit. Kepler, however, did not hesitate to break away from the practice of Copernicus in several important respects, in order to bring the data into closer agreement with his theory. Thus Kepler defined the orbits of the planets with reference to the Sun and not, as Copernicus had done, with reference to the centre of the Earth's eccentric orbit. This was a valuable step towards Kepler's subsequent discoveries; and the data, when recalculated on this basis, were more favourable to his ingenious hypothesis concerning the regular solids. The more accurate data later available, however, did not bear out the hypothesis, and no scientific significance is now attached to it. However, the hope that improved measurements of the planetary distances would confirm his surmise was one of Kepler's motives for seeking access to Tycho Brahe's store of observations; and the *Mysterium Cosmographicum* was the means of introducing him to the Danish astronomer.

Tycho was always rather secretive in the matter of his observations, but he soon set Kepler to work on perfecting a theory of the motions of Mars upon which Longomontanus, one of Tycho's old assistants at Hveen who had followed him to Prague, was then at work. Tycho claimed that, by a suitable combination of epicycles, etc., the theory had already been made to fit his observations of the longitude of Mars when in opposition to the Sun within two minutes of arc. When a planet is in opposition to the Sun its longitude measured from the Earth is the same as its longitude measured from the Sun; but, between oppositions, the distance of the Earth from the Sun is sufficient to produce an appreciable angle (heliocentric parallax) between the apparent direction of the planet as viewed respectively from the Earth and from the Sun. Tycho's theory, as it was laid before Kepler, could not account at all satisfactorily for the observed heliocentric parallaxes of Mars, nor for the observed deviations of the planet from the plane of the ecliptic. Moreover, its agreement with observations at opposition had been exaggerated. The oppositions upon which Tycho had based his theory were oppositions to the *mean* position of the Sun, and not to its *true* position. Kepler objected that this complicated the study of the planet's apparent motion by introducing into the problem any uncertainties affecting the Sun's apparent motion. Again, while Tycho had supposed the *Sun* to describe a circular orbit with a uniform angular velocity about its centre, Kepler assigned this motion to the *Earth*, but with an important modification, namely that the angular velocity was uniform neither about the centre nor about the Sun, but about a third point, the *equant*. He made the same assumption provisionally with regard to the other planets. He endeavoured, however, to keep faith with Tycho by developing his planetary theory along three parallel lines, namely in accordance with the Ptolemaic, Copernican, and Tychonic systems respectively. But every step in the process convinced him more and more of the scientific accuracy of the Copernican system.

Tycho's early death left Kepler a free hand. Assuming Mars to have such an orbit as that described, his first task was to determine its fundamental elements from a combination of suitably chosen observations. He found immediately that the plane of such an orbit would pass through the Sun, and that its inclination to the ecliptic would be invariable and unaffected by such periodic oscillations as had always been assumed in previous planetary theories. His efforts to determine the apse-line and eccentricity of the orbit and the position of the equant from four positions of the planet at true opposition cost Kepler much labour. He could proceed only by a method of trial and error, and the seventy trials which he had to make before he succeeded in fitting his theory to the data occupied four years. The theory so obtained from four oppositions admirably fitted all Tycho's other observed oppositions. But it failed seriously to account for the observed latitudes of the planet, and even for its longitudes when not in opposition.[1]



Illustr. 89.—Diagram to Explain Certain Technical Terms in the Astronomy of Kepler, etc

[1] Most of the technical terms involved in the description of Kepler's planetary theories may be explained by reference to Illustr. 89. In the simplest possible form of the heliocentric theory a planet (P) would uniformly describe a circle (APB) having the Sun at its centre (C). Variations, however, were detected in the angular velocity of the planet about the Sun, and in the length of the *radius vector*, that is the straight line (SP) drawn from the Sun to the planet. Hence it was necessary to suppose that the Sun (S) was not at the centre (C) of the planet's orbit (which thus became an *eccentric* circle), and further that the centre about which the planet's motion was uniform lay neither at C nor at S, but at some third point E (*equant*). The system might be further complicated by making the planet describe a small circle (*epicycle*) about its mean position, which meanwhile traversed the circle APB (*deferent*). The points A and B in which CS, when produced in both directions, met the orbit, were the *apses*, A being the nearer apse, or *perihelion*, and B the farther apse, or *aphelion*, and AB the *apse-line*. The ratio CS to CA was the *eccentricity*. When subsequently Kepler's circular orbit gave place to an ellipse, the apse-line became the major axis of the latter, and the Sun occupied the focus. The plane of the planet's orbit about the Sun is inclined at a certain angle (*inclination*) to the plane of the Earth's orbit (*ecliptic*), angle ASI, and the two planes intersect each other in a straight line (*line of nodes*) passing through the Sun, and cutting the planet's orbit in the two nodes N, N'. The

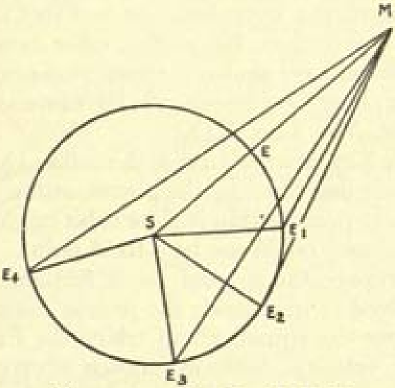
Kepler was thus obliged to begin his task again. Adopting Ptolemy's procedure of "bisecting the eccentricity," i.e. locating the centre of the orbit midway between the Sun and the equant, he reduced the discrepancies to within 8 minutes of arc. But he was not content with this degree of agreement. "For our part," he writes, "since divine goodness has given to us in Tycho Brahe such a painstaking observer, from whose observations an error in these Ptolemaic calculations amounting to 8 minutes is revealed, it is fitting that we should gratefully recognize and use this gift of God. That is to say we should labour . . . finally to trace out the true nature of the celestial motions. . . . For if I had believed these 8 minutes in longitude to be negligible, I should already have sufficiently corrected the hypothesis set out in Cap. XVI (by the bisection of the eccentricity). But as that error cannot be neglected, these 8 minutes alone have shown the way to the complete reformation of astronomy; they have been made the material for a great part of this work" (*Ast. Nov.*, Cap. XIX).

From his failure Kepler inferred that the orbit of Mars could not be regarded as a circle described by the planet with a uniform angular velocity about some point within it. The orbit might be a circle, but in that case the equant could not be a fixed point.

In order to prepare the ground for a further attack on Mars, Kepler now resolved to investigate the precise nature of the Earth's orbit, and to locate the equant about which the Earth moved with uniform angular velocity. Suitably chosen observations of Mars, when at a given point in its orbit showed that the equant must lie on the Earth's apse-line, with the geometrical centre of the orbit midway between it and the Sun (see note on p. 136). This was the arrangement which had given the best results in the case of Mars. From the fact that the equant did not coincide with the centre of the orbit, it followed that the linear velocity of the Earth in its course could not be uniform, and that, at the two apses, the times for describing equal small arcs must be proportional to the distances of the Earth from the Sun at those times. Already in his *Mysterium Cosmographicum* Kepler had suggested that the planets might be impelled in their orbits by an *anima motrix* (a moving spirit) which was located in the Sun, and which acted more powerfully upon planets the nearer they were to the Sun. The force emanating from the Sun, he thought, was confined to the plane of the ecliptic, and

planet's *longitude* is the angular distance between its projection on the ecliptic and a standard point on the ecliptic, as viewed from the Sun (*heliocentric longitude*) or from the Earth (*geocentric longitude*). The planet's *latitude* is its angular distance from the ecliptic as viewed from the Sun (*heliocentric latitude*) or from the Earth (*geocentric latitude*).

therefore varied inversely as the simple distance. The velocity, supposed to be maintained by the force, should therefore obey the same law. Having apparently verified this law in the neighbourhood of the apses, Kepler surmised that the planet might describe equal small arcs *in all parts of its orbit* in times proportional to the lengths of the *radii vectores* drawn to these arcs from the Sun. He accordingly divided the Earth's orbit into three hundred and sixty equal arcs, and computed the length of the *radius vector* to each point of division. He found that the time required for the Earth to travel from one point to another on its orbit was approximately proportional to the sum of the *radii vectores* drawn to the divisions intercepted between these



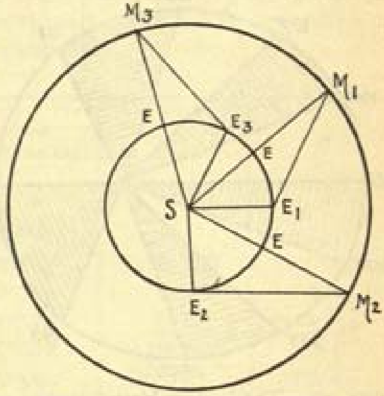
Illustr. 90.—The Earth's Orbit

points. Kepler seems to have simplified his approximate calculations by noting that in an orbit of small eccentricity the sum of such a series of *radii vectores*, each multiplied by the corresponding small arc, was nearly proportional to the area included by the terminal *radii vectores* and the arc of the orbit intercepted by them. And he was eventually led to adopt, as rigorously descriptive of the Earth's orbital velocity, the law that the time taken by the Earth in moving from one point on its orbit to another was proportional to the area swept out by the radius vector during that time.

Kepler's method of determining the eccentricity and orientation of the Earth's orbit may be explained by reference to the above diagram (Illustr. 90).

Let E be the position of the Earth when Mars, the Earth, and the Sun are in a straight line (MES). In 687 days Mars will be in the same place again (M), but the Earth will not yet have completed its second round, so that, instead of being at E, it will only be at E_1. Now the angles of the triangle ME_1S could be measured, and consequently also the relation of SE_1 to SM. In another 687 days Mars will again be in its original position (M), but the Earth will be at E_2. The angles of the triangle ME_2S can now be determined, and also the relation of SE_2 to SM. Similarly with the successive periods of 687 days each, after which Mars always returns to the position M whereas the Earth is successively at positions E_3 and E_4, etc. At these positions the relations of SE_3, SE_4, etc., to SM can be determined. But as SM is constant, Kepler thus got the relations of SE_1 to SE_2 to SE_3 to SE_4, etc., which are the *radii vectores* of the Earth.

Assuming that the Earth described its eccentric in accordance with his new area-law, Kepler now returned to the attack on Mars. But he still found himself unable to construct an entirely satisfactory theory, and began at last to surmise that the orbit of Mars was not a circle at all. Determinations of the relative distances of the planet from the Sun at several parts of its orbit suggested that the required curve was some sort of oval lying wholly within the old eccentric but touching it at the two apses. Only after trying many ovals, all larger at one end than at the other, did it occur to Kepler to try an *ellipse*, the simplest form of oval. He eventually arrived, by trial and error, at an elliptic orbit, for which the area-law was found to hold strictly.



Illustr. 91.—The Orbit of Mars

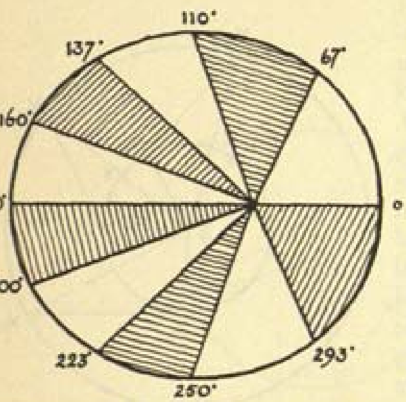
Kepler's method of dealing with Mars will be clear from the above diagram (Illustr. 91).

Let M_1, M_2, M_3, etc., represent different positions of Mars when in opposition to the Sun, that is when Mars, the Earth, and the Sun are in a straight line (MES). And let E_1, E_2, E_3 mark the positions of the Earth 687 days after the respective oppositions when Mars has completed another circuit. By reasoning analogous to that explained above in connection with the determination of the relative distances of the Earth (Illustr. 90) Kepler deduced from the triangles SE_1M_1, SE_2M_2, SE_3M_3, etc., the relative distances of SM_1, SM_2, SM_3, etc. And he found eventually that the positions of M_1, M_2, M_3, etc., are positions on an elliptic orbit with the Sun in one focus.

Kepler found that such an orbit of Mars satisfied everywhere the requirements of Tycho's observations, both in longitude and latitude, on the assumption that the planet described it in accordance with the area-law, and about the Sun in one focus (see Illustr. 91). He was thus able to enunciate, for the case of Mars in the first instance, his first two Laws of planetary motion:

1. The planet describes an ellipse about the Sun in one focus;
2. The radius vector drawn from the Sun to the planet describes equal areas in equal times.

These discoveries, together with an account of the painful process by which they had been arrived at, were published by Kepler in 1609 in his great work *Astronomia Nova αιτιολογετος seu Physica Coelestis, tradita commentariis de Motibus Stellae Martis* (German translation by Max Caspar, München-Berlin, 1929). The two Laws are enunciated in Chapter LIX of this book.



Illustr. 92.—The Radius Vector sweeps out Equal Areas in Equal Times

The full significance of Kepler's contributions to astronomy was not apparent until years after his death, when his Laws were the means of leading Newton to his far more comprehensive Law of Universal Gravitation. But already in his lifetime Kepler put his discoveries to practical account by making them the foundation of his *Rudolphine Tables*. This work remained an indispensable aid to precise astronomy for about a century. Besides tables and rules for predicting the positions of the planets, the work contains Tycho's catalogue of one thousand star places, as well as refraction tables. The laborious task of preparing these tables was lightened somewhat towards the end by the use of logarithms. These had just then been invented independently by Napier, in Scotland, and by Bürgi, a Swiss clockmaker who worked in the Landgrave Wilhelm's observatory at Cassel and later entered the service of Kepler's patron, Rudolph II. A logarithm table computed by Kepler is included in the tables.

Among Kepler's other writings we may note his *Epitome Astronomiae Copernicanae* (1618–21), a comprehensive catechism of Copernican astronomy in which Kepler's two Laws of planetary motion are explicitly extended (though without adequate proof) to the remaining planets, to the Moon, and to the Medicean satellites of Jupiter. Kepler's third, and last, great Law of planetary motion is contained in his *Harmonices Mundi*, 1619 (V, 3), and runs:

3. The squares of the periodic times of the several planets are proportional to the cubes of their respective mean distances from the Sun.

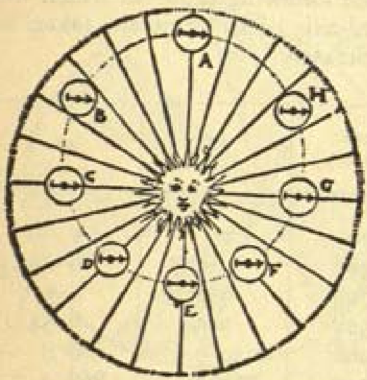
This law is usually expressed by saying that a^3/T^2 is constant—where a stands for the planet's mean distance from the Sun, that is the semi-axis major of the planet's elliptic orbit about the Sun, and T for the planet's periodic time, that is the time it takes to complete its orbit. This may be seen from the following table, in which the Earth's periodic time and the semi-axis of its orbit are taken as units, and the constant = 1 approximately.

Planet	Periodic Time (T)	Mean Distance (a)	T^2	a^3
	Year			
Earth ..	1	1·00	1	1
Mercury	0·24	0·387	0·058	0·058
Venus ..	0·61	0·723	0·378	0·378
Mars ..	1·88	1·524	3·54	3·54
Jupiter ..	11·86	5·202	140·7	140·8
Saturn ..	29·46	9·539	867·9	868 0

A considerable portion of the *Harmonices Mundi* is taken up with a supposed analogy between the angular velocities of the several planets about the Sun and the frequencies of musical notes. As a planet's angular velocity fluctuates in the course of its periodic circuit round the Sun, so the corresponding note alters, returning to its initial frequency when the planet returns to its starting-point. Kepler wrote down in musical notation the tunes thus associated with the several planets. They remind us of the Pythagorean "music of the spheres"; but Kepler did not ascribe to them an audible existence.

Kepler concerned himself much with attempts to explain on physical grounds the remarkable planetary laws which he had obtained inductively; but his speculations in this direction, though significant as initiating a new era, were in themselves of relatively little value. He supposed that the *anima motrix*, located in the Sun, sent out straight lines of force, like the spokes of a wheel, and that as the Sun rotated on its axis these lines exerted a *vis a tergo* (a push) upon each planet, carrying it round the Sun (see Illustr. 93).

It is worth noting that this theory anticipated by some years the actual discovery of the Sun's axial rotation. The differences in the planets' periods arise from differences in their masses and in the orbits which they have to traverse, as well as from the falling off with distance of the solar virtue, which Kepler was inclined to regard as a species of magnetic effluvium. Magnetism also played a part in his explanation of the shape of the planetary orbits. Largely under the inspiration of Gilbert's researches, he supposed that each planet behaved like a huge magnet whose axis kept its direction in space unaltered during the revolution of the planet. The two poles were alternately presented to the Sun, which attracted one and repelled the other. Hence the whole planet was alternately attracted to and repelled from the Sun, and the radius vector thus underwent those fluctuations in length which characterize the elliptic orbit.



Illustr. 93.—Kepler's Idea of the Sun's Action on the Planets

Kepler speculated on the nature of gravity, which he regarded as "a mutual affection between cognate bodies tending towards union or conjunction, similar in kind to magnetism, so that the Earth attracts a stone rather than the stone seeks the Earth. . . . If the Moon and Earth were not held in their orbits by their animal force or some other equivalent, the Earth would rise to the Moon by one fifty-fourth part of their distance apart, and the Moon would fall to the Earth through the other fifty-three parts" (Introduction to the *Commentaries on the Motion of Mars*, where Kepler also suggests that two neighbouring stones "would come together in the intermediate point, each approaching the other by a space proportional to the comparative mass of the other." Presumably he estimated the masses of the Earth and the Moon to be in the ratio of 53 to 1). But Kepler did not succeed in identifying gravity with the force keeping the planets in their orbits.

Kepler has some claim to be regarded as the discoverer of Sun-spots. He endeavoured in 1607 to observe the transit of the planet Mercury across the Sun's disc. When he supposed the transit was due to take place, he formed an image of the Sun upon a screen by admitting direct rays through a narrow opening in a dark room. To his surprise a small, indistinct spot appeared on the bright disc, which he took to be an image of Mercury in transit. There is little doubt, however, that the object was a Sun-spot, for Mercury did not transit on that day, and even if it had done it could not have been seen in the manner described. After the discovery of Sun-spots Kepler withdrew the interpretation which he had too hastily put upon this observation.

Kepler's writings include also short accounts of the new star of 1604, whose lack of diurnal parallax he demonstrated, and a treatise on comets, wherein he suggested that these bodies were condensations in the aether which fills space.

Horrocks

In the Temple of Fame, where the names of Kepler and Newton are writ large, some small place must be consecrated to the memory of a young English astronomer who may be regarded as an intermediate link between them, and who, but for his untimely death at the early age of twenty-four, might conceivably have anticipated some of Newton's greatest discoveries. The astronomer referred to was Jeremiah Horrocks (1617–41). He was almost unknown in his lifetime, and it was not until he had been dead some thirty years that such of his writings as had survived received proper publication (*Opera Posthuma*, edited by J. Wallis, London, 1672).

Horrocks studied astronomy on his own account as a boy and as a Cambridge undergraduate, and he carried on a scientific correspondence with William Crabtree, cloth-dealer and astronomer, to whom he had been introduced by Christopher Towneley, the antiquary. He studied the astronomical tables of Lansberg and Kepler, which he corrected from the results of his own observations, and he satisfied himself that a transit of Venus, unpredicted by Kepler, would take place on November 24 (O.S.), 1639. He communicated his discovery to Crabtree, and when the phenomenon, which had never previously been observed, took place at the predicted time the two men were the only spectators. Horrocks, who was then working as a curate near Preston, missed the beginning of the spectacle owing to his attendance at Church service, but was later able to follow the transit until sunset by throwing an image of the Sun from a telescope on to a screen.

Horrocks was a keen student of Kepler's planetary theory, and he tentatively assigned an elliptic orbit to the Moon, attributing variations in the elements of this orbit to the disturbing action of the Sun. Horrocks' rudimentary lunar theory was afterwards developed by Flamsteed with the aid of his own observations. In his speculations on the forces producing the motions of the planets, Horrocks was one of the pioneers in the theory of universal gravitation. He discovered that the mean rate of Jupiter's motion was greater, and that of Saturn's motion less, than it had been in Kepler's time—an inequality confirmed by Halley. Horrocks also arrived at an improved estimate of the solar parallax, and made some of the earliest systematic observations of the tides. The former of these was noteworthy, because it was practically the first scientific improvement in the estimate of solar parallax since the time of Hipparchus. Horrocks' estimate of the solar parallax (14″ or 15″, as against the present estimate of about 9″) was a great improvement on the values accepted by Ptolemy (2′ 50″), Lansberg (2′ 13″), and Kepler (59″). But Horrocks' method was very questionable and very much in the Keplerian vein. From the approximate telescopic measurements available of the angular diameters of the planets, he calculated what these diameters would be if viewed from the Sun. For this purpose he had to estimate roughly the Sun's distance. Kepler's statement that the parallax of Mars, though about twice that of the Sun, was insensible, gave Horrocks a lower limit for the Sun's distance. When some rather arbitrary adjustments had been made on the score of correcting for irradiation, all the diameters as measured from the Sun came out at about 30″. He *assumed* that the same value would hold of the Earth as viewed from the Sun, giving a parallax of 30″/2 or 15″ (*Astronomia Kepleriana*: Disputatio V, Chap. 5).

(See J. L. E. Dreyer, *Tycho Brahe*, Edinburgh, 1890; J. Kepler, *Opera Omnia*, ed. C. Frisch, Frankfurt, 1858–71; *Tychonis Brahe Dani Opera Omnia*, ed. J. L. E. Dreyer, Hauniae, 1913–29; H. Shapley and H. E. Howarth, *A Source Book in Astronomy*, New York and London, 1929; and the list of books on p. 26.)

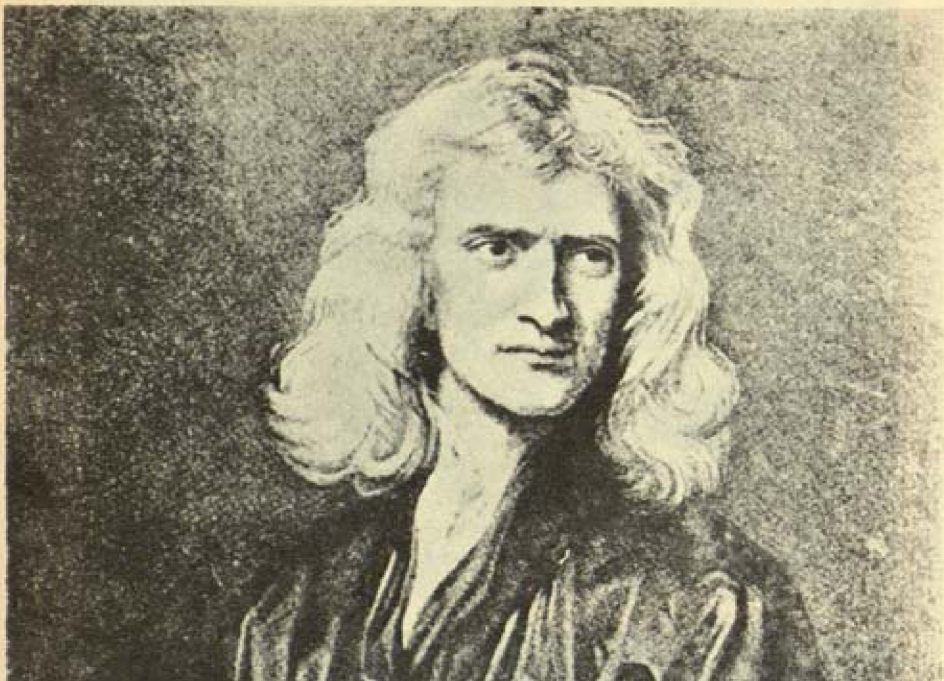
Illustr. 94

Tycho Brahe

Illustr. 95

Johannes Kepler

Illustr. 96



Isaac Newton

CHAPTER VII

THE NEWTONIAN SYNTHESIS

THE whole history of science affords but few parallels to the development of astronomy from Copernicus to Newton. The progress made during that comparatively short period was so continuous and complete as to give it something of the character of a self-contained drama exhibiting the natural unfolding of the logic of events. Beginning with Copernicus' revolutionary conception of the Earth as one of the smaller planets of the solar system, the work of Galilei, Tycho Brahe, and Kepler led progressively to Newton's great synthesis of the physical world. Thereby the traditional cleavage between the sublunar and the superlunar worlds, and the associated demarcation between the natural and supernatural, between this world and other worlds, were abolished or undermined. For it was shown that the whole physical universe is subject to the same law of gravitation and the same laws of motion, so that all physical objects or events in one part of the universe exercise some influence upon all others, and thus together constitute one cosmic system of interconnected parts.

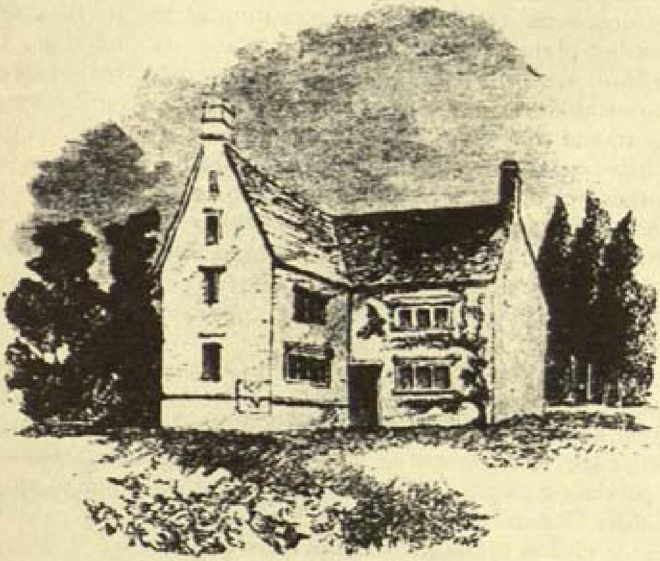
It is profoundly significant that the five principal thinkers whose co-operation resulted in the Newtonian Synthesis belonged to five different nations. The revelation of the physical unity of the universe was thus the achievement of a certain spiritual unity of mankind. The preceding pages have already unfolded the story of the parts played by Copernicus, Galilei, Tycho Brahe, and Kepler. We now reach the climax of the story in the work of Newton.

THE LIFE OF NEWTON

Isaac Newton was born on December 25 (O.S.), 1642, at Woolsthorpe near Grantham, in Lincolnshire, the posthumous son of a farmer of moderate means. He was delicate as a child, but was sent at the age of twelve to the Grammar School at Grantham. Here he eventually became head boy, and excelled in the construction of mechanical toys and models. A water-clock which he contrived at this period remained in use after he had left Grantham, and one of his sundials still survives.

In 1656 Newton's mother, who had married a second time, was again widowed, and Newton was called home to help with the farm at Woolsthorpe. As, however, he showed no interest or skill in farming, he was sent back to school at Grantham, and soon afterwards, at the recommendation of his uncle, William Ayscough, he proceeded to the University of Cambridge.

Newton entered Trinity College in June 1661. As an undergraduate he ranged through the mathematics and optics of the time, but he worked largely on his own account, and attracted little attention. It was not until after he had taken his B.A. degree, early in 1665, that his career of discovery began. In the course of the following two years, 1665 and 1666, much of which he spent



Illustr. 97.—Manor House, Woolsthorpe, the Birthplace of Newton

at Woolsthorpe, owing to the prevalence of the Plague, Newton discovered the binomial theorem, invented the method of fluxions, began his experiments on colour, and took the first steps towards the establishment of the law of universal gravitation. In 1667, after returning to Cambridge, Newton was elected a Fellow of Trinity College. He proceeded to his M.A. in the following year, and in 1669 he succeeded Isaac Barrow as Lucasian Professor of Mathematics. In the meantime he had resumed his interrupted researches in optics; and to this period belong the construction of his reflecting telescopes, and his discovery of the composite nature of sunlight, which he eventually communicated, early in 1672, to the Royal Society, of which he had shortly before been elected a Fellow. At this period also he found time to develop his method of fluxions, and to make experiments in chemistry, which had interested him ever since he was a schoolboy.

Newton's attention was recalled from time to time to the problem of gravitation by conversations and correspondence with scientific friends. In 1684, however, at the instigation of Halley, he entered upon the period of intensive research in theoretical mechanics which culminated in the publication of his *Principia* in July 1687.*

Earlier in that year Newton had appeared in the High Court before Judge Jeffreys as one of a number of delegates representing the University of Cambridge in a dispute with King James II concerning the privileges of the University. From this episode may be dated Newton's increasing participation in public affairs and social life. In 1689 he was elected Member of the Convention Parliament for the University, and later on, in 1701, he returned to Westminster again for a few months in the same capacity. After the Dissolution of Parliament in 1690 Newton appears to have returned to Cambridge, and for some years devoted much attention to the textual study and interpretation of Scripture. About this time he began to suffer in health and spirits from the effects of years of overwork and self-neglect. In 1695, however, he was appointed Warden of the Mint, and threw himself eagerly into his new duties. These were just then peculiarly responsible, as they involved supervising the complete re-coinage of the silver currency, which had become seriously debased. In 1699, when this task had been successfully completed, Newton was made Master of the Mint, an office which he retained until his death. In 1699 also he was elected a Foreign Associate of the French Academy of Sciences. In 1701 he resigned his Fellowship at Trinity College and the Lucasian Professorship, but continued to occupy himself from time to time with minor scientific problems and with the preparation for the Press of the *Opticks*, and of further editions of the *Principia*. In 1703 Newton was elected President of the Royal Society, and he was re-elected annually until his death. In 1705 he received a knighthood from Queen Anne. His closing years were somewhat troubled by his controversies with Flamsteed and with Leibniz. He was taken ill while presiding at a meeting of the Royal Society, and a fortnight later, on March 20, 1727, he died, in his eighty-fifth year. He was buried in Westminster Abbey. All things considered, there are but few men in the earlier history of science whose genius met with such ready recognition at home and abroad as did the genius of Newton. In comparison with Galilei, for example, Newton's good fortune formed a very pleasing contrast.

* A revision by F. Cajori of Motte's English translation (1729) of the *Principia* was published in Cambridge in 1934.

THE DISCOVERY OF UNIVERSAL GRAVITATION

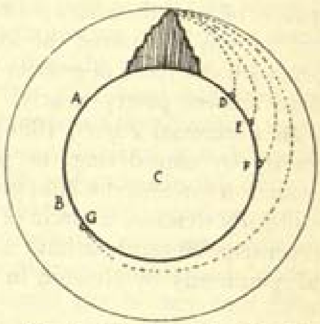
The revolution in dynamical ideas initiated by the researches of Galilei made it necessary to formulate in new terms the problem of assigning a mechanical explanation to the motions of the planets. Galilei's experiments showed that an external force is required, not to maintain, but to alter a body's uniform rectilinear motion. This meant that astronomers had to explain not why the planets continue to move, nor why they fail to move in exact circles, but why they revolve about the Sun in closed curves at all, and do not travel in straight lines into outer space. It was in developing the implications of the new dynamics, and in applying them to the concrete mechanical problems presented by the solar system, that Newton made his greatest contributions to astronomy.

Newton's earliest recorded speculations on gravitation date from the plague year 1666, during his temporary retirement from Cambridge to Woolsthorpe. Our information as to this earliest phase comes from several independent sources, which do not show complete agreement with one another. These include a memorandum in Newton's handwriting, and printed statements in the works of his friends Pemberton and Whiston, claiming to be based on conversations with Newton.

From these accounts it appears that in 1666 Newton began to wonder whether the force of gravity, which is observed to extend to the tops of the highest mountains, might not extend to the Moon and influence that body—perhaps even keep it in its orbit.

At one time Newton appears to have thought of the orbital motion of the Moon and of the other planets as analogous to the movement of a projectile, or as a limiting case of it, in accordance with modified Galilean laws of projectiles. This seems clear from the following passage: "That by means of centripetal forces the planets may be retained in certain orbits, we may easily understand, if we consider the motions of projectiles; for a stone projected is by the pressure of its own weight forced out of the rectilinear path, which by the projection alone it should have pursued, and made to describe a curve line in the air; and through that crooked way is at last brought down to the ground; and the greater the velocity is with which it is projected, the farther it goes before it falls to the Earth. We may therefore suppose the velocity to be so increased, that it would describe an arc of 1, 2, 5, 10, 100, 1,000 miles before it arrived at the Earth, till at last, exceeding the limits of the Earth, it should pass quite by without touching it (Illustr. 98).

"Let AFB represent the surface of the Earth, C its centre, VD, VE, VF, the curve lines which a body would describe if projected in an horizontal direction from the top of a high mountain successively with more and more velocity; and, because the celestial motions are scarcely retarded by the little or no resistance of the spaces in which they are performed, to keep up the parity of cases, let us suppose either that there is no air about the Earth or at least that it is endowed with little or no power of resisting; and for the same reason that the body projected with a less velocity describes the lesser arc VD, and with a greater velocity the greater arc VE, and, augmenting the velocity, it goes farther and farther to F and G, if the velocity was still more and more augmented, it would reach at last quite beyond the circumference of the Earth, and return to the mountain from which it was projected.



Illustr. 98.—The Centripetal Motion of Projectiles

"And since the areas which by this motion it describes by a radius drawn to the centre of the Earth are proportional to the times in which they are described, its velocity, when it returns to the mountain will be no less than it was at first; and, retaining the same velocity, it will describe the same curve over and over by the same law.

"But if we now imagine bodies to be projected in the directions of lines parallel to the horizon from greater heights, as of 5, 10, 100, 1,000, or more miles, or rather as many semi-diameters of the Earth, those bodies, according to their different velocity and the different force of gravity in different heights, will describe arcs either concentric with the Earth, or variously eccentric, and go on revolving through the heavens in those trajectories, just as the planets do in their orbs" (Andrew Motte's translation of Newton's *De Systemate Mundi*, London, 1803, pp. 3–4).

The well-known story that the problems of gravitation were vividly brought home to Newton by the fall of an apple in the orchard at Woolsthorpe, seems to rest upon fairly good authority. To test the possible connection between the force producing the fall of an apple and that keeping the Moon in a closed orbit, it was necessary (1) to ascertain according to what law the force of gravity fell off (as it was then generally supposed to do) with increase of distance from the Earth; (2) to calculate from this law and from the measured acceleration of bodies at the Earth's surface what acceleration gravity should produce in a body at the Moon's distance; (3) to calculate what was the actual centripetal acceleration of the Moon, assuming its orbit to be a circle about the Earth in the centre; and (4) to ascertain whether the accelerations calculated under (2) and (3) were sensibly equal, and so could be regarded as arising from the operation of one and the same force.

This was evidently the procedure essentially followed by Newton, for he writes: "And the same year [1666] I began to think of gravity extending to the Orb of the Moon, and having found out how to estimate the force with which [a] globe revolving within a sphere presses the surface of the sphere from Kepler's Rule of the periodical times of the Planets being in a sesquialterate proportion of their distances from the centres of their Orbs I deduced that the forces which keep the Planets in their Orbs must [be] reciprocally as the squares of their distances from the centres about which they revolve; and thereby compared the force requisite to keep the Moon in her Orb with the force of gravity at the surface of the Earth, and found them answer pretty nearly" (Manuscript quoted in the *Catalogue of the Portsmouth Papers*, 1888). Newton thus ascertained the law of gravity by considering the planets as moving in circles under an attraction towards the Sun, probably somewhat as follows: If a planet uniformly describe a circle of radius r with velocity v in a period T, its centripetal acceleration f is v^2/r (Huygens' formula, but discovered independently by Newton in 1666). We have:

$$f = v^2/r \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$v = 2\pi r/\mathrm{T} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and, from Kepler's Third Law:

$$\mathrm{T}^2/r^3 = \text{constant} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

From (2) and (3) v^2 varies as $1/r$, whence, and from (1), f is proportional to $1/r^2$. Newton made the tentative assumption that this inverse square law governed also the acceleration of bodies under the attraction of the *Earth*, so that the distances through which the Moon and a particle at the Earth's surface respectively fall towards the centre of the Earth in one second should be as the square of the distance of the particle from the Earth's centre to the square of the distance of the Moon from the Earth's centre. This calculation involves merely a knowledge of the ratio of the Moon's distance to the Earth's radius—a ratio known to Newton with an accuracy sufficient for his purpose. But the next stage in the calculation, the determination of the distance actually fallen through by the Moon towards the Earth's centre in one second, requires a knowledge of the radius of the Earth.

According to Pemberton's and Whiston's accounts of the matter, which have been almost universally followed, Newton, being "absent from books," substituted a value of the radius derived from the crude estimate that one degree of latitude measures sixty miles on the Earth's surface, which was the value commonly assumed by seamen at that time. This would have led to a serious discrepancy (of the order of 15 per cent) between the two values of the Moon's acceleration as determined, on the one hand, from its period and the supposed size of its orbit, and, on the other, from the force of gravity at the Moon's distance, as deduced from the inverse square law. According to Whiston this discrepancy in the calculations led Newton to suspect that perhaps a Cartesian vortex might share with gravity the task of keeping the Moon in its orbit. It is further maintained that it was this failure to make out a clear case for his surmise that led Newton to lay aside his speculations on gravitation until recalled to the subject by Hooke in 1679. In the meantime an improved estimate of the Earth's radius had been obtained by Picard and had become known, and it is supposed that Newton, upon substituting this value in his calculations, obtained a satisfactory agreement, and was thus led to resume his researches in this field.

It must be borne in mind, however, that Newton nowhere states what value of the Earth's radius he originally assumed; that on his own account he found the results of his two critical calculations "answer pretty nearly"; and that he could not in any case have looked for exact agreement, seeing that he had treated the Moon's orbit as a circle uniformly described. Moreover, several fairly close estimates of the Earth's radius (e.g. that of Gunter) were current in 1666, and were accessible to Newton, if not at Woolsthorpe, at least after his return to Cambridge.

John Couch Adams and J. W. L. Glaisher and, more recently, Professor Cajori, were accordingly inclined to attribute Newton's delay in publishing his calculations to the difficulty of defining the *effective* distance between the attracting Earth and a small body near its surface. Should this distance be taken as the height of the body above the ground, or its distance from the Earth's centre, or from some other point? Newton must have measured his distances provisionally from the centre of the Earth, but it was not until 1685 that he was able to prove that the Earth attracts external bodies as if it were a massive particle concentrated at its own centre (see F. Cajori's paper on "Newton's Twenty Years' Delay in Announcing the Law of Gravitation," in *Sir Isaac Newton, 1727–1927*, London, 1928).

Somewhere about 1677 Newton had a discussion on gravitation with Wren and Donne, apparently with special reference to the inverse square law. He was recalled to the subject at the end of

1679 by a letter from Hooke urging him to resume his former intercourse with the Royal Society, and asking his opinion concerning Hooke's proposal for "compounding the celestial motions of the planets of a direct motion by the tangent and an attractive motion towards the central body." In his reply Newton stated that he had for some years been "endeavouring to bend myself from philosophy to other studies in so much that I have long grutched the time spent in that study unless it be perhaps at idle hours sometimes for a diversion." However, he threw out a suggestion for demonstrating the Earth's diurnal rotation on its axis, which was that a body falling from a height should suffer a deflection from the vertical towards the east. Hooke, writing to Newton early in 1680, claimed to have performed this experiment successfully; and he now proposed to Newton the problem of determining what path would be followed by a particle moving in the neighbourhood of a centre of attractive force which varied according to the inverse square law. Newton apparently did not reply to this letter, but was stimulated to renew his old calculations which he seems at this time to have refined by the substitution of Picard's improved value of the Earth's radius.

It was at this juncture, too, that, according to his own account, he solved Hooke's problem by showing that the required orbit under an inverse square law of force was an ellipse with the attracting body in one focus. The elliptic orbits of the planets thus received a rational explanation; and Newton went on to prove that, conversely, an elliptic orbit described about a centre of force in one focus necessarily implies an inverse square law of force. He also showed that the law of equable description of areas by the radius vector (of which Kepler's Second Law is a particular case) must hold good of any central orbit, whatever the law of force.

But having obtained these important results, Newton, as he wrote to Halley in 1686, "threw the calculations by, being upon other studies; and so it rested for about five years." Towards the end of this period, however, in January 1684, Halley, who, like Newton, had deduced the inverse square law from Kepler's Third Law, but had been unable to proceed farther, had a conversation on the subject with Wren and Hooke. Wren, too, had got as far as deducing the inverse square law; but Hooke claimed to have arrived at a complete explanation of the planetary motions, based on the law. Wren offered a prize to whichever of his two friends should provide such an explanation within two months. Halley failed to do so; Hooke made an excuse for not putting his alleged explanation forward just then, and it was never forthcoming.

The following August Halley, while on a visit to Cambridge, learned from Newton that he had succeeded in solving the problem. Newton had mislaid his papers, but he reproduced the calculations from memory, and sent them, together with the results of further researches, to Halley in November 1684. Halley visited Cambridge again almost immediately, and examined the manuscript of Newton's recent investigations, which he was using as the basis of his lectures for that year. He pressed Newton to continue these researches, and made him promise to send the results to the Royal Society, so that they could be registered and their priority established. Halley and Paget were appointed by the Society "to put Mr. Newton in mind of his promise"; and in the following February Newton sent an instalment of his propositions on motion to the Society. Writing to his friend Aston about this time, Newton complained that the work was occupying "a greater part of my time than I expected, and a great deal of it to no purpose." Early in 1685, however, Newton succeeded in proving the important theorem that a spherical body whose density is equal at all points equidistant from its centre, attracts an external particle as if the whole mass of the body were concentrated at its centre. Newton could now feel completely justified in treating the various bodies of the solar system as if they were massive particles; and from this time on he devoted himself tirelessly to working out the consequences of his fundamental laws and propositions, until the work was finished. The first book of the future treatise was probably completed about Easter 1685, and the second and third books were ready a little over a year later. On Newton's own estimate he spent less than eighteen months over the work, and carried on researches in chemistry meanwhile.

It appears to have been the original intention to publish the results of Newton's researches in the *Philosophical Transactions*, but after examining the earlier sections the Royal Society resolved to print the work in book form at its own expense. The Society, however, suffered at this period from chronic impecuniosity; and it lacked sufficient funds to publish the book. Halley therefore undertook to do so at his own expense, although he himself was in financial difficulties at the time. Halley must also be given credit, not only for stimulating Newton to pursue his researches, and to carry them to completion, but also for constantly assisting in the preparation of the work by collecting necessary astronomical data, correcting proofs, pointing out obscurities in the text, and arranging for its printing and illustration. Moreover, he made the importance of the new book known by a descriptive review in the *Philosophical Transactions* (No. 186).

The appearance of the book was retarded, not only by delay on the part of the printers, but by the necessity of meeting Hooke's claim to have been the original discoverer of the inverse square law and the prime mover in the whole of Newton's series of discoveries. A compromise was reached by inserting a statement that Hooke had been among those who independently discovered the law of the inverse square. So all difficulties were finally overcome, and the first edition of Newton's great work appeared, in Latin, under the title of *Philosophiae Naturalis Principia Mathematica*, in July 1687. Later editions of the *Principia* brought out during Newton's lifetime were those of 1713 and 1726 (see W. W. Rouse Ball, *An Essay on Newton's Principia*, London, 1893).

Newton's "Principia"

The *Principia* is made up of three books, together with important introductory matter. The first of these books treats generally of the unresisted motion of particles and bodies under specified laws of force, and the second of motion in a resisting medium and of hydromechanics generally, while the third applies the results obtained to elucidate the chief phenomena of the solar system. The fundamental importance and comprehensive scope of the *Principia* may be realized from a brief survey of its principal contents.

The work opens with definitions of the principal concepts of mechanics, e.g. *mass* (the product of a body's volume into its density, and measured by its weight); *momentum* or *quantity of motion* (the product of mass into velocity); and *force* (measured by the rate of change of momentum which it produces)—concepts which Newton was the first to employ with precision, though his definitions of them have not all escaped criticism. The definition of "mass" is a tautology, for "density" is defined as mass per unit volume. "Velocity" and "acceleration" without qualifications imply absolute space and time, which Newton, accordingly, accepts explicitly. In a scholium which follows these definitions he postulates the existence of "absolute, true, and mathematical time" which "flows equably without regard to anything external"; "absolute space" which "remains always similar and immovable"; and "absolute motion" which is "the translation of a body from one absolute place into another." The abandonment of these concepts of an absolute and independent space and time constitutes the fundamental break between Newtonian and twentieth-century physics. Even Newton who, as we shall see, adopted the centre of gravity of the solar system as a point fixed in absolute space, felt the difficulty of distinguishing this space from other spaces in uniform motion relative to it. "It is indeed extremely difficult to discover and distinguish effectively the true motion of particular bodies from the apparent; because the parts of that immovable space in which those motions are performed do by no means come under the observation of our senses."

We come next to Newton's well-known Axioms or Laws of Motion:

1. Every body perseveres in its state of rest, or of uniform motion in a straight line, unless it is compelled to change that state by impressed forces;
2. Change of motion (i.e. rate of change of momentum) is proportional to the impressed force and takes place in the direction in which that force is impressed;
3. To every action there is always opposed an equal reaction; or the mutual actions of two bodies upon eac': other are always equal and opposite.

The first two Laws are direct deductions from the results obtained by Galilei, to whom the credit for them rightly belongs; and the first Law had been clearly enunciated by Descartes (see e.g. *Le Monde*, §7). But the principle expressed in the third Law (which is the only *physical* law of the three), though assumed in the experiments of Wallis, Wren, and Huygens on impact, does not appear to have been clearly formulated by anyone prior to Newton. A few typical experiments intended to illustrate rather than establish the truth of these Laws are briefly described in a scholium. From the Laws of Motion certain important corollaries follow, e.g. that the momentum of a system of bodies in any direction, and the motion of its centre of gravity (mass centre) are unaffected by the mutual reactions of the bodies.

Book I opens with an elementary account of the principles of fluxions as applied to determine the ratios of evanescent quantities, though without any use of Newton's characteristic notation of dotted letters, which he had himself employed from his earliest experiments with this method in 1665. There follows a numerous series of theorems and problems on central orbits, and on the relation of the form of the orbit to the law of force under which it is described. The most important of these (equable description of areas in a central orbit, I, 1; and the law of force in an ellipse described about the focus, I, 11) were the results from which Newton started, as has already been indicated. Of particular importance from the astronomical point of view are Newton's approximate solution of "Kepler's Problem" of finding the position of a body in an inverse square elliptic orbit at any given time after its passage through an apse, and his investigation of revolving orbits. Having dealt with the motion of particles under attractions directed towards immovable centres, "though very probably there is no such thing existent in nature," Newton proceeds to consider the motion of particles under their mutual attractions. He shows that two mutually attracting bodies describe similar orbits about their common centre of gravity and about each other (I, 57). The particular case is considered of the motion of three mutually attracting bodies under such conditions as those obtaining in the Earth–Moon–Sun system; and it is shown that the disturbing action of the body corresponding to the Sun produces, in the orbit of that representing the Moon, just such inequalities and peculiarities as had, in fact, already been detected in the motion of the Moon. The problem is generalized to give results of subsequent use in the explanation of precession and the tides. The question of how the attraction of an extended body depends upon its shape arises at this point, and is dealt with in detail in two sections, treating respectively of the attractions of spherical and of certain non-spherical bodies. The former of these contains Newton's elegant theorems that, under an inverse square law, the force on a particle anywhere inside a homogeneous spherical shell is zero (I, 70), while an external particle is attracted as if the material of the shell were concentrated at its centre (I, 71). This leads immediately to the theorems on the attraction of a homogeneous solid sphere upon an external particle (I, 74) or upon another such sphere (I, 75), and on the revolution of such spheres about each other in conical paths. Book I concludes with propositions on the passage of a corpuscle across the interspace separating two media, one of which attracts the corpuscle. These propositions have a direct bearing upon Newton's theory of the nature of light, and afford an explanation in terms of that theory of the phenomena of refraction and diffraction.

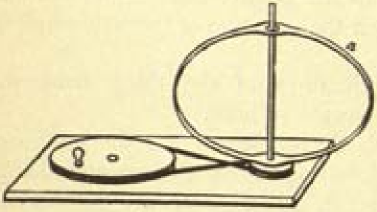
The second Book deals in the first place with the motion of bodies in a medium which offers resistance proportional to the velocity, or to the square of the velocity, of the moving bodies, either with or without interference from gravity, or from central forces. The properties of fluids, whether incompressible or gaseous, and their pressure on immersed solids, with obvious applications to the case of the atmosphere, are next taken up. A section on the motion of pendulums in resisting media embodies Newton's experimentally determined result that the masses of pendulum bobs vary as their weights. Newton attempts also, though with limited success, to deal with hydrodynamical problems such as that of finding the resistance to the motion of a sphere, and of more complicated figures, through a fluid. A section is devoted to wave-motion in elastic fluids, the velocity of propagation being calculated for fluids of given elasticity and density. Newton attempted to apply this result in order to calculate the velocity of sound in air, but he recognized that there was a discrepancy between the calculated and the observed values of this quantity, which he attributed (erroneously) to the particles having a finite size. The second Book concludes with an account of viscosity, which leads to a refutation of Descartes' theory that the planets are carried round the Sun by the motion of a vortex in a fluid filling all space—a theory which was almost universally accepted in the time of Newton's youth. Newton showed that a vortex could not impart to a planet a motion in accordance with the several Laws of Kepler without the velocity of its parts simultaneously obeying several mutually contradictory laws.

Newton is careful to point out that the several physical assumptions made in various parts of the first and second Books (e.g. those relating to the corpuscular nature of light, the law of resistance of fluids, the repulsive forces between the particles of an elastic fluid, etc.) must be taken as leaving open the question of the true physical nature of these things.

Among the astronomical applications of the third Book the following results are particularly worthy of note.

Newton begins by setting forth the evidence that the bodies of the solar system move in accordance with the Copernican theory and with the Laws of Kepler, and in orbits which are determined by mutual gravitation. He lays it down as an hypothesis that the centre of the solar system is fixed; but he recognizes that this centre should be identified with the centre of gravity of the solar system, and not with the Sun. For, under the attractions of the planets, the Sun itself must be in motion relatively to this point, though never receding far from it. The most considerable attraction exerted upon the Sun is that due to Jupiter, the most massive of the planets; and by reference to this attraction Newton was able to account for a discrepancy in Kepler's Third Law as applied to this planet. Newton had devised a simple method of comparing the masses of any of the planets which possess satellites with the mass of the Sun, from a knowledge of the periods of the planet and satellite, and of the radii of their respective orbits. The Sun's pull upon the planet, and the planet's pull upon its satellite (as given by Huygens' formula) enable the attractions of these two bodies (and hence their masses) to be compared.

It had been discovered by Richer in 1672, and by Halley five years later, and afterwards by a number of other observers, that the force of gravity is less intense near the Equator than in higher latitudes, so that pendulum clocks tend to go slower there. This discovery suggested that the Earth might not be a perfect sphere, but perhaps a spheroid flattened at the poles—a surmise favoured by the noticeable flattening of Jupiter. Newton assumed provisionally that the shape of the Earth might correspond to a state of equilibrium between gravitational cohesion and the centrifugal tendency set up by the Earth's rotation. He seeks in the third Book of the *Principia* (III, 19) to calculate the Earth's ellipticity on this assumption. He considers an ideal canal of fluid passing from one of the poles to the centre of the Earth, and another passing from the centre to a point on the Equator. He finds in what proportion the lengths of the two canals should stand to each other in order that the two columns of fluid should be in equilibrium under these opposing pressures. The weight of the Equatorial column is partly neutralized by a centrifugal tendency, and it is hence longer than the polar column, the calculated difference in the lengths of the two columns enabling the ellipticity of the Earth to be estimated. The calculation was a difficult one, and Newton's numerical result was not very accurate. It gave about half the correct ellipticity, owing to his having neglected the self-attraction of the Equatorial bulge. It assumed that the Earth is composed of homogeneous shells of matter, which is apparently not true. But the general conclusion that the Earth bulges measurably in the Equatorial region was a valuable result later confirmed by direct measurements (see Illustr. 99).

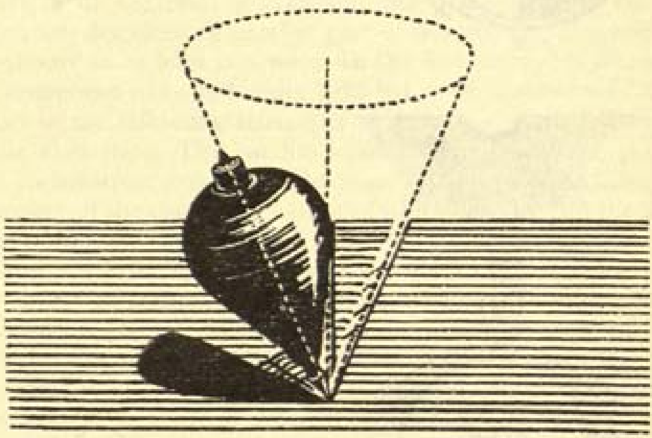


Illustr. 99.—Explanation of the Spheroidal Form of the Earth. The Circular Strip of Brass *a*, when whirled, looks like a Globe bulging at the Equator

The discovery that the Earth is flattened at the poles enabled Newton to account for the precession of the equinoxes. This phenomenon, first clearly recognized by Hipparchus (150 B.C.), could be represented by supposing the Earth's axis of rotation to describe a cone slowly in space. Newton showed that, since the Earth is not exactly spherical, the attractive force of the Moon has a tendency to turn the Earth so as to bring the plane of its Equator into coincidence with that of the Moon's orbit. This effect, combined with the Earth's rotation, imparts to the axis just such a conical motion as that required by observation. A similar effect due to the Sun is combined with that due to the Moon; and Newton predicted that there would be minute fluctuations in the precession, of the type detected, some fifty years later, by Bradley.

By reference to gravitational principles Newton was further able to give an explanation of the more familiar tidal phenomena, but an adequate treatment of this complex problem was beyond his reach. He recognized, however, that the tide-raising force of the Moon is more considerable than that of the Sun, the highest tides occurring at new and full Moon, when the two bodies reinforce each other's attraction; and the lowest tides occur at quadrature, when the Sun and Moon act against each other (see Illustr. 101). By comparing the respective heights of the tides under these two different circumstances, Newton sought to compare the mass of the Moon with that of the Sun, and hence with that of the Earth, but he fell into serious inaccuracy in consequence of the many difficulties incident to this method.

In the third Book, the lunar inequalities, already touched upon,

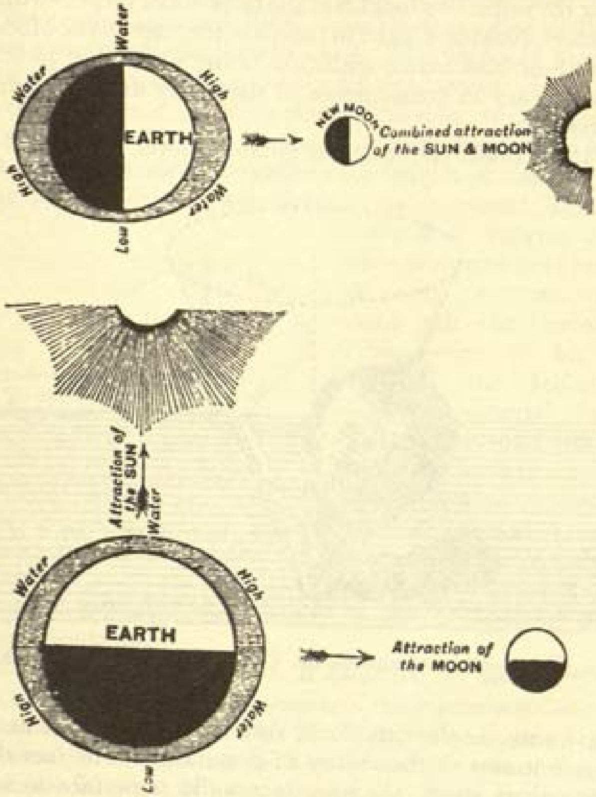


Illustr. 100.—Diagram to Illustrate the Conical Motion of the Earth's Axis

as we have seen, in the first Book, receive more detailed numerical treatment in terms of the theory of gravitation. The fact that the Moon, librations apart, always presents the same face to us is referred to the tide-raising force exerted by the Earth upon the Moon and controlling its rate of rotation.

An important section of the *Principia*, in view of the development which it afterwards received from Halley, was that devoted to comets. These bodies, if they move under the gravitational attraction of the Sun, must describe conics with the Sun in one focus. Newton showed that the observed motions of the comet of 1680, and of a number of others, were indeed consistent with their moving in parabolas or elongated ellipses—it was impossible to say which from the limited extent of its orbit over which one of these bodies could be followed. Thus comets, which a century before had been regarded as transient and capricious atmospheric phenomena, were brought under the law of universal gravitation.

In the closing pages of the second edition of the *Principia* Newton discusses the nature of the power of gravity by reference to which he has explained the phenomena of the solar system with such



Illustr. 101.—The Tides and Lunar and Solar Gravitation

signal success. He holds that this power "must proceed from a cause that penetrates to the very centres of the Sun and planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes use to do), but according to the quantity of the solid matter which they contain, and propagates its virtue on all sides to immense distances, decreasing always in the duplicate proportion of the distances. . . . But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy." In his closing words, however, he hints at the possibility of accounting for gravity, electrical attractions, etc., by reference to some all-pervading medium.

The publication of the *Principia* did not mark the end of Newton's services to dynamical astronomy, for under Halley's persuasion he continued his efforts to improve his lunar theory, with the aid of observations made by Flamsteed at Greenwich.

Newton's greatest contributions to astronomy consisted in his establishment of theoretical mechanics and his formulation of the principle of universal gravitation. But his researches in optics, which are described in another part of this volume, also bore upon astronomy in at least two ways. In the first place, his discovery of the composite nature of white light led to his discovery of the real defect of the refracting telescopes of that time, namely, their chromatic aberration. This, as has already been explained, prompted him to construct reflecting telescopes. In the second place, by his discovery of the composite nature of white light he laid the foundations of modern spectroscopy.

Newton's *Principia* is commonly described as the greatest work in the history of science. Certainly no other scientific masterpiece has exercised a greater influence upon contemporary and subsequent thought. For more than two hundred years it formed the basis of all astronomical and cosmological thought. It was a stupendous achievement to show in detail how the same principle of gravitation and the same laws of motion apply to the smallest particles of terrestrial matter and to the largest celestial bodies, to phenomena of obvious regularity, and also to such seemingly irregular happenings as the tidal movements of water and the fiery rush of comets. Small wonder that the phenomenal success of Newtonian mechanics so impressed even workers in such vastly different fields as those of psychology, economics, and sociology that they attempted to follow mechanical or quasi-mechanical models in the solution of their several problems. But with the advent of Einstein and Relativity Newtonian mechanics has apparently received a check. There is no finality in science. But, on the other hand, if great scientific achievements are never final, they are also never futile. In the view of some of those who are most competent to judge, the new methods have not come to destroy but to supplement and to fulfil the great physical synthesis achieved by Newton.

(See L. T. More, *Isaac Newton*, 1934; *Sir Isaac Newton, 1727–1927*, History of Science Society, 1928.)

CHAPTER VIII

ASTRONOMERS AND OBSERVATORIES IN THE AGE OF NEWTON

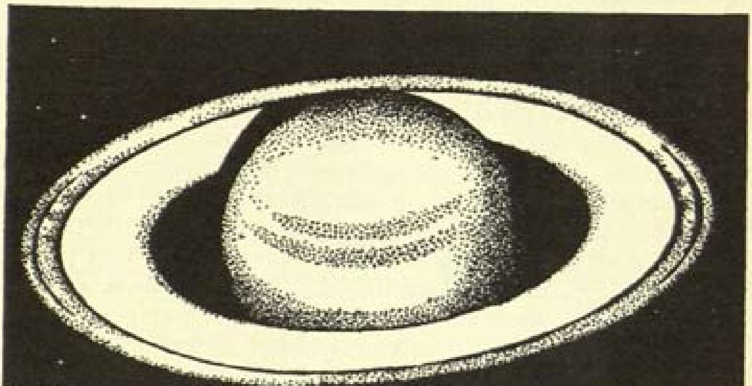
In order to complete the story of the progress of astronomy in the age of Newton it is necessary to give an account of his chief contemporaries and of the work done at the Paris and Greenwich Observatories, with one or other of which most of them were associated in some way. Of the astronomers to be dealt with in this chapter, Huygens, Picard, Auzout, and Cassini were connected with the Paris Observatory. So was Römer to some extent, though most of his work was done at Copenhagen. Flamsteed and Halley were intimately connected with the Greenwich Observatory. Hevelius had his own observatory in Danzig (see Illustr. 117, p. 181).

Huygens

Christian Huygens was born April 14, 1629, at The Hague, the son of Constantijn Huygens, a distinguished poet and diplomatist. He studied at Leiden and Breda, and early began to make valuable contributions to mathematics, applied mechanics, astronomy, and optics. He travelled considerably, and visited England on several occasions. He was made a member of the Royal Society; and in 1666 he accepted an invitation to Paris to become a member of the newly formed *Académie des Sciences*, remaining there until 1681, when he returned to Holland on account of ill-health. The revocation of the Edict of Nantes in 1685 led him to remain in Holland, where he continued his researches until his death on June 8, 1695. Huygens learned correspondence was very extensive, and fills ten volumes of the complete edition of his works now in course of preparation. Newton expressed great admiration for the genius of his contemporary, whom he called *Summus Hugenius*, and from whose works he derived much inspiration; and Huygens, though nearly sixty when he read the *Principia*, immediately embraced its doctrines.

Reference has already been made to some of Huygens' most important contributions to astronomy, namely, his successful application of the pendulum to regulate clocks, and certain improvements in the telescope. These improvements enabled him to make several interesting new discoveries.

While he was still in Holland, Huygens, working with his elder brother, had succeeded in figuring and polishing telescope lenses with an accuracy not before attained; and he was rewarded by the solution of a long-standing astronomical mystery. Galilei, in 1610, had observed Saturn through his telescope and had noticed two remarkable appendages to the planet. He had found that they varied in an obscure manner with lapse of time, and disappeared occasionally. Since then they had been studied by several astronomers without their true significance being understood, though Hevel showed that the changes which they underwent occurred periodically. Huygens, upon turning his improved telescope on the planet, in 1655, recognized that the peculiar appearance of Saturn was due to its being encircled by a thin plane ring, inclined to the ecliptic (Illustr. 102).



Illustr. 102.—The Rings of Saturn

In the same year he discovered the first of the numerous satellites of the same planet. He announced these discoveries in the form of anagrams in the first instance; but a few years later, having studied the appearance of the planet from many aspects, he published his book, *Systema Saturnium* (1659), in which he describes his discoveries, defines the position of the ring, and explains the phenomena of its appearance and disappearance. In the course of these observations Huygens employed a *micrometer*, which we shall describe along with those of his colleagues of the Paris Observatory. He was also the inventor of the telescopic eyepiece which bears his name, and which consists of two convex lenses whose focal lengths and distance apart are chosen so as to reduce the defects of the image to a minimum.

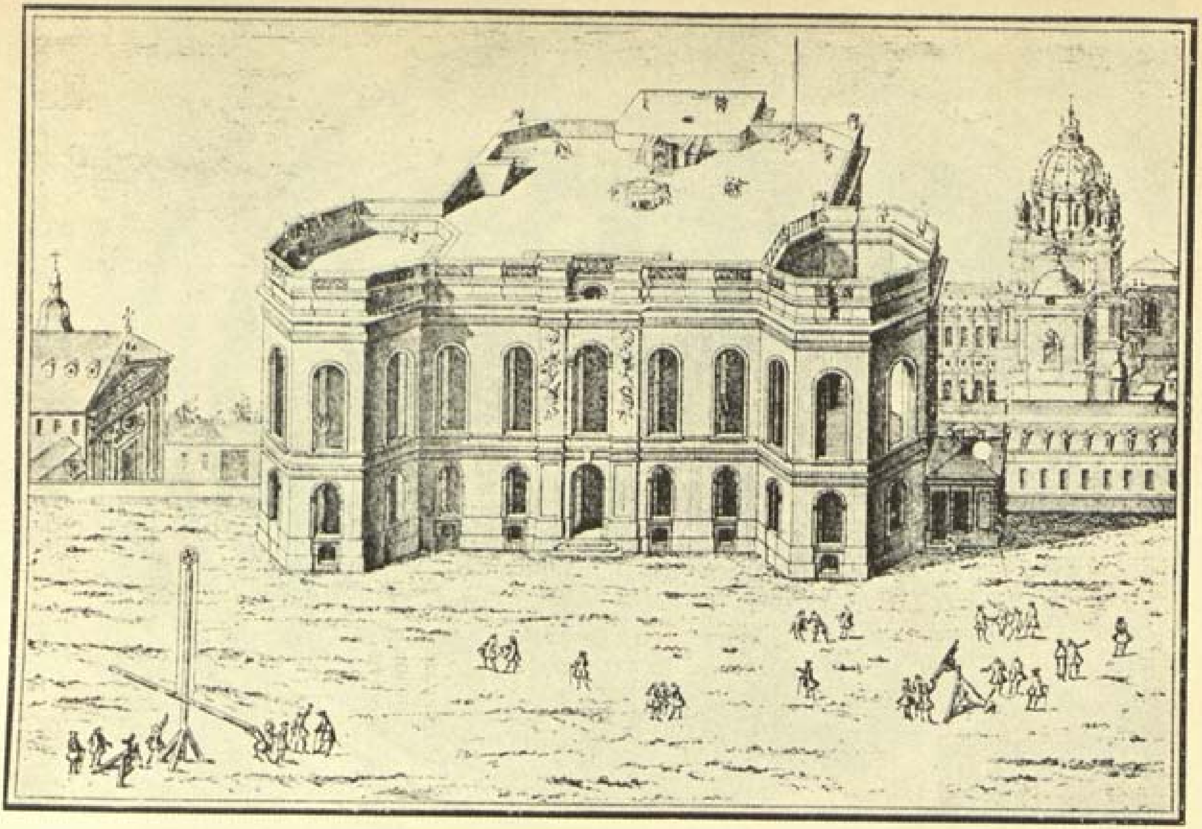
Among the results of importance to astronomy contained in the *Horologium Oscillatorium* were the familiar formula connecting the period of vibration of a simple pendulum with its length and the acceleration of gravity; and the equally important expression for the centripetal force necessary to maintain a body in uniform circular motion—a result obtained independently by Newton. Huygens, while at Paris, determined the length of a simple pendulum whose period of vibration was one second, and he applied his pendulum formula to deduce a more accurate value for g, the acceleration of gravity, than could be obtained by direct measurements of this quantity. He obtained for $g/2$, the distance of free fall of a body from rest in one second, the value 15 Paris feet, 1 inch, approximately; and he proposed the length of the seconds pendulum as a unit of length, but without success. His researches on rotating bodies led Huygens to anticipate the flattening of the Earth and the falling off in gravity with decrease of latitude, which were subsequently established.

Besides accurately estimating, from the length of the seconds pendulum, the gravitational acceleration of falling bodies, Huygens further sought to give a mechanical explanation of gravity, as he had done for light. His speculations on this problem are to be found in his *Discours de la cause de la pesanteur*, which appeared in 1690 as a supplement to his treatise on light. Huygens' point of view was that gravity should not be attributed to a quality or propensity of bodies, but should be explained, like every other natural process, in terms of motion. He acknowledges that his hypothesis is closely related to that of Descartes, who had tried to conceive gravity as due to the motion of a material vortex surrounding the Earth. Gravity, says Huygens, operates in so mysterious a manner that the senses are unable to discover anything about its nature. He points out that, while its operations were previously ascribed to the inherent qualities of bodies, this amounted merely to introducing obscure principles without explaining the causes. Descartes, on the other hand, had recognized that physical processes should be referred to concepts which do not transcend our power of comprehension; and for Huygens, as for Descartes, such concepts were those of matter, devoid of qualities, and its motion.

In these inquiries Huygens started out from the following experiment. He covered the bottom of a cylindrical vessel with small fragments of some solid substance (e.g. sealing-wax). He then partly filled the vessel with water and whirled it about its axis by means of a revolving table, whereupon the sealing-wax travelled out to the sides of the vessel. The table and vessel being suddenly brought to a standstill, the water continued to rotate for some little time, but it was observed that the bits of sealing-wax, being checked by their contact with the bottom of the vessel, were driven in spiral paths towards its centre. Huygens supposed that, just as the water rotated in the vessel, so an aether, which must be regarded as

Illustr. 103

Christian Huygens



The Paris Observatory (Front)

incomparably more fluid than water, must rotate about the Earth. Any gross bodies situated in this aether will not, as the experiment shows, share its rapid motion, but will be thrust towards the centre of that motion. Gravity is therefore "the action of the aether which circulates about the centre of the Earth striving to travel away from the centre and to force those bodies which do not share its motion to take its place." Huygens even ventured to estimate the rate at which this circulation of the aether must take place. Several more recent attempts to furnish mechanical explanations of gravity are based in the last resort upon the notions here developed by Huygens. (See A. E. Bell, *Christian Huygens*, London, 1947.)

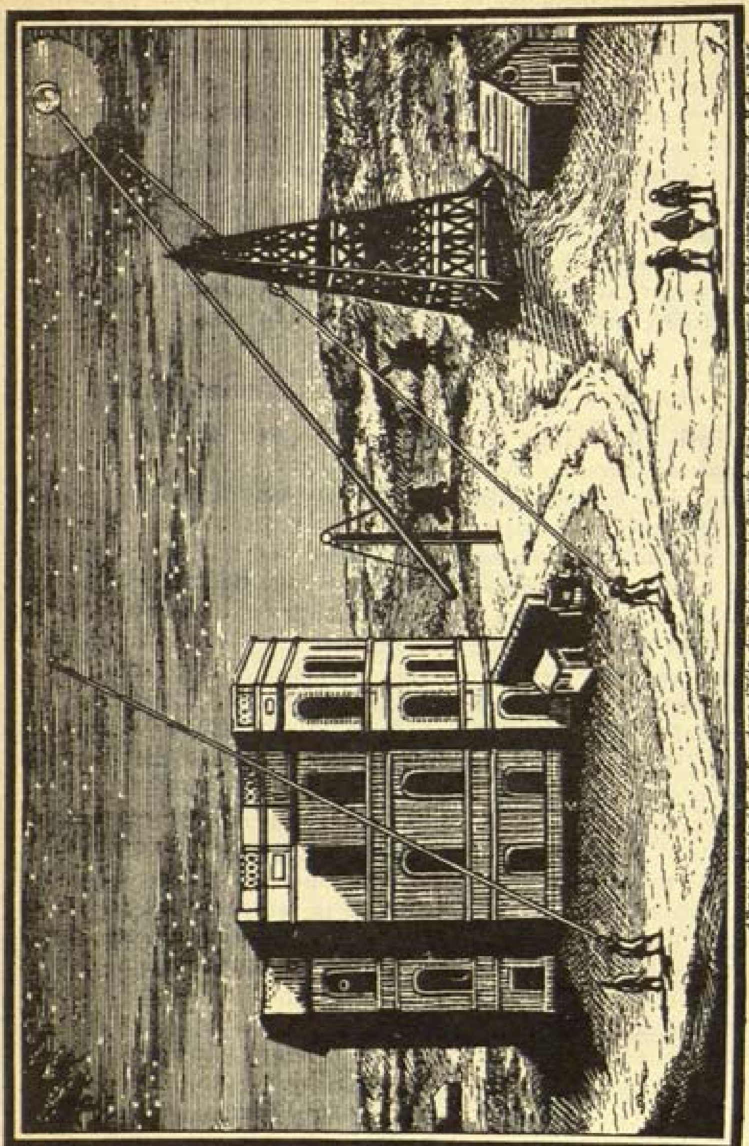
Huygens' wave-theory of light, which had many astronomical applications, will be dealt with in a later chapter.

The Paris Observatory: Picard, Auzout, Cassini

The Paris Observatory was, as we have seen, an offshoot of the *Académie des Sciences*. The foundations were laid in 1667, and the building was completed in 1672. It was built with lofty windows and a flat roof; but the design soon proved unsuited to the needs of the observers, whose methods of work were rapidly developing along new lines. The building was used, not only for astronomical purposes, but also for physical experiments of all descriptions, the well of the staircase, which extended from roof to cellar, affording excellent opportunities for studying the behaviour of falling bodies.

The first astronomers who worked here included Huygens, Jean Picard (1620–82), Adrien Auzout († 1691), and Giovanni Domenico Cassini (1625–1712). Their most notable services to astronomy lay in the increased refinement which they introduced into the processes of observation by the application of the telescope to the older instruments of precision, and by the use of the pendulum clock.

At the time when the Paris Observatory was being established many astronomers were endeavouring to increase the power, and improve the quality, of their telescopes by using object-glasses of great focal length. This tendency led to the construction of instruments of enormous size, and means had to be devised to obviate the flexure occurring in such long tubes. One way was to dispense with the tube altogether, as in the *aerial telescope* devised by Huygens (Illustr. 41). A telescope in which the object-glass and the eyepiece were in separate pieces was independently introduced by Cassini. The Academicians, however, also had several instruments in which the object-glass and eyepiece were fixed to the ends of a yard-arm which was slung by cords and pulleys from a mast on the terrace of the Observatory. This mast was not strong enough for the larger



Illustr. 105.—The Paris Observatory (Side)

telescopes, and a disused wooden water-tower, 120 feet in height, was transferred from Marly, and was used until long-focus lenses went out of fashion.

The evolution of the telescope as an instrument of precision down to the time of Picard is worth consideration.

The fundamental operation in all precise astronomy is that of accurately measuring the angle subtended at the observer's eye by two given points on the celestial sphere. Hence the fundamental instrument of precise astronomy has always consisted essentially of a circle or arc graduated in angle, and traversed by a radial index carrying sights and pivoted at the centre of the circle. The observer makes the plane of the circle coincide with the plane determined by his eye and the two points whose separation is required; the index is then successively directed to the two points, and the required angle is read off the graduated scale. Now the accuracy with which this operation can be performed is restricted, in the last resort, by the limited resolving power of the human eye. If the angle to be measured is less than about 2′, the two points appear as one and the same point, so that all naked-eye determinations of celestial angles are necessarily subject to an uncertainty of about that order.

The invention of the telescope early in the seventeenth century, and more especially the invention of the "Keplerian" or "astronomical" telescope (which was first used by Scheiner about 1618, and which had a common focus of object-glass and eyepiece in which wires could be placed in the same focal plane as the image of a distant object), afforded a means of magnifying the angles under which the eye sees distant objects, and, therefore, of reducing the *proportional* uncertainty in its estimates of those angles. The telescope, however, was not systematically used in precise astronomy until about fifty years after its invention. During that period its riumphs were restricted almost entirely to the field of descriptive astronomy, and necessarily so.

When at length the telescope *was* applied to precise work, it could be used in either of two ways.

(1) It could be attached as the radial index of the graduated arc. For this purpose it was necessary to equip the telescope in such a way that it could define a precise direction in space, and this was usually effected by fixing in the focal plane of the object-glass a pair of hairs intersecting at right angles. These cross-hairs, being then in the same plane as the images of the stars, etc., formed by the object-glass, were simultaneously in focus with those images when the eyepiece was properly adjusted. The line of collimation of the telescope, i.e. the line joining the cross to the optical centre

of the object-glass, played the part of the line of sights in the primitive instrument; while, besides the magnification of the angles to be measured, there was the additional advantage that the images and the cross-hairs could be simultaneously viewed with the same adjustment of the eye. This had not been possible with the "open sights," of Tycho Brahe and his successors, in which it was necessary to focus the eye alternately on the near sights and the distant star. This application of the telescope was made by Picard in 1668. (See C. Wolf, *Histoire de l'Observatoire de Paris de sa fondation à 1793*, p. 136, and Le Monnier, *Histoire Céleste*, Paris, 1741, pp. 1, 2, 11, 31.)

(2) The telescope could be used, without the need of an external graduated circle, to measure the small angular separations of objects simultaneously visible in the field of view of the instrument. For this purpose it had to be fitted with a *micrometer*—a mechanism for measuring such small angles. This, too, was perfected at the Paris Observatory.

MICROMETERS

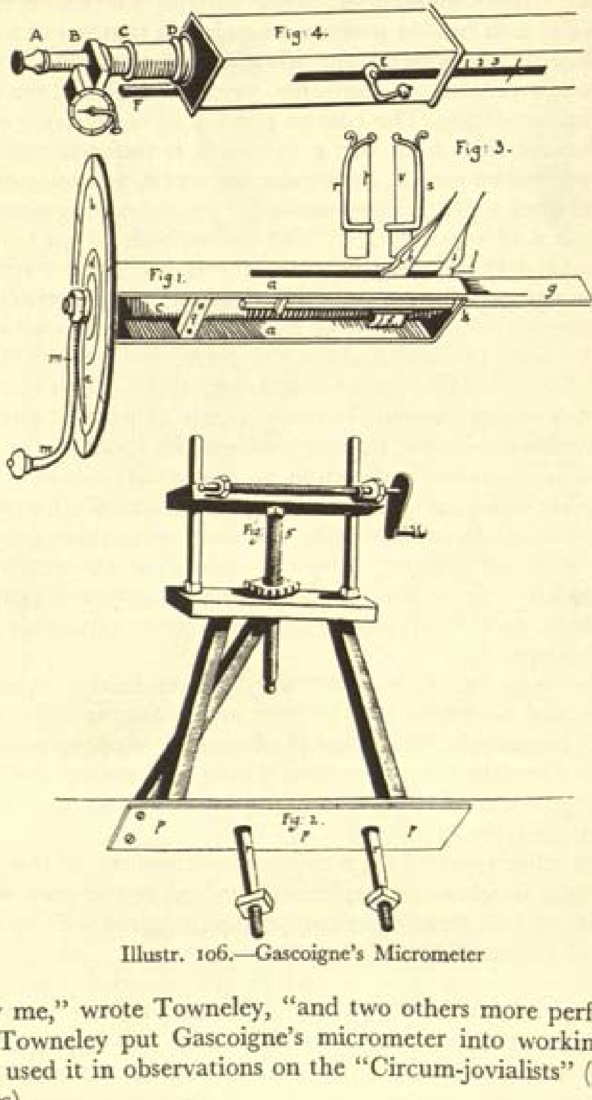
Both these applications of the telescope seem, however, to have been anticipated by William Gascoigne about 1640. They are described in letters from Gascoigne to Oughtred in that and the following year (S. P. Rigaud, *Correspondence of Scientific Men of the Seventeenth Century*, etc., Oxford, 1841, 1862, Letters 19 and 20):

"I have either found out, or stumbled on . . . a most certain and easy way, whereby the distance between any the least stars, visible only by a perspective glass, may be readily given, I suppose to a second; affording the diminutions and augmentations of the planets strangely precise. . . ."

Gascoigne seems to have discovered the principle accidentally "when it pleased the All Disposer, at whose direction a spider's line drawn in an opened case could first give me by its perfect apparition, when I was with two convexes [presumably a convex object-glass and a convex eyepiece] trying experiments about the Sun, the unexpected knowledge."

Gascoigne's letters also clearly describe the application of the telescope to a graduated quadrant.

Gascoigne fell as a Royalist at the battle of Marston Moor, in 1644, and his inventions were forgotten for the time being. However, about twenty years later, a letter from Auzout to Oldenburg was published in the *Philosophical Transactions* (Vol. I, No. 21) claiming that the micrometric method which Auzout and Picard had introduced, and which is described below, was giving the diameters of the Sun, Moon, and planets correct to a few seconds. And this letter prompted Richard Towneley to write to Dr. Croune stating that, even before the time of the Civil War, Gascoigne (some of whose papers had fallen into Towneley's hands) had devised an instrument as sensitive as that of the French astronomers, and had used it for some years. "The very Instrument he first made I have



Illustr. 106.—Gascoigne's Micrometer

now by me," wrote Towneley, "and two others more perfected by him." Towneley put Gascoigne's micrometer into working order, and he used it in observations on the "Circum-jovialists" (Jupiter's satellites).

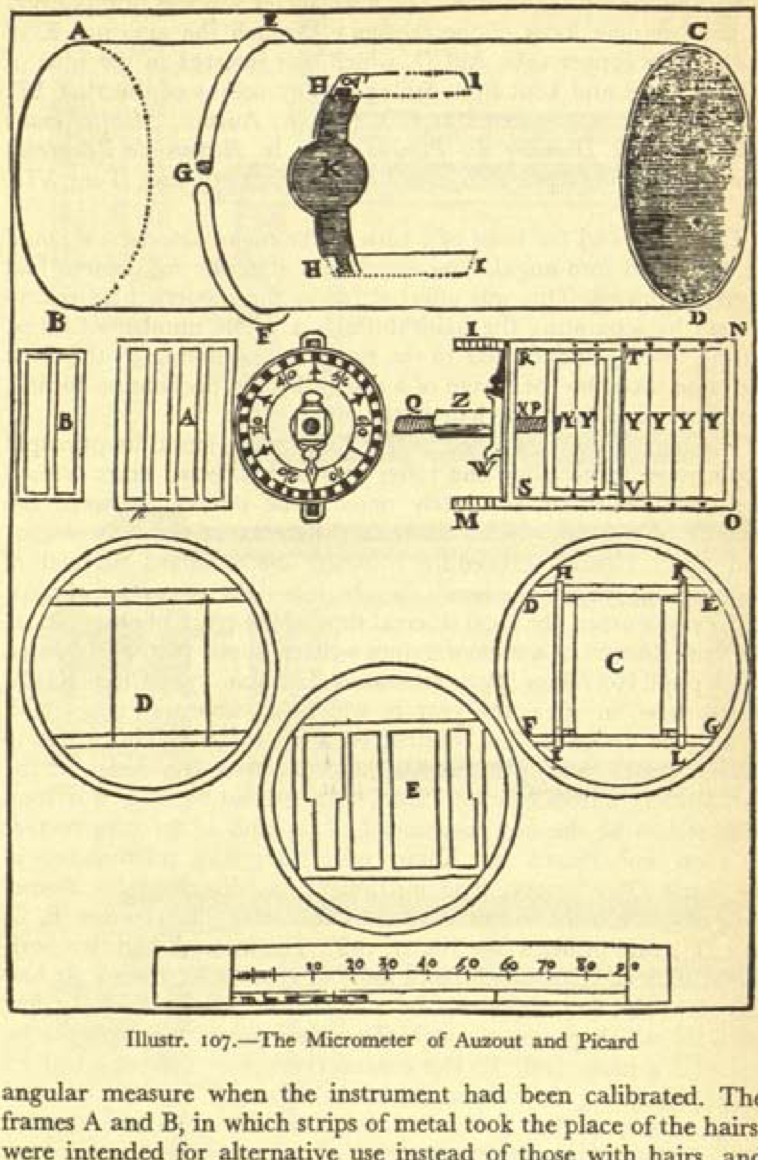
A description of Towneley's micrometer, with illustrations, was prepared by Hooke and published in the *Philosophical Transactions*

(1667, Vol. II, No. 29). A general view of the instrument, with the cover removed, is shown in Illustr. 106 (1), where *aaa* is an oblong box of brass about 6 inches in length, to one end of which is screwed a brass disc *bbb* whose circumference is divided into 100 equal parts. A carefully constructed screw extends the whole length of the box and is so held in position at each end that it can be turned by the handle *mm* without any unsteadiness of motion. The third of the screw nearest the plate has twice as fine a thread as the remaining two-thirds. The coarser portion of the screw works in a socket *f* fixed to a long bar *g* to which is fastened the sight *h*, which can thus be moved, by turning the screw, nearer to or farther from the fixed sight *i*. The reading of an index *l* against a scale on the bar *g*, of which each division corresponds to one turn of the screw, enables the distance apart of the sights *h* and *i* to be read off to the nearest whole turn, while the index *e* on the brass plate shows additional hundredths of a revolution. To the socket *q*, in which the finer portion of the screw turns, is fixed the plate *ppp* (Illustr. 106, 2) which is also attached to the telescope by the screws *rr*. Turning the handle consequently causes the micrometer to move relatively to the telescope at half the speed of the moving sight and in the opposite direction, so that points midway between the movable sights may always lie upon the axis of the telescope. Illustr. 106 (3) shows how hairs *t*, *v*, fixed in suitable frames *r*, *s*, may be used for sighting purposes instead of the edges of the sights *h* and *i*. Illustr. 106 (4) shows the micrometer in position on the telescope, and Illustr. 106 (5) shows the *rest* or adjustable support of the telescope.

Another type of micrometer was independently invented by Huygens, and was described by him in his *Systema Saturnium*. He measured the angular diameters of planets by slipping brass plates of various breadths across the focal plane, and noting the breadth of plate required to hide the planet, from which its angular diameter could ultimately be calculated.

Various other types of micrometer were invented at this period, but the type which in principle has survived to our own day was one similar to Gascoigne's but independently introduced by Auzout and Picard about 1666.

The micrometer of Auzout and Picard consisted essentially of two frames LMNO and RSTV, of which the latter could be moved to and fro in grooves cut in the former. This was done by turning the screw PQ to which was attached a pointer moving over the circle W which was graduated to show sixtieths of a complete turn. Each frame carried a system of parallel hairs YY fixed at equal distances apart. The image to be measured was comprised between a convenient pair of hairs on the two frames, and the separation of these two hairs was measured in whole turns and a fraction of a turn of the screw. These arbitrary units could be converted into



Illustr. 107.—The Micrometer of Auzout and Picard

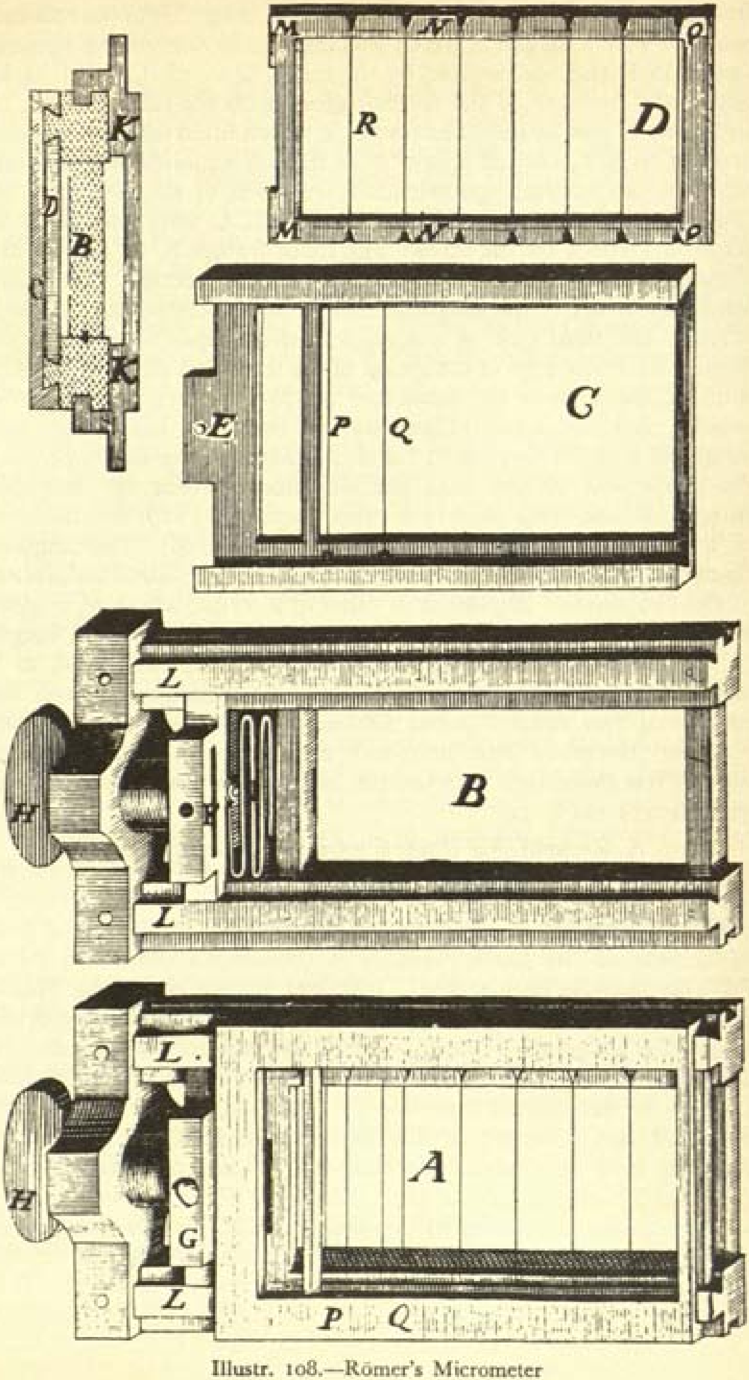
angular measure when the instrument had been calibrated. The frames A and B, in which strips of metal took the place of the hairs, were intended for alternative use instead of those with hairs, and were placed in the positions TVON and RSVT respectively. C and

D were simple forms of frames to which hairs might be attached as desired, and which might then be inserted at the common focus of eyepiece and object-glass, while E was made up of metal strips of various known thicknesses. The micrometer was slid into position at the common focus of the telescope through the aperture K of the iron or copper tube ABCD, which was inserted in the tube of the telescope and kept from falling out by means of the ring EF. The eyepiece was inserted at CD. (See A. Auzout, *Manière exacte pour prendre le Diamètre des Planètes*, etc., in *Histoire de l'Académie Royale des Sciences depuis 1666 jusqu'à 1699*, Paris, 1733, etc., Tom. VII, pp. 118–130.)

The turns and fractions of a turn of the micrometer screw could be converted into angular measurements after the instrument had been calibrated. This was effected (as in the modern filar micrometer) by separating the hairs through a whole number of turns, setting them perpendicular to the Equator and noting by the clock the time taken by the image of a star of known declination to drift across the field from one hair to the other.

The pendulum clock, used in this calibration, found its principal application in enabling the times at which selected stars crossed the meridian to be accurately noted. The interval between the transits of two stars measures their difference of right ascension; and upon Picard's procedure is based the standard method of determining, on the one hand, the absolute right ascensions of stars, and, on the other, the local sidereal time of the place of observation.

Olaus Römer, in a memorandum written about 1676 and quoted by his pupil Horrebow (*Basis Astronomiae*, Havniae, 1735, Chap. XIII), relates how, in 1672, the year in which he came to France with Picard, he designed and constructed a micrometer. He claims to have invented this instrument without previous knowledge of the one already introduced by Picard and Auzout, and it was soon admitted to be the best instrument of its kind so far constructed. By 1676 both Picard and Römer were using such micrometers in the Paris Observatory. The instrument, as described by Römer (*loc. cit.*), consisted essentially of three rectangular frames B, C, and D, made of brass (see Illustr. 108). The frame B had two horizontal bars L, L, in which three pairs of grooves were sunk. It had also three fixed cross-pieces, and a sliding stop F moved by a fine screw H which passed through the outermost of the cross-pieces, and had a blunt end. To the central cross-piece was attached an M-shaped spring I pressing the movable stop F against the end of the screw, so that F moved steadily as the screw was turned, and inaccuracies due to the wearing of the screw were minimized, which was one of the principal merits of this instrument. The



Illustr. 108.—Römer's Micrometer

frame C carried a thread Q and a metal strip P, for alternative use, and had a tongue E which was inserted in the oblong mortice shown in F and was secured by the screw G, so that C slid backwards and forwards in the exterior grooves on the sides of L, L, as the screw H was turned. The frame D, which fitted into the interior grooves on L, L, carried a number of threads equidistant from each other by an amount equivalent to ten turns of the screw (or at other convenient distances), and the sides L, L, were graduated to serve as a check on the screw. The three frames B, C, and D are shown fitted together in the figure A, and, in section, in the top left-hand corner of the diagram. KK is part of the telescope which fits into the third pair of grooves and thus grips the micrometer. Round the outer edge of the socket of the screw is a circle graduated into ten equal parts for measuring tenths of a turn of the screw, smaller divisions being judged by the eye. The instrument was mounted with its threads in the focal plane of the telescope, and the screw was turned until the distance between the movable thread and one of the fixed ones exactly coincided with the diameter of a planet (or other small arc to be measured). The angular diameter of the planet was calculated from the measured separation of the two threads (expressed in turns of a screw, whose equivalent in linear units had to be known) combined with the focal length of the telescope; or else the instrument could be calibrated, as is now the practice, by measuring with it the distance between the images of two distant points subtending a known angle at the observer. Horrebow later used such an instrument at Copenhagen until it was destroyed, with the rest of Römer's instruments, by the great fire of 1728.

(See J. A. Repsold, *Zur Gesch. d. astron. Messwerkzeuge*, 1908.)

PICARD

Picard obtained an improved value for the length of a degree of latitude on the Earth's surface in the neighbourhood of Paris. For this purpose he measured (1669–70) an arc extending from a point near Amiens to a point near Paris, and astronomically determined the difference of latitude at its extremities. To increase the accuracy of the survey, he connected the arc to be measured with a carefully determined base-line by triangulation—a method first proposed and practised by the Dutch mathematician Willebrord Snell in 1615–17 (*Eratosthenes Batavus*, Leiden, 1617). The publication of Picard's results, in 1671, may have been one of the factors which stimulated Newton to proceed with his researches on gravitation. (Picard's researches are described in his *Ouvrages de Mathématique*, La Haye, 1731.)

CASSINI

Picard's influence on the work undertaken at the Paris Observatory gradually declined with the rise of the Italian Cassini, who, soon after his arrival in 1669, became virtually the Director of the Observatory. As, however, he never held that title officially (see C. Wolf, *Histoire de l'Observatoire de Paris*, Paris, 1902, chap. xiii), conditions at Paris in the early years were rather different from those obtaining at Greenwich. There was no central authority and no fixed programme of work; each observer worked at what he pleased, and very often at his own home, as the Observatory was rather out of the way; hence the Parisian contribution to astronomy was not so solid as that of Greenwich, until the reorganization of the Observatory of Paris after the Revolution.

While still in Italy, where he was a highly placed civil engineer, Cassini made a name as an astronomer by his measurement of the periodic rotations of the planets Mars and Jupiter, and by his construction of tables defining the motions of Jupiter's satellites. At Paris he continued his observations, which were rewarded by his discovery of four satellites of Saturn additional to that found by Huygens—two of them by means of a tubeless telescope of the aerial type. He also observed that Saturn's ring is divided into two concentric portions by a cleavage still known as the "Cassini Division" (see the division between the outer and inner rings in Illustr. 102, p. 163); and he correctly suggested that the ring is composed of an assemblage of small satellites to the planet. He was also among the earliest to note the white polar caps of the planet Mars, and to compare them with the ice-covered polar regions of the Earth.

Cassini was one of the astronomers who collaborated with Jean Richer in the determination of the parallax, or distance, of Mars, at the opposition of 1672. Richer's observations of Mars from Cayenne, when compared with those of his colleagues at Paris, gave the alteration in the apparent direction of the planet consequent upon a displacement of the observer from Paris to Cayenne. The determination of the planet's distance thus resulted from the solution of a triangle whose base and base-angles were known. From the combined observations Cassini was able to deduce the distance of the planet and hence that of the Sun, which was the real objective. He estimated the Sun's parallax as being of the order $9''\cdot5$, which corresponds to a distance of about 87,000,000 miles. This estimate compares well with the modern value (based on the 1901 opposition of Eros) of $8''\cdot8$, corresponding to a mean distance of 92,800,000 miles; and it was a great improvement on the gross under-estimate of the Sun's distance which had come down from Alexandrian times to the seventeenth century. (The Richer–Cassini determination of the Sun's parallax is reported in Cassini's *Divers Ouvrages d'Astronomie*, La Haye, 1731, pp. 129 ff.)

Cassini joined in the prevailing search for annual stellar parallax, which was to be expected on the Copernican hypothesis; but his methods were not sufficiently refined, and the errors due to atmospheric refraction were too large and too uncertain for his observations to be of any value for this purpose.

Towards the end of his life, Cassini became involved in the controversies concerning the shape of the Earth which arose out of the discovery, by Richer and Halley, of the shortening of the seconds pendulum near the Equator. Newton had rightly surmised that the Earth is flattened at the poles and bulges at the Equator, having the form of an oblate spheroid, as was noticeably the case with the rapidly rotating Jupiter. Cassini, however, maintained the view that the Earth is flattened at the *Equator*, the polar radius being greater than the Equatorial radius. This view received apparent support from several measurements of the lengths of meridian arcs carried out in France under Cassini's auspices; and it was not until the middle of the eighteenth century that the question was settled. French scientific expeditions were then sent to Peru and Lapland for the purpose of measuring meridian arcs, and the results then and since obtained concerning the shape of the meridians have borne out Newton's surmise as against Cassini's. The analogous but more difficult problem of ascertaining the precise shape of the *Equator* is still exercising geodesists.

The Cassini family enjoyed a long association with the Paris Observatory, G. D. Cassini's son, grandson, and great-grandson successively controlling the destinies of the institution down to the time of the French Revolution.

Cassini's table of Jupiter's satellites, already mentioned, was intended to serve for the accurate determination of longitudes by a method suggested by Galilei. The instant of the eclipse of one of these bodies could be predicted, with the aid of the tables, in the *standard* time of some prime meridian; the observer in some remote part of the world then noted the *local* time of the eclipse, and the difference between the standard time and the local time of the phenomenon measured the longitude of the observer from the prime meridian. It was partly in order, by this method, to measure the longitudes of important places throughout the world that the Academy of Sciences had fitted out a number of expeditions. Two of these have already been mentioned—that of Picard to Uraniborg in 1671, and that of Richer to Cayenne in 1672.

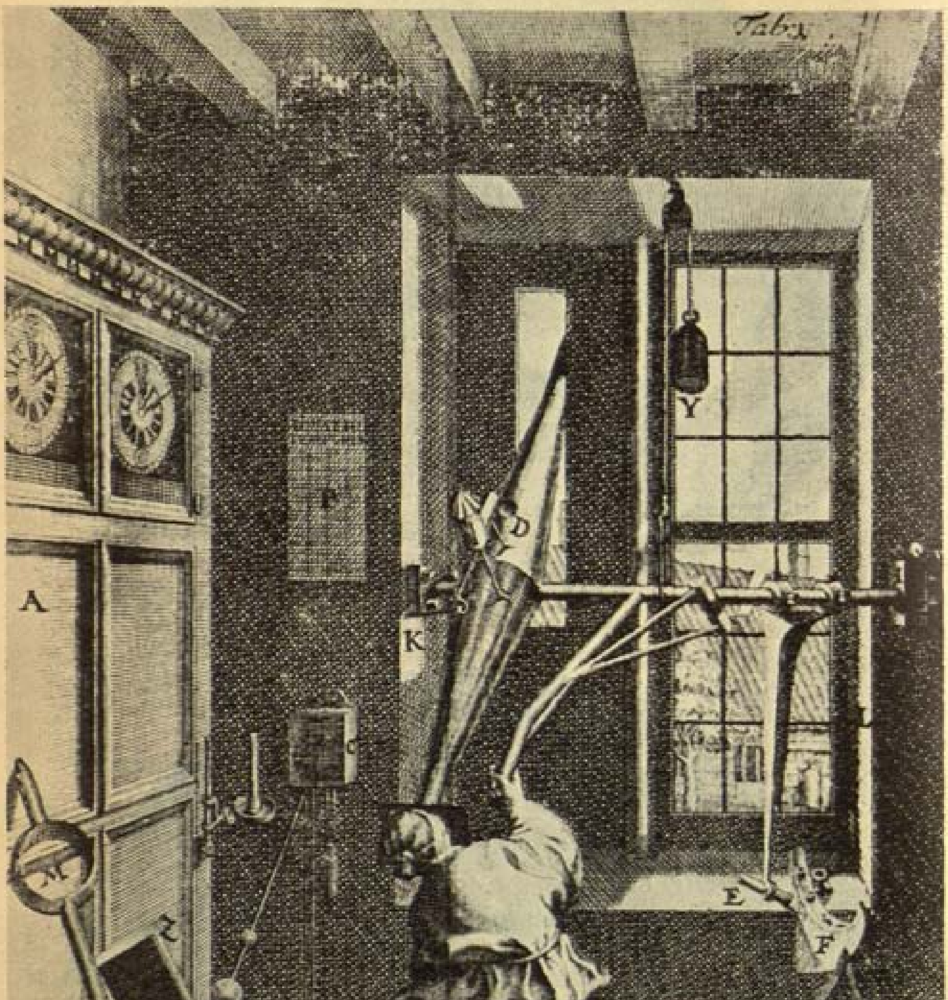
Illustr. 109

Illustr. 110

Olaus Römer

Jean D. Cassini

Illustr. 111



Römer's Transit Instrument

RÖMER

Picard's astronomical expedition to Uraniborg, in 1671, was indirectly the means of introducing Olaus Römer (1644–1710) to the Academy of Sciences at Paris. During his stay there, Römer was initiated into the observational *technique* of Picard and his colleagues, and after his return to Denmark he made an important contribution to astronomy by the invention of the transit-circle—of which, however, he may have obtained an inkling from some of Picard's instruments.

Römer observed at first from the Round Tower at Copenhagen, which King Christian IV had built in 1637 as an observatory for Longomontanus, formerly Tycho Brahe's assistant. But later Römer observed in his own house, where he set up his transit instrument about 1690. This instrument consisted essentially of a small telescope turning in the meridian about a horizontal axis lying due east and west, and having, in the common focus of object-glass and eyepiece, a web of horizontal and vertical wires. These wires were illuminated by means of a lamp, a lens, and a reflector, which directed a beam of light on to them through a hole in the side of the telescope. The instant of transit across each upright wire was recorded with the aid of a clock audibly ticking out seconds, and the time of transit across the meridian could thus be calculated. Errors of the instrument were deduced from suitable combinations of observations as in a modern observatory. With this instrument differences in the right ascensions of stars could be easily and accurately ascertained. The corresponding declinations were read off through a microscope carried round by an index perpendicular to the axis of the instrument, and travelling over a graduated circle (see Illustr. 111).

The superiority of Römer's method of observing lay in the avoidance of cumbrous instruments for measuring celestial angles. Such instruments were expensive; it required much gear and expenditure of time, and several assistants to work them, and the results immediately obtained were not the right ascensions and declinations required; these could be deduced from the observations only after much tedious calculation.

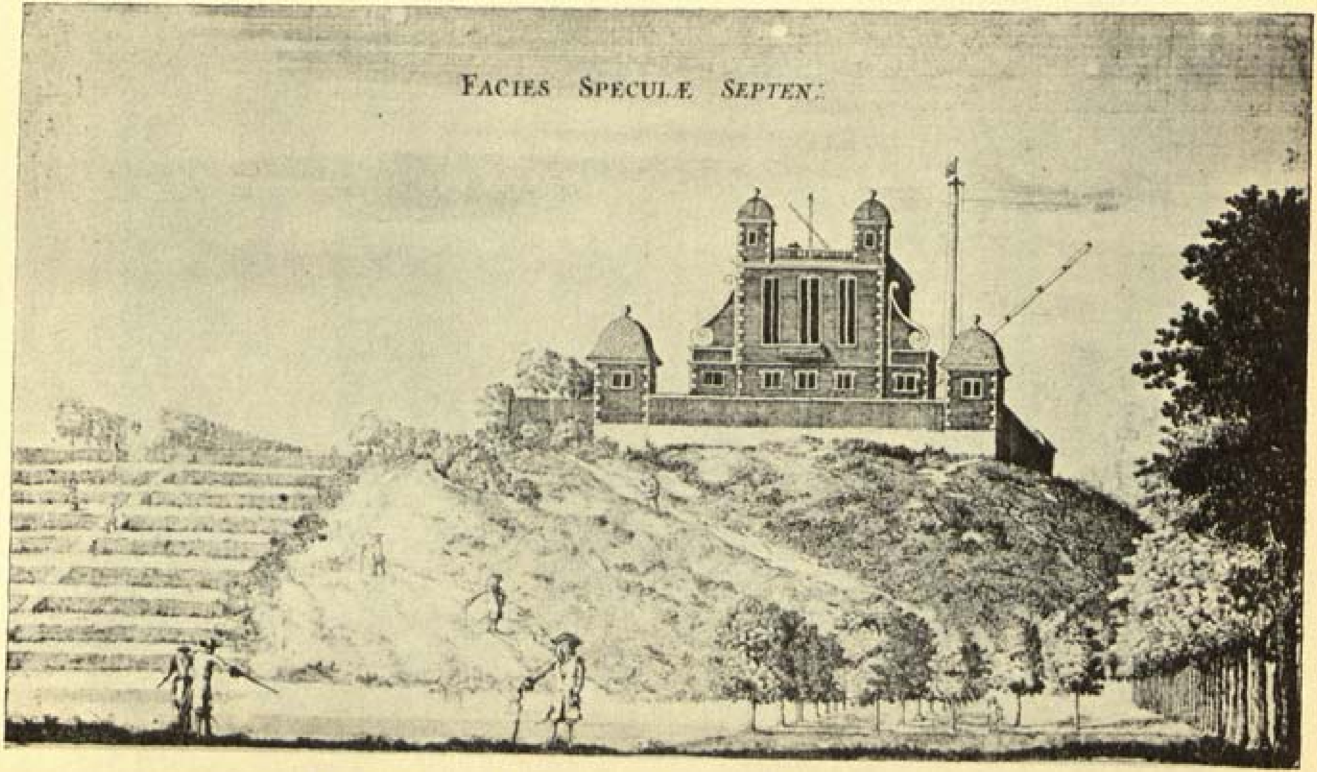
Römer's instruments, and almost all the records of his observations, were destroyed in the great fire which devastated Copenhagen in October 1728. His instruments and methods, however, were minutely described from memory, and with the aid of Römer's manuscript memoranda, by his devoted pupil Peder Horrebow in his book *Basis Astronomiae*, Havniae, 1735, from which the above particulars are derived.

(See E. Philipsen, *Olaus Römer*, Christiania, 1860.)

The Greenwich Observatory: Flamsteed

The early history of Greenwich Observatory is closely bound up with the life-story of John Flamsteed, the first astronomer to hold office there. Had it not been for Flamsteed's enterprise it is doubtful whether the Observatory would have been founded when it was, or would have achieved so much even if it had been.

John Flamsteed was born on August 19, 1646, near Derby. Ill-health interfered with his schooling and threw him much on to his own resources, and he devoted much of his youth to the private study of mathematics and astronomy. An ephemeris for the year 1670 which he prepared and sent to the Royal Society won him the friendship of the Secretary, Henry Oldenburg, whom he visited later in that year. While in London he was taken by John Collins to the Tower to see Sir Jonas Moore, the Surveyor of the Ordnance and later a distinguished mathematical member of the Royal Society. Moore gave Flamsteed a micrometer of Towneley's, and furnished him with lenses with which he shortly afterwards constructed a telescope and set up a little observatory at Derby. Here he concentrated on micrometric observations, leading to improvements in lunar theory on the lines of Horrocks' work. Flamsteed in return presented a pair of "weather-glasses" (thermometer and barometer) to Moore, who himself made a similar pair for Charles II, to whom he often talked of the young astronomer. Soon afterwards Flamsteed went to Cambridge University, where he made the acquaintance of Newton and Barrow; and having obtained the M.A. degree, he took Orders with a view to a career in the Church. In 1675, however, Moore summoned him to London to take charge of an observatory which he meant shortly to found at Chelsea College, then the property of the Royal Society. Meanwhile he got Flamsteed made a member of a Commission, which included Brouncker, Wren, and Hooke, besides Moore himself, and which had been appointed to consider a proposal made by a French nobleman, Le Sieur de St. Pierre, for obtaining longitude at sea by a method involving the precise determination of the Moon's place among the stars. Flamsteed pointed out that this method was not, even in theory, the best available, and that in practice it was out of the question, owing to the uncertainty of the contemporary lunar tables and star catalogues. His objections were reported to Charles II, who "said, with some vehemence, 'He must have them [star-places and Moon's motion] anew observed, examined, and corrected, for the use of his seamen.' . . . And when he was asked, Who could, or who should, do it? 'The person (says he) that informs you of them' " (Baily, p. 38). In March 1675 Moore gave



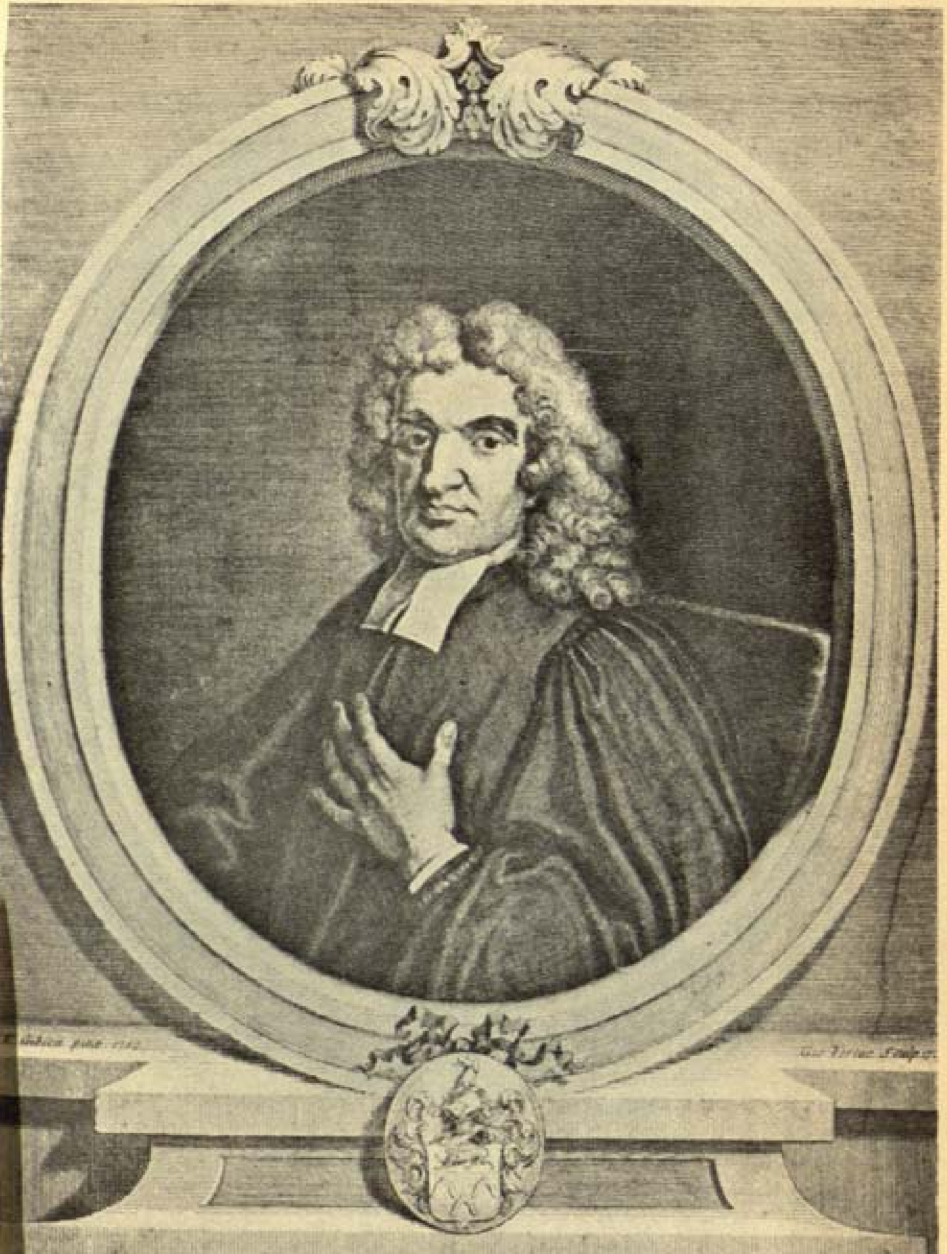
The Greenwich Observatory in Flamsteed's Time (Exterior)
(*Reproduced by permission of the Astronomer Royal*)

Illustr. 113

The Greenwich Observatory in Flamsteed's Time (Interior)

(Reproduced by permission of the Astronomer Royal)

Illustr. 114



John Flamsteed

Illustr. 115

Edmund Halley

Flamsteed a warrant from the King appointing him "Our Astronomical Observator" at a salary of £100 *per annum*. Moore's project of an observatory at Chelsea was brought forward; Hyde Park was also mentioned as a possible site, but finally Wren's suggestion of Greenwich Hill was agreed to. Charles signed a warrant establishing Greenwich Observatory on June 12/22, 1675. The foundations were laid in August, and by July of the following year Flamsteed was installed.

Before he could begin serious work, however, Flamsteed had to furnish his new observatory with instruments. He already possessed a small quadrant, and a sextant which he had had made at the Tower, and Moore gave him two clocks; but in order to complete the equipment of the Observatory he had to expend both money and labour without obtaining the recompense to which he was entitled. His instruments were mostly of the type in use at the Paris Observatory and elsewhere at this period, consisting of graduated arcs traversed by telescopic sights. Upon the graduation and calibration of the scales and micrometer-screws, which were to serve for the measurement of celestial angles, Flamsteed bestowed much ingenuity and labour, often assisted only by " an ill workman, who respected nothing but the getting of wages by his work." Flamsteed's finest instrument was a mural arc of 140 degrees which he constructed with the aid of his friend Abraham Sharp at a cost of £120, and which was completed, in 1689, after more than a year's work.

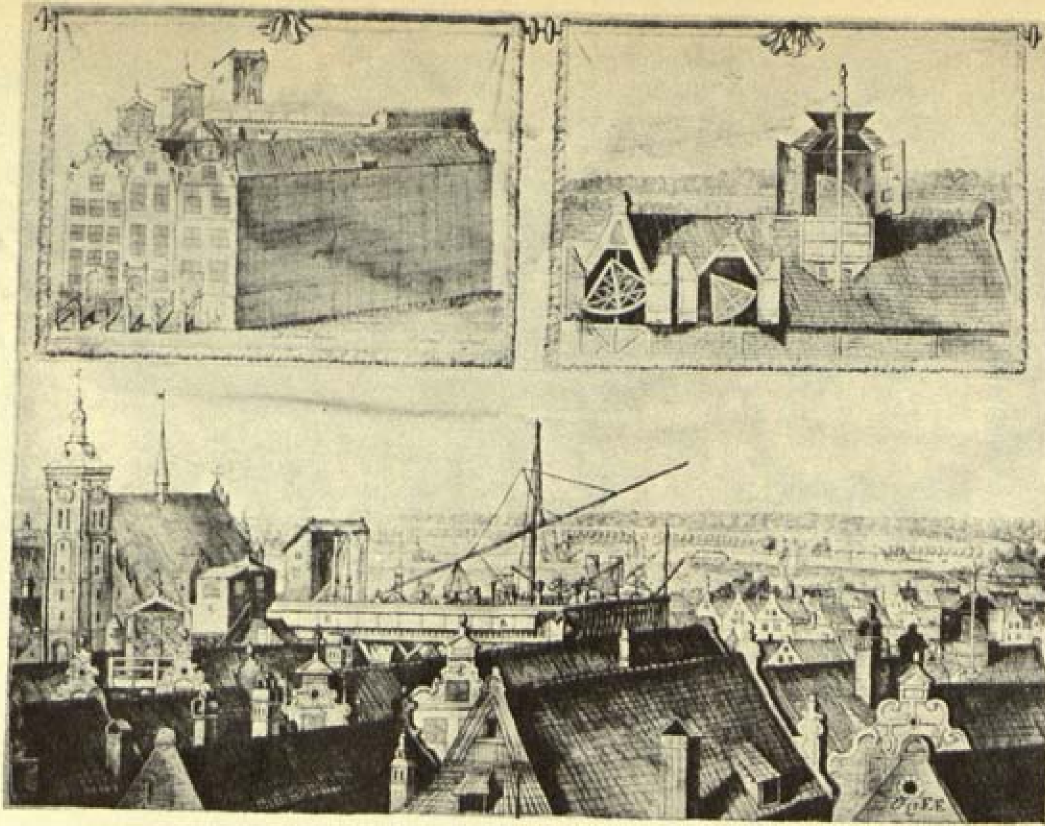
The principal fruit of Flamsteed's years of labour at Greenwich was the construction of a star-catalogue which superseded all previous ones both by its accuracy and by the number of stars which it contained. It marks an important stage in the development of modern precise astronomy. Flamsteed's round of work also included the frequent observation of the Sun, Moon, and planets, and the correction of their tables. He was fertile in inventing fresh methods of observation, such as the procedure, which still bears his name, for determining the vernal equinoctial point—the origin of graduations on the ecliptic and the Equator. In constructing his catalogue, Flamsteed generally proceeded by measuring the angular separations of pairs of stars by means of his sextant, thus gradually building up a network of such "intermutual distances" all over the visible heavens. The places of the stars included in this survey were then connected by calculation with the places of certain selected fundamental stars. The absolute co-ordinates of the latter (and thus ultimately of all the rest) were determined with the aid of the mural arc and a pendulum clock which enabled the times and altitudes of their meridian transits to be ascertained. The mural arc and clock were also frequently used to determine star-places directly, in accordance with the practice of Römer, the two methods serving as a check on each other. In this way a catalogue of nearly three thousand star-places was built up.

The forty-five years which Flamsteed spent at Greenwich were darkened by ill-health and shortage of money; and he became involved in a long and painful dispute with Newton and Halley over the publication of his results. The point of this dispute was, briefly, that Flamsteed was anxious not to publish his catalogue until he had carried it to the highest possible degree of perfection. He supposed that, as he had spent some £2,000 of his own money on the work without any Government repayment except his salary, he had some right to publish his observations at his own time. On the other hand, Newton, who was latterly one of the Visitors of the Observatory, seems to have taken the view that Flamsteed was a Government official whose observations were national property and should be published with expedition for the common benefit. Newton was also anxious to establish his theory of Universal Gravitation before he died, by showing its conformity with the most accurate observations available. Flamsteed, however, showed little interest in Newton's theoretical researches, and he accused Newton of not making proper acknowledgment of the lunar observations with which he kept supplying him, and which Newton used for the correction of his lunar theory. Flamsteed seems also to have had a particularly strong dislike of Halley. This was probably due, in the first instance, to the freedom of Halley's theological views; but the quarrel was aggravated by the publication, in 1712, under the editorship of Halley, and without the concurrence of Flamsteed, of an edition of the Greenwich observations in a curtailed and mutilated form which seriously diminished their scientific value. Even the portions printed did not represent Flamsteed's latest and best work, but were largely earlier observations which had been communicated to Newton merely as a guarantee that Flamsteed would complete his catalogue in due course.

Flamsteed resolved to reprint his observations and catalogue in his own way and at his own expense. He managed to buy up three hundred of the original four hundred copies of the 1712 edition. From these he separated the pages giving his early sextant observations which he had himself prepared for press, and he incorporated these in his new edition, to form the bulk of the first volume. Of the remaining portions of Halley's edition he made a bonfire. He died (December 31, 1719 O.S.), however, before he could finish preparing his later observations and star-places for the press; but this task was completed by his friends Crosthwait and Sharp, and the *Historia*

Illustr. 116

Johannes Hevelius



Hevelius' Observatory

Coelestis Britannica was finally published in three volumes in 1725.

Important memoranda and correspondence of Flamsteed containing much autobiographical material were first examined and printed by Francis Baily (*An Account of the Rev. John Flamsteed*, etc., London, 1835). Baily recalculated Flamsteed's star-places from his observations, and from his edition of Flamsteed's papers the foregoing particulars are largely derived. (Baily's account of the disputes shows a certain hostility to Newton and Halley; for a concise statement of their case, see Whewell's pamphlet, *Newton and Flamsteed*, Cambridge, 1836.)

(See E. W. Maunder, *The Royal Observatory, Greenwich*, 1900.)

HALLEY AND HEVELIUS

Edmond Halley was born in London on November 8, 1656. As a schoolboy, and later as a student at Oxford, he studied astronomy, and observed the heavens on his own account. At the age of nineteen he contributed a paper to the Royal Society suggesting a method more direct than those in use for determining the elements of a planetary orbit. This paper revealed his remarkable command of geometry; but it was chiefly of importance because it reverted to Kepler's Second Law and rejected the alternative hypothesis, which was then finding favour, that a planet revolves uniformly about the vacant focus of its elliptic orbit.

Halley's early observations of the planets, made with crude, home-made instruments, had revealed to him certain discrepancies betwen the true places of Jupiter and Saturn and those predicted in the current tables. Like Tycho Brahe a century before, Halley was anxious to reform the planetary tables; but he, too, recognized that such an attempt would be a waste of time without a more correct catalogue of the fixed stars. It was no use his competing with observers like Flamsteed and Hevelius on their own ground. He therefore resolved to supplement their work by cataloguing the stars of the southern celestial hemisphere, which could not be observed from Greenwich or Danzig, and whose places were known only through the crude observations of sailors. Halley fixed on St. Helena, which was then the most southerly of British dominions, as the site of his temporary observatory. His father undertook to meet the cost of the expedition, while Sir Joseph Williamson, the President of the Royal Society, and Sir Jonas Moore, the patron of Flamsteed, brought the scheme to the notice of Charles II. The King commended Halley to the East India Company, who were then in control of St. Helena, and who offered him a passage to the island when the fleet sailed. Equipped with the necessary instruments, Halley arrived at St. Helena early in 1677, and pitched his camp on one of the northern spurs of Diana's Peak, a central mountain which dominates the island. Here he remained observing for nearly eighteen months. Bad weather seriously hampered his observations, but by working without intermission upon every opportunity he succeeded in determining nearly 350 star-places before the time came for his return in 1678. He filled up the time when he could not observe with investigations of physical and meteorological phenomena, and was impressed by the unfamiliar forms of life that met his eye.

Halley's procedure in the construction of his catalogue, which he published in 1679, was to measure, by means of a telescopic sextant (that is, a telescope traversing a graduated arc of 60 degrees, not a "nautical sextant") which he took with him, the angular distances of each unknown star from at least two stars whose places were known from Tycho Brahe's catalogue. The required celestial longitude and latitude were then obtained by calculation, Halley himself reducing the observations. He enhanced the value of his catalogue by including the actual data from which the co-ordinates were deduced. This he did in order that the accuracy of his calculations could be checked, and in order that all his southern star-places could be recalculated when Flamsteed's and Hevelius' catalogues, with their improved values for the fundamental stars, should have appeared. Halley's results could thus be rediscussed by Sharp (who amalgamated most of his star-places with those of Flamsteed and published them in the *Historia Coelestis*), and, in more recent times, by Baily (*Mem. R.A.S.*, Vol. XIII, 1843). Halley retained the traditional constellation names, but introduced one new group (which has not survived), viz. *Robur Caroli*, in honour of his royal patron and of the oak which preserved him. It is noteworthy that Halley's star-catalogue was the first to be based on telescopic observations.

Immediately after his return to England, Halley was elected a Fellow of the Royal Society. He kept up a lifelong connection with the Society, becoming its Secretary in 1713; and he also undertook for some years the editorship of the *Philosophical Transactions*, in which some eighty of his papers are to be found, extending over a period of more than sixty years.

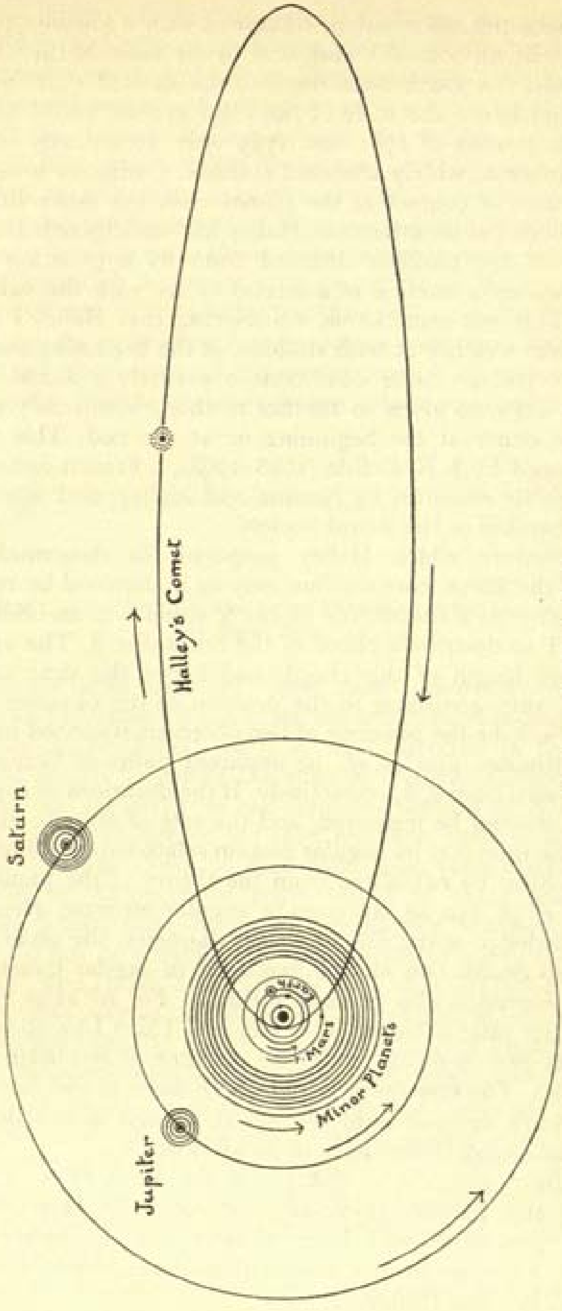
The first duty which the Royal Society laid upon Halley brought him into contact with a much older astronomer, Johann Hevel (1611–87), called Hevelius, of Danzig. This brilliant but opinionated observer made skilful use of the telescope for examining the Moon, planets, and comets. The books in which his observations are described and depicted are among the finest products of seventeenth-century descriptive astronomy. But when it came to the precise measurements involved in constructing his star-catalogue, which included about 1,500 stars, Hevelius (as has already been mentioned) maintained the superiority of open sights as against the telescopic sights which were then being almost universally adopted. His obduracy on this point led to a long and sometimes acrimonious discussion with Hooke, the champion of the telescopic sight. Hooke impugned the accuracy of Hevelius, who thereupon appealed to the Royal Society to send someone to judge at first hand of the quality of his work. Halley was chosen. He arrived at Danzig in May 1679 with a telescopic quadrant, and the two men observed in friendly rivalry for two months. Halley testified to Hevelius' great skill, and to the precision of his instruments (all of which were destroyed in a fire about two months later). But neither astronomer altered his convictions, and we find all Hevelius' objections to telescopic sights reiterated in his last, posthumous book.

In the course of a tour on the Continent, begun in 1680, Halley visited the Paris Observatory, and observed the great comet of that year with Cassini. In the years immediately following his return he was closely in touch with Newton, encouraging his researches in mechanics, smoothing over difficulties in the way of their publication, correcting the proofs of the *Principia*, and even meeting the cost of its production out of his own pocket. After a short spell of duty at the Mint at Chester (1696–8), Halley was placed in command of a warship and sent on an expedition "to improve the knowledge of the Longitude and variations of the Compasse" in the Atlantic Ocean, and to search for unknown land to the south. After many months spent at sea, Halley was made Savilian Professor of Geometry at Oxford, in 1703, and was then immersed for years in studying Arabic and editing ancient mathematical texts. To this period also belongs his edition (1712) of Flamsteed's observations, to which reference has already been made. Upon Flamsteed's death, Halley succeeded him as Astronomer Royal, an appointment he held from the beginning of 1720 until his own death on January 14, 1742.

Halley found that Greenwich Observatory had been stripped of nearly all its instruments by the executors of Flamsteed. He obtained a grant of £500 from the Government for new equipment, but it was late in 1721 before he got properly started. In that year he introduced the first transit instrument which had been seen at Greenwich Observatory. The telescope was of Hooke's construction, and still hangs on the wall of the transit-house. Later he added a large iron quadrant constructed by Graham. At Greenwich, Halley devoted his attention almost exclusively to lunar observations for the correction of the tables, in the hope that these might provide a means of determining longitude at sea. With this object he made almost daily observations of the Moon throughout a sarotic period of about eighteen years, the results of which first appeared in 1749. Halley's day-to-day observations at Greenwich have, however, never been reduced and published. They were examined by Francis Baily (*Mem. R.A.S.*, Vol. VIII, 1835), who was led to form rather a low opinion of the conditions at Greenwich during Halley's term of office there.

As an astronomer Halley is best known for his connection with the comet which bears his name. We have seen that Newton, in his *Principia*, showed that the observed motion of the comet of 1680 could be attributed to its having moved in a parabola under the gravitational attraction of the Sun. Newton's methods for determining the orbits of comets were later applied successfully to a number of these bodies whose movements had been sufficiently well recorded. In this work Halley took an important part. He determined the elements of the orbits of twenty-four comets whose appearances dated from 1337 to 1698. His results appeared in the *Philosophical Transactions* for 1705. Newton had thought that some comets at least might move in elongated ellipses about the Sun, in which case they should show periodic returns to perihelion. Halley noticed a close similarity among the orbital elements of the comets of 1531, 1607, and 1682; and he suspected that here was a case of a single comet describing a closed orbit with a period of about seventy-five years (see Illustr. 118). He attributed slight discrepancies in the elements and in the intervals between successive appearances to the disturbing action of Jupiter upon the comet; and he predicted the return of the comet about 1758. It duly appeared at the close of that year (sixteen years after Halley's death), and has made subsequent returns in 1835 and 1910. Its appearances previous to the three considered by Halley have been partially traced back to dates before the Christian era by Hind, and by Cowell and Crommelin.

While at St. Helena, in 1677, Halley observed a transit of Mercury across the Sun's disc. From the duration of the transit, which he measured as carefully as possible, he arrived, by a complicated calculation, at an inaccurate estimate of the Sun's parallax. It occurred to him, however, that more precise results might be obtained from concerted observations of a transit of Venus, which approaches nearer to the Earth than Mercury. An ephemeris of such phenomena which he published in 1691 showed that the next transit of Venus was due in May 1761. Although aware that he could not expect to live until then, Halley carefully outlined, in 1716, a plan of campaign for taking full advantage of the phenomenon. His method involved measuring the durations of the transit as viewed from at least two stations differing in latitude by a known



Illustr. 118.—Halley's Comet

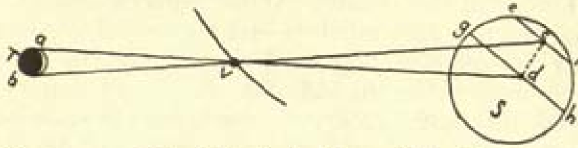
amount. From this information, combined with a knowledge of the rate of synodic motion of Venus, and of the ratio of the distances of Venus and the Earth from the Sun, he showed that the latter distance, and hence the scale of the solar system, could be ascertained. The transits of 1761 and 1769 were accordingly observed from a number of widely scattered stations. Owing to uncertainty in the instants of contact of the planet with the Sun's limb, the method proved less sensitive than Halley had anticipated. However, the values of the parallax deduced from its application mostly agreed to within a fraction of a second of arc with the value now accepted. This was remarkable, considering that Halley's method required clear weather at both stations, at the beginning *and* at the end of the transit. Such conditions are rarely realized. Hence preference was soon given to another method, which only requires observation either at the beginning or at the end. This method was introduced by J. N. Delisle (1688–1768), a French astronomer, who was highly esteemed by Newton and Halley, and was elected a foreign member of the Royal Society.

The procedure which Halley proposed for determining the distance of the Earth from the Sun may be understood by reference to Illustr. 119. At a transit, the planet V appears to an observer on the Earth T to describe a chord of the Sun's disc S. The apparent position and length of this chord, and hence the time taken to traverse it, vary according to the position of the observer on the Earth. Let *a*, *b*, be the positions of two observers stationed in widely different latitudes, and *gh*, *ef*, he apparent paths of Venus across the Sun as seen from *a*, *b*, respectively. If the durations of the transit at the two stations be measured, and the rate of synodic motion of Venus at the time (i.e. its angular motion relative to the line joining Earth and Sun) be calculated from the theory of the planet, then the chords *ef*, *gh*, can be expressed in angular measure. Hence, and from a knowledge of the Sun's angular diameter, the separation *cd* of these two chords can also be expressed in *angular* measure. But *cd* can be expressed also in *linear* measure. For $cd : ab = SV : VT$, and this latter ratio is known from Kepler's Third Law to be about 2·6, so that $cd = 2{\cdot}6 \,.\, ab$, while the distance *ab* is obtained from geodetic data. The knowledge of *cd* in both angular and linear units gives the Sun's distance from the Earth, though in practice additional complicating factors have to be allowed for.

Halley drew attention to changes in the mean rates of motion of Jupiter and Saturn, previously suspected by Horrocks, and becoming appreciable with lapse of time; and he suspected the existence of a minute secular acceleration of the Moon, which was later established and explained.

A total eclipse of the Sun, which occurred in 1715, afforded Halley an opportunity of observing and describing the appearance of the Sun's corona, which he supposed might be the Moon's atmosphere. He seems also on this occasion to have caught a glimpse of the chromosphere. Halley applied the method of fluxions to determine, by a now familiar method, at what elongation from the Sun Venus has its maximum brightness. He also made some advance towards a modern explanation of meteors, which he found it hard to attribute in all cases to the ignition of atmospheric vapours.

While preparing his star-catalogue of 1679, Halley found the brightness of several stars to be less than that recorded by Ptolemy, or even by an early seventeenth-century observer, Bayer. Other stars seemed altogether to have disappeared. This seemed to point to "mutability" in the heavenly bodies, which was further suggested



Illustr. 119.—Halley's Method of Determining the Distance of the Earth from the Sun

by the remarkable series of "new stars" discovered during the sixteenth and seventeenth centuries. Several of these showed periodic fluctuations in brightness, such as were detected in the sixteen-eighties by Kirch in one of the stars of Cygnus. Halley, in 1715, confirmed the regularity of the light-fluctuations of Kirch's star, and prepared a synopsis of the chief new and variable stars which had been observed down to that date. Two years later he made the important discovery of *proper motion* in three bright stars, Aldebaran, Sirius, and Arcturus. These showed changes of latitude which could not be attributed to the slow alteration in the plane of the ecliptic; and Halley's surmise that they were in motion relatively to their neighbours was confirmed and extended to other stars during the eighteenth century. Halley was a precursor of Herschel in the study of "nebulae," which began to be discovered in the seventeenth century. Star-clusters, of which Halley himself discovered two, were at that time, and long afterwards, classed among nebulae. Halley supposed that such objects are composed of a diffuse, self-luminous medium, and he had some notion of their enormous extent.

Halley's numerous researches in pure physics and geophysics are described elsewhere.

(See E. F. MacPike, *Correspondence and Papers of Edmond Halley*, Oxford, 1932, and *Hevelius Flamsteed and Halley: Three Contemporary Philosophers and their Mutual Relations*, London, 1938.)

CHAPTER IX

MATHEMATICS

ANTECEDENTS

The twelfth century saw the earliest considerable infiltrations of Graeco-Arabic mathematical learning into Western Christendom. These chiefly took the form of translations, by Christian or Jewish scholars, of such textbooks as were current in the Moorish schools of Spain. Of these books the most important was the *Elements* of Euclid, which was translated from Arabic into Latin by the English monk Adelhard of Bath about 1120, and which soon established itself as a standard textbook at the mediaeval Universities. Another important feature of this period was the gradual introduction into Western Europe of the so-called Arabic numeral system, with its symbols for the first nine numbers and for zero. This system took root slowly at first, but became firmly established, at least for scientific purposes, before the end of the fourteenth century.

During the thirteenth century the mathematical knowledge thus derived from the Arabs was expounded, with original developments, in several Latin works, which helped to lay the foundations for the later, independent growth of Algebra in Europe. The first of these was the *Liber Abaci* (1202) of Leonardo of Pisa, a widely read and travelled Italian. In his book Leonardo solves simple and quadratic equations, and sums elementary series by methods which he derived from Arabic authorities, and which he illustrates by numerous examples. He was one of the earliest advocates of the use of Arabic numerals, and his work long exercised an important influence. Contemporary with Leonardo was Jordanus Nemorarius, a German Dominican monk who wrote both on geometry and on algebra. He appears to have been one of the first to denote algebraic quantities, known and unknown, by arbitrary letters of the alphabet, instead of by initials or other abbreviations of words. There is some trace of this practice in the work of Leonardo of Pisa, but it did not become general until several centuries later. Roger Bacon also belongs to this period. He made little if any technical contribution to mathematics, but he realized, almost alone among his contemporaries, what a powerful instrument mathematics could be for the study of nature.

The fourteenth and fifteenth centuries were relatively barren from the point of view of progress in mathematics, but the fifteenth century saw the invention of printing and the recovery of many ancient mathematical classics in the original Greek. One of the earliest European scholars to be inspired by these original texts was the astronomer Regiomontanus, whose treatise, *De Triangulis*, is a landmark in the modern development of trigonometry. This work was written in the middle of the fifteenth century but was not published until 1533. Trigonometry was one of the few branches of mathematics which had received substantial development at the hands of the Hindus and Arabs. In tabulating the chords corresponding to various angles in a circle of given radius, they had substituted, for the simple chord of the Alexandrians, the half-chord of twice the angle, which is equivalent to the modern *sine*. They had also introduced, in principle, the cosine and tangent. Regiomontanus systematically summed up the work of both the Greek and Arab pioneers in plane and spherical trigonometry. His own special contribution was the application, to the solution of special triangles, of algebraic methods of reasoning derived from Diophantus, though without the use of abbreviations.

The shortening of the solutions of algebraic problems made possible by the use of abbreviations for the unknown quantity and its powers and for the words *plus*, *minus*, etc., was exemplified in the work of the Italian Friar Luca Pacioli, who lived in the latter part of the fifteenth century, and whose *Summa* appeared in 1494. Pacioli, however, did not reach the stage, represented by modern algebra, where symbolic statements take the place of sentences of ordinary prose. His theoretical work, moreover, represented little improvement upon that of Leonardo of Pisa.

The next advance in algebra was due to Michael Stifel (1486–1567), a Lutheran clergyman who revived Jordanus' practice of denoting unknown quantities by arbitrary letters of the alphabet. He represented the unknown and its successive powers by the current symbols R for *res* or *radix* (x), Z for *zensus* (x^2), C for *cubus* (x^3), etc. Stifel also occasionally represented powers by repeating the unknown the requisite number of times, e.g. writing it twice for a square, three times for a cube, and so on—a practice revived by Harriot at the beginning of the seventeenth century.

VIETA

The most important advance, however, in the development of algebra into an independent language, based upon an international shorthand, was the introduction, by the French mathematician Vieta, of the use of general symbols for quantities and operations, in place of mere abbreviations of the words for these things. François Viète (1540–1603), generally known by his latinized name of Franciscus Vieta, was a lawyer who rose to high official rank under King Henry IV of France, but found time for important mathematical researches, and was recognized as the most brilliant French mathematician of his time. He put his genius to practical account during the war between France and Spain by deciphering intercepted enemy despatches. Vieta's chief treatise on algebra, *In Artem Analyticam Isagoge* (Tours, 1591), was the means of establishing a number of improvements, some of which had been anticipated by earlier writers but had not taken root. Besides denoting the algebraic quantities by arbitrary letters (unknowns by vowels, given quantities by consonants), Vieta represented successive powers of a quantity (squares, cubes, etc.) not by using new letters, but by adding the words *quadratus*, *cubus*, etc. He thus economized in the number of symbols used, and saved the reader much bewilderment.

Vieta also applied algebra to trigonometry. The trigonometrical terms *tangent* and *secant* were being introduced at about this period, and most of the recognized abbreviations for the trigonometrical terms were coming into use, although with many variants. Vieta showed how the trigonometrical ratios could be transformed and related to one another in a variety of ways, by algebraic processes. He was thus the founder of the branch of trigonometry sometimes known as *goniometry*. For instance, he devised formulae giving $\sin na$ and $\cos na$ in terms of $\sin a$ and $\cos a$. Vieta succeeded in expressing π in the form of an infinite product, and he calculated its value to ten places of decimals.

Vieta's work on the theory of equations is mostly contained in his *De Numerosa Potestatum Resolutione* (Paris, 1600) and in his posthumously published *De Aequationum Recognitione et Emendatione* (1615). In this domain he gave rules for approximating to roots of equations which could not be directly solved, but he was still of the opinion that the solutions of an equation were represented only by its positive roots.

TARTAGLIA

An important achievement of sixteenth-century algebra was the solution of equations involving the cube of the unknown quantity. The ancients were acquainted with geometrical problems analogous to that of solving cubic equations, e.g. the classic problems of duplicating the cube, of trisecting the angle, and of dividing the sphere in a given proportion by means of an intersecting plane. One cubic equation is considered in the *Arithmetica* of Diophantus, and some of the Arabs gave approximate geometrical solutions of such equations. But the algebraic treatment of the problem dates from the beginning of the sixteenth century, when rules were found for solving certain types of cubic equations. The discovery of the most comprehensive of these rules is now usually attributed to the

Italian, Niccolo Tartaglia (1500–57), who may or may not have originated it, but who employed it to his own advantage in a contest which he waged with a rival mathematician. But Tartaglia's solution became generally known in 1545 through Girolamo Cardan of Milan (1501–76), who is said to have learned the method from Tartaglia in confidence, which he subsequently betrayed.

Much of Tartaglia's other writings deal with contemporary methods in commercial arithmetic, e.g. methods of calculating the interest due on sums of money. Such calculations were known even in ancient times, and compound interest reckonings were known among the Indians and the mediaeval Italian merchants. Stevin, however, was the first to publish tables for the calculation of interest, simple and compound.

A method of solving equations of the fourth degree, depending upon Tartaglia's solution of the cubic, was discovered by Ferrari, a pupil of Cardan. Attempts to solve algebraically equations of the fifth and of higher degrees continued until early in the nineteenth century, when this problem was shown by Abel to be generally insoluble.

Cardan's chief original contributions to algebra also related to the theory of equations. He paid attention to negative and imaginary roots of equations. He also anticipated certain relations between the roots and the coefficients of equations more clearly formulated later; and he did some pioneer work on probability.

GIRARD

It was not until the seventeenth century, however, that negative roots won complete recognition as real solutions. The law that, in general, the roots of a given equation are equal in number to its degree (the highest power of the unknown quantity occurring in it) was deduced by the Lorraine mathematician, Albert Girard (1595–1632), from the connections which he recognized between the roots of an equation and its coefficients. These connections follow from the fact that if $f(x) = 0$ is an equation of the nth degree, whose roots are $a_1, a_2, a_3 \dots a_n$, and in which the coefficient of x^n is unity, then

$$f(x) = (x - a_1)(x - a_2)(x - a_3) \dots (x - a_n)$$

The results of Girard's researches appeared in his *Invention Nouvelle en l'Algèbre* (Amsterdam, 1629). They immediately justified the existence of imaginary and negative roots, which had hitherto been neglected, but which Girard found it necessary to include with the other roots in order to bring their total number up to the degree of the equation, and so to satisfy his law. Girard noted that negative roots of equations could conveniently be represented geometrically as segments of straight lines set off in the sense opposite to that in which segments corresponding to positive roots were set off. This idea was afterwards applied by Descartes to a whole series of problems. Girard also studied the properties of spherical triangles, and he arrived at the simple formula, which bears his name, giving the area of such a triangle. This formula shortly afterwards received a more rigorous proof from Cavalieri.

Vieta's results were systematized and further developed on analytical lines by Thomas Harriot (1560–1621) in his posthumous *Artis Analyticae Praxis* (London, 1631). The *Clavis Mathematicae*, published in the same year (at first under a different title) by William Oughtred (1575–1660), the inventor of the slide-rule, became a standard textbook in which Newton made his first acquaintance with mathematics.

Mathematical Symbols

Most of the operational and other symbols now used in elementary arithmetic and algebra date from the sixteenth and seventeenth centuries. A considerable variety prevailed at first in such symbolism, and many other signs which have not survived were employed by individual mathematicians of the period.

The signs for addition (+) and for subtraction (−) are found as commercial symbols in a work on arithmetic by Johannes Widman published as early as 1489; but more than half a century elapsed before they were used as operational symbols by Stifel and others, and they did not come into general use until the beginning of the seventeenth century. The sign of equality (=) was suggested in 1557 by Robert Recorde in his *Whetstone of Witte* (the first English book to contain the + and − signs); it was possibly already known at that date, but did not become firmly established for another century. The symbol for multiplication (×) was introduced in 1631 by Oughtred, whose *Clavis Mathematicae* was extremely rich in symbols. The use of a dot as a symbol of multiplication is due to Leibniz. The symbol for division (÷) was first printed in 1659 in a work by a Swiss mathematician, J. H. Rahn. The symbols for "greater than" (>) and "less than" (<) were introduced early in the seventeenth century by Harriot. A radical sign, with numbers affixed to denote which root was to be taken, seems to have made its first appearance in 1484, in the manuscript of a French physician, Chuquet, who also had a notation for negative indices. But the radical first became generally known through its use by Rudolff early in the sixteenth century. The divers methods of indicating powers of algebraic quantities were finally developed into the modern index notation (for positive integral powers) by Descartes in 1637. This notation was extended to represent roots and the reciprocals of powers by Wallis and Newton, who showed how fractional and negative indices could be used for this purpose. The use of fractional powers, however, had been anticipated to some extent by the fourteenth-century Nicole Oresme (who devised a special notation for them) and by Simon Stevin.

Stevin also rendered a valuable service to arithmetic alike for everyday and for scientific purposes, by suggesting, in 1585, a notation for decimal fractions, which he preferred to the customary sexagesimal fractions. He drew attention to the value of decimal notations and methods of calculation, and he urged the Government to introduce decimal coinage and weights and measures—a wish which was first realized two hundred years later by the men of the French Revolution. Stevin's method of writing decimal fractions, however, was rather inconvenient. He subjoined to each figure an index showing the place which it occupied to the right of the unit's place. For instance, he would have written the decimal fraction 0·3469 in the form

$$3 \; ① \; 4 \; ② \; 6 \; ③ \; 9 \; ④.$$

Stevin had an alternative notation in which this fraction would appear as $3' \, 4'' \, 6''' \, 9''''$. About the beginning of the seventeenth century, however, following a proposal of Vieta's, the modern Continental method arose of writing decimals with a comma prefixed. The use of the point occurs in a work of Napier's which appeared in 1617, but a variety of notations long persisted.

Many of the pictographic geometrical symbols now in use (e.g. $\odot$, $\triangle$, $\square$, etc.) are of ancient origin, but were revived and increased considerably during the early seventeenth century, especially by Hérigone and Oughtred.

The symbol π for the ratio of the circumference of a circle to its diameter seems to have been first employed by William Jones in 1706, but it was not widely used until the latter half of the eighteenth century. (See F. Cajori, *A History of Mathematical Notations*, Vol. I, Chicago, 1928.)

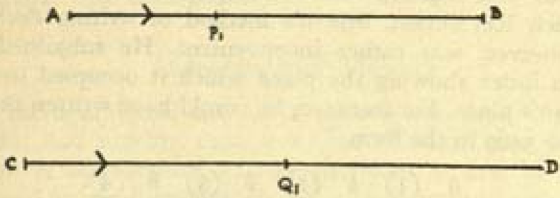
Logarithms

An extremely important advance in the art of calculation was made early in the seventeenth century with the invention of logarithms, whereby multiplication and division were reduced to addition and subtraction, and the extraction of roots to simple division. The idea underlying this aid to calculation seems to have occurred independently to at least two mathematicians of the period—to John Napier, Baron of Merchiston, a Scottish nobleman and theologian, and to Joost Bürgi, the Swiss astronomer, Kepler's friend.

NAPIER

John Napier (1550–1617) made his invention known in his *Mirifici Logarithmorum Canonis Descriptio* (1614), which contained his table with rules for its use, but no account of the method of its construction. This was first explained in Napier's posthumous book, *Mirifici Logarithmorum Canonis Constructio* (1619), which, however, was actually written at an earlier date than the *Descriptio*.

Napier had to arrive at his conception of logarithms without the mental aid of the index notation, which nowadays affords the easiest



Illustr. 120.—Napier's Conception of Logarithms

approach to them. He considered a point P moving along a straight line such as AB (10^7 units in length) with a velocity proportional at each point P_1 to the remaining distance P_1B, while another point, Q, was supposed to move along an unlimited line CD with a uniform velocity equal to that of the first point when it was at A (Illustr. 120). Supposing the points to start from A, C, simultaneously, then the logarithm of the number measured by P_1B was defined as measured by CQ_1.

In the absence of anything like our logarithmic series, Napier had to approximate to the value of each logarithm which he required by evaluating certain limits within which it must lie. This he effected by means of two formulae and of auxiliary tables constructed for the purpose, from which the required number was obtained by interpolation.

Napier intended his logarithms to serve primarily for the solution of plane and spherical triangles. Hence his tables were constructed to show the logarithms of the natural sines of angles from 0° to 90° for every minute of arc. Logarithmic cosines and tangents could also be read off from them. Napier took the sine of 90° as 10^7, so that his table covered the numbers between 0 and 10^7.

These numbers, however, were not the natural numbers, but the sines of angles which increased by regular increments. Again, these original logarithms were not the same as those now called "Napierian" or "natural" logarithms. For in Napier's system the logarithm of 10^7, i.e. of sin 90°, is 0, and as the number decreases in geometrical progression towards zero its logarithm increases in arithmetical progression towards infinity.

The practical value of logarithms was immediately recognized by Napier's friend Henry Briggs (1561–1631), the Gresham Professor of Geometry, who had much to do with the subsequent development and rapid spread of Napier's invention. Napier and Briggs both came to see the advantage of a system in which $\log 1 = 0$, and in which the logarithms increase with the numbers. Tables based on this principle were calculated by Briggs, whose *Arithmetica Logarithmica* (1624) gave the common logarithms, to 14 decimal places, of 30,000 numbers. This collection was supplemented, to cover all numbers from 1 to 100,000, by the Dutch mathematician Adrian Vlacq in 1628.

John Napier also made some contributions to spherical trigonometry, including a useful mnemonic, the "rule of circular parts," for the formulae of the right-angled spherical triangle. (See E. W. Hobson's *John Napier*, Cambridge, 1914.)

BÜRGI

Joost Bürgi (1552–1632) introduced a crude table of *antilogarithms*, i.e. of numbers whose logarithms formed a series of natural numbers. This system was in all probability independent of Napier's, though first published six years later (1620); but it was definitely inferior, and soon suffered eclipse. Kepler expressed high admiration for Napier, but he constructed tables on lines of his own, which he published in 1624–5, and later included in his *Rudolphine Tables*.

The laborious early methods of calculating logarithms were superseded later in the seventeenth century by the use of logarithmic series depending upon the recognition of the logarithm as an index. Such a series enables a logarithm to be obtained to any required degree of accuracy by the summation of a sufficient number of terms in a convergent series. The important series for $\log(1 + x)$ was first stated generally by Wallis, who obtained modified forms of it having the practical advantage of more rapid convergence.

Analytical Geometry

DESCARTES

Descartes' treatise, *La Géométrie*, first appeared as one of the appendices to his *Discours de la Méthode* (1637). (See facsimile edition with annotated English translation by David E. Smith and Marcia L. Latham, Chicago and London, 1925.) It is divided into three books, of which the first two are devoted to analytical geometry, while the third deals mainly with the theory of equations. The book was written intentionally in a somewhat obscure style, much of the analysis and many implications of the results obtained being omitted "so as to leave to posterity the pleasure of discovering them."

Descartes begins by establishing analogies between ruler-and-compass constructions in geometry and the standard processes of arithmetic; and he points out the advantages of introducing algebraic ideas and notation into geometry. Thus he denotes segments of straight lines by letters. He also forms products and powers of such letters, or of combinations of them, without attempting to interpret them geometrically (as areas or volumes), and is thus able to use quantities like a^4, a^5, a^6 . . ., which correspond to no known geometrical forms, and which without this provision would have been unintelligible. In order to denote such powers he employs the system of writing indices which we use to-day, except that he uses aa or a^2 indifferently. He usually makes use of the letters a, b, c . . . to denote known or constant segments of lines, and x, y, z for unknown or variable segments.

For the solution of geometrical problems Descartes recommends the *analytical* method (already familiar to the Greeks and formulated by Pappus) of supposing the problem solved and then writing down all the implicit relations which must hold between the lengths of the various lines involved in the construction. Each of these relations is represented by an equation; and the solution of the problem then depends upon the solution of all these simultaneous equations. For the problem to be determinate, the number of equations connecting the unknown lines must equal the number of such lines involved in the problem.

Descartes applies his method to a problem enunciated by Pappus, which had exercised the classical geometers, who could solve only particular cases of it. In its general form it runs: Given a number of fixed straight lines and an equal number of variable lines each making a given angle with one of the fixed lines and all passing through one point, it is required to find the locus upon which that point must lie so as to make the product of the lengths of some of the variable lines multiplied together stand in a certain ratio to the product of the remaining variable lines. Descartes shows that it is possible to express all the variable lengths involved in this problem in terms of two variable lengths (which he calls x and y) and of constant, known data of the problem. The constant ratio of the two products can thus be expressed as an equation in x and y and their powers and products. When one of these quantities is known, the other becomes determined; and the equation as it stands is the analytical counterpart of the required locus. "If we take successively an infinite number of different values for the line y, we obtain an infinite number of values for the line x, and therefore an infinity of different points by means of which the required curve can be drawn."

We have here the germ of co-ordinate geometry, in which the position of a point in a plane is defined by its distances, x and y, from two fixed axes, and in which a given relation between x and y corresponds to a definite geometrical locus upon which the point must lie, and *vice versa*. It is this conception which marks Descartes' essential contribution to geometry; the mere application of algebra to geometry was no novelty, but goes back to many centuries before. The modern use of arbitrary co-ordinate axes upon which the abscissa and ordinate of a point are set off from a common origin does not, however, occur in Descartes' book. For purposes of reference he makes use of any convenient line in the figure. The modern system of using two axes in plane problems was introduced in the eighteenth century, when the terms "abscissa" and "ordinate" also came into use.

The degree of the equation arising from any particular case of Pappus' Problem cannot exceed the number of lines multiplied together on either side of the equation. Descartes shows that when three or four lines are involved, the required locus is a conic, but that for increasing numbers of lines it is represented by curves of ever higher degree. In Book II he proceeds to discuss these curves and the possibility of generating them artificially.

The classical geometers generally excluded from consideration curves requiring for their construction mechanical devices other than the ruler and compass, though they admitted the conic sections. Descartes, however, insists that any curve is a proper object of geometrical investigation provided its mode of generation can be clearly conceived, since, in geometry, exactness of reasoning is all that matters. If all "mechanical" curves are to be excluded, there is no justification for retaining the straight line and circle, since even these require a ruler and a pair of compasses for their construction. Descartes, therefore, includes in his survey all curves determined by the intersection of two moving lines whose rates of motion have a definite, known relation to each other. These are the "geometric" curves whose properties can be defined in each case by a single algebraic equation in two variables. He still excludes, however, curves generated by independent motions whose relations cannot be exactly specified—a class which includes many of the special curves (e.g. the spiral and quadratrix) of the Greeks.

Geometric curves are grouped by Descartes in successive classes according as their equations are of the second degree (his first class), of the third or fourth degrees (second class), of the fifth or sixth (third class), and so on.

Descartes describes a mechanical device in which two straight edges slide over each other and generate an hyperbola as the locus of their point of intersection. By substituting this hyperbola, or any other curve of his first class, for one of the straight edges, Descartes proposed to obtain a curve of the second class as the locus of intersections, and, by the similar employment of this curve, a third-class curve, and so on. Reverting to Pappus' Problem, Descartes shows how the class of the required locus depends upon the number of variable lines involved, and he investigates the conditions under which, in the simplest case, the several types of conics appear. This section constitutes an almost complete treatment of the elements of the analytical geometry of conics.

Descartes next shows how to define the direction of the normal (and hence of the tangent) at any given point on a curve whose equation is known. His method is to find a circle which just touches the curve at the given point without cutting it, its equation having only one pair of roots in common with that of the curve. The centre of this circle lies on the required normal and defines its direction.

Descartes concludes his second book with an account of certain curves, since called "Cartesian Ovals," which have important optical properties. They generate, by revolution, reflecting or refracting surfaces which make rays of light, emanating from a point source, all pass through a real or virtual point image.

Among the results in the theory of equations contained in the third book is that still known as "Descartes' Rule of Signs." As formulated by Descartes, this states that an equation, written in the zero form (that is, with all terms on one side), can have as many "true" (i.e. real, positive) roots as its successive terms show changes of sign from $+$ to $-$, or from $-$ to $+$, and can have as many "false" (by which Descartes means real, negative) roots as the number of times two $+$ or two $-$ signs occur in successive terms. This result had been partially anticipated by Cardan; its limitations were understood after the establishment of the notion of imaginary roots (which Descartes himself was among the first so to designate). It is explained also how the roots may have their signs reversed, or may be increased, diminished, multiplied, or divided by a given number, by appropriate transformations of the original equation. The book concludes with rules for the graphical solution of cubic and biquadratic equations by the intersection of circles with a parabola, and with suggestions for using curves of higher classes for the solution of equations of higher degree.

DESARGUES

Conceptions of great value for the future development of pure geometry were introduced by the French mathematician, Desargues, almost simultaneously with Descartes' publication of his discoveries in analytical geometry.

Girard Desargues was born at Lyons in 1593. He spent much of his early life in Paris, where he made the acquaintance, and won the esteem, of Descartes and of many other men who later helped to form the French Academies. An architect by profession, Desargues saw service as a military engineer at the siege of La Rochelle. He subsequently wrote several technical books on perspective and stone-cutting, but his masterpiece was his *Brouillon project d'une atteinte aux événemens des rencontres d'un cône avec un plan*, etc. (Paris, 1639).

Desargues regarded a cone or a cylinder as generated by an infinite straight line passing through a fixed point and moving round the circumference of a circle. He obtained the various types of conic sections by cutting such a cone or cylinder by a plane in various ways, and he showed how to derive the properties of the conic sections, as a class, from the simpler properties of the circle forming the base of the cone. Among other points to be noted in this work are Desargues' conception of parallel straight lines as lines meeting at a point infinitely distant, and of parallel planes as planes meeting in a line at infinity; his theory of the involution of sets of points on a line, subsequently extended by Chasles; the principle of the pole-and-polar relation of points and lines with regard to conics, and its extension to solid geometry; and many other important results.

Desargues' writings, like those of Descartes, are somewhat obscure, and his nomenclature is original and complicated. Many of his results are given without proof or elaboration, and his ideas have partly to be extracted from the works of his pupil, Abraham Bosse, to which Desargues made many avowed contributions, besides inspiring the whole treatment. Desargues' methods at first aroused much bitter criticism from men not of the first rank. On the other hand, they were appreciated and further applied by Pascal; but after the deaths of both Pascal and Desargues, in 1662, the latter was soon forgotten, and his book became unprocurable. The methods of Descartes, later supplemented by the calculus, were more appropriate than those of Desargues in dealing with the problems in physics and astronomy which were receiving most attention at that period. Hence Desargues had to wait for full recognition until the nineteenth century, when his ideas formed the basis for the rapid development of projective geometry in the hands of Poncelet, Chasles, and Steiner. (See *Œuvres de Desargues . . . réunies et analysées par M. Poudra*, Paris, 1864.)

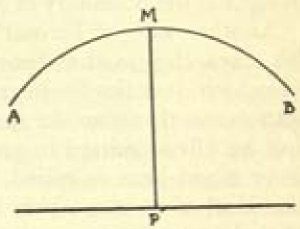
FERMAT

The credit for the establishment of analytical geometry must be shared by Descartes' contemporary, Pierre de Fermat (1601–65), one of the greatest of French mathematicians. Fermat devoted his main energies to official duties at Toulouse, but occupied his leisure with mathematical researches. Ten years before the appearance of Descartes' book, Fermat was already experimenting with the application of algebra to geometry. He appears independently to have hit upon the fundamental idea of representing a curve by means of an equation from which its characteristic properties could be derived. Like Descartes, he took the geometry of the ancients as his starting-point, and he laboured (though in vain) to restore the *Porisms* of Euclid, which survives only in extracts made by Pappus.

Fermat's fundamentally important work on analytical geometry, *Ad locos planos et solidos isagoge*, was both clearer and more exhaustive than the work of Descartes. Equations, he says, can be conveniently represented by drawing two straight lines making with each other a given angle (which is most suitably taken as a right angle), and by setting off from their point of intersection, taken as origin, distances respectively proportional to the variables of the equation. Fermat calls the origin N, and he denotes the distances set off therefrom in perpendicular directions by A and E (corresponding to our x and y). Constant quantities are expressed by means of the letters B, D, G. The equation of a straight line passing through the origin occurs for the first time in Fermat's work in the form $D \cdot A = B \cdot E$ (cp. $ax = by$). He writes the equation of a parabola $A^2 = D \cdot E$ (cp. $x^2 = ay$), and that of a circle $B^2 - A^2 = E^2$ (cp. $r^2 - x^2 = y^2$), and so on.

It is difficult to pronounce upon Fermat's claim to priority over Descartes in the invention of analytical geometry. Fermat published very little, and most of his discoveries were announced in letters to Parisian mathematicians—particularly to Mersenne. His works, and much of his correspondence, were not published until after his death (*Opera Varia*, Toulouse, 1670–9; see also the modern edition by Tannery and Henry, 1891–6).

Fermat was one of the first mathematicians to find a general method of solving questions on maxima and minima. He employed a principle which is of fundamental importance in algebra and analytical geometry, and in their application to physics. The earliest known problem of this nature occurs in the *Elements* of Euclid (VI, 27). Though expressed in geometrical terms, it is equivalent to finding the greatest possible value of the product $x(a - x)$. It is shown that the product is a maximum when $x = \frac{a}{2}$. It was also known to the ancients that the circle has the greatest area of all plane figures with a given perimeter, and that the sphere has the greatest volume of all solids with a given surface. Several mathematicians of the fifteenth and sixteenth centuries investigated isolated problems of this nature, but the effective treatment of them dates from the work of Kepler, Cavalieri, and Fermat in the seventeenth century.



Illustr. 121.—A Variable Quantity at its Maximum

Kepler had noticed incidentally that a variable quantity, as it approaches a maximum, shows but little change in value. For instance, if PM (Illustr. 121) be the greatest ordinate of the curve AMB, its length shows no perceptible alteration for infinitesimal displacements parallel to itself in either direction. Kepler clearly grasped this principle, though he was unable to prove it.

Fermat's method, which he was already using in 1629, is substantially that applied to-day to elementary problems of this type. In the expression whose values are to be investigated he substitutes for the independent variable, say x, the new value $(x - E)$, or $(x + E)$, where E is a vanishingly small quantity. He then equates the new value of the expression to the former value, thereby determining the value of x as that which makes the expression a maximum or minimum. After reduction and division by E, E is made equal to zero, and the solution of the resulting equation gives the required value or values of x. An example of Fermat's, in modern notation, will help to make the matter clear. Required to find the value of x which makes $x^2(a - x)$ a maximum. The solution runs

$$x^2(a - x) = (x + E)^2\{a - (x + E)\}$$

which reduces to

$$2ax - 3x^2 + E(a - 3x - E) = 0$$

Putting $E = 0$,

$$2ax - 3x^2 = 0, \text{ and } x = \frac{2a}{3} \text{ is obtained as the value of } x$$

which actually makes the expression a maximum. Fermat's method, however, afforded no criterion for distinguishing between maxima, minima, and points of inflection at which the tangent is horizontal. A general method of distinguishing between these was first afforded by the calculus.

Fermat made many applications of this method to particular problems in what we should call differential geometry. His important physical application of it to the problem of the refraction of a ray of light at the boundary of two different media is described elsewhere.

Another side of Fermat's mathematical genius was revealed by his researches in the theory of numbers, the modern development of which practically began with him. He discovered numerous important theorems in this field, some of which bear his name; but he often omitted to give their proofs, and in certain cases these have never been supplied. An instance of this, and an example of the kind of results which he obtained, is afforded by his theorem that the equation

$$x^n + y^n = z^n$$

cannot be satisfied by integral values of x, y, and z when n is an integer greater than 2—a general proof of which has not yet been discovered.

Fermat and his contemporary, Pascal, were also practically the founders of the mathematical theory of probability, and of the closely related theory of combinations.

Infinitesimals, Fluxions, and the Calculus

From the beginning of the seventeenth century progress began to be made in establishing a mathematical method of treating infinitesimal quantities. The elaboration of this method into one of the most powerful instruments of scientific investigation by the invention of the infinitesimal calculus was reserved for Newton and Leibniz. First among the pioneers in this field, however, must be placed Kepler and Cavalieri, an Italian disciple of Galilei.

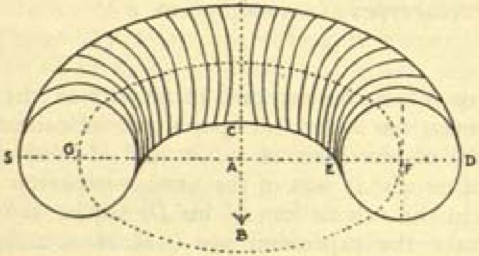
The ancients, and particularly Archimedes, had already recognized that many geometrical problems could not be solved with the aid of elementary mathematics. This had led to the employment of a process known as the "method of exhaustion" for the mensuration of curved figures. In order to estimate the length of a closed curve, or the area which it contained, a polygon was inscribed to the curve, and another polygon circumscribed to it. The perimeters and the areas of these rectilinear figures could be ascertained, and hence those of the curve, which lay between them, were known to lie within certain limits. By increasing the number of sides of the polygons these limits of uncertainty could be correspondingly narrowed, and approximate values of the perimeter or area of the curve were thus obtained. The volumes of curved solids could similarly be measured with the aid of inscribed and circumscribed rectilinear solids of suitable form. By such roundabout methods the Greek mathematicians had obtained the perimeters, areas, and volumes of several types of curved figures.

KEPLER

The progress of astronomy and of physics in the seventeenth century, however, was dependent upon the development of a general method for the measurement of curves and of solids generated by the motion of curves. It was of the utmost importance to Kepler, for instance, in the calculations of his *De motibus stellae Martis*, to be able to take the expression $\pi(a + b)$ as a sufficiently close approximation to the perimeter of an ellipse whose major and minor axes, a and b, differ only slightly. Many important results which would now be expressed as definite integrals of trigonometrical functions occur incidentally and implicitly in Kepler's astronomical writings. Kepler, however, dealt more systematically with the determination of the volumes of solids of revolution in his *Nova Stereometria doliorum vinariorum* (Linz, 1615). (For a German edition of this book, see Ostwald's *Klassiker*, Bd. 165.)

Kepler was originally led to make the calculations set forth in the book by a desire to improve upon the crude methods then in use for estimating the contents of wine-casks and other vessels. While buying wine he noticed that the vintners determined the contents of the casks by passing a measuring-rod through the bung-hole as far as the opposite staves without taking account of the curvature of the latter. By rotating the longitudinal section of the cask about its axis, a body equal in volume to the cask would be formed. Kepler's plan was to divide up such solids of rotation into an infinite number of elementary parts, and to sum these; and in his *Stereometria* he applies this method to some ninety special cases. Kepler regarded infinitely small arcs as straight lines, infinitely narrow planes as lines, and infinitely thin bodies as planes. His conception of infinitely small magnitudes was one which the ancients had in general avoided, but which a little later formed the basis of Cavalieri's method.

As one example of Kepler's argument we take his approximate quadrature of the circle, which may be compared with that of Archimedes. He regards the circumference as made up of an infinite number of segments of straight lines each of which is the base of an isoceles triangle having its vertex at the centre of the circle. If now all these bases, whose sum equals the circumference, be placed side by side in a straight line whose distance from the centre equals the radius, and be all joined to the centre, a triangle will be obtained made up of an infinite number of small triangles and equal in area to the circle. In a similar manner the content of a



Illustr. 122.—Kepler's Cubature of the Anchor-Ring

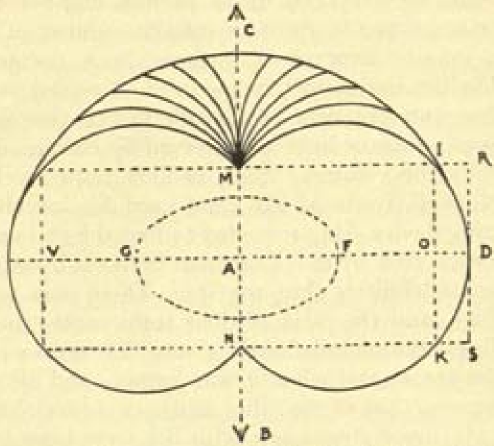
sphere is calculated by regarding it as divisible into an infinite number of cones having a common vertex at the centre.

One of the most instructive examples of Kepler's procedure is his cubature of the anchor-ring (Illustr. 122). This was first divided into an infinite number of discs by planes through the axis A. These discs are not uniformly thick, but are thinner in the parts towards A and thicker in the opposite parts. These inequalities cancel each other out, however, and the volume of the ring is equal to that of a cylinder whose base equals the cross-section of the ring and whose height equals the circumference of the circle described by the centre F of this cross-section, supposing it to rotate about the axis A.

Kepler leads off with the few solids of revolution treated by the ancients, but he goes on to consider a multitude of new figures—ninety-two in all—many of them generated by the revolution of conic sections about their diameters, chords, or tangents, or about exterior lines, as axes. Many of the bodies so generated Kepler names after fruits. His "apple," shown in Illustr. 123, was produced by the rotation, about its chord, of a circular segment larger than a semicircle. The similar rotation of the smaller segment gave rise to a "gourd" (*citrium*, not *citreum*, a citron).

The rigour of Kepler's demonstrations did not attain to that of Euclid or of Archimedes. For this a further development was necessary of the methods of handling infinitesimal quantities. In many cases Kepler had to be satisfied with conclusions of merely probable correctness, and in others he missed the correct solution altogether.

In his *Stereometria*, Kepler also investigated what form a vessel should have in order to hold as much as possible while requiring



Illustr. 123.—Kepler's "Apple"

for its construction as little material as possible. Such *isoperimetrical* problems, as they are called, were known to the ancients, and they came into prominence again in the eighteenth century, when they played an important part in the development of higher mathematical analysis. As an example of Kepler's researches in this realm may be mentioned his proposition that the cube is the greatest parallelopiped which can be inscribed in a sphere. Kepler's reference to the important criterion for the maxima (and minima) of a variable, viz. that the variable remains practically stationary in value in the immediate neighbourhood of a maximum, is discussed elsewhere, as is also his work on logarithms.

Of some significance was Kepler's recognition (in his *Ad Vitellionem paralipomena*) of the continuity of the several types of conic sections, whereby we may pass by an unbroken transition from the circle through the ellipse, parabola, and hyperbola to the line-pair. He introduced the term *focus* into this branch of geometry.

CAVALIERI

The next stage in the development of new methods foreshadowing the integral calculus is represented by the work of Bonaventura Cavalieri (1598–1647), a Jesuit professor of mathematics at Bologna. In his *Geometria indivisibilibus continuorum nova quadam ratione promota* (1635; final edition 1653), Cavalieri, who had studied Kepler's *Stereometria*, explains the *method of indivisibles* (which he had invented some ten years before), but without making clear what his indivisibles precisely were. His procedure gave the impression that he regarded a line as composed of an infinite number of successive points, a surface as made up of an infinite number of lines, and a solid of an infinite number of surfaces, such points, lines, and surfaces being the indivisibles in question. This led to much misunderstanding and criticism of Cavalieri. For the elements into which volumes, areas, or lines are resolved by continual subdivision must themselves be volumes, areas, or lines respectively. Cavalieri was probably well aware of this, and used his indivisibles simply as a calculating device. He proceeded to find the ratio of a required area to a known area by dividing them both up by equally spaced parallel lines, indefinitely close together. These lines were summed for each figure, and the ratio of their sums ascertained when the number of lines became indefinitely great. This ratio was equivalent to that of the areas; and when it was known, and also the area of one of the figures, that of the other figure could be obtained.

For example, draw a rectangle with the same base and height as a given triangle, and draw across both figures a number of equally spaced lines parallel to the base. The sum of the lines intercepted in the triangle is easily shown to be half that of the intercepts in the whole rectangle, whence Cavalieri concludes that the surfaces also stand in the ratio of 1 : 2. By a similar process it was proved that the area of an ellipse is to that of a circle whose diameter is equal to one axis of the ellipse, as the other axis of the ellipse is to the diameter of the circle.

In applying his methods to the mensuration of solid bodies, Cavalieri employed equally spaced parallel planes in place of lines to divide the bodies. If such planes cut two bodies in surfaces whose areas stand to each other in a certain proportion throughout, then the volumes of the bodies will be in the same proportion—a theorem which still bears the name of Cavalieri.

As compared with the methods of Kepler, who set himself definite problems, Cavalieri's methods possessed the advantage of greater generality and more abstract treatment. Despite the opposition which both men encountered, the conception of infinitesimals which they introduced was the most valuable idea which has ever enriched mathematics. Its fruitfulness was first fully revealed after the invention of analytical geometry, from whose combination with the conception of infinitesimals the differential and integral calculus emerged.

GULDINUS

Among the men of science who opposed Kepler and Cavalieri was Paul Guldin or Guldinus (1577–1643). His voluminous book, *Centrobaryca* (Vienna, 1635–42), deals mainly with the determination of the centres of gravity of curves, surfaces, and solids, which he worked out more thoroughly than had yet been done. He also calculated the volumes of a number of solids of revolution. For this purpose he employed a theorem which he resuscitated from the work of Pappus, and which states that the *volume* of a solid generated by the revolution of a plane figure about an axis in its plane equals the area of that figure multiplied by the arc described by its centroid. This theorem and a related one which enables the *surface* of a solid of revolution to be evaluated, are now called indifferently after Pappus or Guldin.

Guldin did not succeed in giving a satisfactory proof of this theorem, but rather inferred its truth from the fact that, with its aid, the same conclusions could be reached as by other, more rigorous but longer, methods. Many of Guldin's examples were the same as those which Kepler had worked out. But while Kepler's method (which Guldin opposed as unscientific) contained the germ of higher mathematics, Guldin's procedure remained without appreciable influence upon the further development of the science. In fact, it was found that, in many cases, the quadrature of the plane figure and the determination of its centroid were much more difficult than the direct cubature of the solid of revolution which it generates. Accordingly the theorems are now most commonly used to determine the centroid of a plane figure when its area and the volume of the solid which it generates are already known.

ROBERVAL

Some of the supposed logical objections to Cavalieri's indivisibles were removed by the French mathematician, Giles Persone de Roberval (1602–75), who claimed to have invented the method independently. He regarded lines as made up of elementary lines, areas as made up of elementary areas, and solids of elementary solids. He succeeded in finding by the method of indivisibles the areas of a number of curves—in particular, of the cycloid.

Roberval tried to make use of kinematic ideas in the solution of geometrical problems. Thus he regarded certain types of curves (e.g. the parabola) as traced out by a point whose motion, at each instant, was compounded of two simple motions. He sought to construct the tangent to such a curve at any given point by analysing the motion of the generating point into its component motions, and applying the parallelogram of velocities to obtain the tangent.

PASCAL

Blaise Pascal (1623–62), a friend and contemporary of Fermat, also worked with the method of indivisibles, which he modified in the same way as Roberval. Pascal showed precocious mathematical genius, but his activities in this direction were checked by religious scruples and cut short by his early death. Nevertheless, he made notable advances in several different branches of mathematics and physics.

At the age of only sixteen Pascal wrote an essay on conic sections, now mostly lost. In it he employed the geometrical methods of Desargues, and stated the theorem, still known as Pascal's Theorem, on a remarkable property of a hexagon inscribed in a conic. Late in his life, when he had abandoned mathematics for some years, Pascal returned to it for a brief space in order to study the properties of the cycloid—the curve traced out by a point on the circumference of a circle which rolls along a fixed straight line without slipping. The area of the cycloid had already been found by Roberval and by Torricelli independently of each other, while Fermat and Descartes had solved the problem of drawing tangents to it. Pascal took up some difficult problems on the mensuration of the solids generated by rotating a cycloid about special lines in its plane and on the centres of gravity of such solids. He is said to have solved them within a week, employing the method of indivisibles, which enabled him to arrive at results equivalent to certain fundamental trigonometrical integrals.

WALLIS

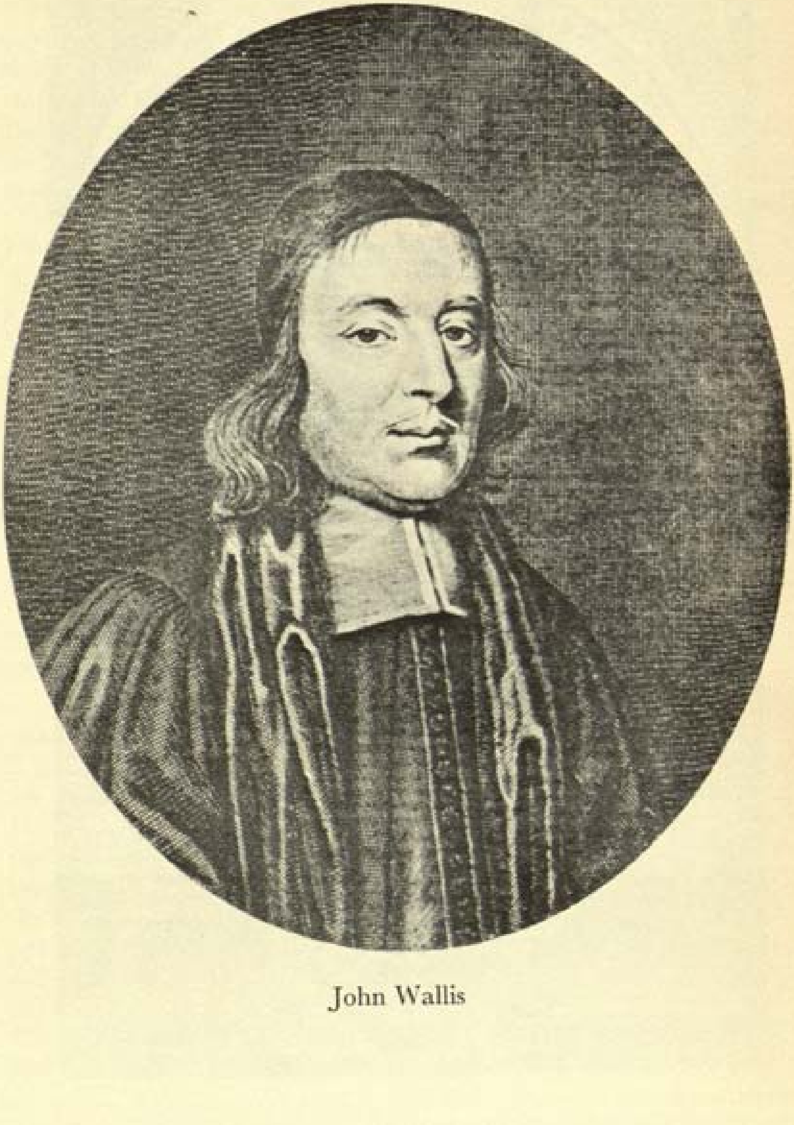
The synthesis of the methods of Cavalieri with analytical geometry and more advanced algebraic analysis was the chief contribution to the development of seventeenth-century mathematics made by John Wallis (1616–1703) in his *Arithmetica Infinitorum* (Oxford, 1655). As a mathematician and divine, Wallis was one of the most distinguished men of the generation which was responsible for the foundation of the Royal Society. He occupied a position intermediate between Descartes and Newton, and was a close friend of the latter.

Wallis' method of effecting the quadrature of a curve was to

John Napier

Illustr. 125

Blaise Pascal



John Wallis

Isaac Barrow

divide it by parallel ordinates into strips which were approximately parallelograms, to sum these, and to find the value which the sum approached when the number of strips was indefinitely increased. Wallis applied this method in particular to curves whose equations were of the general form $y = x^m$. While previous investigators had confined themselves to cases where m was a positive integer, Wallis recognized the significance of negative and fractional values of m, and he studied the results obtained when such values were assumed (so far as he could interpret them). These investigations led Wallis to attempt the quadrature of the circle. His methods readily gave him the areas enclosed by the axes of co-ordinates, and the curve $y = (1 - x^2)^m$, so long as m was a positive integer. Putting $m = \frac{1}{2}$, the equation of the circle was obtained: $y = \sqrt{(1 - x^2)}$, or $x^2 + y^2 = 1$; and the enclosed area was now that of a quadrant. Wallis saw that if this could be evaluated it would immediately yield an expression for the area of the circle, or for π. He formed the series corresponding to $m = 0, 1, 2, 3, \ldots$, and tried to find, by interpolation, the expression which would correspond to $m = \frac{1}{2}$. By an indirect method he succeeded in expressing π in the form of an infinite product which Lord Brouncker transformed into an endless continued fraction. Also Wallis' attempt led Newton some ten years later to follow up the matter and to discover the binomial theorem.

To Wallis is due the first general statement of the logarithmic series. He was also the first to apply Cartesian methods systematically to the geometry of conics.

BARROW

The immediate forerunner of Newton was Isaac Barrow (1630–77), his teacher and predecessor in the Lucasian Chair at Cambridge. In his *Lectiones opticae et geometricae* (1669), Barrow suggested a method for drawing a tangent at a given point on a curve, which may have influenced Newton in his invention of fluxions. From the equation of the curve Barrow calculated the slope of the chord joining the given point to a second point on the curve close to the first, and he found the value which this slope assumed when the difference in the co-ordinates of the two points became vanishingly small. This was the slope of the required tangent, which could accordingly be drawn.

NEWTON

Newton's numerous contributions to pure mathematics are overshadowed in importance by his invention of fluxions, which constitute one of the two chief tributaries from which the modern differential and integral calculus is derived.

The invention of fluxions, like the discovery of universal gravitation, seems to have originated from some mathematical experiments which Newton tried during his temporary retirement from Cambridge in the plague years of 1665 and 1666. Several of Newton's manuscript notes relating to these early attempts have survived (see S. P. Rigaud, *Historical Essay on the First Publication of Newton's "Principia,"* Oxford, 1838, Appendix, pp. 20–24). The solution of a problem dated November 13, 1665, may serve to illustrate his methods and the type of problem with which they were devised to deal. The problem runs:

"An equation being given expressing the relation of two or more lines x, y, z, etc., described in the same time by two or more moving bodies A, B, C, etc., to find the relation of their velocities p, q, r, etc."

Newton assumes that the "infinitely little lines" which the bodies describe each moment are proportional to their velocities while they are describing them. Hence if the body A, with velocity p, describes "the infinitely little line O in one moment," the body B, moving with velocity q, will meanwhile describe the line $\frac{Oq}{p}$, and so on. Hence the length of the line x described by A becomes $x + O$, y becomes $y + \frac{Oq}{p}$, etc. These new quantities must satisfy the same given equation as x, y, etc., and upon substituting them in that equation the required relation between the velocities is found.

Newton considers as an example the equation

$$rx + x^2 - y^2 = 0$$

involving the two variables x and y; and his procedure, expressed in modern index notation, is essentially as follows:

Substitution of the new values for x and y gives

$$rx + rO + x^2 + 2xO + O^2 - y^2 - \frac{2qOy}{p} - \frac{q^2O^2}{p^2} = 0$$

Subtract the original equation

$$rx + x^2 - y^2 = 0$$

and there remains

$$rO + 2xO + O^2 - \frac{2qOy}{p} - \frac{q^2O^2}{p^2} = 0$$

Dividing by O,

$$r + 2x + O - \frac{2qy}{p} - \frac{Oq^2}{p^2} = 0$$

"Also those terms in which O is, are infinitely less than those in which it is not. Therefore, blotting them out, there rests

$$r + 2x - \frac{2qy}{p} = 0, \quad \text{or} \quad pr + 2px = 2qy,"$$

which is the required relation between the velocities p and q.

Newton applied his method in drawing the tangent to a curve (thus systematizing Barrow's construction), and in determining the radius of curvature at any point on the curve.

These early exercises already illustrate Newton's fundamental conception of mathematical quantities as being generated by a continuous motion analogous to that of a point tracing out a line with a definite velocity. This idea was developed in Newton's successive writings upon the subject, and was explained, and applied to problems, in terms of a definite nomenclature and notation. "Lines are described, and thereby generated, not by the apposition of parts, but by the continued motion of points; superficies by the motion of lines; solids by the motion of superficies; angles by the rotation of the sides; portions of time by continual flux; and so in other quantities. . . . Therefore, considering that quantities, which increase in equal times, and by increasing are generated, become greater or less according to the greater or less velocity with which they increase or are generated; I sought a method of determining quantities from the velocities of the motions or increments with which they are generated; and calling these velocities of the motions or increments *Fluxions*, and the generated quantities *Fluents*, I fell by degrees upon the Method of Fluxions . . . in the years 1665 and 1666" (*Quadratura Curvarum*, 1704). Newton denoted the fluxion of any fluent quantity x by the symbol $\dot{x}$. Unless the rate of generation of the fluent be uniform, its fluxion has a finite fluxion of its own, which Newton denoted by $\ddot{x}$, and so on. Newton further defined the moments of quantities as "their indefinitely small parts, by the accession of which, in infinitely small portions of time, they are continually increased." He denoted these quantities, in his notation, by the symbols $O\dot{x}$, $O\dot{y}$, etc., where $\dot{x}$, $\dot{y}$ are the fluxions of x and y, and O is "an infinitely small quantity" of time or of some other equably increasing fluent. Newton's use of the above notation of "pricked letters" goes right back to his jottings of 1665, but he allowed many years to pass before publishing any formal or complete account of his new methods and their characteristic notation.

He first communicated a sketch of his method, with geometrical applications, to Barrow in 1669, but it was not until 1711 that this tract was published. He also referred to his method, with an application to the theory of equations, in a letter to Collins (December 10, 1672) which later figured prominently in the controversy with Leibniz. He worked out his ideas on fluxions more fully in a manuscript, *Methodus Fluxionum*, written in 1671. This first appeared posthumously sixty-five years later in an English translation (*Method of Fluxions*, 1736), though the substance of the manuscript was meanwhile given in the *De Quadratura Curvarum*, which formed an appendix to the *Opticks* of 1704.

From the beginning Newton recognized two types of fluxional problems inverse to each other: (1) "the relation of the fluents being given, to find the relation of their fluxions," and (2) the relation between the fluxions being given, to find the relation between the original fluents. The first problem (1) is illustrated by the simple example given above and corresponds to differentiation. The second problem (2) arose in connection with processes corresponding to integration and the solution of differential equations. Already in 1666, Newton was experimenting with the inverse process of constructing fluents from their fluxions, and of ascertaining the area of a given curve, regarded as the quantity whose fluxion was the ordinate. When he wrote his *Method of Fluxions* Newton seems to have been already familiar with processes analogous to partial differentiation and partial integration.

In the first section of Book I of the *Principia* (1687) Newton explained the elements of his theory of prime and ultimate ratios, i.e. of the limiting values of the ratios of two nascent or evanescent quantities, which forms the logical basis of the differential calculus. "By the ultimate ratio of vanishing quantities," he writes, "is to be understood the ratio of the quantities, not before they vanish, nor after they vanish, but with which they vanish. Similarly the prime ratio of nascent quantities is the ratio with which they begin their existence. . . . The ultimate ratios with which quantities vanish are not really the ratios of ultimate quantities, but the limits to which the ratios of quantities diminishing without limit perpetually approach." By means of this notion of a limit Newton was already seeking to escape, though not entirely successfully, from the difficulties involved in the use of infinitesimals, to which he rightly felt an objection. The fluxionary notation nowhere appears in the *Principia*, the methods of proof employed being those of pure geometry, which had undergone but little alteration since Alexandrian times. This was partly in deference to the opinions of contemporary mathematicians, few of whom would have followed analytical, and still fewer fluxionary, methods of proof with much ease or conviction. There can be little doubt, however, that Newton arrived at many of his results with the aid of fluxionary methods, which were especially suited to the type of problems with which he had to deal. It was in the *Principia*, moreover, that the properties of fluents were first openly discussed, and the word for fluxions (*fluxiones*), in the special sense, was first printed.

The earliest printed account of Newton's invention which employed the notation of "pricked letters" was that which he contributed to the Latin edition of Wallis' *Algebra* (1693). This is written in the third person; the earliest printed account to appear *under Newton's own name* was that given in his *Quadratura curvarum* (1704), which marks his greatest freedom from the use of infinitesimal quantities, of which he had made free use in his earlier manuscripts. He here insists that "there is no necessity of introducing figures infinitely small into geometry," for the fluxions of variable quantities are generally finite—a point the neglect of which led to much misunderstanding and confusion among eighteenth-century writers on the subject.

One of the unfortunate consequences of Newton's delay in giving his method of fluxions proper publication was the subsequent controversy with Leibniz concerning the latter's claim to be regarded as an independent inventor of the calculus.

Newton's discovery of the Binomial Theorem also belongs roughly to the early period of his retirement from Cambridge. He enunciated the formula, explained his methods of deriving it, and applied it geometrically, in two letters which he wrote, in 1676, to Oldenburg for the information of Leibniz. He appears to have arrived at the theorem by considering Wallis' unsolved problem of evaluating the area of the quadrant lying under the curve of $y = (1 - x^2)^{\frac{1}{2}}$ taken between the values $x = 0$ and $x = 1$. He studied the quadrature of curves having equations of the form $y = (1 - x^2)^m$, where m had the successive integral values 0, 1, 2, 3, . . . He found regularities in the resulting series which enabled him to obtain, by interpolation, the form assumed by the series when m was $\frac{1}{2}$, and he eventually arrived at a general expression for the expansion of any power of a binomial. He was thus able to express π in the form of an infinite series (as Wallis had sought to do), and he was able to extend Wallis' method to the quadrature and rectification of any curve whose ordinate was represented by a rational power of a binomial involving the abscissa x, since this could now be developed as an infinite series of terms in ascending powers of x to each of which Wallis' method could be applied. His partially completed researches in this direction were published in 1704.

In the first of his two letters to Oldenburg of 1676, Newton showed how to express an angle in the form of an infinite series of ascending powers of its sine, which he then reversed into another infinite series giving the sine in terms of the angle. He also alluded in the second letter to his method of fluxions, but the only vital portions were concealed in anagrams from which Leibniz, for whose information he was writing, could have gleaned nothing. A letter from Leibniz in the following year, however, showed that he had already developed his own form of the calculus, with its characteristic notation *dx*, *dy*, for increments in the co-ordinates of points on the curve.

As Lucasian Professor, Newton gave annual courses of lectures mainly embodying the results of his own researches. The contents of his early optical lectures are treated elsewhere; later he turned to algebra, and particularly the theory of equations, to which he made many contributions of a technical character. Chief of these were an important method (partly anticipated by Vieta) for approximating to the roots of numerical equations, and an expression for the sum of any given positive integral powers of the roots of an equation in terms of its coefficients. Much of Newton's work in algebra was later published in his *Arithmetica Universalis* (1707). As appendices to the first edition of the *Opticks* (1704) Newton added two mathematical tracts, *Enumeratio Linearum Tertii Ordinis* and *De Quadratura Curvarum*. Of these, the former applies analytical geometry to investigate the properties of cubic curves, but it contains much of general importance in the study of higher plane curves. The latter tract explains Newton's method of evaluating the areas and perimeters of curves by the extension of Wallis' method.

LEIBNIZ

The German philosopher, Gottfried Wilhelm Leibniz (1646–1716), has a place in the history of mathematics as being probably an independent inventor of the infinitesimal calculus, and certainly the originator of the notation now almost universally employed in that branch of mathematics. His claims, moreover, gave rise to a controversy which influenced the course of development of mathematics in Europe for more than a century.

Leibniz appears to have been led to the serious study of mathematics, in the first instance, by his meeting with Huygens while on a political mission to Paris in 1672. In the following year he spent some time in London, where he became acquainted with Oldenburg and the Royal Society. At this period Leibniz' researches in higher mathematics dealt chiefly with the quadrature of the circle, and of other curves, in terms of infinite series. He was led on, however, to consider general methods of summing the elements of which curves are composed, and in 1674, according to his subsequent claim, he invented the differential and integral calculus.

Manuscript notebooks of Leibniz, dated 1675–6, contain tentative experiments foreshadowing the processes of the differentiation and integration of the simplest expressions. Leibniz' ideas show a slow development, his characteristic notation, however, appearing almost from the first.

In 1677 Leibniz communicated his methods of drawing tangents to curves, and of solving inverse problems corresponding to integration, in a letter to Oldenburg. This was sent in reply to the second of the two letters in which Newton, in 1676, had enunciated the binomial theorem and had formulated (under cover of anagrams) the problem of finding fluxions from equations between fluents. Newton later made due acknowledgment of Leibniz' achievements in this field. In the first edition of the *Principia* (1687) he inserted a passage which may be rendered: "In letters which passed between me and that most excellent geometer, G. W. Leibniz, ten years ago, when I signified that I knew a method of determining maxima and minima, of drawing tangents and the like, and when I concealed it in transposed letters . . . that most distinguished man wrote back that he had also fallen upon a method of the same kind, and communicated his method, which hardly differed from mine, except in his forms of words and symbols." This sentence was retained in the second edition of the *Principia* of 1713, but the reference to Leibniz was suppressed in the third edition of 1726.

Meanwhile Leibniz had formally published particulars of his calculus in contributions to the *Acta Eruditorum*. A paper of his which appeared in 1684 laid down the principles of the differential calculus. He characterized its main problem as that of calculating the increment in the value of an expression consequent upon an infinitesimal increment in the variable upon which the value of the expression depends. Such an increment he called the "difference." He denoted it by the addition of the letter *d*, which in his notebooks he at first wrote in the denominator, but subsequently prefixed to the expression to be differentiated, as in the modern practice. Leibniz' symbol for the difference dx or dy was an improvement upon the arbitrary letters previously used, e.g. by Fermat, to denote small increments in a variable quantity. Whether, however, such differences were to be regarded as finite or infinitesimal in all cases was not made quite clear. Leibniz later employed a special notation for partial differentiation. In a second paper, appearing in 1686, Leibniz dealt chiefly with the integral calculus, in which the problem is the inverse one of determining the form of an expression, being given its difference. He recognized from the first the close connection between this process and that of the quadrature or cubature of figures. In his notebooks Leibniz began by denoting the sum of successive elements of a quantity by prefixing the word *omnia* (abbreviated *omn*), but he subsequently (1675) wrote for this the symbol ∫ (a long *s*—the initial letter of *summa*). This symbol first appeared in print in 1686. The term *calculus integralis* was suggested to Leibniz by Johann Bernoulli. Leibniz soon gained command of methods of differentiating sums and differences, products and quotients, and powers and roots of simple quantities (though with occasional misapprehensions), and he gave rules for determining maxima and minima.

Following Newton's acknowledgment to Leibniz in 1687, it was long assumed that the two men were independent inventors of their respective systems. In 1699, however, the Swiss mathematician, Fatio de Duillier, in a paper communicated to the Royal Society, suggested that Leibniz had obtained his ideas from Newton; but the Royal Society dissociated itself from this charge, though Newton did not appear in the controversy at this point. In 1705 Leibniz, in an anonymous review of Newton's *Opticks* and its mathematical appendices, which he wrote for the *Acta Eruditorum*, insinuated that Newton's fluxions were an adaptation of Leibniz' differences. This charge of plagiarism was hurled back at Leibniz in 1708 by John Keill, a lecturer on experimental physics at Oxford, and later Savilian Professor of Astronomy there. In response to Leibniz' appeal against this accusation, the Royal Society in 1712 appointed a committee (mainly composed of friends of Newton), which examined the documents bearing upon the controversy and published a report (*Commercium Epistolicum*, 1712). This report, however, merely asserted the priority of Newton against charges of plagiarism from Leibniz: it did not pronounce upon Leibniz' originality or upon the truth of Keill's charges. Moreover, it was hostile in tone to Leibniz, whose interests had not been properly represented on the committee. Further, the committee based its judgment upon the assumption that Leibniz had seen, in 1676, a certain document which might have given him valuable clues, whereas it was established by De Morgan in 1852 that he never received this document at all, but only a copy of it from which the vital portion was omitted. When Leibniz complained to the Royal Society of being unfairly treated, the Society disclaimed responsibility for the committee's report. Later, however, the matter was considered by a meeting at the Royal Society at which the foreign ambassadors were present. At the suggestion of one of these, Newton started personal negotiations with Leibniz. But Leibniz died before any conclusion was reached, and the controversy continued to rage for many years.

If Leibniz, contrary to his own consistent assertions, really derived the ideas underlying his calculus from Newton, it must have been not later than 1675 and presumably from some of Newton's early manuscripts on fluxions (if he was capable of understanding them), since he could have made nothing of Newton's anagrams, and there is no reason to think that he obtained any information of value during his visit to London in 1673. That Leibniz had access to such sources has never been established; but the possibility cannot be ruled out, and hence the controversy is, strictly, indeterminate, since most authorities hold that Leibniz' own testimony is not above suspicion. Yet the balance of opinion has steadily swung in Leibniz' favour, especially since the strong and reasoned advocacy of his case by De Morgan.

Leibniz' valuable invention of the differential and integral notation has never been contested, and De Morgan even thought that Newton derived from Leibniz "the idea of the permanent use of an organized mode of mathematical expression," in place of the occasional use of dots, and so was stimulated to systematize and to publish his own notation. Leibniz always attached great importance to the question of mathematical notation. He was deliberate in his introduction of new symbols, and discussed their value with contemporary mathematicians before adopting and publishing them. He attributed his discoveries to his use of improved notations, and most of his innovations have survived. Leibniz' differential notation was used in a book by John Craig, published in London as early as 1685—eight years before Newton's notation was printed. Generally speaking, however, Newton's notation was adhered to by English mathematicians throughout the eighteenth century, and is still extensively used by English writers on mechanics, while that of Leibniz was mainly, but by no means exclusively, employed by the French and German mathematicians. This created something of a barrier between English and Continental mathematics which, while it lasted, was injurious to both, but especially to the English school.

In his successive contributions to the *Acta Eruditorum*, from 1684 onwards, Leibniz worked out the principles and processes of elementary differentiation and integration, with geometrical and mechanical applications. In differential geometry he established the theory of envelopes. He also touched on other branches of mathematics, anticipating in his correspondence the use of *determinants* for the abbreviation of certain algebraic expressions, and devising a special notation to represent these quantities. Leibniz' contributions to mechanics are of less importance and of uneven value.

(See A. De Morgan, *Essays on the Life and Work of Newton*, edited by P. E. B. Jourdain, Chicago and London, 1914. G. A. Gibson, in the *Proceedings of the Edinburgh Mathematical Society*, Vol. XIV, 1895–6. F. Cajori, *A History of Mathematical Notations*, Chicago, 1928, 1929, Vol. II; *The Early Mathematical Manuscripts of Leibniz*, tr. by J. M. Child, 1920; F. Cajori, *A History of the Conceptions of Limits and Fluxions in Great Britain from Newton to Woodhouse*, 1919, and *A History of Mathematics*, New York, 1919; W. W. R. Ball, *A Short Account of the History of Mathematics*, 1908; D. E. Smith, *History of Mathematics*, Boston, 1923, 1925, and *A Source Book in Mathematics*, New York, 1929; J. F. Scott, *The Mathematical Work of John Wallis*, London, 1938; P. H. Osmund, *Isaac Barrow*, London, 1944; H. W. Turnbull, *The Mathematical Discoveries of Newton*, London and Glasgow, 1945.)

CHAPTER X

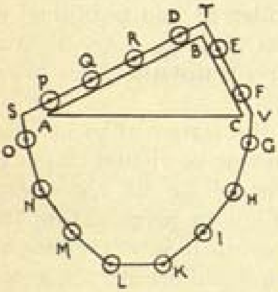
MECHANICS

STEVINUS

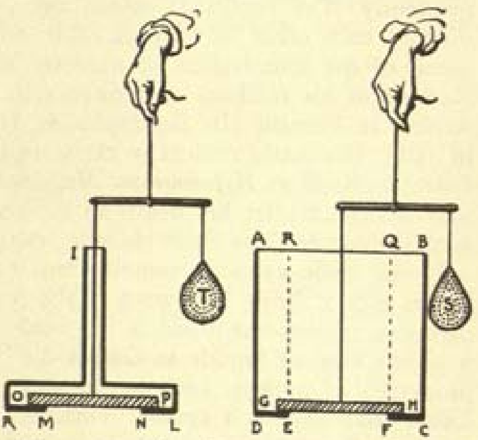
The first investigator to concern himself seriously with hydromechanical problems after the Renaissance was Simon Stevin, or Stevinus, of Bruges (1548–1620), a Flemish engineer and inventor who rose to high rank in the Dutch Army. Stevin and Galilei were almost contemporaries, but they carried on their researches independently. The results at which they arrived, however, supplemented each other in a remarkable manner, and together form much of the foundations of modern Mechanics. Stevin gave an account of his methods and discoveries in Mechanics in a book written in Flemish (*De Beghinselen der Weeghconst*) which appeared in 1586. His mathematical works were collected and published in Latin by Snell as *Hypomnemata Mathematica* (Leiden, 1605–8), and they appeared after his death in a French translation, *Les œuvres mathématiques de Simon Stevin* (Leiden, 1634).

Stevin made valuable contributions to the statics alike of solid bodies and of fluids. The germ of the principle of virtual displacements or velocities is found in his work, though he did not extend it to the case of liquids as Galilei did. Thus in investigating the properties of pulleys, both single and combined into tackles, he found that any such system remained in equilibrium when the product of each of the weights supported, into the distance through which it was moved by any given displacement of the system (or, what came to the same thing, into its velocity), was the same throughout the system.

Stevin arrived, by an original process of thought, at the conditions for equilibrium which hold for the inclined plane, and, ultimately, at the law of the parallelogram of forces. He intuitively apprehended rather than proved the truth of these results, which he deduced as follows. He considers an upright triangle, or a prism of triangular section, ABC (Illustr. 128), whose base AC is horizontal, and around which is slung a closed chain composed of equal and equally spaced masses P, Q, R, D . . . which can be slid without friction over the inclined sides of the prism. Such a chain must necessarily be in equilibrium, for otherwise it would be in a state of perpetual motion, which Stevinus assumes to be impossible. Further, this equilibrium cannot be disturbed by removing the equally heavy and symmetrical portions SL, VK, of the chain situated below the base. But in that case the shorter portion of the chain, resting on BC, must hold in equilibrium the longer portion resting on AB. But the weights of these portions are obviously as their lengths, i.e. as BC, AB. It follows, therefore, that two masses on the inclined planes AB, BC, connected by a string lying along both the planes, will remain in equilibrium if they are proportional to the lengths of these planes. If BC is perpendicular to AB, the law of the inclined plane takes the simpler form that the mass on BC must stand to the mass on BA in the same proportion as does the height of the plane to its horizontal length. Stevin himself was so astonished at the result of his investigation that he exclaimed: "Wonder en is gheen Wonder" (A wonder, and yet it is no wonder).



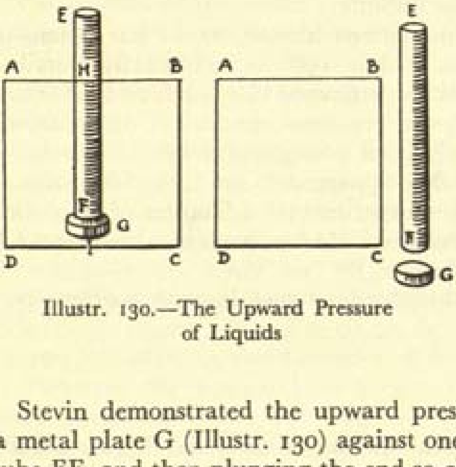
Illustr. 128.—Equilibrium on an Inclined Plane



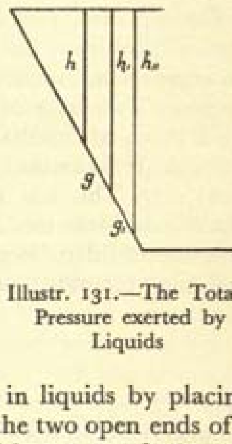
Illustr. 129.—Experimental Demonstration of the Hydrostatic Paradox

By considering a body as supported on an inclined plane by two strings respectively parallel and perpendicular to the plane, Stevin arrived at the law of the parallelogram, or at least of the rectangle, of forces, in its restricted application to statical problems.

Among Stevin's greatest achievements was his discovery of some of the most important laws of hydrostatics. For example, he gave an experimental demonstration of the so-called "hydrostatic paradox," i.e. of the law that the force exerted by a liquid on the bottom of a vessel containing it depends only upon the size of the surface under pressure and the height of the column of liquid above it, and not upon the shape of the vessel. Stevin demonstrated this property by means of the experiment represented in Illustr. 129. A vessel ABCD is filled with water and has at the bottom a round opening EF covered by a wooden disc GH. A second vessel IRL of the same height as the first and also filled with water has an opening of equal size in the bottom which is also covered by a wooden disc OP of the same weight as GH. It is found in the experiment that the discs GH, OP do not rise to the surface but remain pressed against the openings, and that in fact they are subject to equal pressures. This is proved by showing that these pressures are counterpoised, and that the discs are just raised, by the weights S, T, which are equal to each other and to the weight of the column of water ERQF over the disc GH. Stevin observes that in this manner one pound of water in a narrow tube could easily exert a pressure of one hundred thousand pounds' weight upon a plug in a wide vessel. It was upon this principle that the invention of the hydraulic press was later based.



Illustr. 130.—The Upward Pressure of Liquids



Illustr. 131.—The Total Pressure exerted by Liquids

Stevin demonstrated the upward pressure in liquids by placing a metal plate G (Illustr. 130) against one of the two open ends of a tube EF, and then plunging the end so closed into a vessel of water ABCD. It was observed that the plate did not fall off but remained pressed against the tube by the upward pressure of the liquid. Both the experiments just described are still shown to students of physics.

Stevin implicitly assumed the principle, later formulated by Pascal, that the pressure at any point in a liquid is the same in all directions. In order to compute the total pressure exerted upon a portion of the side of a vessel filled with water, Stevin proceeds by dividing up this portion by horizontal lines into a series of little parallel rectangular strips g, g_1, etc. (Illustr. 131). The strip g is under a pressure greater than that due to a prism of water of base g and height h, but less than that due to a prism of base g and height h_1. Similar limits can be set to the values of the pressure on the other strips. By summing the upper limits Stevin obtained a value for the total pressure which is too large, and by summing the lower limits, a value which is too small. As the strips are made narrower the two sums approximate from opposite sides to the same value, which is that of the required total pressure.

Finally Stevin investigated the conditions for the equilibrium of floating bodies. He found that the centre of gravity of such a body must lie in the same vertical line as that of the displaced liquid (centre of buoyancy). He supposed that, for stability, the centre of gravity of the body must lie below that of the displaced liquid, and that the lower it lay below the latter the greater would be the degree of stability. This latter statement, however, is not quite correct, for it is the position of the centre of gravity of the body in relation to a third point, the *metacentre* (that is, the point through which the resultant upward pressure of the fluid passes) which is now known to determine the stability.

For the most part Stevin confined himself, as we have seen, to statical problems, but in his book of 1586 he incidentally describes an experiment on falling bodies performed by himself and his friend Grotius. Two balls of lead, one ten times the weight of the other, were dropped simultaneously from a height of about 30 feet on to a plank. It was noted that they appeared to reach the latter simultaneously. This was the first experimental refutation of Aristotle's dynamical ideas (see M. Steichen: *Mémoire sur la vie et les travaux de Simon Stevin*, Bruxelles, 1846, p. 25).

Stevin's contributions to the introduction of decimals are described elsewhere.

TORRICELLI

Several of Galilei's disciples extended their researches to include the mechanics of liquids and gases. Their leader in this field was Torricelli, the greatest of them all.

Evangelista Torricelli was born in 1608 at Faenza, of a distinguished family. He went to Rome at the age of twenty and studied there under Castelli, the close friend of Galilei and propagator of his ideas. Torricelli was stimulated by Galilei's *Discorsi* of 1638 to write for himself on mechanics with the object of proving some of the Galilean laws of motion. His book was the means of attaching him to Galilei, under whose direction he worked until the old man's death. Torricelli was the natural heir to the position and the authority of Galilei, in whose spirit he continued to work at Florence until his own early death in 1647.

Torricelli supplemented the dynamics of solid bodies, established by Galilei, by creating the dynamics of liquids. His fundamental work in hydrodynamics is to be found in his *Opera Geometrica*

(Florence, 1644), in the section entitled *De motu gravium naturaliter descendentium.* He here proves that a jet issuing from a hole in the side of a vessel full of water follows a parabolic path. He further shows that its velocity of efflux (and hence also the quantity escaping in unit time) is proportional to the velocity acquired by a body falling freely from the level of the water-surface to the level of the hole, and hence proportional to the square root of the height of the column of water above the hole. The exact relation between the velocity of efflux and head of pressure was formulated later by Johann and Daniel Bernoulli. The time required to empty such a vessel depends upon the velocity of efflux at each instant, and it follows from the above law that the times in which similar vessels are emptied through holes of equal size are as the square roots of the heights of the water columns above the holes. If the hole is situated in the horizontal bottom of the vessel, it follows, according to Torricelli, that the quantities of water escaping in successive equal intervals of time fall off in proportion to successive odd numbers. For example, if the time necessary completely to empty the vessel is 6 seconds, and the quantity flowing out in the last second be represented by 1, then the quantities flowing out in the 5th, 4th, 3rd . . . seconds will be represented by 3, 5 7. . . .

Torricelli describes also how fountains rise nearly to the levels of their respective heads, and he attributes the slight differences in level partly to air-resistance and partly to the weight of the superincumbent column pressing down on the water issuing from the jet.

Some advances in the dynamics of solid bodies were also made by Torricelli. He seems to have been acquainted with the principle that a connected system of weights at rest will start moving under gravity only if the motion results in the descent of the centre of gravity of the system. Torricelli made a special study of the motion of projectiles. He showed that the trajectories of all projectiles discharged with equal velocities from a given point are enveloped by a paraboloid; and further, that with a given initial velocity, the range corresponding to any given elevation of $(45^\circ + a)$ equals that for an elevation of $(45^\circ - a)$.

Torricelli's important researches in pneumatics are described in connection with the history of the barometer.

PASCAL

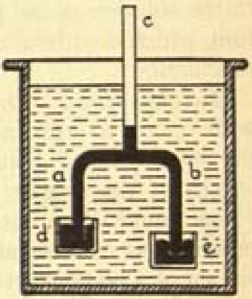
An important landmark in the development of mechanics during the seventeenth century was the publication of Blaise Pascal's *Traitez de l'équilibre des liqueurs et de la pesanteur de la masse de l'air* (Paris, 1663). This book consists of two separate treatises, dealing respectively with hydrostatics and pneumatics, and some concluding reflections. It was completed in 1653, but first appeared after Pascal's death. It marked an advance on the work of Stevin and Galilei, and it is remarkable alike for the clarity of its style and for the convincing experiments which it describes.

Pascal based his investigations in hydrostatics upon the principle that the pressure at any point in a fluid is the same in all directions. He further followed the precedent of Galilei in applying the principle of virtual velocities or displacements to hydrostatics, but with an important development. He considered every portion of fluid imprisoned in a containing vessel as a machine in which the forces acting are brought into equilibrium in accordance with certain definite relations, as in the case of the lever and the other simple machines. We may, for example, consider with Pascal two communicating cylinders of different cross-sectional areas, containing a fluid and closed by pistons. If the pistons are loaded with weights proportionate to their areas, equilibrium is maintained. Pascal regards such a system as a machine analogous to a lever with unequal arms. He recognizes that in both cases equilibrium involves the same relation between the forces acting and the motions corresponding to any hypothetical displacement of the system. "It is surprising to find," he writes, "in this new machine that invariable regularity which occurs in all the old ones, such as the lever, the pulley, the endless screw, etc., namely, the distances vary [inversely] as the forces (*le chemin est augmenté en mesme proportion que la force*). . . . This can be taken as the true reason for this effect, for it is clear that it is the same thing to move a hundred pounds of water through one inch as to move one pound of water through a hundred inches. And thus when a pound of water is in such adjustment with a hundred pounds of water that the hundred pounds cannot move itself an inch without moving the pound through a hundred inches, they must remain in equilibrium, one pound having as much power (*force*) to move a hundred pounds through one inch as a hundred pounds have to move one pound through a hundred inches." This passage anticipates the Principle of Virtual Work, and reveals Pascal's underlying assumptions.

Pascal proceeds to the more general result that a fluid contained in a vessel in whose sides are openings stopped with pistons is in equilibrium when the forces on these pistons are proportional to their areas.

Some of Pascal's most important contributions to pneumatics belong to an account of the invention of the barometer. He attributed the behaviour of this instrument to atmospheric pressure, as against the alleged *horror vacui*, and in 1648 he published in a tract the results of the experiments which he had devised to establish this view. He returns to this subject in the pneumatical sections of his larger work, where he explains how many familiar phenomena must be conceived as the effects of atmospheric pressure. To this agency are ascribed the phenomena of suction, cupping, pumping, siphoning, breathing, etc., and, erroneously, of the adhesion between polished plates.

One of the most important advances made by Pascal was his recognition of the correspondence between phenomena produced by atmospheric pressure and phenomena due to the pressure exerted by a liquid. As an example of this principle and of the kind of experimental demonstrations given by Pascal, we may describe his apparatus for operating a siphon by the pressure of water. He took a bifurcated tube *abc* (Illustr. 132), open at its three ends, and having the limb *b* longer than the limb *a*; and he dipped these limbs into cups of mercury *d*, *e*, at different levels. The apparatus was immersed in a vessel of water, and upon its being lowered to a sufficient depth the mercury rose in the two branches until the columns flowed together, whereupon mercury flowed from the higher cup *d* to the lower cup *e*. The water served by its pressure upon the external mercury surfaces to maintain the mercury at a sufficient level in the tube for the flow to take place, and so played a part similar to that of the air in an ordinary siphon. That the effect was not due to *horror vacui* was clear, since the air was free to fill the portions of the tube unoccupied by mercury.



Illustr. 132.—Siphon Operated by the Pressure of Water

HUYGENS

Huygens did not restrict his investigations on pendular motion to the case of the simple pendulum, in which a particle is supposed to swing at the end of a flexible weightless thread. Soon after Galilei's researches had become known in northern Europe, Mersenne raised the question, according to what laws did extended bodies of arbitrary shape vibrate if free to turn under gravity about fixed axes. Several keen mathematicians, including Descartes and Huygens (who was then only seventeen years of age), took up the problem. For the time being it defied solution, but Descartes introduced the notion of a "centre of agitation" in any compound pendulum (analogous to a centre of gravity), whose distance from the point of suspension determined the period of vibration of the pendulum. Huygens gave a clear definition of this point (which he termed the "centre of oscillation") and a general method of determining its position for a given pendulum, in his *Horologium Oscillatorium* (1673) twenty-seven years after the problem had been first propounded.

Only the first and last of the five parts into which this book is divided are primarily concerned with clocks. The second part treats of the motions of particles under gravity when allowed to fall freely or when constrained to move along smooth planes or curves, and it culminates in a proof of the tautochronous property of the cycloid; while in the third part the theory of the evolutes of curves is established. These subjects are investigated for the sake of their application to the construction of strictly isochronous pendulums. Huygens devotes the fourth part of his book, however, to the solution of the problem of the physical or compound pendulum, which is without question his greatest achievement in theoretical mechanics.

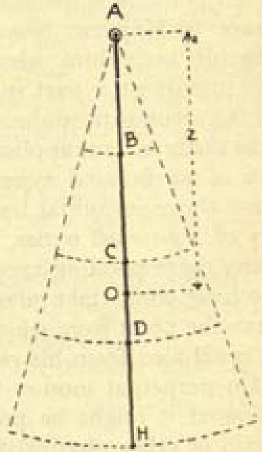
Huygens defines the centre of oscillation of any given figure, with regard to a given axis of suspension, as the point on the axis of the figure whose distance from the axis of suspension equals the length of the simple pendulum whose period equals that of the given figure. The entire mass of the swinging body can thus be regarded as concentrated into a particle situated at this point, just as that of a body at rest can be regarded as concentrated at its centre of gravity.

The simplest form of the problem of finding a centre of oscillation is represented in Illustr. 133. Two particles a and b are suspended rigidly from o in the line oba, and it is required to find the length ox of the equivalent simple pendulum which has the same period as the combination of a and b. The particle a retards the particle b, while b accelerates a, so that b vibrates more slowly and a more rapidly than each would, if separately suspended. Hence the required point x must lie somewhere between a and b.

The most general form of this problem is that of compounding the motions of the infinite number of rigidly connected particles which together make up a physical pendulum. Thus, in Illustr. 134, given that B, C, D . . . are a series of rigidly connected particles having masses m_1, m_2, m_3 . . ., and situated at distances a_1, a_2 a_3 . . . from the axis A, about which the system swings; it is required to find the centre of oscillation O, or the length AO ($= z$, say) of the equivalent simple pendulum.

Huygens' solution is based primarily upon a mechanical principle which he was the first to formulate, and which proved to be of the greatest consequence both in this and in subsequent applications. He formulated it in the words: "If any masses begin to move under the force of their own gravity, their common centre of gravity cannot rise higher than it was when the motion began." He lays it down as experimentally established that the centre of gravity of a pendulum would describe equal arcs as it alternately falls and rises, if all resistance due to air and other factors were eliminated. Huygens further makes use of the fact, known from the laws of falling bodies, that the height to which any given particle of the pendulum rises above its lowest level is proportional to the square of the velocity which it acquires in falling to that level. Again, the velocities of the particles are proportional to their respective distances from the axis

Illustr. 133.—Centre of Oscillation



Illustr. 134.—Centre of Oscillation (General Form)

of suspension. With the aid of these considerations, Huygens arrives at a general solution of his problem in which the required distance AO ($= z$) is given by the sum of the products of the masses of the particles into the squares of their respective distances from the axis of suspension, divided by the sum of the products of the masses into their respective simple distances. That is:

$$z = \frac{m_1a_1^2 + m_2a_2^2 + m_3a_3^2 + \ldots}{m_1a_1 + m_2a_2 + m_3a_3 + \ldots} \text{ or, more shortly,}$$

$$z = \frac{\Sigma(ma^2)}{\Sigma(ma)}.$$

When Euler had introduced the expression "moment of inertia" for the quantity $\Sigma(ma^2)$, Huygens' Law could be expressed, as it is now, by saying that the distance of the centre of oscillation of a compound pendulum from the axis of suspension is obtained by dividing the moment of inertia of the pendulum about that axis by its statical moment about the same axis.

Huygens goes on to apply this fundamental result to ascertain the centres of oscillation of several geometrical figures, including the circle, rectangle, parabolic segment, cone, sphere, etc. He shows that the vibrations of a pendulum about parallel axes equidistant from the centre of gravity are isochronous; and also that the period is not altered when the pendulum is suspended by a new axis through its centre of oscillation parallel to the old one, in which case the old point of suspension becomes the new centre of oscillation. Hence to Huygens must be attributed the idea underlying the reversible pendulum which, during the nineteenth century played so important a part in the more precise estimation of the length o. the seconds pendulum.

Huygens made several applications of his principle that the centre of gravity of an isolated system of bodies moving under gravity cannot rise above its initial level. He employed it to disprove the possibility of a *perpetual motion*, in which force would be generated without any corresponding expenditure. Such a generation of force out of nothing could take place only if the mass concerned rose higher than the point from which it previously fell. While, however, Huygens concluded from his principle that it would be impossible to maintain perpetual motion by purely "mechanical" means, he still considered it might be possible to achieve perpetual motion with the aid of other physical forces, e.g. by employing a magnet. On the other hand Mersenne, as early as 1644, denied that a perpetual motion was possible at all, and he likened the efforts to construct one to the search for the Philosophers' Stone. Huygens' principle was later erected by Johann Bernoulli into a general law of nature, and it was called the "Principle of the Conservation of *vis viva*." By the *vis viva* of a particle is meant the product of its mass into the square of its velocity (mv^2); the expression is due to Leibniz, who was already reflecting upon the total quantity of force present in the Universe.

At the conclusion of his book on the pendulum clock Huygens gives his fundamental propositions on the so-called "centrifugal force." Here also it is an extension of Galilei's doctrine of pendular motion which is involved.

In order to constrain a body, initially moving uniformly in a straight line, to move uniformly in a circle, it is necessary to exert upon it a pull directed radially towards the centre of the circle—say by means of the tension in a string joining it to the centre. The equal and opposite reaction of the body is represented by a pull directed radially outward from the centre. This is the centrifugal force; and Huygens proves that it varies directly as the square of the velocity of the body, and inversely as the radius of the circle.

Already in 1669 Huygens had communicated this and a number of other results to Oldenburg in the form of anagrams, and he dealt with the subject in greater detail in his *Tractatus de vi centrifuga*, which was published posthumously in 1703. (It has been edited in German in Ostwald's *Klassiker*, No. 138.) Before this date Newton had dealt with the theory of centrifugal force from a much more general standpoint. He must have discovered independently Huygens' formula for motion in a circle, but from this restricted case he extended his investigation of such problems to include the elliptical motions of the planets. Most of the propositions in Huygens' tract had already been enunciated without proof in his *Horologium Oscillatorium*. The most important of them are the following:—

"If equal bodies revolve uniformly in equal circles (or in the same circle) with unequal velocities, the centrifugal forces are as the squares of the velocities" (Prop. II);

"If equal bodies revolve with equal velocities in unequal circles, the centrifugal forces are inversely as the diameters, so that the said force is greater in the smaller circle" (Prop. III).

Huygens' results may be expressed in modern notation by the relations

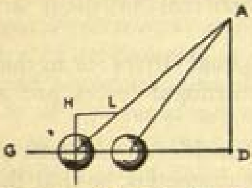
$$P = \frac{mv^2}{r} = mr\omega^2$$

where P is the centrifugal force, m the mass, v the linear velocity, and ω the angular velocity of the particle, and r the radius of the circle.

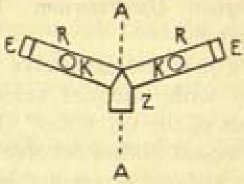
In the course of his tract Huygens investigates how great the velocity of a body in a given circular path must be in order that the centrifugal force may overcome gravity. He further discusses the centrifugal force arising from pendular motion, and finds, for example, that a simple pendulum whose bob swings through a complete quadrant on each side will, as it passes through its lowest point, exert a tension upon the string three times as great as when it hangs at rest.

He finally considers in detail the case of the conical pendulum in which a particle at one end of a string uniformly describes a horizontal circle while the other end of the string remains fixed. He finds that, for two such pendulums (Illustr. 135) with equal masses, and of the same height, AD, but of unequal lengths, the tensions in the strings are proportional to their lengths (Prop. XV). Proofs of the other principal properties of the conical pendulum are also to be found in this portion of the work (Props. VIII-XIV).

Among the experiments on centrifugal force which Huygens performed, the following are particularly remarkable. He placed some wooden balls in a vessel full of water and then set the vessel in rotation about its axis. The balls immediately moved towards the axis, thus proving that the centrifugal force of revolving bodies depends upon their specific gravities. Nowadays this experiment is performed by placing wooden balls in the tubes RR of the apparatus shown in Illustr. 136, which is rotated about the axis AA. If the tubes contain air, the balls move out from the axis and, when the rotation is sufficiently rapid, they move upwards. If, however, the tubes are completely filled with water, the wooden balls, being specifically lighter, move towards the axis. This descent of wooden



Illustr. 135.—The Tension in the Strings of Pendulums



Illustr. 136.—The Centrifugal Force of Bodies and their Specific Gravity

balls in tubes filled with water at first causes surprise; but a familiar technical use is made of this effect in machines for separating the watery constituents of milk from the specifically lighter cream which it contains. Huygens made the results of a somewhat similar experiment the basis for his theory of gravitation. On another occasion he tried the effect of a centrifugal force on a clay sphere which he set in rapid rotation about a diameter. He knew that every particle of a rotating body not lying in the axis of rotation is subject to a centrifugal force which increases with its distance from the axis. He concluded that if the particles were not rigidly connected—for example, if the body was made of a plastic material—deformations should appear. He found, in fact, that his rotating clay sphere assumed the form of a spheroid flattened at the poles. From this experiment and the reflections to which it gave rise, Huygens was able to interpret the flattening of Jupiter which he had observed and which appeared to him as the surest indication that the planet, like the Earth, rotates on an axis. He further concluded that the notion of the spherical figure of the Earth, underlying all previous attempts to measure the length of a degree on its surface, was presumably incorrect. For if the Earth rotates and is not an absolutely rigid body, it too must depart from a spherical form. Huygens made calculations which pointed to a value of 1 : 587 for the Earth's ellipticity, and this, with the more refined calculations of Newton, helped to focus the attention of astronomers upon the problem of the figure of the Earth.

Huygens was led to anticipate certain restricted forms of the law of the conservation of *vis viva*, not only by his above-mentioned investigation of the motion of pendulums, but more particularly by some researches which he and several contemporary physicists carried out on the impact of bodies.

Impact

During the first half of the seventeenth century there was nothing like a general theory of impact, though some particular cases of the collision of elastic bodies were correctly treated by Marcus Marci in 1639, and Galilei discussed the nature of impulsive forces, which he contrasted with static pressures. Galilei had intended to deal with the problem of impact in his *Discorsi* of 1638. This section of the book is incomplete, but it seems certain that Galilei arrived at no general conclusions on the subject. Descartes, in his *Principia* of 1644, formulates eight laws of impact, but these are largely incorrect, containing such statements as the following: If a body C is greater than a body B and is at rest, then with whatever speed B approaches C it can never move C, whose resistance is greater the greater the speed of B. B rebounds towards the point whence it came (II, § 49). Descartes, moreover, failed to distinguish clearly between elastic and inelastic bodies. A few further particular cases of the problem were studied by Borelli.

In 1668, however, the recently established Royal Society set certain of its members the problem of investigating the laws of impact so as to make good this deficiency in the principles of mechanics. In response to the Society's invitation, Wallis, Wren, and Huygens shortly afterwards sent in papers dealing, each in his own way, with the problem before them.

WALLIS

The first to lay his results before the Society was Wallis, whose paper was read on November 26, 1668, and was subsequently published in the *Philosophical Transactions* (Vol. III, No. 43, *A Summary Account given by Dr. John Wallis of the General Laws of Motion*). Wallis considers primarily the impact of inelastic bodies travelling in the straight line joining their centres of gravity, though he also makes some provision in his paper for cases of oblique impact, and subsequently (1671) published results on elastic impact.

In deriving his formulae Wallis employs the conception of quantity of motion already occurring in the writings of Descartes. He regards the force (*vis*) moving a given body as proportional jointly to the weight (*pondus*) and to the speed (*celeritas*) of that body. If we call the masses of the impinging bodies m and m_1, their respective velocities before impact v and v_1, and the common velocity of the masses after impact u, the equation at which Wallis arrived may be expressed in the form

$$u = \frac{mv + m_1 v_1}{m + m_1}$$

when the bodies are initially moving in the same direction, and the form

$$u = \frac{mv - m_1 v_1}{m + m_1}$$

when they are initially moving in opposite directions.

WREN

A second, slighter, contribution to the problem proposed by the Royal Society was made by one of its founder-members, Dr. (later Sir) Christopher Wren, celebrated as the architect of St. Paul's Cathedral and of numerous other public buildings of this period. By experiments with suspended bodies made in collaboration with Rooke, Wren discovered the empirical laws of impact for elastic bodies without, however, being able to derive them theoretically. Wren's results were submitted to the Society on December 17, 1668 (see *Phil. Trans.*, III, No. 43). A little later (January 4, 1669) Oldenburg received from Huygens an account of the laws of central elastic impact without theoretical proofs (see *Phil. Trans.*, IV, No. 46). The results obtained by Wren and Huygens, although very similar, were arrived at independently. Huygens, who noticed that the motion of the centre of gravity of impinging bodies was unaffected by their impact, also communicated his results to the Paris Academy about the same time. He seems, judging from some of his correspondence, to have arrived at them at least as early as 1656 (see Felix Hausdorff in Ostwald's *Klassiker*, No. 138).

Experiments on impact more systematic than those of Wren were carried out by Mariotte and described by him in his *Traité de la percussion ou choc des corps* (Paris, 1677).

HUYGENS

Huygens dealt with the subject of impact in greater detail (giving proofs of his propositions) in his *Tractatus de motu corporum ex per-*

cussione, which was published in 1703, eight years after his death (for a German edition, see Ostwald's *Klassiker*, No. 138, Pt. I).

This book, which is of fundamental importance on the subject, consists of five hypotheses and thirteen propositions. The first hypothesis is Newton's First Law. The second implicitly postulates perfect elasticity of impact, although Huygens nowhere uses the expression. It contains the sentence: "If two equal bodies with equal velocities impinge upon each other directly, and from opposite directions, each recoils with the same velocity with which it came." Next comes Huygens' own important axiom on relative motion, according to which the motion of bodies, and the equality or inequality of their velocities, must be conceived relatively, i.e. with regard to other bodies considered as at rest, even though they may be affected by a further motion common to the whole system. By way of example Huygens explains the case in which a passenger on a moving ship makes two equal spheres collide with each other with equal velocities (relative to the ship) in the direction of the ship's course. The spheres will appear to him to rebound from each other with equal velocities. To a spectator standing on the shore, however, supposing the velocities of the spheres to be equal to that of the ship, one sphere must appear motionless after the impact while the other recoils with a velocity twice as great as that which the passenger originally imparted to it.

All the propositions in Huygens' tract relate to central impact, but as the proportions of the masses and velocities of the impinging bodies are altered, a variety of particular cases arises for consideration. Among the most noteworthy of these cases is the following: "If upon a body at rest another body equal to it impinges, the latter will come to rest after impact, while the body initially at rest will acquire the velocity of the impinging body" (Prop. I). This proposition is a particular case of the following: "If two equal bodies collide with unequal velocities, they will move after impact with interchanged velocities" (Prop. II). This proposition, and more especially the celebrated Proposition XI, are expressions of the comprehensive principle that the total energy of motion of perfectly elastic bodies is unaltered by impact. The eleventh proposition runs: "In the mutual impact of two bodies the sum of the products of the masses into the squares of the respective velocities is the same before and after impact." It was this product of the body's mass into the square of its velocity which was called, following Leibniz, *vis viva*, and so, in this law of Huygens, already formulated in his paper on impact in 1669, the principle of the conservation of *vis viva*, the most comprehensive principle of Mechanics, found partial expression for the first time. Its full significance could only be realized later when heat had come to be recognized as a particular form of motion (or energy). A declaration of the universal validity of such a law is already found in the words of Leibniz, who wrote: "The universe is a system of bodies which do not communicate with other bodies. Therefore there remains in it always the same force" (*Mathematische Schriften*, Halle, 1860, II Abt., Bd. II, p. 434). Leibniz elsewhere remarks that any force absorbed by the smallest particles upon impact is not lost to the universe.

MARIOTTE

Among the friends and contemporaries of Huygens was the French priest, Edmé Mariotte (?1620–84). He was one of the earliest members of the *Académie des Sciences* (which he joined in the year of its foundation, 1666), and he made contributions to several branches of science, including mechanics, optics, heat, and meteorology.

Mariotte advanced hydromechanics by his *Traité du mouvement des eaux et des autres corps fluides* (1686). This deals with the equilibrium of liquids and floating bodies, and in particular with the efflux of liquids from vessels, and with the resulting friction, by reference to which Mariotte explained many discrepancies between theory and experiment. In this book he describes the familiar apparatus still known as "Mariotte's Bottle" which enables the pressure under which a liquid flows out of an orifice to be kept constant for an appreciable time. He gives the earliest rules for comparing the strength of the walls of cylindrical tubes exposed to internal pressure. He discusses the motion of water in such tubes, the impact of fluids, the heights to which fountains rise, and many other questions of equal scientific and technical importance. Mariotte appears to have been stimulated to undertake these researches in hydrostatics and hydrodynamics by the magnificent waterworks at Versailles.

Mariotte also occupied himself with the mechanics of solid bodies, and devoted the greater part of his *Traité de la percussion ou choc des corps* (Paris, 1677) to the laws of impact, which he had experimentally investigated by means of an apparatus specially contrived for the purpose. This consisted of two balls, made of soft clay or of ivory, according to the type of impact desired, which were suspended from a wooden framework by threads, so as to be in horizontal contact. These balls could be drawn aside over graduated arcs and allowed to impinge upon each other at velocities which could be controlled and calculated according to the initial deflections, the motion after impact being studied. With this apparatus Mariotte was able to demonstrate the elementary laws of impact, including that of the conservation of "quantity of movement" (jointly depending upon the weights and the velocities of the impinging bodies). Mariotte further describes experiments with a sort of ballistic pendulum, in which a small suspended cannon was made to discharge a suspended cylinder, the subsequent speeds of the cannon and the cylinder in opposite directions (known from the heights to which they rose) being shown to be inversely as their weights. Other sections of the work are devoted to the impact of fluids upon solid bodies and to the stroke of "thunderbolts." (See p. 243 *infra*.)

NEWTON

Newton summed up the results of earlier work on impact in the introductory Scholium to Book I of his *Principia*. He makes allowance for the fact that no body in nature is perfectly elastic, so that the relative velocities of impinging bodies are diminished (as well as reversed) by their impact, in a definite proportion, depending upon the substance of which they are composed. Newton himself experimentally determined this proportion (often called the *coefficient of restitution*) for wood, cork, steel, and glass.

PNEUMATICS

The sections on the thermometer, barometer, and air-pump in Chapter V have already dealt generally with the study of the physical properties of air. The story of the study of its chemical properties will be told in Chapter XV. In the present section it is proposed to give an account of the discovery and establishment of what is variously known as Boyle's Law, Mariotte's Law, or Boyle–Mariotte Law, namely the law that, at constant temperature, the pressure of a gas multiplied by its volume (pv) is constant. The credit for the discovery may be divided between Boyle, Hooke, and Towneley—Boyle receiving the lion's share. Mariotte did little more than repeat a few of Boyle's experiments, and advertise, on the Continent, the importance of the discovery.

BOYLE'S LAW

His experiments with the air-pump (or "new pneumatical engine") suggested to Boyle that the air has a "spring." He supposed that the air contains parts which can be bent or compressed either by the weight of the superincumbent layers of the atmosphere or by other kinds of pressure; and that the compressed air can recover its previous dimensions when the said pressure is removed. His ideas and experiments on the subject are fully reported in his *New Experiments Physico-Mechanicall Touching the Spring of the Air and its Effects,* *Made for the most part in a New Pneumatical Engine* (Oxford, 1660). His idea of the "spring" of the air is explained by him as follows:

"This notion may perhaps be somewhat further explained by conceiving the Air near the Earth to be such a heap of little Bodies, lying one upon another, as may be resembled to a Fleece of Wooll. For this (to omit other likenesses betwixt them) consists of many slender and flexible Hairs; each of which may indeed, like a little Spring, be easily bent or rouled up; but will also, like a Spring, be still endeavouring to stretch itself out again. For though both these Haires and the Aerial Corpuscles to which we liken them, do easily yield to external pressures; yet each of them (by virtue of its structure) is endow'd with a Power or Principle of self-Dilatation" (*op. cit.*, p. 23; *Works*, ed. 1772, Vol. I, p. 11).

Boyle was aware that Descartes had offered a different explanation, but he found his own easier, although he was not willing to decide between them. Moreover, his purpose (he said) was to demonstrate, not to explain, the "Spring" of the air; and most of his experiments were directed to this end.

In one of these experiments Boyle took a partially inflated lamb's bladder, securely tied at the neck, and placed it in the receiver of his air-pump. As evacuation proceeded the bladder swelled as if it had been blown out; whereas, when air was readmitted into the receiver the bladder grew flaccid again. To prove that this phenomenon was due to the "spring" of the enclosed air, Boyle showed that two other bladders, from one of which all the air had been pressed out and which had been securely tied at the neck and the other of which contained about one-fifth of the air formerly contained in the first bladder and was not tied at the neck, did not exhibit the phenomenon. Other tied bladders were shown to burst when the receiver was sufficiently evacuated. These results showed that the air possessed a "spring."

But it was Experiment 17 that Boyle considered "the principal fruit" of his "Engine." He knew that in the Torricellian experiment the mercury in a closed tube remained at a height of 27 digits above the level of the mercury in which the tube was inverted; and he thought that if it was the case that the mercury remained at this height solely because "at that altitude the Mercurial Cylinder in the Tube is in an Aequilibrium with the Cylinder of Air, suppos'd to reach from the adjacent Mercury to the top of the Atmosphere" (*op. cit.*, p. 106; *Works*, ed. 1772, Vol. I, p. 33), then, if the experiment could be tried so as to exclude the atmosphere, the mercury in the tube would fall to the same level as that of the mercury in the open vessel in which the tube was inverted, since there would be no air pressure to resist the weight of the mercury in the tube.

If the experiment could be tried in his "Engine," he expected that the mercury would fall in proportion to the exhaustion of the air. Accordingly, a glass tube closed at one end and well filled with mercury, was inverted over a vessel containing mercury and then placed in the receiver of the air-pump. The upper end of the tube passed through a hole in the cover of the receiver made air-tight by a cement. As soon as pumping began the mercury fell as predicted, the extent of successive falls diminishing with each stroke of the pump. The inner column of mercury, however, could not be brought down completely to the level of the outer mercury, but remained an inch higher. This difference Boyle attributed to the inward leakage of air. The evidence convinced Boyle that the standing of the column of mercury in a closed tube at a determinate height was due to the equilibrium between the pressure of the mercury and that of the external air. The experiment was repeated in the presence of "those excellent and deservedly Famous Mathematic Professors, Dr. Wallis, Dr. Ward, and Mr. Wren . . . and 'twas by their guess, that the top of the Quick-silver in the Tube was defin'd to be brought within an Inch of the surface of that in the Vessel" (*op. cit.*, pp. 111, 112; *Works*, ed. 1772, Vol. I, p. 34). Boyle also found that, by compressing more air into the receiver by means of the pump, the mercury "would ascend much above the wonted height of 27 digits, and immediately upon the letting out of that Air would fall again to the height it rested at before" (*op. cit.*, p. 119; *Works*, ed. 1772, Vol. I, p. 36).

In another experiment Boyle attempted to weigh air. A glass bulb "about the bigness of a small Hen-egge" was blown and sealed off "with as little rarefaction as might be." It was fastened to one of the scales of Boyle's "exact pair of Ballances," and was counterpoised with a piece of lead. The whole was then put in the receiver which was thereupon evacuated. The pan containing the bulb was depressed, its depression increasing as evacuation proceeded. On admitting air, the former equilibrium was restored. At this stage a weight of three-quarters of a grain was added to the pan containing the lead, and the experiment was repeated. At length the beam became horizontal, as evacuation was continued; but this position could not be attained when a further quarter of a grain was added to the lead. Boyle estimated that the weight of the air in the bulb was "above a grain," recognizing that evacuation was not complete. He repeated the experiment with the bulb unsealed and found that in these circumstances it did not outweigh the lead when evacuation was carried out—"so that by the help of our Engine we can weigh the Aire, as we weigh other Bodies, in its natural or ordinary consistence without at all condensing it" (*op. cit.*, p. 275; *Works*, ed. 1772, Vol. I, p. 82). Unfortunately, when the bulb was being filled with water to determine its volume, it broke, and Boyle had no other at hand.

Another attempt was made to weigh the air. Boyle heated an aeolipile as hot as possible, closed its orifice with wax, allowed it to cool, and then weighed it. The wax was then perforated with a needle, the air rushed in, and the apparatus was reweighed. The difference in weight was 11 grains; and Boyle admitted that some air must have remained in the heated apparatus. The aeolipile was found to hold 21½ ounces of water, so that "the proportion in gravity of Air to Water of the same bulk will be as one to 938" (*op. cit.*, p. 290; *Works*, ed. 1772, Vol. I, p. 86). Ricciolus had estimated it as 1 to 10,000; and Galilei had computed it as 1 to 400.

Now Boyle in his Experiment 17 had noticed that the mercury column in the tube fell with each stroke of the pump until it was nearly at the same level as the external mercury, and he had hoped "from the descent of the Quick-silver in the Tube upon the first suck, to derive this advantage; that I should thence be enabled to give a nearer guess at the proportion of force betwixt the pressure of the Air (according to its various states, as to Density and Rarefaction) and the gravity of Quick-silver, than hitherto hath been done" (*op. cit.*, p. 115; *Works*, ed. 1772, Vol. I, p. 35). The capacities of the receiver and of the cylinder could be determined, but there were "difficulties that require more skill in Mathematicks than I pretend to" (*op. cit.*, p. 117; *Works*, ed. 1772, Vol. I, p. 36), and Boyle merely hinted at the possibilities of a valuable discovery. It is to be noted, however, that he had suggested this at least as early as December 1659, when his book went to press.

In 1661 Franciscus Linus attacked Boyle's views, as expressed in the *New Experiments* of 1660, in his *De corporum inseparabilitate*. While admitting that the air possessed both "spring" and weight, Linus argued that the "spring" of the air was not great enough to sustain the column of mercury in the Torricellian experiment. He proposed instead to explain the phenomena in this and other vacuum experiments by means of a "Funiculus," an extremely thin substance which, when forcibly distended, violently attracted all neighbouring bodies. This, according to Linus, is the real support of the mercury in a Torricellian tube, and exercises the pull that is felt when the top of such a tube is closed by the finger.

Boyle criticized this theory as "partly precarious, partly unintelligible, and partly insufficient, and besides needless," in the second edition of his book. This new volume appeared under the title *New Experiments Physico-Mechanicall, Touching the Spring of the Air, Whereunto is added A Defence of the Author's Explication of the Experi-*

ments Against the Objections of Franciscus Linus and Thomas Hobbes (Oxford, 1662; *Works*, ed. 1772, Vol. I). And it was in this *Defence* that Boyle first published the hypothesis since known as "Boyle's Law."

Before proceeding to describe Boyle's derivation of this law, some comment is necessary on the history of the idea. Boyle himself stated that Richard Towneley, as a result of reading the first edition of the *New Experiments*, and, in all probability, Experiment 17 in particular, had suggested the hypothesis "that supposes the pressures and expansions to be in reciprocal proportion." Boyle also stated that, when he first reported this suggestion to a certain person (Hooke, no doubt, to judge from the context, and other evidence), the latter informed him that he had already in 1660 carried out experiments on rarefaction that agreed with such an hypothesis. Boyle adds that Lord Brouncker had made similar experiments about the same time. In 1665 Hooke published an account of his experiments in his *Micrographia* (pp. 222–7), where he concluded that "the Elater of the Air is reciprocal to its extension, or at least very neer," on the evidence of experiments which he had made, or repeated, after Boyle had told him about Towneley's Hypothesis. The Register Book of the Royal Society indicates that Boyle gave an account of his experimental verification of this hypothesis at a meeting of the Society held on September 11, 1661.

Boyle's experiment was described by its author as follows: "We took then a long glass tube which by a dexterous hand and the help of a lamp was in such a manner crooked at the bottom that the part turned up was almost parallel to the rest of the tube, and the orifice of this shorter leg of the siphon (if I may so call the whole instrument) being hermetically sealed the length of it was divided into inches (each of which was subdivided into eight parts) by a straight list of paper, which containing those divisions was carefully pasted all along it: then putting in as much quicksilver as served to fill the arch or bended part of the siphon, that the mercury standing in a level might reach in the one leg to the bottom of the divided paper and just to the same height or horizontal line in the other, we took care, by frequently inclining the tube, so that the air might freely pass from one leg into the other by the sides of the mercury (we took, I say, care), that the air at last included in the shorter cylinder should be of the same laxity with the rest of the air about it. This done, we began to pour quicksilver into the longer leg of the siphon which by its weight pressing up that in the shorter leg did by degrees streighten the included air; and continuing this pouring in of quicksilver till the air in the shorter leg was by condensation reduced to take up but half the space it possessed (I say possessed, not filled) before; we cast our eyes upon the longer leg of the glass, on which was likewise pasted a list of paper carefully divided into inches and parts, and we observed not without delight and satisfaction that the quicksilver in that longer part of the tube was 29 inches higher than the other. Now that this observation does both very well agree with and confirm our hypothesis, will be easily discerned by him, that takes notice that we teach, and Monsieur Pascall and our English friends' experiments prove, that the greater the weight is that leans upon the air, the more forcible is its endeavour of dilatation, and consequently its power of resistance (as other springs are stronger when bent by greater weights). For this being considered, it will appear to agree rarely-well with the hypothesis, that as according to it the Air in that degree of density and correspondent measure of resistance to which the weight of the incumbent atmosphere had brought it, was able to counterbalance and resist the pressure of a mercurial cylinder of about 29 inches, as we are taught by the Torricellian experiment: so here the same air being brought to a degree of density about twice as great as that it had before, obtains a spring twice as strong as formerly. As may appear by its being able to sustain or resist a cylinder of 29 inches in the longer tube, together with the weight of the atmospherical cylinder, that lean'd upon those 29 inches of mercury, and, as we just now inferred from the Torricellian experiment, was equivalent to them" (*Defence*, pp. 58, 59; *Works*, ed. 1772, Vol. I, pp. 156, 157).

The tube in which these experiments were performed was broken, so Boyle obtained another and longer tube of the same shape. It was in fact so long that he could not use it in a room, and had to suspend it from a staircase by means of strings. The mercury was poured in by one person on the stairs at the direction of another at the foot, who observed the shrinkage of the air. Numerous readings were taken, and Boyle tabulated his results as shown in the accompanying "Table of the Condensation of the Air" (taken from *Defence*, p. 60; *Works*, ed. 1772, I, p. 158). It will be observed (see Columns D and E) that Boyle compared the results experimentally obtained with those calculated by "the hypothesis that supposes the pressures and expansions to be in reciprocal proportion"; and that his range of pressures was from 1 to 4 atmospheres. Within the limits of the experimental error to be expected, the observed and the calculated values agreed very well.

In a further series of experiments Boyle tested the hypothesis for pressures below atmospheric. He took a narrow glass tube open at both ends, pasted on it "a list of paper divided into inches and half-quarters," pushed the tube down into the mercury until all but an inch of the tube was submerged, and then closed the end by melting sealing-wax over it. The tube was then allowed to cool, and was gradually raised from the mercury, the length of the column of air and the height of the column of mercury being noted at various positions, until the air was dilated to a length of 32 inches. A Torricellian glass showed that the barometric height at the

A table of the condensation of the air

A	*A*	*B*	*C*	*D*	*E*
48	12	00	Added to $29\frac{1}{8}$ makes	$29\frac{2}{16}$	$29\frac{2}{16}$
46	$11\frac{1}{2}$	$01\frac{7}{16}$		$30\frac{9}{16}$	$33\frac{6}{16}$
44	11	$02\frac{13}{16}$		$31\frac{15}{16}$	$31\frac{12}{16}$
42	$10\frac{1}{2}$	$04\frac{6}{16}$		$33\frac{8}{16}$	$33\frac{1}{7}$
40	10	$06\frac{3}{16}$		$35\frac{5}{16}$	35
38	$9\frac{1}{2}$	$07\frac{14}{16}$		37	$36\frac{15}{19}$
36	9	$10\frac{2}{16}$		$39\frac{5}{16}$	$38\frac{7}{8}$
34	$8\frac{1}{2}$	$12\frac{8}{16}$		$41\frac{10}{16}$	$41\frac{2}{17}$
32	8	$15\frac{1}{16}$		$44\frac{3}{16}$	$43\frac{11}{16}$
30	$7\frac{1}{2}$	$17\frac{15}{16}$		$47\frac{1}{16}$	$46\frac{3}{5}$
28	7	$21\frac{3}{16}$		$50\frac{5}{16}$	50
26	$6\frac{1}{2}$	$25\frac{3}{16}$		$54\frac{5}{16}$	$53\frac{10}{13}$
24	6	$29\frac{11}{16}$		$58\frac{13}{16}$	$58\frac{2}{8}$
23	$5\frac{3}{4}$	$32\frac{3}{16}$		$61\frac{5}{16}$	$60\frac{18}{23}$
22	$5\frac{1}{2}$	$34\frac{15}{16}$		$64\frac{1}{16}$	$63\frac{6}{11}$
21	$5\frac{1}{4}$	$37\frac{15}{16}$		$67\frac{1}{16}$	$66\frac{4}{7}$
20	5	$41\frac{9}{16}$		$70\frac{11}{16}$	70
19	$4\frac{3}{4}$	45		$74\frac{2}{16}$	$73\frac{11}{19}$
18	$4\frac{1}{2}$	$48\frac{12}{16}$		$77\frac{14}{16}$	$77\frac{2}{3}$
17	$4\frac{1}{4}$	$53\frac{11}{16}$		$82\frac{12}{16}$	$82\frac{4}{17}$
16	4	$58\frac{2}{16}$		$87\frac{14}{16}$	$87\frac{3}{8}$
15	$3\frac{3}{4}$	$63\frac{15}{16}$		$93\frac{1}{16}$	$93\frac{1}{5}$
14	$3\frac{1}{2}$	$71\frac{5}{16}$		$100\frac{7}{16}$	$99\frac{6}{7}$
13	$3\frac{1}{4}$	$78\frac{11}{16}$		$107\frac{13}{16}$	$107\frac{7}{13}$
12	3	$88\frac{7}{16}$		$117\frac{9}{16}$	$116\frac{4}{8}$

AA. The number of equal spaces in the shorter leg, that contained the same parcel of air diversly extended.

B. The height of the mercurial cylinder in the longer leg, that compressed the air into those dimensions.

C. The height of the mercurial cylinder, that counterbalanced the pressure of the atmosphere.

D. The aggregate of the two last columns *B* and *C*, exhibiting the pressure sustained by the included air.

E. What that pressure should be according to the hypothesis, that supposes the pressures and expansions to be in reciprocal proportion.

time of the experiment was $29\frac{1}{8}$ inches. The experimental values were compared with those calculated according to the "hypothesis" and were found to be in good agreement with them. Boyle tabulated his results as shown in the table on page 242 (taken from *Defence*, p. 64; *Works*, ed. 1772, I, p. 160).

Returning to the work described by Hooke in his *Micrographia* it appears that Hooke had, in 1660, made similar experiments on the rarefaction of air, measuring the pressures of a column of air in expanding from an atmospheric pressure of 30 inches of mercury down to 3 inches. He did not apply these results to test any hypothesis, not being aware of Towneley's idea at that time. On August 2, 1661, after being informed of Towneley's hypothesis, he repeated these experiments and carried out others for pressures above atmospheric, working up to a pressure of two atmospheres in an apparatus similar to Boyle's. His results agreed with the hypothesis within the limits of experimental error, and, as indicated above, he concluded that the "Elater of the Air is reciprocal to its extension, or at least very near." However, Boyle may fairly be credited with the discovery (although he did not announce his results to the Royal Society until September 11th of that year), since he informed

A table of the rarefaction of the air

A. The number of equal spaces at the top of the tube, that contained the same parcel of air.

B. The height of the mercurial cylinder, that together with the spring of the included air counterbalanced the pressure of the atmosphere.

C. The pressure of the atmosphere.

D. The complement of *B* to *C*, exhibiting the pressure sustained by the included air.

E. What that pressure should be, according to the hypothesis.

A	*B*	*C*	*D*	*E*
1	$00\frac{0}{8}$	Subtracted from $29\frac{3}{4}$ leaves	$29\frac{3}{4}$	$29\frac{3}{4}$
$1\frac{1}{2}$	$10\frac{5}{8}$		$19\frac{1}{8}$	$19\frac{5}{6}$
2	$15\frac{3}{8}$		$14\frac{3}{8}$	$14\frac{7}{8}$
3	$20\frac{2}{8}$		$9\frac{4}{8}$	$9\frac{11}{12}$
4	$22\frac{5}{8}$		$7\frac{1}{8}$	$7\frac{7}{16}$
5	$24\frac{1}{8}$		$5\frac{5}{8}$	$5\frac{19}{20}$
6	$24\frac{7}{8}$		$4\frac{7}{8}$	$4\frac{23}{24}$
7	$25\frac{4}{8}$		$4\frac{2}{8}$	$4\frac{1}{4}$
8	$26\frac{0}{8}$		$3\frac{6}{8}$	$3\frac{23}{32}$
9	$26\frac{3}{8}$		$3\frac{3}{8}$	$3\frac{11}{36}$
10	$26\frac{6}{8}$		$3\frac{0}{8}$	$2\frac{39}{40}$
12	$27\frac{1}{8}$		$2\frac{5}{8}$	$2\frac{23}{48}$
14	$27\frac{4}{8}$		$2\frac{2}{8}$	$2\frac{1}{8}$
16	$27\frac{6}{8}$		$2\frac{0}{8}$	$1\frac{55}{64}$
18	$27\frac{7}{8}$		$1\frac{7}{8}$	$1\frac{47}{72}$
20	$28\frac{0}{8}$		$1\frac{6}{8}$	$1\frac{39}{80}$
24	$28\frac{2}{8}$		$1\frac{4}{8}$	$1\frac{23}{96}$
28	$28\frac{3}{8}$		$1\frac{3}{8}$	$1\frac{1}{16}$
32	$28\frac{4}{8}$		$1\frac{2}{8}$	$0\frac{119}{128}$

Hooke of Towneley's hypothesis—and Hooke probably worked with Boyle's apparatus. In the absence of any real knowledge at the time of a variety of gases, neither Boyle nor Hooke realized the importance of the discovery. "Boyle's Law" is, however, very properly, the name by which this generalization is known.

The rival claims of Mariotte to have the law called by his name, whether solely or in conjunction with that of Boyle, rest on slender grounds. The relevant considerations are the following. In his *Essay de la nature de l'air* (1679) he contended that air must have weight, and adduced as evidence the fact that the mercury in a barometer rose 3 inches when the barometer was immersed in $3\frac{1}{2}$ feet of water. He argued that as this increase can only be due to the weight of the water on the exposed surface of the mercury, the column of mercury in the tube must have been maintained at its previous height by the weight of the atmosphere. He asserted that air must have a *vertu de ressort* (obviously Boyle's "spring" and Hooke's "elater") so that it could be compressed or expanded; and he thought that the air near the surface of the Earth is compressed by the air above it, whereas the air in the highest region of the atmosphere must have unrestricted freedom of expansion (*la liberté entière de se dilater*). He formulated the law that air is compressed in proportion to the weight that is acting upon it (*l'air se condense à proportion des poids dont il est chargé*). He experimented with pressures below atmospheric by means of apparatus which was just like Boyle's. The only figures he gave were those for such simple cases as when the volume of air was doubled or was increased by a third; he said that he had also tried other cases, but gave no figures. His experiments with pressures greater than atmospheric were likewise made with apparatus just like Boyle's; and again he mentioned but few numerical results. There is every reason to suppose that Mariotte was familiar with the work of Boyle, who had published his detailed results seventeen years earlier. And among his contemporaries Mariotte was not beyond suspicion of exploiting the work of others. Newton, for example, paid him the ambiguous compliment of remarking on his having considered it worth while to write a whole volume (namely, *Traité de la percussion*, etc.) on the substance of a few pendulum experiments which Wren had made before the Royal Society, and which were published in the *Philosophical Transactions* (*Principia*, 1687, p. 20—Axiomata, Corol. VI, Schol.). The seventeenth century, as will be pointed out again in connection with Mayow's relation to Boyle and Lower, had charitable views on what would now be condemned as plagiarism. And it would not be altogether fair to charge Mariotte with it. But that is not a sufficient reason for crediting him with Boyle's discovery, even if Mariotte may be credited with a realization of the importance of Boyle's Law and with the promotion of its recognition on the Continent. If any other name deserves to be associated with that of Boyle in connection with this law it is the names of Towneley and Hooke, not that of Mariotte (see W. S. James in *Science Progress*, 1928, 23, pp. 269 ff.).

(See E. Mach, *The Science of Mechanics*, tr. by T. J. McCormack, 5th edition, 1942.)

CHAPTER XI

PHYSICS

I. LIGHT

THE modern history of the science of Optics may be taken as beginning with the fundamental researches of Kepler, or with the accurate formulation of the law of refraction, early in the seventeenth century. In order, however, to realize the state of the science at the beginning of the modern period, and to understand how some of its fundamental concepts originated, it is desirable to begin with a rapid survey of previous work in this field.

ANTECEDENTS

The Greeks were the first to subject the phenomena of light to mathematical treatment, and Euclid in his *Optics* summed up what was known or surmised about the subject up to his time. It was known that the rays of light, which intervene between the eye and the object observed, are straight lines, and that, in reflection from a plane mirror, the angles of incidence and reflection are equal. The phenomenon of refraction was also recognized, and was studied by Ptolemy in one of the few recorded experimental investigations of the ancient world. He stated that the angle of incidence is proportional to the angle of refraction for a given pair of media, though his refraction tables do not agree with this simple relation.

According to most ancient theories vision was due to something emanating from the eye and falling upon the object seen, or mingling with another emanation proceeding from it. On the other hand, the Epicureans seem to have taught that vision is caused by a succession of thin films which are emitted from the surface of an object and which enter our eyes, giving us a continuous impression of that object. In contrast with these conceptions of light as a substance, Aristotle held that it is a quality of the medium intervening between the eye and the object. The conflict between these two types of explanation has continued down to the present day.

The greatest of the mediaeval opticians was the Arab Ibn al-Haitham or Alhazen (eleventh century), whose book remained a standard authority down to the seventeenth century. He taught that light spreads out spherically from each point on a visible object, and he determined refractive indices experimentally, recognizing that Ptolemy's crude law of refraction holds only for small angles. Alhazen investigated many particular cases of reflection and refraction, and drew attention to the light-ray's property of retracing its path when reversed. His account of the structure and functions of the eye was first superseded in the seventeenth century. Mediaeval European opticians, from Vitellio and Roger Bacon in the thirteenth century to Maurolycus and Porta in the sixteenth, mainly concerned themselves with the discussion of minor problems in connection with which we shall have later occasion to mention some of them. Knowledge of optical phenomena throughout this period was very limited, and there was no satisfactory theory of colours, which were generally attributed to the mixture of light and darkness in various proportions.

KEPLER

At the threshold of the modern period stands Johann Kepler, the astronomer, whose principal contributions to optics dealt with refraction, the properties of lenses, and the theory of vision. Kepler set forth the results of his optical researches in two books, *Ad Vitellionem Paralipomena* (Frankfurt, 1604), which treats of the whole science of light, and *Dioptrice* (Augsburg, 1611), which is concerned primarily with refraction. The fundamental researches contained in these books mark a notable advance on the achievements of Kepler's predecessors in this field.

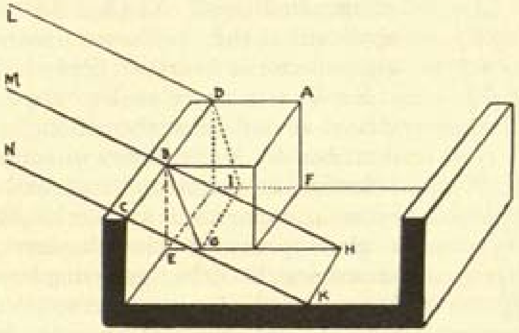
In the earlier of these works Kepler clearly formulated for the first time, though only by an appeal to intuition, the fundamental law of photometry, namely that the intensity of the light emanating from a point-source varies, with increase of distance of the illuminated object from the source, in inverse proportion to the surface of a sphere having that distance as radius; in other words, it varies inversely as the square of the distance from the source (*Paralipomena*, I, 9). In the same work Kepler maintained that light is capable of being propagated into illimitable space (*ibid.*, I, 3), and that it requires no time for its propagation, since, being immaterial, it offers no resistance to the moving force, which, in accordance with Aristotelian mechanics, therefore gives it an infinite velocity (*ibid.*, I, 5). He was unable to account satisfactorily for colours, which he supposed to arise from different degrees of transparency and density of the coloured substance (*ibid.*, I, 15); and he subscribed to the erroneous opinion that refraction takes place at the boundary of two media because of the greater resistance and proportionately greater refracting power of the denser medium (*ibid.*, I, 20). Very soon after the publication of this book, however, Kepler's attention was drawn by Harriot to the fact that oil, though less dense than water, refracts light much more strongly. Kepler also concerned himself with the problem, previously discussed by

Maurolycus, of explaining why, when sunlight is admitted through a small opening into a dark room, the image formed on a screen is always round, whatever the shape of the opening. He arrived at the correct explanation through a geometrical construction (*ibid.*, II). Taking a book, he placed between it and the wall a screen in which was an angular aperture. He attached a thread to one corner of the book, passed it through the aperture, and, keeping it in a straight line, drew it along the edge of the aperture, tracing on the wall with a piece of chalk attached to the other end of the thread a figure similar to that of the aperture. He repeated the process with the thread attached successively to other points of the book, and obtained a number of partially overlapping outlines of the aperture which all lay within a single outline, having the shape of the book. Taking the book to represent a luminous body, and the thread the limiting rays of light, Kepler was thus able to clear up this ancient problem. Explaining how we are able to judge the distance of an object, Kepler maintained that we unconsciously solve the triangle whose base is the distance between our eyes and whose sides are the lines of sight drawn from each eye to the object (*ibid.*, III, 8). He devotes separate sections of his *Paralipomena* to refraction—especially astronomical refraction, for which he drew up a table—and to the theory of vision. These branches of optics, however, were taken up again by Kepler in his *Dioptrice*, and we shall confine our attention to this later presentation of them.

The invention of the telescope in 1609 stimulated Kepler to occupy himself afresh with optics, and to furnish a geometrical explanation of this instrument. The *Dioptrice* was the result of his meditations, supported by only moderate experimental resources. It was especially through this book that Kepler became the founder of modern optics, to which he stands in the same relation as Galilei to mechanics and Gilbert to the science of magnetism and electricity.

In judging Kepler's work on refraction, it must be remembered that, in his time, the ratio of the angle of incidence to the angle of refraction was commonly assumed to be constant. Kepler took as his fundamental experimental law the rule that rays of light, upon passing from a rarer to a denser medium, are bent towards the normal drawn to the surface of separation of the two media at the point of incidence (*Dioptrice*, II). His apparatus for measuring refraction is shown in Illustr. 137.

Rays of sunlight L, M, N, cast the shadow of the straight edge CBD of an upright screen upon the horizontal base of the apparatus. Some of these rays are unrefracted and cast the shadow HK, while others pass through a cube of the transparent substance under investigation and cast the shadow IG. From the height BE of the screen, and the lengths EH, EG of the shadows formed in the two cases, the ratio of the angles of incidence and refraction at the surface of the cube can easily be deduced *(ibid.*, IV). In the course of his researches Kepler discovered the total internal reflection of a ray of light travelling in glass and incident upon the surface of separation between glass and air at an angle exceeding 42 degrees *(ibid.*, XIII). Despite numerous measurements of angles of incidence and the corresponding angles of refraction, Kepler could find no regular relation between the two quantities, though he showed that angles of incidence less than 30 degrees bore an approximately constant ratio to the corresponding angles of refraction *(ibid.*, VII),

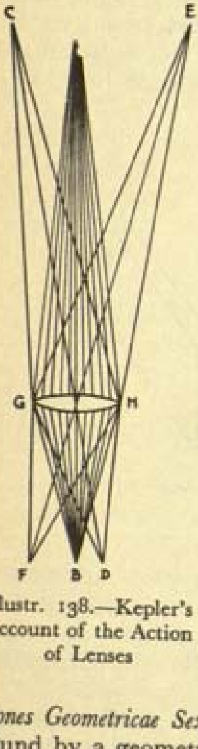


Illustr. 137.—The Determination of the Angle of Refraction of Light

for a given pair of media, this ratio being about 3 to 2, for glass or rock-crystal *(ibid.*, VIII). But this ratio, he showed, did not hold good for larger angles of incidence. Kepler's attempts to find a general trigonometrical expression for the ratio, though on the right lines, were unsuccessful.

Although thus ignorant both of the general law of refraction and of the relation connecting conjugate points of a lens (first obtained by Halley), Kepler was nevertheless able to give an approximate theory of the action of lenses and lens-systems. Taking the ratio of refraction as 3 : 2, and considering only rays incident at small angles, he depicted the course of such rays through various types of lenses or lens-combinations, and obtained results by reasoning from the geometrical properties of the diagrams. His usual procedure was to consider two cones of rays, having the lens as their common base, whose vertices respectively coincided with a point of the object and the corresponding point of the image. Three such pencils of rays are shown in Illustr. 138.

The points F, D of the image respectively correspond to the points E, C of the object, so that the law that a convex lens gives inverted images is immediately evident (*ibid.*, XLV). This method of construction by means of pencils consisting of innumerable rays was an innovation due to Kepler, his predecessors having always traced the course of single rays. The new method enabled him to ascertain the positions and sizes of images much more correctly. Thus he discovered, for instance, that an object placed on the axis of a biconvex lens and distant from it by twice the focal length gives rise to an image equal in size and at an equal distance on the opposite side of the lens (*ibid.*, XLIX). Among practical applications, the "bull's-eye" lantern with lens and reflector is described (*ibid.*, LIII).



Illustr. 138.—Kepler's Account of the Action of Lenses

Kepler was aware of the complications now classed as "spherical aberration," and already described by Roger Bacon in connection with reflection at concave mirrors, and by Maurolycus in connection with refraction through a glass sphere. Kepler, however, suggested remedying this defect by giving lenses a hyperbolic instead of a circular section, believing, with the anatomists of his time, that the lens of the eye was hyperboloidal on its reverse side, thus giving us sharp images unaffected by spherical aberration (*Paralip.* V, 1; *Diopt.* LX). Progress along the lines of Kepler's work in geometrical optics was made by Cavalieri who, in 1647, proved the correct relation between the focal length of any thin lens and the radii of curvature of its surfaces (*Exercitationes Geometricae Sex*, 1647, p. 462), and by Barrow who, in 1674, found by a geometrical method the image formed by a thick lens upon which an axial pencil falls (*Lectiones Opticae et Geometricae*, 1674, pp. 96–102). Such cumbrous geometrical investigations involving the separate consideration of numerous particular cases, were eventually superseded by the analytical methods of Descartes, which Halley, in 1693, successfully applied to the problem of finding the general formula of the thick lens (*Phil. Trans.*, No. 205).

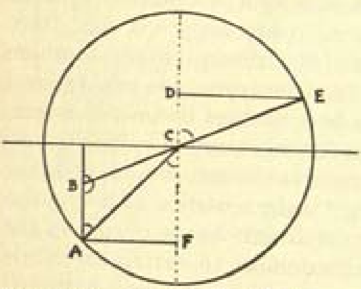
The theory of vision accepted in Kepler's time was largely based upon the ideas of Alhazen, and was unsound. Kepler devoted several years to the study of vision, and was able to give a more satisfactory account of the functions of the eye than his predecessors (*Paralip.* V). He explained the retina as the part of the eye which receives the image formed by the crystalline lens (*Diopt.* LX) and he was of opinion that if the opaque outer tunics of the eye were removed, an inverted, diminished image of any object in the field of view would be seen (*Paralip.* V, 2). He seems to have based this surmise (later confirmed experimentally by Scheiner and others) on Porta's experiments with the *camera obscura*, the similarity of which to the eye had, however, previously been pointed out by Leonardo da Vinci.

Porta and Maurolycus had supposed that, from each point on a luminous body in the field of view, a ray of light enters the eye through the pupil. Porta regarded the lens as the perceptive organ upon which the image is formed, the posterior wall of the eye serving as a concave mirror from which light is reflected towards the centre. Kepler, however, more correctly supposed that from the several points of the object cones of rays diverge whose common base is the pupil. These are refracted by the crystalline lens to form converging cones (*cp.* Illustr. 104), whose vertices lie on the retina, which plays the part of the screen in a *camera obscura*. Kepler's theory of the activity of the retina shows remarkable accordance with the most recent ideas. "Sight," he writes, "is the sensation of a stimulation of the retina" (*Diopt.* LX). A substantial change occurs in the retina when light falls upon it, for it contains an extremely subtle material, *spiritus visivus*, which is decomposed by the light collected by the lens, just as a combustible substance is altered by the application of a burning-glass. The image thus formed is of some duration, as Kepler proves by adducing the after-images which are seen upon closing the eyes or turning them away after looking at a bright object (*ibid.*). These speculations later received a measure of confirmation through the discovery of the chemically transformable "visual purple." Kepler rightly remarked that the formation of an image upon the retina does not of itself constitute the entire act of vision, but that the image must be transmitted "by a spiritual current" to the seat of the faculty of sight in the brain (*ibid.*). He explains the fact that our two eyes give us the perception of only one image on the ground that the two retinas are similarly stimulated (*Diopt.* LXXII). He also discussed the question why we see objects erect although their images formed on the retina are inverted, but he could not find a satisfactory answer. However, he correctly explained short-sightedness and long-sightedness as respectively arising when the cones of rays, coming from the several points of the object and suffering refraction in the crystalline lens, come to a focus before reaching the retina, or reach the retina before coming to a focus (*ibid.*, LXIV). In either case points of the object transform into discs on the retina, and the image is blurred. Kepler devoted a section of his book to the use of lenses as aids to vision (*ibid.*, LXVI *et seqq.*). He also explained the *accommodation* of the normal eye to variations in the distance of the object, as due to a displacement of the lens or of the retina (*ibid.*, LXIV). Descartes inclined to the view, later shown to be correct, that the lens alters its curvature in consequence of a varying pressure exerted upon it.

Kepler's contributions to the development of the telescope are dealt with in the history of that instrument.

SNELL

An important advance in the development of optics was made by Willebrord Snell (1591–1626), Professor of Mathematics at Leiden, who, in 1621, formulated the exact law of refraction, although not in the familiar form which to-day bears his name. In the course of experiments on refraction, probably on the same lines as those of Kepler, Snell discovered that the length of the path CA (Illustr. 139) of a ray ECA passing from air to water and falling upon the vertical side of the containing vessel, bears a constant proportion to the path CB, which the ray would have traversed if it had not been deflected from its original direction (C. Huygens: *Dioptrica*, in *Opuscula Postuma*, 1703, p. 2). From this result the law of refraction can easily be deduced in its modern form. For, with the usual notation,



Illustr. 139.—The Angle of Refraction of Light

$$\frac{\sin i}{\sin r}=\frac{\sin B\hat{C}F}{\sin A\hat{C}F}=\frac{AF}{CB}\Big/\frac{AF}{CA}=\frac{CA}{CB}= \text{a constant, by Snell's result.}$$

Snell did not publish his result, but it was seen in manuscript by Huygens and possibly also by Descartes. It is therefore doubtful whether Descartes discovered the law of refraction independently, though he was the first to publish the law and to attempt a physical proof of it (in *Discours* II of his *Dioptrique* of 1637).

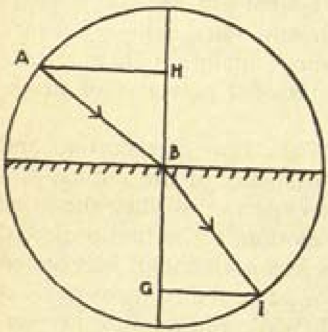
DESCARTES

In the opening pages of his *Dioptrique* (*Discours* I) Descartes compares vision to the process by which a blind man feels the objects around him with the aid of his stick. Light, he holds, is an action or pressure which is transmitted by the luminous body to our eyes through the intervening medium, just as the motion or resistance of an object is communicated through the blind man's stick to his hand. Colours he attributed to the different rates of rotation of the particles of the luminiferous medium. The analogy tended to confirm Descartes in his belief that light is propagated "in an instant." This conception of the nature of light is more fully worked out in Descartes' other books, *Le Monde* (1664, but written about 1630) and *Principia Philosophiae* (1644). The most important sources of light in his system are the fiery cores of the vortices. These cores are the Sun and stars, whose outward pressures, besides illuminating the planets, maintain their respective vortices in existence against the outward pressures of neighbouring ones.

In attempting a mechanical proof of the laws of reflection and refraction, Descartes assumed that light, being of the nature of a thrust, or tendency to motion, may be expected to obey the same mechanical laws as a body actually in motion, e.g. a ball projected from a tennis-racket. He argued that when such a ball is reflected from a hard, even surface, the resolute (or part) of its velocity parallel to the surface is practically unaffected, while the resolute perpendicular to it is reversed by the impact (*Diopt.* II). It easily follows that the angle of incidence must equal the angle of reflection. To represent refraction from a denser to a rarer medium, Descartes supposed the ball to be hit through a thin cloth so that the perpendicular resolute (or part) of the velocity was reduced in a certain proportion while the horizontal resolute was unchanged (*ibid.*). In the case of refraction from a rarer to a denser medium, the ball was supposed to receive a further impulse at the point of incidence, causing it to travel on with an increased velocity. These analogies involve the supposition that light travels more "easily" and rapidly in a denser medium than in a rarer, for which Descartes furnished a mechanical reason. Light, he argues, consists of a motion in a medium, and is therefore deadened more easily by impinging on the soft and loosely joined particles of air than on the harder and more firmly connected particles of water or glass, just as a ball rolls less easily on a carpet than on a bare table. The above considerations immediately led to the law of refraction, which Descartes enunciated in the following form:

Let ABI represent a ray refracted at B (Illustr. 140) upon passing into a different medium. Draw a circle of arbitrary radius about B in the plane of incidence of the ray, intersecting the ray in A and I. Draw perpendiculars AH, IG to the normal through B. Then the ratio AH : IG is constant, for the two media, whatever the angle of incidence of AB (*ibid.*).

The remainder of the *Dioptrique* deals with the anatomy of the eye and with vision, in accordance with Descartes' general theory of the physiology of the senses. It describes how to demonstrate experimentally with a bull's eye the formation of retinal images, and how to illustrate the accommodation of the eye by applying pressure to the lens (*Discours* V). Describing the use of lenses as aids to vision, Descartes advocated making them with elliptic or hyperbolic instead of circular sections (*Discours* VIII), so as to eliminate their defects; and he described machinery for grinding them (*Discours* X).



Illustr. 140.—Descartes' Law of Refraction

Descartes' contributions to the theory of the rainbow, in *Les Météores*, will be mentioned in connection with Newton's improvement of that theory.

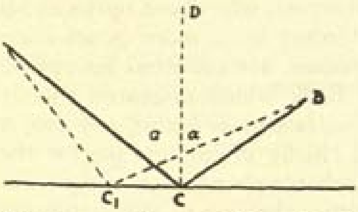
Descartes' proofs of the laws of optics, sketched above, are unconvincing, especially as they involve the comparison of an instantaneously propagated pressure with a body moving at a finite and variable velocity. His supposition that light increases its velocity upon passing into a denser medium met with much opposition from contemporary physicists.

FERMAT

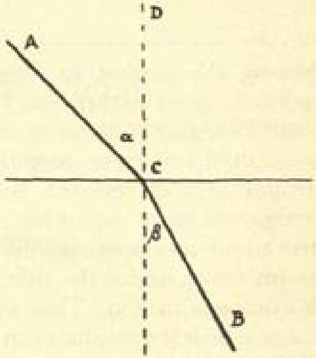
Among Descartes' critics was the mathematician Fermat, who proved Snell's Law in a very different manner by applying to the problem of refraction his general method for determining the maximum and minimum values of a variable quantity (Huygens' *Traité de la Lumière*, Chap. III end). This method was based upon the principle that the value of a quantity, when near a maximum or minimum, is not sensibly altered by small changes in the quantities upon which that value depends.

The ancients had explained the rectilinear propagation of light teleologically, supposing that light travels in straight lines so as to reach an object by the shortest possible path or in the least possible time. Hero of Alexandria had further shown in his *Catoptrica*, that the law of reflection illustrates the same principle. He pointed out that a ray passing from a given point A to a given point B (Illustr. 141) with an intermediate reflection at any point C of a given surface, has a minimum distance to go when $A\hat{C}D = B\hat{C}D$, CD being normal to the surface. It is easy to prove that any other path (e.g. AC_1B) would be longer.

Fermat assumed tentatively that the path of a ray passing between two given points A and B in two different media (Illustr. 142) and undergoing refraction at some point C, would correspond to a minimum of some sort. The *distance* AC + CB was obviously not a minimum, but by merely assuming the total resistance encountered, or the *time* required, in traversing ACB to be minimal, and the respective velocities v_1 and v_2 in the first and second media to be constant but different, Fermat was able by his method to



Illustr. 141.—Rays of Light take the Shortest Paths



Illustr. 142.—The Refraction of Light and the Principle of Least Time

deduce Snell's Law, with the further result that $\sin \alpha : \sin \beta = v_1 : v_2$. The greater velocity is here associated with the rarer medium, in conflict with the result of Descartes' investigation and with emission theories generally, but in agreement with the later result of Foucault's crucial experiment in the nineteenth century.

Snell's Law, regarded as the result of experiment, in its turn tended to justify Fermat's belief in a general principle of Least Action characterizing the processes of Nature. Further attempts on the same lines to account for the fundamental laws of optics were made later by Leibniz and Maupertuis, who assumed that light travelled so as to make one quantity or another a minimum.

From these purely *mathematical* derivations of Snell's Law we turn to the more important attempts of the seventeenth century to provide *physical* explanations of the law in terms of various hypotheses as to the nature of light. These hypotheses had to take account of an increasing number of light-phenomena which were discovered during the seventeenth century, and hence became gradually more complicated. Two principal types of hypotheses arose, according as light was conceived as a wave-motion in an all-pervading medium (on the analogy of water-waves) or as made up of particles emitted by luminous bodies (on the analogy of projectiles). The two types of theory developed simultaneously in the latter half of the seventeenth century. They interacted upon each other, and have divided the opinions of physicists down to the present day.

It is worth noting that traces of an undulatory theory of light are to be found in the writings of Leonardo da Vinci and in the letters of Galilei. None of the seventeenth-century advocates of the undulatory theory claimed it as an original idea of his own.

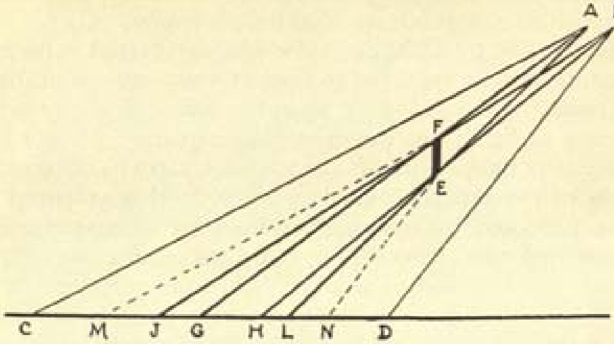
GRIMALDI

Among the earliest to suggest seriously that light is wave-like or periodic in its nature was Francesco Maria Grimaldi (1618–63), a Jesuit Professor of Mathematics at Bologna. Grimaldi was a deeply learned man and an accomplished observer, who chose optics as his principal field of research, and subjected it to more penetrating investigation than any of his predecessors. He collected his optical observations and speculations in a book which appeared shortly after his death under the title *Physico-Mathesis de lumine, coloribus, et iride* (Bologna, 1665). This work is chiefly of importance for the account which it contains of the remarkable phenomena of *diffraction* (*diffractio*) which appeared to contradict the law of the rectilinear propagation of light (I, 1).

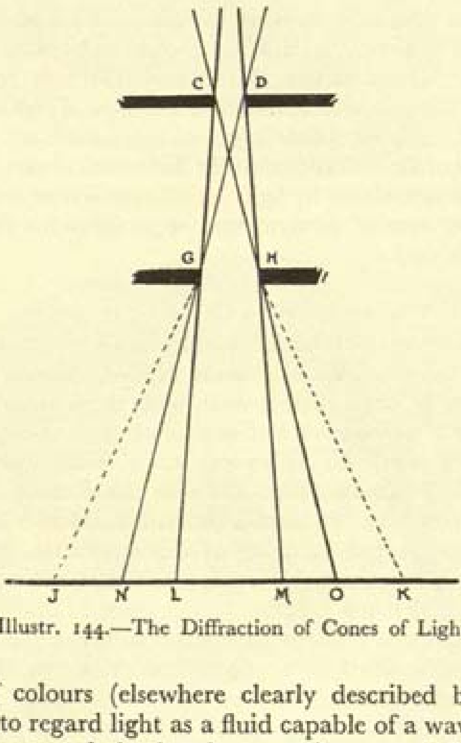
Grimaldi admitted a beam of sunlight into a dark room through a small aperture AB in a shutter (Illustr. 143), and carefully studied the shadow cast upon a screen CD by a small opaque body EF placed in the beam at some distance from the aperture. He found that the breadth MN of the shadow was greater than was to be expected from the dimensions of the apparatus, supposing light to travel past the obstacle in straight lines, and further, that the shadow was bordered externally by coloured bands parallel to its edge. With sufficiently bright illumination, similar bands appeared inside the shadow. Grimaldi accurately described and illustrated these effects, which became more complicated when the edge of the obstacle contained sharp angles.

Another of Grimaldi's experiments is shown in Illustr. 144, in which, when a cone of light was allowed to pass through two circular apertures CD and GH, the diameter JK of the disc of light cast on a screen was greater than the diameter NO obtained by geometrical construction, assuming the rectilinear propagation of light.

These phenomena, and especially that of the coloured borders of shadows, which was obviously not identical with the refractive



Illustr. 143.—The Diffraction of Light



Illustr. 144.—The Diffraction of Cones of Light

dispersion of colours (elsewhere clearly described by Grimaldi), inclined him to regard light as a fluid capable of a wave-like motion (I, 2). He compared the bands appearing round the shadows of opaque objects with the circular waves formed when a stone is thrown into water. The idea that light consists of a fine fluid in a state of undulation frequently recurs in Grimaldi's explanations, this fluid being supposed to suffer diffusion through transparent media at an immeasurably great but not an infinite velocity.

An experiment by Grimaldi in which the partial superposition of two spots of light appeared to lead to a diminution of illumination is sometimes regarded as an anticipation of the principle of interference of light; but the effect was explained by Mach as of physiological origin. Grimaldi, however, partly anticipated the invention of the reflection grating, showing that coloured bands could be produced by reflecting sunlight on to a screen from a finely scratched metal plate (I, 29). It did not occur to him, however, to attempt the composition of white light from coloured. By reference to the action of his scratched plates upon light, Grimaldi partly explained the iridescent colours frequently occurring in the animal kingdom, on birds' feathers, insects' wings, etc.

Grimaldi repeatedly states that colours are not something different from light and somehow existing in coloured bodies even without the presence of light (I, 45). They are modifications of light produced by the fine structure of the bodies which reflect it, and probably consisting in an alteration in the type of motion and in the velocity of the light. As the different musical notes are produced by different kinds of air-vibrations, so the different colours are produced when the eye is stimulated by light oscillations whose velocities differ. All these views were of fundamental importance for the subsequent development of optics.

HOOKE

In 1665, the year in which Grimaldi's book was published, there appeared the *Micrographia* of Robert Hooke. Among a multitude of other topics, he dealt in this work with the iridescent colours of thin, transparent films, such as flakes of mica, soap-bubbles, blown glass, mother-of-pearl, oil on water, etc. (Observation IX: *Of the Colours observable in Muscovy Glass and other thin Bodies*).

While studying the properties of mica, Hooke noticed that rainbow hues appeared in flakes of this substance within certain definite limits of thickness. He recognized that the colour at each part of the flake depended upon the thickness of that part, graduations in the thickness leading to corresponding graduations in the colours, which recurred in a regular order, as in the secondary rainbow. He obtained similar colour-effects upon pressing two plates of glass together with an air-film between them (*Microg.*, Observation IX, p. 50). Hooke was unable to establish any definite relation between the thickness of the film and the colour-effects

Illustr. 145

Willebrord Snell

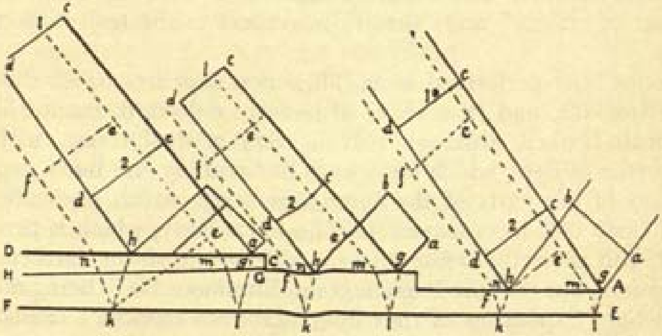
Illustr. 146

Pierre Fermat

Francesco Maria Grimaldi

produced, but he helped to prepare the way for Newton's more exact investigations in this department.

Hooke's explanation of these colour-phenomena was bound up with his theory of light. He supposed light to be a rapid, vibratory motion of small amplitude, of the particles of the luminous body, and to be propagated in straight lines in all directions with an immeasurable though not necessarily infinite velocity ("to the greatest imaginable distance in the least imaginable time; though I see no reason to affirm that it must be in an instant"), through an all-pervading, homogeneous medium, and through transparent bodies. The vibrations spread out as a series of spherical pulses, each sphere ordinarily cutting the light-rays at right angles. Light



Illustr. 148.—Colours of Thin Films

becomes coloured when the "orbicular pulse" or wave-surface becomes obliquely inclined (for instance, by refraction) to the direction of the light. In these circumstances one side of each of the pulses constituting a beam moves in advance of the other side. The primary colours are blue and red; "all the intermediate ones . . . arise from the composition and dilutings of these two." Blue is observed where the precedent portion of the pulse adjoins the edge of the beam (being weakened by contiguity to the dark medium), and red is observed where the rearward portion of the pulse adjoins the edge. Thus he held "that Blue is an impression on the Retina of an oblique and confused pulse of light, whose weakest part precedes, and whose strongest follows; and that Red is an impression, on the Retina, of an oblique and confused pulse of light whose strongest part precedes and whose weakest follows." When light falls upon a thin, transparent film each pulse is reflected partly from the front surface and partly from the back (Illustr. 148), and thus gives rise to two reflected pulses parallel to each other but separated by an interval. Hooke supposed the resulting colour-sensation to depend upon whether the reflected pulses from the upper surface closely preceded or closely followed weaker reflected pulses from the lower surface (*Micrographia*, Observation IX).

His explanation completely ignored any sort of interference between the pulses, such as is now invoked to explain these colours; but it had at least the merit (which Newton's explanation had not) of making *both* reflecting surfaces play a part in producing the phenomenon.

Hooke devised a construction for the refracted ray which was a rather crude anticipation of that of Huygens, but which involved Descartes' assumption that light travels more rapidly the denser the medium. Hooke, however, discriminated between "density in respect of gravity" and "density in respect to the trajection of the Rays."

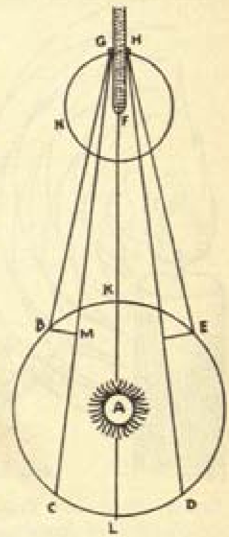
Hooke later performed some diffraction experiments on the lines of Grimaldi's, and in a series of lectures delivered about 1680–2, he dealt further, although only in very general terms, with the properties of light, which he defined as "nothing else but a peculiar Motion of the parts of the Luminous Body, which does affect a fluid Body that incompasses the Luminous Body, which is perfectly fluid, and perfectly Dense, so as not to admit of any farther Condensation; but that the Parts next the Luminous Body being moved, the whole Expansum of that fluid is moved likewise" (*Posthumous Works*, published by R. Waller, 1705, p. 113).

Hooke regarded the velocity of light as too great for experimental determination. Down to the seventeenth century it had usually been regarded as infinite, and Kepler, and perhaps also Descartes, seem to have held this view. Descartes, as we have seen, believed that light was not a moving substance, nor a motion at all, but a tendency to motion, or a thrust exerted by the luminous body; and he supposed that this thrust, being incorporeal, required no time for its propagation. He was the first, however, to attempt to decide the matter on astronomical evidence. He pointed out that if light took an appreciable time to travel from the Earth to the Moon, the latter would not appear to us to be directly opposite to the Sun when totally eclipsed, but would appear displaced from that position. But observation shows no such displacement. Huygens, however, pointed out that if the velocity of light were considerable, the displacement would be lost among the errors of observation, so that Descartes' argument, while proof that the velocity was high, was no proof that it was infinite. Galilei and, following him, the Florentine Academicians had attempted unsuccessfully to ascertain the velocity of light by means of reciprocal terrestrial light signals.

The various interpretations of the law of refraction as due to *differences* in the velocity of light in different media, however, suggested that these velocities must be finite. This principle was first established, and a fair estimate of the velocity obtained, by the labours of Olaus Römer, Picard's Danish colleague at the Paris Observatory.

RÖMER

While at Paris, about 1672–6, Römer observed a series of eclipses of the innermost of the satellites of Jupiter, which completes a revolution round the planet in about forty-two and a half hours, and is eclipsed in its shadow-cone once in each revolution (*Histoire de l'Académie Royale des Sciences*, 1666–99, Paris, 1733, anno 1676). Let A be the Sun (Illustr. 149), BCDE the Earth's orbit, F Jupiter, and GN the orbit of the satellite. Then Römer noticed that the satellite's period seemed longer, and eclipses succeeded one another more slowly, when the Earth was receding from the planet in the portion BC of its orbit, than when it was approaching it along DE. He explained this effect by supposing that light has a finite velocity. He deduced from his data that it would take about eleven minutes to traverse a radius of the Earth's orbit, and that the velocity of light must be about 48,000 leagues (approximately 193,120 km., or 120,000 miles) per second. The view that light travels with a finite velocity was rejected by many of Römer's contemporaries, especially by the Cartesians, who continued to believe in the infinite velocity of light. Römer's hypothesis was finally verified by Bradley's discovery of aberration (1726).



Illustr. 149.—Römer's Determination of the Velocity of Light

Römer's estimate of eleven minutes as the time it would take light to traverse a radius of the Earth's orbit was modified by several of his contemporaries, and is now put at about eight minutes and twenty seconds. The corresponding value for the velocity of light in terms of terrestrial units was long subject to uncertainties arising from imperfections in the methods of observation, and from doubts as to the true size of the Earth's orbit. It is now usually estimated at about 299,778 km. per second in air and 299,796 km. per second *in vacuo*.

HUYGENS

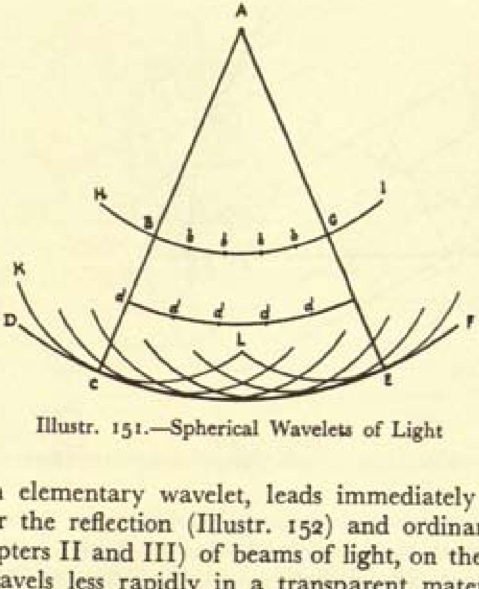
Römer's discovery established that the propagation of light is a *process* taking place at a finite rate. The phenomena both of ordinary and of double refraction suggested that the rate of this process varies according to conditions. Diffraction and iridescence phenomena suggested that the process is an undulatory motion. These ideas were all taken up by Huygens and elaborated into a theory which, although based on only a few underlying assumptions, succeeded in explaining and correlating most of the optical phenomena then known. Huygens' theory was worked out during his years of residence in France; it was communicated to the *Académie des Sciences* in 1678, and published, with additions, under the title *Traité de la Lumière*, in 1690.

Illustr. 150.—The Propagation of Light

Huygens (*Traité*, Chapter I) conceives the minute particles composing a luminous body as communicating impulses to the neighbouring particles of an all-pervading medium. These impulses (unlike those of sound) come from each individual particle of the body, and occur at irregular intervals. The medium, again, through which light travels is not the air (since light can pass through a vacuum), but it is an aether made up of small, hard, elastic particles each of which transmits any impulses which it receives to all the particles in contact with it, but does not itself suffer any permanent displacement. In this way each excited particle becomes the centre of a spherical wavelet. From his researches on the impact of elastic balls, Huygens had learned that such an assemblage of particles, though not themselves in motion, could simultaneously propagate impulses travelling in all directions, so that beams of light could cross each other without any mutual interference. From these considerations it followed that each of the particles A, B, C of a luminous body, e.g. a candle-flame (Illustr. 150), sends out its own set of concentric spherical wavelets.

If now a wave from a point-source A (Illustr. 151) reaches the position BG at any given instant, each of the particles *bbbb* in the wave-front immediately sends out a spherical wavelet. These wavelets are too weak to give any sensible effect except in the region where they reinforce one another, that is, in the surface CE which touches them all when the time necessary for traversing BC has elapsed. CE is then the new wave-front. If BG is an aperture in a screen, then, at points outside the cone ACE, the wavelets from BG will not all arrive together so as to produce an appreciable joint effect, but will straggle in one after the other, having had different distances to come, and will thus produce no sensible effect. Shadows, and the rectilinear propagation of light, are thus accounted for; but Huygens ignored the wave which, on this view, should be propagated back towards the source, and also the serious weakening which a ray would suffer through the lateral dissipation of the impulses.

Huygens' Principle that each point on a wave-front acts as the

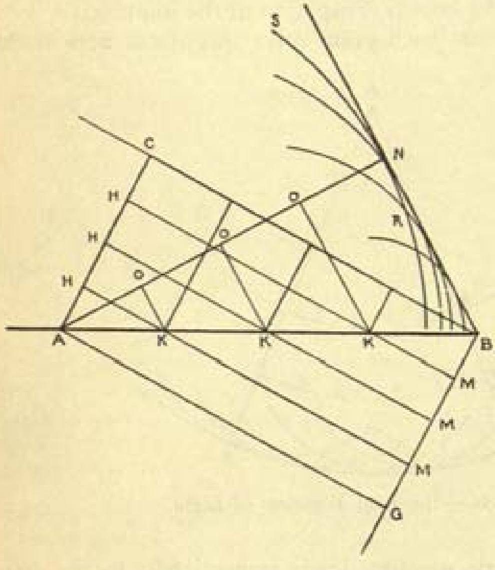


Illustr. 151.—Spherical Wavelets of Light

centre of an elementary wavelet, leads immediately to his constructions for the reflection (Illustr. 152) and ordinary refraction (*Traité*, Chapters II and III) of beams of light, on the assumption that light travels less rapidly in a transparent material medium than in a vacuum. Huygens explains the passage of light through transparent solids by supposing the aether to fill the pores between the solid particles. Light-waves are propagated through this aether, but rather less rapidly than in free space, through having to make *détours* round the particles. In double refraction, however, the extraordinary wave is propagated through both the aethereal and the material particles. Opaque substances are those which contain soft particles which damp the aetheric vibrations.

In 1670, while Huygens was in Paris, Erasmus Bartholinus, a Danish scientist, announced his discovery of *double refraction* in Iceland spar or calcite (*Experimenta crystalli Islandici*, Hafniae, 1670). Small objects observed through crystals of this substance appear double, two refracted rays being formed, one of which, the ordinary ray, obeys Snell's Law, while the other, the extraordinary ray, does not.

Huygens devotes the longest and most vital section of his *Traité* (Chapter V) to a discussion of this remarkable phenomenon, which appeared to upset his explanation of ordinary refraction. His treatment of the problem forms an unsurpassed model of the co-operation of experimental investigation and penetrating analysis. The result was that he found it possible to account for most of the peculiarities

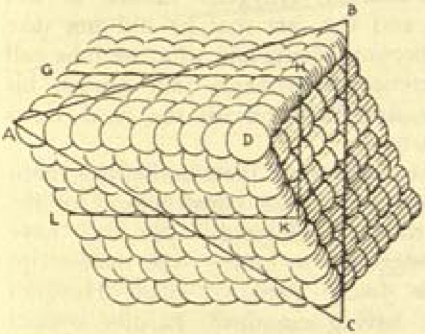


Illustr. 152.—Huygens' Construction for the Reflection of Light

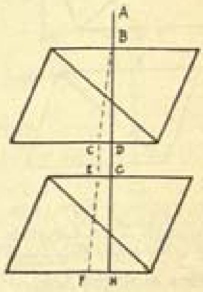
of double refraction and ordinary refraction on the same general hypothesis, which thus received further confirmation.

Huygens repeated Bartholinus' observations of calcite with increased accuracy, and himself discovered a less pronounced double refraction in rock-crystal. By careful observation he found that, while it was possible to attribute the ordinary ray in calcite to the propagation of a spherical wave through the crystal, yet in order to account for the behaviour of the extraordinary ray, it was necessary to imagine that the light spreads out so that the wave-front is an ellipsoid of revolution. The minor axis of this ellipsoid (which is the axis of revolution) was found to be parallel to the optic axis of the crystal, the velocity along the axis being the same for both rays. That is to say, supposing light to travel out in all directions from any point inside a calcite crystal, the wave-front has the form of a double surface made up of a sphere and an ellipsoid. These touch each other at the extremities of the minor axis of the latter, the radius of the sphere being to the major axis of the ellipsoid as 8 : 9. Huygens shows how to construct the direction of the extraordinary ray graphically by a method analogous to that already applied to ordinary refraction, but more complicated.

Huygens tried to connect the spheroidal shape of the extraordinary wave with the fine structure of the crystal. He supposed that the geometrically regular shapes of crystals must depend upon the shape and arrangement of the ultimate particles of which they are composed. He thus tried to explain the cleavage of Iceland spar, and the propagation of ellipsoidal waves therein, by supposing the crystal to be regularly built up of minute ellipsoidal particles



Illustr. 153.—The Cleavage of Iceland Spar

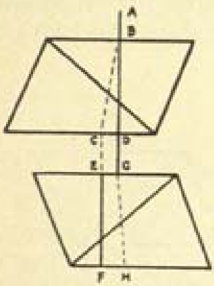


Illustr. 154.—A Ray of Light passing through Two Crystals having their Corresponding Sides Parallel

having definite orientations in regard to the optic axis of the crystal (Illustr. 153).

Huygens' theory of double refraction, however, was unable to account for certain further phenomena which he discovered upon passing a ray of light through two crystals in succession. He found that when the two crystals were placed with their corresponding sides parallel (Illustr. 154) and a ray was passed through the first, the resulting two rays did not undergo any further duplication in the second, but the ordinary ray of the first underwent an ordinary refraction in the second, and the extraordinary ray of the first underwent an extraordinary refraction in the second. At first Huygens thought that each ray in passing through the first crystal had lost the power of propagating the other kind of undulation in the second. But he found that when the two crystals were placed with their principal sections at right angles (Illustr. 155) the ordinary ray of the first became the extraordinary ray of the second, and the extraordinary ray of the first became the ordinary ray of the second. Moreover, for other relative orientations of the two crystals, each ray emerging from the first was found to be duplicated by the second, so that four rays in all resulted. Their relative intensities depended in a regular manner upon the relative orientations of the two principal sections, but their joint intensity did not exceed that of the incident ray.

Huygens was obliged to leave this enigma unsolved. "Pour dire comment cela se fait," he writes, "je n'ai rien trouvé jusqu'ici qui me satisfasse." The matter remained a mystery until longitudinal light-waves were abandoned in favour of transverse, in the nineteenth century. Huygens' failure at this point, and the fact that he did not take into account colours and what we now call interference phenomena, told against his theory, and in favour of the rival views of Newton.



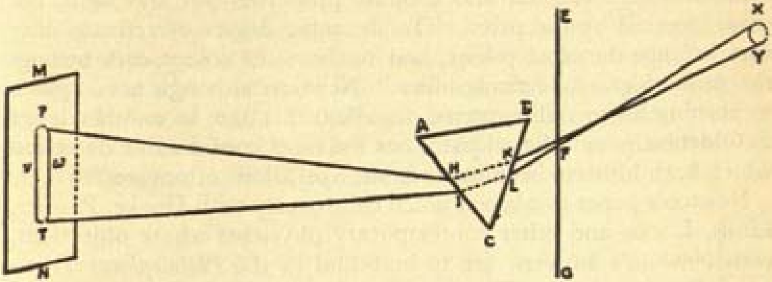
Illustr. 155.—A Ray passing through Two Crystals placed with their Principal Sections at Right Angles

Some of Huygens' ideas on light, perhaps even the germ of his constructions for the reflected and refracted rays, may have been suggested to him by the manuscript of the Jesuit Pardies, which Huygens admits having examined. Pardies' optical work was not published, but some of his ideas seem to have been incorporated by Ango, another Jesuit, in his own *Optique*, published in 1682.

NEWTON

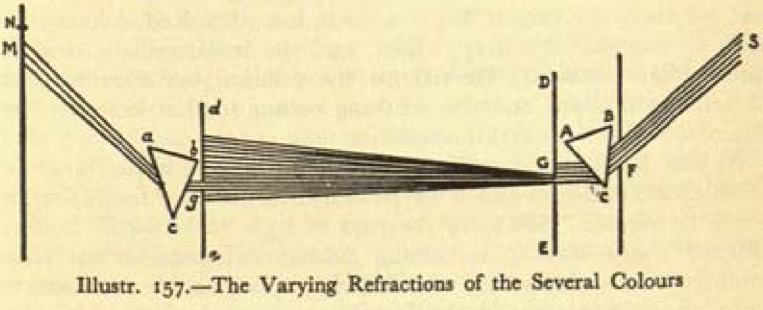
Newton became interested in optical problems while he was still an undergraduate, when he made attempts to construct telescopes and to eliminate their defects. In the hope of finding some means of getting rid of the chromatic aberration, which produces coloured edges round the images formed by a refracting telescope, he determined to make a study of the phenomena of colours. He bought a prism for this purpose in 1666, but his experiments were interrupted for two years by the Plague, and it was not until 1672 that he published an account of them in the *Philosophical Transactions*—his first scientific paper.

This account took the form of a letter to Oldenburg, in which Newton related how "having darkened my chamber, and made a small hole in my window-shuts, to let in a convenient quantity of the Sun's light, I placed my prism at his entrance, that it might be thereby refracted to the opposite wall. It was at first a very pleasing diversion to view the vivid and intense colours produced thereby; but after a while applying myself to consider them more circumspectly, I was surprised to see them in an oblong form; which, according to the received laws of refraction, I expected would have been circular" (*Phil. Trans.*, No. 80). See Illustr. 156.



Illustr. 156.—The Spectrum of Light

Setting his prism at the position of minimum deviation, Newton found that the length of the spectrum was about five times that of the spot of light thrown by the beam when unrefracted. He thought of various explanations, as that the light might be scattered by irregularities in the glass; but a second, inverted prism completely neutralized the effect of the first. The rays, he thought, might follow curved paths after refraction, but this was found not to be so. Newton finally isolated the several colours in succession, and upon refracting each beam with a second prism (Illustr. 157), he found



Illustr. 157.—The Varying Refractions of the Several Colours

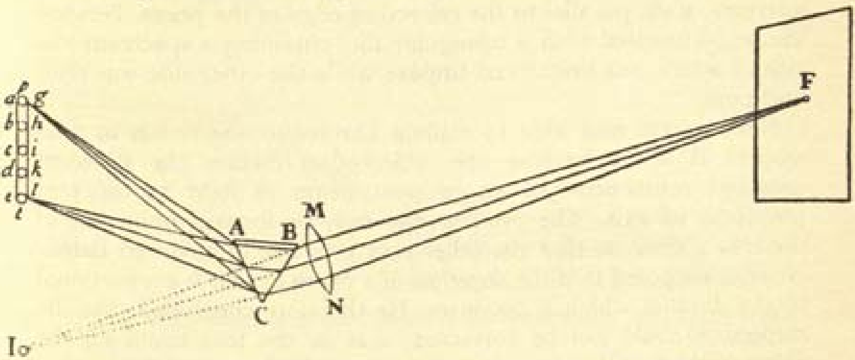
that the several colours showed unequal amounts of refraction. This experiment Newton, in Baconian phrase, called his *experimentum crucis* (crucial experiment). He later supplemented it by another experiment in which light, after suffering refraction at one prism so as to form an upright band on the wall, was passed through a second prism with its axis perpendicular to that of the first. The breadth of the resulting band of colours was not increased by the second refraction, but the spectrum became oblique, the colours which suffered the greater refraction at the first prism suffering the greater refraction at the second. He concluded that sunlight, and white light generally, is composed of rays of every colour, such colours being "original and connate properties" of the light, not manufactured by the prism. "To the same degree of refrangibility ever belongs the same colour, and to the same colour ever belongs the same degree of refrangibility." Newton, although never given to pushing his own discoveries, described this one, in another letter to Oldenburg, as "the oddest if not the most considerable detection which hath hitherto been made in the operations of nature."

Newton's paper awakened much controversy with Hooke, Pardies, Linus, Lucas, and other contemporary physicists whose objections, with Newton's answers, are to be found in the *Philosophical Transactions* for several years following 1672. As a result of these discussions, Newton's ideas on the *nature* of light gradually crystallized, and were brought into some relation with those of Hooke, who was his principal critic. The main difference between Newton and Hooke was this. Newton regarded colours as constituents of white light, while Hooke regarded them as produced by the modification undergone by white light when the light-pulse was made oblique to the light-rays, as he supposed happened in refraction. When he was replying to some of Hooke's objections in 1672 (*Phil. Trans.*, No. 88), Newton submitted that, on such a theory of light as Hooke's, and assuming the aether-vibrations to be periodic, these might be of various sizes, and "if by any means those of unequal bignesses be separated from one another, the largest beget a sensation of a Red colour, the least or shortest of a deep Violet, and the intermediate ones, of intermediate colours." He likened the relation between the size of aether-vibrations and the resulting colour to that between the size of air-vibrations and the resulting note.

At first Newton was inclined to explain light by a combination of an emission-theory and a wave-theory. In his reply to Hooke, in 1672, he wrote: "Assuming the rays of light to be small bodies, emitted every way from shining substances, those, when they impinge on any refracting or reflecting superficies, must as necessarily excite vibrations in the aether, as stones do in water when thrown into it" (*Phil. Trans.*, No. 88).

In 1675 Newton further developed his views on the elastic aether in which he supposed these vibrations to occur (in his "Hypothesis" communicated to the Royal Society: Brewster's *Memoirs*, etc., Vol. I, App. II). He rejected a purely undulatory theory of light, however, as he could not reconcile it with the rectilinear propagation of light.

His aether was largely intended to provide an explanation of gravitational attraction. Professor E. T. Whittaker, however, points out (in the Preface to his edition of Newton's *Opticks*) that when this explanation was shortly afterwards superseded by the descriptive inverse square law of universal gravitation, Newton lost much of his interest in aether theories, especially as it was difficult to reconcile the existence of an aethereal medium with the apparently unresisted motions of the planets. The discovery of polarization, again, seemed explicable only by comparing light to some form of corpuscle. Newton thus tended more and more towards a corpuscular hypothesis, relegating discussions on the aether to the more speculative sections of the *Principia*, and to the *Queries* of the *Opticks*.



Illustr. 158.—The Sine-Law of Refraction True for each Colour

Newton gave an account of his more important results in optics, together with much elementary matter, in some lectures at Cambridge, which were published in 1728. A comprehensive and more readable account of his work is to be found in his *Opticks* of 1704 (enlarged edition, 1717, with additions, 1718; edited in 1931 by E. T. Whittaker, F.R.S.). Newton begins this treatise with the words, "My Design in this Book is not to explain the Properties of Light by Hypotheses, but to propose and prove them by Reason and Experiments." In practice, however, he frequently relies implicitly upon the corpuscular hypothesis as an aid to explanation.

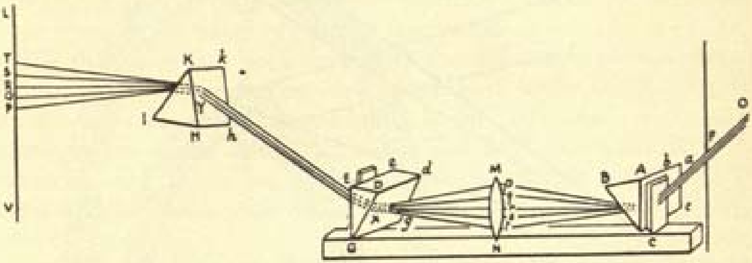
Book I of the *Opticks* contains Newton's fundamental experiments on the spectrum: its formation, the measurement of its length, and the connection between colour and refrangibility. This latter result is further confirmed by a number of subsidiary experiments. Newton next describes how he obtained a pure spectrum with the aid of a lens placed before a prism, and how he showed that the sine law of refraction is true for each colour individually (Illustr. 158).

Newton wished to obtain a spectrum in which the successive coloured images of the hole in the shutter formed on the screen by the prism should be as little as possible superimposed upon one another. Each colour would then be seen unmixed with its neighbours. In order to produce such a "pure" spectrum, he brought the light diverging from the aperture F (Illustr. 158) to a focus on the screen at I by means of the lens MN, and then placed the prism behind the lens in the converging beam. A sharp image of the aperture in each colour was now formed at *pt*. By diminishing the size of the aperture the purity was increased, but the breadth of the spectrum was seriously reduced. A spectrum at once pure and broad was, however, obtained by using in place of a circular aperture, a slit parallel to the refracting edge of the prism. Newton also experimented with a triangular slit, obtaining a spectrum one side of which was bright and impure while the other side was faint and pure.

Newton was now able to explain chromatic aberration in telescopes. It arises because the object-glass focuses the different coloured constituents of an incident beam of light at different points on its axis. The eye-lens can only be focused upon one of these at a time, so that the other rays give rise to coloured bands. Newton supposed that the *dispersion* of a prism or lens is proportional to the *deviation* which it produces. He therefore concluded that the dispersion could not be corrected, that is, the lens could not be achromatized, without ceasing to be a lens. His despair of eliminating chromatic aberration led him to give up refracting telescopes altogether in favour of reflecting ones, which he was probably the first to construct. Those who preferred refracting telescopes sought to make good their defects by constructing object-glasses of great focal length. Telescopes thus gradually increased in size. The difficulty of constructing sufficiently rigid tubes of such length was sometimes got over (as has already been explained) by dispensing with the tube altogether, as in the "aerial telescope" of Huygens.

Newton further devised a number of experiments to demonstrate the recombination of the spectral colours to form white light. One of these is shown in Illustr. 159. The prism ABC was made to cast a spectrum *pqrst* on the lens MN which, in turn, focused the coloured rays on the point X, where a second prism DEG parallel to the first neutralized the effect of the first prism and the lens and sent a parallel beam of white light towards Y. This beam behaved like ordinary sunlight, and could be refracted again by a third prism IHK to form a spectrum PQRST. Upon intercepting any of the colours *p*, *q*, *r*, *s*, *t* at the lens, the corresponding colour was found to disappear from the spectrum PQRST. This proved that the coloured constituents forming the ray XY were identical with those into which the ray was resolved. The colours were thus shown not to be due merely to a modification of the light consequent upon refraction, but to a separation and recombination of rays each possessing a certain colour.

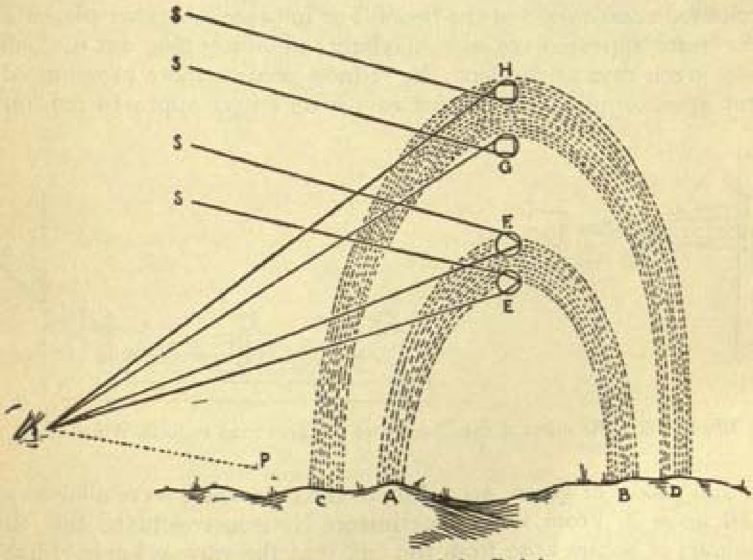
Newton used the same apparatus to examine the cause of the colours of bodies. He placed various bodies in the beam XY and found that they appeared in the same colours as when viewed by daylight, but that these colours arose from the corresponding coloured constituents of the beam. For instance, cinnabar placed in the beam appeared red as in daylight; upon stopping out the blue and green rays at the lens, the redness became more pronounced, but upon stopping out the red rays it no longer appeared red, but



Illustr. 159.—Reunion of the Colours of the Spectrum to form White Light

a dull yellow or green, according to the rays which were allowed to fall upon it. From these experiments Newton concluded that the colours of bodies arise from the fact that the various kinds of light incident upon them are reflected in different proportions from the surfaces of different bodies, according to the varying thickness of the films of which, he supposed, their surfaces were composed. Newton's theory of colours marked a great advance on the Aristotelian doctrine that these arise from the mixture of light and darkness in various proportions, which was held even in the seventeenth century, and was then scarcely improved upon by the crude views of Isaac Barrow, Newton's teacher.

An interesting by-product of Newton's investigations of colour-phenomena was his explanation of the rainbow. It had been recognized as early as the beginning of the fourteenth century by Theodoric of Saxony, that rainbows, both primary and secondary, are due to multiple refraction and reflection of sunlight in rain-drops. This explanation became known to a wider circle three centuries later through Antonio de Dominis. Descartes tabulated the deviation of a ray in passing through a drop against its angle of incidence on the surface, and showed from his table that, for a certain angle of incidence, this deviation was a minimum. Hence sunbeams incident on the drops at about this angle emerge as approximately parallel beams, and have an appreciable effect upon the eye. On this principle Descartes could explain the circular form and constant angular radius of the rainbow, and why it is always exactly opposite to the Sun, though his explanation of the colours was worthless. In this respect, however, Descartes' work was supple-



Illustr. 160.—Formation of the Rainbow

mented by Newton, who showed that each colour produces its own bow, partially overlapping the neighbouring ones (see Illustr. 160).

The second book of the *Opticks* deals with the colours of thin films. The central topic is the familiar phenomenon known as "Newton's Rings," which he originally produced by pressing the flat side of a plano-convex lens against a double convex lens of great focal length. He found the thicknesses of the air film corresponding to the dark rings to be as 0, 2, 4, 6 . . . and those corresponding to the most vivid portions of the bright rings to be as 1, 3, 5, 7. . . . He explains the phenomenon in terms of the corpuscular hypothesis with the aid of an *ad hoc* hypothesis of "fits" of easy transmission and reflection: ". . . the Rays of Light, by impinging on any refracting or reflecting Surface, excite vibrations in the . . . Medium . . . the vibrations thus excited are propagated . . . and move faster than the Rays so as to overtake them; and . . . when any Ray is in that part of the vibration which conspires with its Motion, it easily breaks through a refracting Surface, but when it is in the contrary part of the vibration which impedes its Motion, it is easily reflected. . . ." In this manner Newton took account of the obvious periodicity involved in the properties of the rings. Since, however, he supposed that any light reflected from the upper surface had nothing to do with the phenomenon, his explanation was actually inferior to Hooke's. Newton similarly invoked "fits" in order to explain why light incident upon a transparent body is partly reflected and partly refracted. He also tried to establish an analogy between the colours of thin transparent films and the permanent colours of bodies whose ultimate particles, he supposed, were transparent flakes of definite thicknesses.

The third and last Book deals with the diffraction phenomena of Grimaldi, which Newton produced for himself under many different conditions, and which he attributed to an "inflexion" of the rays passing close to the diffracting edge. The Book closes (in the later editions) with thirty-one *Queries* suggesting various hypotheses to explain light phenomena and gravitation, and pointing out further lines of inquiry.

Among his Queries Newton included some speculations on double refraction. He took the first step towards explaining the curious phenomena observed by Huygens upon passing a ray through two calcites whose principal sections were variously inclined to each other. Newton suggested that "every Ray of Light has . . . two opposite Sides, originally endued with a Property on which the unusual Refraction depends, and the other two opposite Sides not endued with that Property" (Query 26). He supposed that the particles of the crystal must have a similar duality, and he likened the two conditions in the rays and the particles to magnetic polarity. This analogy led to the idea of the "polarization" of light. It told against the undulatory theory, however, since only longitudinal waves were thought admissible, and these could not have different properties in different directions perpendicular to their line of propagation.

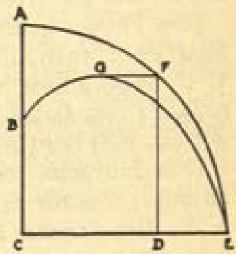
MARIOTTE

Mariotte devoted one of his physical essays, *Traité de la nature des couleurs* (1686), to the phenomena of light, in which he describes experiments with the prism, and criticizes Newton's theory of colour and Descartes' mechanical explanation of light. He made a notable contribution to atmospheric optics with his explanation of the halos which are occasionally visible round the Sun and Moon, as well as of mock-suns and mock-moons. His theory of the production of the halos of 23 degrees radius was based upon some suggestions of Descartes and, so far as it goes, is essentially that accepted to-day. He explained the phenomenon on the assumption that, in the upper regions of the atmosphere, minute prismatic crystals of ice are sometimes formed and float suspended. Rays of light falling on these suffer a twofold refraction, and the light forming the halo is composed of rays which pass through the crystals at minimum deviation. The method of proof is somewhat similar to that employed by Descartes in his explanation of the rainbow. The ice-needles are orientated in every possible manner, but there must always be a sufficient number whose axes are perpendicular to the line joining the observer's eye to the Sun or to the Moon. For this position of the needles, calculation gives the observed angular radius of 23 degrees for the resulting halo.

In optics Mariotte made the remarkable discovery of the "blind spot" in the eye, which he announced to the Academy in 1666. He describes how he had frequently observed in dissecting human and animal eyes that the optic nerve does not enter the eyeball exactly opposite to the pupil, but in man rather higher, and more towards the nose. In order to observe what happened when light fell exactly on the optic nerve, Mariotte fastened a small white paper disc to a dark screen about level with his eyes. He then fastened a second disc, approximately four inches in diameter, about two feet from the first, to the right but somewhat lower. He fixed his right eye upon the first disc and closed his left eye. Upon gradually moving back from the screen he found that, at a distance of about nine feet the second disc vanished from his sight, though objects all round it remained visible, and a slight displacement of the eye brought it into view again. He obtained the same effect upon altering his distance from the screen, and the distance apart of the discs, in the same proportion, and again when he used dark discs on a white ground, and again upon performing a corresponding experiment with his left eye. He thus convinced himself that the phenomenon is due to a defect of the optic nerve, which is insensitive to light at the point where it enters the retina. Mariotte's experiment created a considerable sensation, and it was repeated successfully in 1668 at a meeting of the Royal Society of London. The discovery led Mariotte, however, to the false conclusion that it is not the retina but the underlying choroid which is the seat of vision.

TSCHIRNHAUS

Ehrenfried Walter von Tschirnhaus (1651–1708), a German nobleman, friend of Leibniz and Spinoza and foreign member of the French *Académie des Sciences*, belonged to the class of rich scientific amateurs which included also Hevelius and von Guericke. He spent much of his wealth upon the manufacture of physical, and especially of optical, apparatus. Some of his concave mirrors of copper, the largest of which is still preserved as a curiosity, had a diameter of about three yards and a focal length of about two yards. They were capable of melting a dollar-piece within five minutes, though it was noticed that they produced no appreciable heating effect when used to focus moonlight. Tschirnhaus' lenses ranged up to 80 cm. in diameter. One of these found its way to Florence, where it was employed, in 1695, in experiments on the inflammability of diamond. It melted porcelain and pumice-stone placed at its focus, and within half an hour burned a diamond weighing 140 grains. Tschirnhaus was led by his experiments with burning mirrors to undertake theoretical investigations in optics, and he was one of the pioneers in the study of the caustics arising in connection with the reflection of light at such mirrors.



Illustr. 161.—The Caustic Curve of Intersection of Reflected Rays

It had been known to Roger Bacon that rays of light from a point object do not all pass through a single point after reflection from a concave mirror. The corresponding phenomenon in the refraction of light through a lens had been pointed out by Maurolycus and had been studied by Barrow. The rays reflected from a concave mirror are actually enveloped by a surface to which they are all tangents, and it was the curves obtained by cutting such surfaces by planes through their axes of symmetry, whose geometrical properties were studied by Tschirnhaus and his contemporaries. One branch of such a curve, arising from the reflection of a parallel beam at the mirror AFE, is shown in Illustr. 161.

The ray DF of the beam is reflected in the direction FG. Another ray adjacent to DF gives rise to a reflected ray very slightly inclined to FG and intersecting it in G. The caustic curve EGB is the locus of such points of intersection of consecutive reflected rays.

Huygens was probably the first correctly to ascertain the properties of such a caustic formed by a parallel beam falling upon a concave mirror (*Traité de la Lumière*, Chapter VI). Before his book was published, however, Tschirnhaus published a paper (*Acta Erudit.*, 1682) giving a construction for such a caustic, though he later revised his work after de la Hire had drawn his attention to an error in the calculation. Among the results obtained by Tschirnhaus was the relation

$$\text{length of arc EG} = \text{DF} + \text{FG}.$$

More substantial contributions to the theory both of *catacaustics* (formed by reflection) and of *diacaustics* (formed by refraction) were made at the close of the seventeenth century by Johann and Jakob Bernoulli (who introduced these terms), and by the Marquis de l'Hôpital.

(See E. Mach, *The Principles of Physical Optics*, tr. by J. S. Anderson and A. F. A. Young, 1926; E. T. Whittaker, *A History of the Theories of Aether and Electricity*, 1910; Michael Roberts and E. R. Thomas, *Newton and the Origin of Colours*, London, 1934.)

(On Physics generally, see F. Cajori, *History of Physics*, 2nd ed., New York, 1929; E. Gerland, *Geschichte der Physik*, Munich, 1913; J. C. Poggendorff, *Geschichte der Physik*, Leipzig, 1879; F. Rosenberger, *Geschichte der Physik*, Braunschweig, 1882–1890; W. F. Magie, *A Source Book in Physics*, New York and London, 1935.)

(For Huygens, see also *Oeuvres Complètes*, La Haye, 1888, etc., 20 vols.; *Treatise on Light*, trans. S. P. Thompson, London, 1912; A. E. Bell, *Christian Huygens*, London, 1947.)

CHAPTER XII

PHYSICS

II. HEAT   III. SOUND

II. HEAT

THE more obvious phenomena of heat—combustion, evaporation, melting, freezing, etc.—were of course familiar from early times, and the legend of Prometheus bears witness to the great importance which the ancients attached to fire. Even the leading conceptions concerning the nature of heat are pretty old. There was the so-called Aristotelian, but really pre-Platonic, conception of fire as one of the four material elements, and the Platonic view that heat is a kind of motion. But throughout the centuries—in fact up to the time of Robert Hooke—little or no distinction was made between heat, fire, and flame. Even Robert Boyle did not discriminate between them. Hence part of the story of the study of the phenomena of heat is given in the chapter on the beginnings of modern chemistry (Chapter XV).

FIRE ATOMS VERSUS MOLECULAR MOTION

At the beginning of the modern period we find Pierre Gassendi upholding the view that heat consists of special kinds of atoms, whereas Francis Bacon advocated the view that heat is a kind of motion. Bacon based his view on empirical evidence in accordance with the methods of induction explained in his *Novum Organum* (1620). There is a modern ring about some of his phrases, but his views on heat were not really very clear, as may be seen from the following passage.

"When I say of motion that it is the genus of which heat is a species, I would be understood to mean, not that heat generates motion, or that motion generates heat (though both are true in certain cases), but that heat itself, its essence and quiddity [quality], is motion and nothing else. . . . Heat is a motion of expansion, not uniformly of the whole body together, but in the smaller parts of it, and at the same time checked, repelled, and beaten back, so that the body acquires a motion alternative, perpetually quivering, striving, and irritated by repercussion, whence spring the fury of fire and heat" (*Novum Organum*, Book II, § xxi).

The experimental study of heat was taken in hand by Boyle. It is noteworthy, in view of the subsequent researches by Count Rumford, that among the experimental evidence adduced by

Boyle, in support of the view that heat is the rapid agitation of the parts of a substance, was the heat generated during the boring of guns. This view is brought out clearly in the following passage from Boyle: "And it will be convenient to begin with an instance or two of the production of heat, wherein there appears not to intervene any thing in the part of the agent or patient, but local motion, and the natural effects of it. And as to this sort of experiments, a little attention and reflection may make some familiar phenomenon apposite to our present purpose. When, for example, a smith does hastily hammer a nail, or such like piece of iron, the hammered metal will grow exceedingly hot, and yet there appears not anything to make it so, save the forcible motion of the hammer, which impresses a vehement, and variously determined agitation of the small parts of the iron; which being a cold body before, by that superinduced commotion of its small parts, becomes in divers senses hot; first, in a more lax acceptation of the word in reference to some other bodies, in respect of whom it was cold before, and then sensibly hot; because this newly gained agitation, surpasses that of the parts of our fingers. And in this instance, it is not to be overlooked, that oftentimes neither the hammer, by which, nor the anvil, on which a cold piece of iron is forged (for all iron does not require precedent ignition to make it obey the hammer), continue cold, after the operation is ended; which shews, that the heat acquired by the forged piece of iron was not communicated by the hammer or anvil as heat, but produced in it by motion, which was great enough to put so small a body, as the piece of iron, into a strong and confused motion of its parts, without being able to have the like operation upon so much greater masses of metal, as the hammer and the anvil; though, if the percussions were often and nimbly renewed, and the hammer were but small, this also might be heated (though not so soon, nor so much, as the iron;), by which one may also take notice, that it is not necessary a body should be itself hot, to be calorifick. And now I speak of striking an iron with a hammer, I am put in mind of an observation, that seems to contradict, but does indeed confirm our theory; namely, that if a somewhat large nail be driven by a hammer into a plank, or piece of wood, it will receive divers strokes on the head before it grows hot; but when it is driven to the head, so that it can go no further, a few strokes will suffice to give it a considerable heat; for whilst, at every blow of the hammer, the nail enters further and further into the wood, the motion, that is produced, is chiefly progressive, and is of the whole nail tending one way; whereas, when that motion is stopped, then the impulse given by the stroke, being unable either to drive the nail further on, or destroy its intireness, must be spent in making a various vehement and intestine commotion of the parts among themselves, and in such an one we formerly observed the nature of heat to consist" (*Of the Mechanical Origin of Heat and Cold*, 1675, Section II, Experiment VI, pp. 59–62; *Works*, ed. Birch, 1772, Vol. IV, pp. 249–50).

Yet alongside of the oft-repeated view that "heat seems principally to consist in that mechanical property of matter called motion," Boyle also spoke repeatedly of "atoms of fire," and attributed the gain in weight on the part of a metal when calcined to its absorption of such atoms of fire during calcination. He was never really inclined to regard cold also as something positive and consisting of "peculiar frigorific agents," presumably analogous to the atoms of heat. And when he froze a weighed quantity of water and could observe no change of weight in the resulting ice, he concluded that the search for frigorific particles was futile.

The failure to discriminate between combustion and other forms of heat naturally induced some people to extend to all forms of heat what they believed to be true of combustion. Boyle examined experimentally the view that air is necessary to the production of heat, and arrived at an adverse verdict. He says: "For the sake of those that think the attrition of contiguous air is necessary to the production of manifest heat, I thought, among other things, of the following experiment, and made trial of it. We took some hard black pitch, and having, in a bason, porringer, or some such vessel, placed it a convenient distance under water, we cast on it, with a good burning-glass, the sun-beams, in such a manner, that, notwithstanding the refraction, that they suffered in the passage through the interposed water, the focus fell upon the pitch; wherein it would produce sometimes bubbles, sometimes smoak, and quickly communicated a degree of heat capable to make pitch melt, if not also to boil" (*Of the Mechanical Origin of Heat and Cold*, 1675, Section II, Experiment IX, pp. 66–7; *Works*, ed. Birch, 1772, Vol. IV, p. 251).

He also showed that a piece of red-hot iron placed in the receiver underwent no manifest change when the air was evacuated, and the sides of the receiver were hot even then (*New Experiments Physico-Mechanical*, 1660, pp. 80–2; *Works*, Vol. I, p. 28); that when two closely fitting pieces of brass, one concave and the other convex, placed in an exhausted receiver, were made to rub against each other, by means of suitable revolving apparatus fixed outside the receiver, they became very hot; and that lime slaked in a vacuum produced heat in the same way as when slaked in the air (*A Continuation of New Experiments Physico-Mechanical*, 1669, pp. 154–8; *Works*, ed. 1772, Vol. III, pp. 265–7).

The solution of some of the problems of heat was carried a stage further by Robert Hooke. He made experiments like those of Boyle, examined sparks under the microscope, and arrived at more consistent conclusions than Boyle had done. According to Hooke heat is "a property of a body arising from the motion or agitation of its parts." And he distinguished mere heat from fire and flame, which he described as the effects produced by the action of air on heated bodies. He ridiculed, with considerable gusto, the idea of fire atoms coursing through the pores of hot bodies. "We need not trouble ourselves to find out what kind of pores they are, both in flint and steel, that contain the atoms of fire, nor how these atoms come to be hindered from running all out when a passage in their pores is made by the concussion; nor need we trouble ourselves to examine by what Prometheus the element of fire came to be fetched down from above the regions of air, in what cells or boxes it is kept, and what Epimetheus lets it go; nor to consider what it is that causes so great a conflux of the atomical particles of fire, which are said to fly to a flaming body like vultures or eagles to a putrefying carcase and there to make a very great pudder" (*Micrographia*, Observation VIII, p. 46). Heat, then, "being nothing else but a very brisk and vehement agitation of the parts of a body," and since, according to Hooke, "the parts of all bodies though never so solid do yet vibrate," it follows that "all bodies have some degree of heat in them," and nothing is "perfectly cold." In this way Hooke rejected the conception of cold as something positive, and denied the existence of frigorific particles as well as the existence of fire atoms.

THERMAL CAPACITY

The conception of "thermal capacity" (in the sense of specific heat) appears to have originated with the Accademia del Cimento. Some of its members conducted a variety of experiments on the conduction of heat and the associated phenomenon, thermal capacity. They made a mercury thermometer and a water thermometer of the same size as their ordinary alcohol thermometers. Placing the thermometers in liquids of various temperatures they noticed that the mercury thermometer changed its level more quickly than did the water thermometer, although of course the actual extent of the change in the column of mercury was much less than that in the column of water. (Compare Halley's similar experiment described in Chapter V.) They also made experiments by pouring equal amounts of different liquids, which had been heated to the same temperature, upon ice. They found that the quantity of ice melted by each liquid was different, in spite of the fact that the liquids were all of the same temperature. But the problems of specific heat, thus attempted by the Accademia del Cimento, had to wait for their solution till Joseph Black took them in hand.

RADIATION OF HEAT AND COLD

Although for long centuries men had known the use of burning mirrors and lenses for bringing the Sun's rays to a focus at which combustibles could then be ignited, Francis Bacon (*Novum Organum*, Book II, § xii) first suggested burning-glasses for focusing invisible heat rays: "Let a burning-glass be tried with a heat that does not emit rays or light, such as the heat of iron or stone which has been heated but not ignited, or the heat of boiling water, and the like; and observe whether there ensue an increase of the heat, as in the case of the Sun's rays." In his *De Dignitate et Augmentis Scientiarum* (1623, *Lib.* V, *Cap.* II), he wondered whether cold also could be concentrated, like heat, by means of a mirror. But Baptista Porta (*Magia Naturalis*, 1589, p. 264) had already shown that a concave glass (mirror) reflected heat, light, cold, and sound. He noted that the heat and light of a candle set before a mirror became sensible to the eye when placed at the conjugate focus, "but this is more wonderful, that, as heat, so cold should be reflected: if you put snow in that place, if it come to the eye . . . it will presently feel the cold."

The Accademia del Cimento first demonstrated clearly the reflection of cold (*Essays of Natural Experiments*, etc., trans. R. Waller, 1684, p. 103), by placing 500 lb. of ice before a concave mirror and putting a sensitive 400° thermometer at the focus. The liquid in the thermometer fell immediately; but the ice was near the thermometer, and, to test whether "the direct or reflected rays of cold were more efficacious," they covered the mirror and found that the thermometer liquid rose. This seemed definite proof, but they wrote, "for all this, we dare not be positive; but there might be some other cause thereof besides the want of reflection from the glass; since we were deficient in making all the trials necessary to clear the experiment."

Mariotte about this time (1679) discovered another remarkable fact concerning the difference between the radiant heat and the light in the rays from a fire (*Histoire de l'Académie Royale des Sciences depuis son Etablissement en* 1666 *jusqu'à* 1686, Paris, 1773, Vol. I, pp. 303, 344). He showed that, whereas the heat from the Sun is not separated from the light in passing through transparent bodies, the opposite is true in the case of the rays from a fire. He put a concave metal mirror before a fire. At its focus the hand could not long endure the heat; but, when a glass plate was placed over the mirror the light at the focus was undiminished, or nearly so, whereas no heat could now be felt. This was shown to the *Académie* in 1682. But, writing of it in 1686 (*Traité de la nature des couleurs—Œuvres*, ed. 1740, Vol. I, p. 288), he seems to have realized that the hand was rather insensitive as a means of detecting this heat, and said that, in the transmission of the rays of the fire through the glass, the heat either does not pass through at all, or passes through only in a very small degree. This was the first proof that the radiant heat of a fire could be separated from the light. In this work Mariotte showed also that the original heating effect of the Sun's rays at the focus fell to four-fifths when the mirror was covered with the glass, and that this loss was of the same order as that suffered by the light in reflection at the glass surfaces in traversing it twice.

Hooke confirmed this (Birch, *History of the Royal Society of London*, 1757, IV, p. 137) in 1682, proving to the Royal Society that the heat of a fire was not propagated through glass in the same way as the heat of the Sun. He focused the heat of a fire with a concave metal mirror and placed a glass plate between the fire and the mirror; the light passed through almost undiminished, but there was practically no heat in the focus of the mirror. (Hooke probably used his hand to detect the heat, as Mariotte had done.)

Mariotte also performed a celebrated experiment in which gunpowder was ignited by means of a lens made of ice. He obtained clear ice from pure water which had been boiled for half an hour so as to expel all air, and which was then frozen in the form of a plate several inches thick, free from bubbles and transparent. He placed a portion of this ice plate in a small spherically concave vessel which he brought near to a fire. He allowed it to go on melting, while repeatedly turning it over, until it had assumed, on both sides, the spherical form of the vessel. He then seized the piece of ice by the edges with his gloved hand and brought it into the sunlight, where he soon managed by its means to ignite some gunpowder placed at its focus (*Traité de la nature des couleurs—Œuvres*, ed. 1740, Vol. II, pp. 607 ff.).

Influenced by such or similar observations Newton leaned towards a theory of heat radiation by means of the vibrations of a medium much more subtle than air, existing even where there is no air, and possibly spread through the whole immensity of celestial space in virtue of its great elastic force. Newton did not definitely embrace this view; rather he suggested it in the queries contained in the second edition of his *Opticks* (1717). Curiously enough, it was Newton's own rejection of the imponderable aether, in 1702, that hindered the proper consideration of his suggestion relating to the manner of heat radiation. The result was that for a long time the dominant tendency was to regard heat as a material substance. This was natural. In an age in which the phlogiston theory flourished caloric theories were more or less inevitable. But the history of all this must be held over for a later chapter.

(See E. Mach, *Prinzipien der Wärmelehre*, Leipzig, 1923.)

III. SOUND

The phenomena of sound engaged the attention of many people from early times. Their interest was mainly directed to music, though Pythagoras, Aristotle, Vitruvius, and possibly others in ancient and mediaeval times also made a purely scientific study of the physics of these phenomena. The modern study of this branch of physics dates from Galilei and his contemporaries. The story is somewhat tangled and scrappy. For the sake of simplicity the account here given is divided into sections dealing with the main problems of sound, namely, the determination of the conditions affecting the pitch of notes, the velocity of sounds, the medium of their transmission, and the conditions affecting the intensity of sounds. As these problems are not entirely disconnected a slight amount of overlapping is inevitable.

The Pitch of Sounds

Galilei's discoveries relating to the laws of the oscillations of the pendulum led him to direct his attention to the vibrations of strings, and especially to the phenomenon of so-called sympathetic vibration, which was popularly explained as due to some kind of sympathy on the part of other strings with the vibrating string. First of all Galilei showed the dependence of the pitch of a note upon the rate of vibration, that is upon the number of vibrations occurring in a given time. He did this by means of the following experiment. He moved a sharp piece of iron across a plate of brass. Whenever a distinct note was thus produced, he noticed a number of fine lines (scratches) on the plate at equal distances from one another. When, by means of a quick movement, he produced a high note, then the lines were close together; when the note was lower the lines were farther apart. Evidently the closeness and number of the lines corresponded to the greater or smaller number of vibrations of the iron, for the vibrating of the iron could be distinctly felt by the hand that held it. Galilei next utilized the number of lines, which appeared in a unit of time, whenever a certain note was produced, in order to study the phenomena of sound quantitatively. He produced, for instance, two notes by successively stroking the brass plate more rapidly and less rapidly, and when he obtained two consonant notes which in Music are said to constitute the "fifth," he counted the lines on the brass plate and measured their mutual distances, and discovered that there were forty-five lines (and therefore vibrations) for the higher note to thirty lines (and therefore vibrations) for the lower note. Experiments on the relation of notes to the strings producing them were, of course, very old. Pythagoras (sixth century B.C.) had already instituted such experiments. But the relation hitherto studied had been solely that between the pitch of a note and the *length* of the string. Galilei first drew attention to the rate of *vibrations* (or frequency) as the really important factor in determining the pitch of a note produced by a sounding body. By simple experiments like the above Galilei discovered that the rates of vibration for the fundamental note, the fourth, the fifth, and the octave above it are in the proportion of 1 : 4/3 : 3/2 : 2, that is, as 6 : 8 : 9 : 12. Another interesting experiment in connection with the pitch of notes consisted in a demonstration of stationary water-wavelets which varied in height and in number according to the note produced on the glass vessel containing the water. Galilei, namely, partly filled a glass vessel with water, and produced notes by suitably stroking the glass. Wavelets appeared on the surface of the water, and remained stationary so long as the same note lasted. When the note was suddenly made an octave higher, then each wavelet divided into two wavelets. (For references, see Chapter III.)

Largely under the influence of Galilei the study of sound was taken up by Mersenne, to whom we owe some of our information concerning Galilei's work in acoustics. Mersenne carried out numerous experiments in order to determine the correlation between the pitch of a note and the length, thickness, and tension of a string of given material on which the note is produced. Using n and n' for the pitch of two different notes (or their rates of vibration), l and l' for different lengths of the same kind of string, d and d' for different diameters of the strings, p and p' for different weights stretching the strings, and q and q' for different weights of the strings themselves, Mersenne submitted the following equations:—

(1) When the strings are of equal length and diameter, but stretched by unequal weights, then $n/n' = \sqrt{p}/\sqrt{p'}$.

(2) When the strings are equally long and equally stretched, but are of different weights, then $n/n' = \sqrt{q'}/\sqrt{q}$.

(3) When the strings are of equal diameter and tension, but of different lengths, then $n/n' = l'/l$.

(4) When strings composed of the same material are of equal length and tension, but of different diameters, then $n/n' = d'/d$.

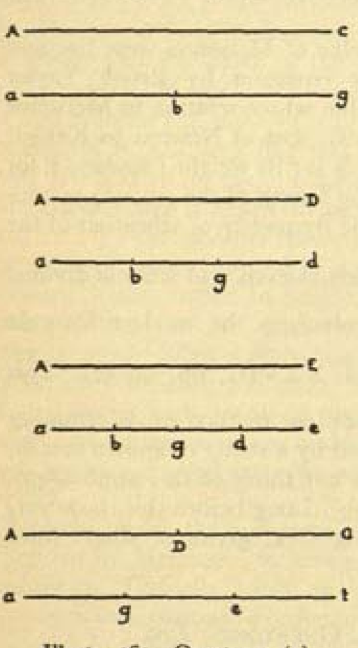
Mersenne also experimented with strings made of various metals—gold, silver, copper, brass, and iron—and found that with strings of the same length, thickness, and tension, the pitch of the note varied inversely with the specific gravity of the metal (*Harmonie Universelle*, 1636).

The seemingly disconnected formulae of Mersenne were brought together under one comprehensive equation by Brook Taylor (1685–1731), an English mathematician whose relation to Mersenne may to some extent be compared with that of Newton to Kepler. Using L for the length of the string, N for its weight (*pondus*), P for the weight stretching it, and D for the length of the seconds pendulum, Brook Taylor's formula gives the frequency of vibration of the string as $\frac{c}{d}\sqrt{\frac{PD}{NL}}$, where $\frac{c}{d}=$ the circumference of a circle divided by its diameter $= \pi$. This is equivalent to the modern formula $n = \frac{1}{2l}\sqrt{\frac{T}{m}}$ (*Phil. Trans.*, 1713, Vol. XXVIII, pp. 26–32). This equation incidentally supplied an excellent method of determining absolutely the pitch of a note produced by a string of known length, weight, and tension. But Taylor did not think of this application, though Euler did so afterwards (1739). Long before this, however, John Shore had invented the tuning fork, giving a single pure musical tone of constant pitch (1711).

Sympathetic Vibration, Overtones, Etc.

The study of sympathetic vibrations, which had stimulated Galilei's interest in the physics of sound, made some progress in the course of the seventeenth century, and was completed in the course of the eighteenth century. Intimately connected with it was the discovery or rather the explanation of overtones or harmonics, and the discovery of longitudinal vibrations in solids as distinct from transverse vibrations, which were the only ones known to the earlier investigators.

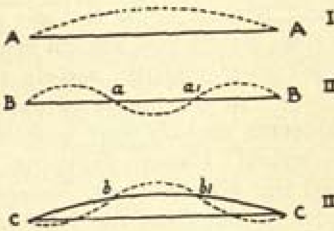
Mersenne discovered that a vibrating string produces overtones in addition to the fundamental note. The fundamental tone is, of course, obvious when a string vibrates freely, but when this tone becomes weaker after a while it is comparatively easy to perceive certain other tones which continue a little longer than the fundamental note. In this way Mersenne heard the twelfth and the seventeenth notes above the fundamental. When Mersenne communicated his discovery to Descartes the latter suggested that the harmonics or overtones might be due to the fact that the parts of a vibrating string vibrate each on its own account. This idea was put to an experimental test, about the year 1673, by William Noble, of Merton College, Oxford, and Thomas Pigot, of Wadham College, Oxford, apparently quite independently of Descartes and each other. These experiments were first reported by John Wallis in the *Philosophical Transactions*, April 1677 (Vol. XII, pp. 839–42). Wallis begins by referring to the then already well-known fact that if a stretched viol or lute string is plucked or bowed, then another string on the same instrument, or near at hand, if in unison with the first, or if it stands in some simple relation of consonance to it, vibrates of its own accord. (Wallis adds, in a postscript, that a string will respond in this way not only to another string, but also to a consonant note on a wind-instrument, such as the organ). The new discovery of Noble and Pigot was that "not the whole of that other string doth thus tremble, but the several parts severally, according as they are Unisons to the whole or the parts of that string which is so struck." The method of Noble and Pigot may be readily explained with the aid of the above diagram. Let AC and *ag* [Illustr. 125] represent two neighbouring strings, such that the fundamental note produced by the vibration of AC is the first octave to that of *ag*, and therefore AC is in unison with each half of *ag*. Now if, while *ag* is open, AC is struck, the two halves of *ag*, namely *ab* and *bg*, will both tremble, but not the middle point *b*. This can easily be observed if a little bit of paper (called a "rider") be lightly wrapped about the string *ag*, and moved gradually from one end of the string to the other. The bit of paper will be found to rest at *b*. Similarly if the string AD, whose note is the twelfth above that of the string *ad*, be struck, *ad* will vibrate in three equal parts, *ab*, *bg*, *gd*, while the points *b*, *g* remain at rest. Again, if AE



Illustr. 162—Overtones (1)

be the double octave to *ae*, the latter will vibrate in four parts, *ab*, *bg*, *gd*, *de*, the points *b*, *g*, *d* remaining at rest. And so on. If, however, AG be a fifth to *at*, so that each half of AG is in unison with each third of *at*, then, when AG is struck, each part *ag*, *ge*, *et* will vibrate separately; and when *at* is struck each part of AG, namely AD, DG, will vibrate. The like will hold in less concords, but the less remarkably as the number of divisions increases.

Wallis verified these results for himself, and noticed that a string plucked at any point which divided the string in simple ratios (half, third, etc.), did not produce such a clear sound as when plucked elsewhere. In the latter case, he thought, the clearness must be due to the simultaneous vibrations of the several unison parts; whereas when struck at the points of division the incongruity must result from the disturbance of the points which should be at rest.



Illustr. 163.—Overtones (2)

The discoveries of Noble and Pigot were also made independently later by Joseph Sauveur (1653–1716), and further developed by him (1700). His experiments were partly the same as those of his two predecessors and partly by means of a monochord on which he produced the overtones in an easily apprehensible form by lightly touching the vibrating string at the several points of rest (*b*, *g*, *d*, in the above diagrams), or *nodes*, as he called them.

Sauveur showed that a string may oscillate along its whole length, as in AA [Illustr. 163 (I)], or the oscillations may take place along parts of the string and in opposite directions, as in BB [Illustr. 163 (II)], and separated by nodes a, a_1, that is by points where the string is at rest. In the first case (AA) the string produces its fundamental, in the second case (BB) it produces overtones or harmonics. Lastly, it is possible for the two types of vibration to take place simultaneously, as in CC [Illustr. 163 (III)]. This, in fact, is the usual type, where no special steps are taken to prevent either of the two types of transverse oscillation or vibration (*Mém. de l'Acad. des Sciences*, Paris, 1701, pp. 347 f.).

The Velocity of Sound

Of the various problems relating to acoustics that of the velocity of sound attracted the greatest amount of attention during the seventeenth century. The first experiments in this connection appear to have been made by Pierre Gassendi (1592–1655). He was led to this problem by the study of the pitch of sounds. According to the Aristotelian view high notes are transmitted through the air more rapidly than are low notes. Gassendi's experiment showed the inaccuracy of this view. A cannon and a musket were fired towards suitably distant points, and measurements were made of the time which elapsed between the moment when suitably placed observers saw the flash and the moment when they heard the explosion. The velocity of the sound was obtained by dividing the distance between the cannon (or the musket) and the observer by the time-interval between the perception of the flash and of the explosion. The velocity appeared to be the same in both cases, namely, 1,473 Paris feet per second. The result was much too high. Mersenne repeated the experiment, and obtained a somewhat better result, namely 1,380 feet per second. About twenty years later, in 1656, Borelli and Viviani made similar experiments and obtained a still lower velocity, namely 1,077 feet per second. Other values obtained in the course of the seventeenth and early part of the eighteenth century were as follows: Robert Boyle, 1,126 Paris feet per second; Cassini, Huygens, Picard, and Römer, 1,097; Flamsteed and Halley, 1,071. Newton took up the problem, not experimentally, but from the standpoint of mechanics or mathematical physics, and arrived at the following equation correlating the velocity of sound in air (v) with the elasticity (e) and the density (d) of the air: $v = \sqrt{e/d}$ (*Principia*, Book II, § 8). For moderate temperatures the velocity, according to this formula, should be 906 Paris feet per second, which is too low. The formula, as was subsequently shown by Lagrange, assumed that the elasticity of the air is simply proportional to its pressure, and failed to take into account changes in the elasticity of the air due to the changes in heat caused by the very propagation of sound through the air. The equation was later modified by Laplace by inserting this correction (*Ann. de Chimie*, 1816, and *Mécanique Céleste*, Book XII). In 1738 a commission appointed by the Paris Academy of Sciences to determine the velocity of sound through the air obtained the value 1,038 Paris feet per second (*Mém. de l'Acad. des Sciences*, 1738). In most of these experiments on the velocity of sound little or no attention was paid to the influence of such factors as variations of temperature and direction of wind. Other experimenters, however, did make a special study of these factors, though their efforts were not particularly successful.

Gassendi's observations led him to the negative result that the direction of the wind does not affect the velocity of sound, which is the same whether the wind blows in the same direction as the sound travels or in the opposite direction. He was mistaken. So were Borelli and Viviani, who arrived at the same negative result. William Derham (1657–1735), however, corrected this error in 1705, when he found that the direction of the wind does influence the velocity of sound (*Phil. Trans.*, 1708, Vol. XXVI, pp. 1–35).

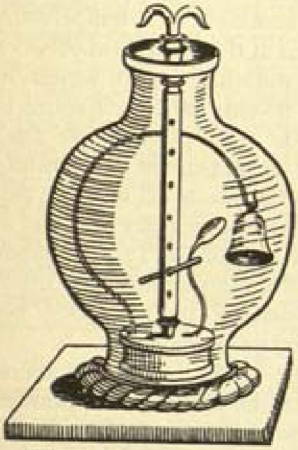
A careful study of the influence of variations of temperature on the velocity of sound was apparently not undertaken until 1740, when Bianconi carried out some experiments for the purpose at Bologna. Cannons were fired at Bologna and observed at St. Urbano, thirty miles distant, and the time-interval between the perception of the flash and of the detonation was measured by means of a pendulum. Bianconi found that in summer, at a temperature of 28° R. there were seventy-six oscillations of the pendulum between the observation of the flash and of the detonation; but in winter, at a temperature of $-1{\cdot}2°$ R., there were seventy-nine oscillations of the pendulum in the interval between the perception of the flash and of the detonation. He concluded that an increase in temperature increases the velocity of sound (*Della diversa velocità del suono*, 1746). In the same year also La Condamine studied the velocity of sound at Quito and found it to be 339 metres per second. In 1744 he made similar observations at Cayenne, where the temperature was much higher than at Quito, and found the velocity to be 357 metres per second, thus confirming Bianconi's general conclusion.

At least one way in which a knowledge of the velocity of sound might be put to practical use was suggested by Derham. He pointed out that once we know the velocity of sound we can estimate the distance of a storm area by noting the time interval between our perception of the flash of the lightning and our perception of the clap of the thunder (*Phil. Trans.*, 1708, Vol. XXVI, No. 313).

Derham also attempted to determine the influence of variations of temperature, of the direction of the wind, and of the moisture of the atmosphere on the intensity of sounds. But his results were rather vague. In general he found that sounds are weaker in summer than in winter; that they are stronger and harsher when there are easterly or northerly winds than when there are westerly winds; and that the sound of firearms is not weakened in wet weather, but is sometimes only barely audible in fine dry weather (*ibid.*).

The Medium of Sound

From the days of Aristotle, possibly earlier, it was believed that air is the usual medium through which sounds are propagated. The belief was generally accepted at the beginning of the modern period, though some thinkers regarded certain parts of the air, rather than the air in its entirety, as the medium concerned. Thus Gassendi, for example, allocated this function to special atoms, while Derham considered it an open question whether it was air itself or certain aethereal or even material particles in it that conveyed sounds. Mairan (1719) even went so far as to suggest that sounds of different pitch are severally conveyed by air-particles of corresponding elasticity, otherwise he could not understand how the same mass of air can transmit at the same time so many sounds of different pitch (*Mém. de l'Acad. des Sciences*, 1737). In any case, the whole question of the function of the air in connection with the transmission of sound could only be a matter of speculation until the invention of the air-pump. With the coming of this instrument experiments could be, and were, instituted.



Illustr. 164.—Air as the Medium of Sound

Guericke, the inventor of the air-pump, was naturally the first to carry out experiments concerning the relation of air to our perception of sounds. In the receiver of his air-pump he suspended, by means of a thread, a bell with a clock-work arrangement to make it strike. He noted that as the air was being exhausted from the receiver, the sound of the bell became weaker and weaker. Similar experiments were carried out by Boyle, and after him by Papin. Beginning with an evacuated receiver containing a bell or a whistle the sound of which was inaudible at first, air was admitted into the receiver very gradually by means of a tube, or through the aperture of the whistle, and the sound became more and more audible (see Illustr. 164).

In 1705 Hauksbee repeated these experiments with an ingenious improvement. A small sphere containing air and a bell was placed inside a larger sphere. The inner sphere communicated with the outer air by means of an open tube. The space between the two spheres was evacuated by means of an air-pump. When the communicating tube was closed the sound of the bell was barely heard; but when the tube was open the bell was heard clearly. In another experiment he placed a bell in a glass flask containing air at atmospheric pressure. The sound of the bell could then be heard distinctly up to a distance of about thirty yards. When the air in the flask was compressed to two atmospheres, the bell was heard distinctly up to a distance of about sixty yards. And when the air in the flask was compressed to three atmospheres, the bell could be heard distinctly up to a distance of about ninety yards (*Phil. Trans.*, Vol. XXIV, No. 297). Priestley and Perolle showed subsequently that other gases than air can serve as media for the transmission of sound.

Guericke, who was the first to prove experimentally the propagation of sound through air, also discovered that sounds are transmitted likewise through water and even through solids. His evidence relating to water as a medium of sound was not conclusive. He relied on the fact that fish can be taught to come to be fed at the sound of a bell. The question whether such fish are lured by sound or sight is still under dispute. Hauksbee's experiments furnished better evidence of the propagation of sound through water. A glass flask containing air and a bell was lowered into water by a string. When the bell sounded it could be heard very clearly, though the sound seemed more harsh and rough than usual. Still better evidence was supplied by Arderon in 1748. With the help of divers he ascertained that all kinds of sounds could be heard under the water. No attempt seems to have been made up to that time to determine the velocity of sound through water. The case of solid media was no better. Hooke had, indeed, tried some experiments with long taut strings; but he arrived at the erroneous conclusion that the transmission of sound through solids is instantaneous.

(See books on Physics on p. 274.)

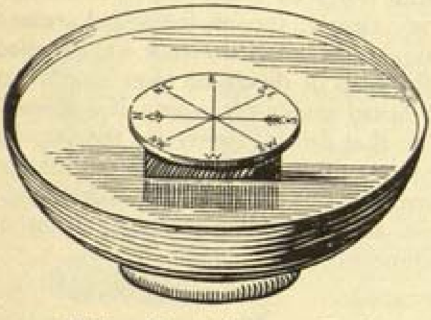
CHAPTER XIII

PHYSICS

IV. MAGNETISM AND ELECTRICITY

ANTECEDENTS

A magnetic oxide of iron occurs in a natural state in various parts of the world, and numerous references to the properties of lodestones are to be found in Greek and Latin literature. From these notices it is evident that the ancients were familiar with the lodestone's power of attracting or repelling pieces of iron, and of communicating to them properties similar to its own. The Chinese are said further to have been acquainted from an early date with the magnet's property of pointing north and south when freely suspended; but this important application was apparently unknown in the West, until the twelfth century, when references to the mariner's compass as a novel instrument of navigation begin to appear in European literature. It is uncertain whether the instrument was introduced from the East by the Arabs or by European sailors, or whether it was independently discovered. Writers of the thirteenth and succeeding centuries showed considerable interest in the properties of the compass-needle, which they variously supposed to point to the Great Bear, to the Pole Star, to some mysterious mountain, and so forth. Early forms of the instrument were mainly water-compasses in which a magnetized piece of iron was floated on wood in a vessel of water, the direction in which it set being noted. Sometimes a magnetized iron float was used. Later came the pivoted needle, and the compass-card—a light disc surmounting the needle and divided into thirty-two equal "points," the true north being denoted by a *fleur-de-lys*. These instruments were enclosed in wooden bowls covered with glass, and were so mounted as to be practically unaffected by the ship's motion.



Illustr. 165.—A Compass Card

The earliest known account of a careful experimental investigation of the properties of the lodestone is found in the manuscript *Epistola de magnete* of the Picard Petrus Peregrinus, dated 1269 (English translation, *Epistle Concerning the Magnet*, by Silvanus P. Thompson, London, 1902). Peregrinus experimented with a globular lodestone; he located the two poles where the magnetic virtue is especially strong, and recognized their distinctive north-seeking and south-seeking tendencies; he showed that like poles repel, and unlike poles attract, each other, that the polarity of a stone can be reversed by forcing two like poles together, and that the result of breaking a lodestone is to form two magnets instead of one. He saw an analogy between the influence of the globular lodestone upon an exploratory needle, held near it, and the supposed influence of the celestial sphere upon the compass-needle—a step towards the more valuable analogy of Gilbert.

During the fifteenth century it was discovered (when and by whom is uncertain) that the compass-needle does not, in general, point due north, but is inclined to the astronomical meridian at a small angle which (as Columbus discovered on his voyage of 1492) varies from place to place and occasionally vanishes. This magnetic *declination*, or *variation of the compass*, was measured by sailors in various parts of the world during the sixteenth century. Many of the early determinations must have been very crude, and were often obtained merely by looking towards the Pole Star along a compass-needle and noting the deviation. Gilbert, in his *De magnete* (IV, 12), describes several more refined contemporary methods of determining the variation. In one of these an instrument was employed consisting of a compass-needle for giving the magnetic meridian and an upright style for casting a shadow whose length and direction enabled the Sun's altitude and azimuth from the *magnetic* meridian at any instant to be ascertained. Two such azimuths were taken when the Sun was at equal altitudes before and after noon, and hence equidistant from the *geographical* meridian. Half the difference of the azimuths in the two cases then gave the variation of the compass. In another instrument, more particularly for use at sea, the angular distance of the rising point of the Sun, or of a known star, from the magnetic north was measured and compared with the computed distance from the true north, the variation being immediately deduced.

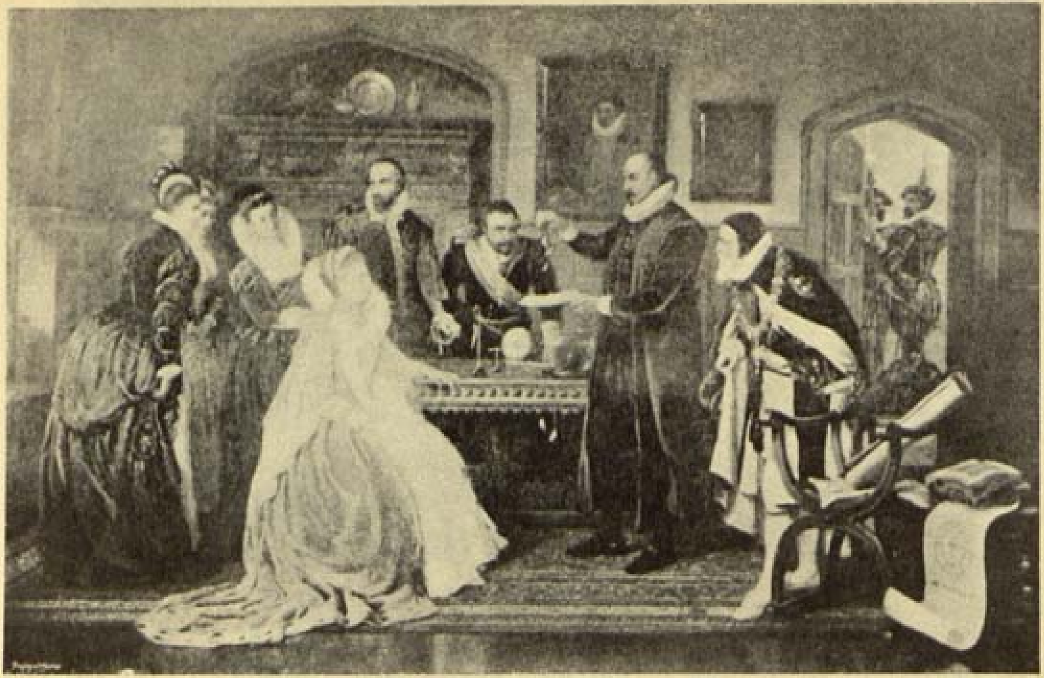
These measurements had a direct practical motive, as it was clearly desirable for the seaman to know, and allow for, the variation in whatever part of the world he might happen to be. But they were also pursued in the hope that they might afford a solution of the problem of determining longitude at sea, which continued to perplex navigators down to the eighteenth century. The position of the observer on the Earth is ordinarily defined by reference to the meridians of longitude and the parallels of latitude; but any other two intersecting systems of curves would serve the purpose. Suppose now that the magnetic declination varied over the Earth's surface in such a manner that the lines drawn through sets of points where it had equal values (*isogonics*) formed on the map a family of closed curves intersecting the parallels of latitude. Then an observer, by determining the magnetic declination and the latitude, could, in general, define his position as the intersection of two loci (viz. an isogonic and a parallel of latitude), and could thus ascertain his longitude indirectly. It was hoped at first that the isogonics would form a regular pattern which a few scattered observations would suffice to reveal, and several charts were constructed on these lines. But the magnetic exploration of the sixteenth century showed that the distribution was irregular, and a reason for this was assigned by Gilbert. About 1620 an attempt was made by a Milanese Jesuit, Borri, to base an isogonic chart of the Atlantic and Indian Oceans upon observation, by joining all the places which had given variations by a series of curves (see A. Kircher, *Magnes*, 1641, p. 503). But soon after this it was discovered that even such a chart could not be of permanent value, since the variation was everywhere slowly changing with lapse of time.

Further attempts were made later in the seventeenth century, as we shall see, to determine longitude by means of another variable magnetic element—the *dip* or *inclination* to the horizontal of a magnetized needle freely suspended at its centre of gravity. This phenomenon appears to have been discovered in 1544 by Georg Hartmann, a German clergyman, but his observation was inexact, and his account of it long remained inaccessible (see Hellmann's *Neudrucke*, No. 10). Hence the phenomenon first became generally known through its independent discoverer, Robert Norman, a Wapping compass-maker, who measured the dip in London in 1576 with a dipping-needle of his own manufacture, and gave it as 71° 50′. Norman's book, *The newe Attractive* (1581), was the first printed book entirely devoted to terrestrial magnetism. It contained a suggestion that the "poynt Respective" towards which the needle turned lay within the Earth, and might be found by observing the directions of the needle at various places: these directions would all meet at the required point. This important notion that the centre of attraction of the needle was situated in the Earth, and not in the heavens or in some fabulous mountain, seems also to have been taught by Gerhard Mercator, and was an important step towards the synthesis of Gilbert.

Illustr. 166

William Gilbert

Illustr. 167

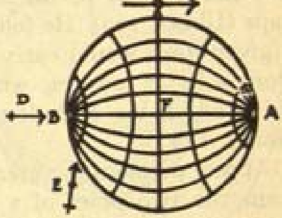


Queen Elizabeth watching Gilbert's Experiments

Gilbert of Colchester

The modern development of the sciences of magnetism and electricity owes very much to the work of William Gilbert, the greatest English experimentalist of his time. Gilbert was born at Colchester in 1540 (according to some authorities, in 1544), and lived from 1573 in London, where he practised as a doctor. He became physician to Queen Elizabeth, and died in the year of her death (1603).

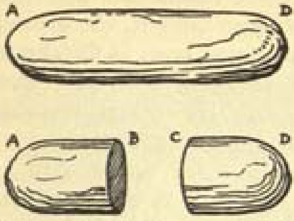
Gilbert set forth the results of seventeen years' research in his great book, *De magnete, magneticisque corporibus, et de magno magnete tellure; Physiologia Nova* (London, 1600)—*On the Magnet, Magnetic Bodies Also, and on the Great Magnet the Earth, a new Physiology* (English version by Silvanus P. Thompson, London, 1900). This work deals primarily with magnetism, only one chapter being devoted to electricity. It is characterized almost throughout by its reliance upon the results of experiment, in accordance with the teachings of Francis Bacon, and in contrast to the practice of Porta and of other earlier writers of works on the subject.



Illustr. 168.—Globular Lodestone (or Terrella) with Versoria

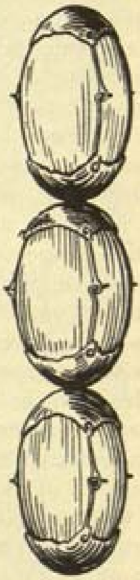
Gilbert begins his work by reviewing previous works on the subject and refuting the old wives' tales which they relate of alleged miraculous and curative properties of the lodestone (as, for instance, that a lodestone loses its virtue when smeared with garlic, but regains it when bathed in goat's blood, and so forth). He next describes the occurrence and appearance of the lodestone in its several species. He shows how to determine its poles, using for this purpose a powerful globular lodestone of convenient size and a short piece of iron wire, or a *versorium* consisting of a minute compass-needle on a pivot. The wire, or the *versorium*, was laid upon the surface of the globular stone, and the direction in which it set itself was marked on the stone by a chalk line which was produced to form a great circle. The wire was then placed at another point and another great circle obtained, and so on. All these circles were found to pass approximately through two diametrically opposite points on the stone: these were the two magnetic poles (A and B in Illustr. 168), whose properties form the principal topic of Book I of the *De magnete*. Although many of the topics here discussed had been familiar to Gilbert's predecessors, they had never before been set out in such clear scientific language.

In order to investigate the effect of dividing a magnetic body, Gilbert took an elongated lodestone AD (Illustr. 169) having its north pole at A and its south pole at D, and cut it into two equal parts. Upon allowing the parts to float in wooden vessels upon water, he found that, while A and D retained their original polarities, a new south pole had appeared at B and a new north pole at C, so that there were now two magnets in place of one. Discussing the behaviour of such floating magnets, Gilbert notes that the Earth orientates the magnet, but does not displace it as a whole (as had already been recognized by Norman). Gilbert increased the effectiveness of his lodestones by "arming" them with steel caps (Illustr. 170). He found that the maximum load of iron which a given stone would carry was thus increased from 4 to 12 ounces, while it was possible to form chains of lodestones, as shown in Illustr. 170.



Illustr. 169.—An Elongated Lodestone Divided in Halves

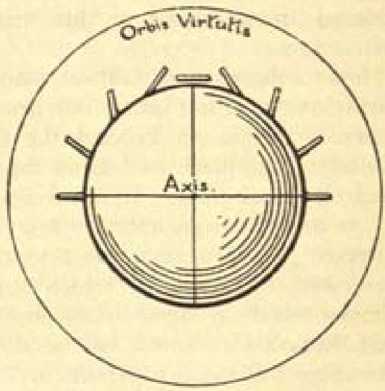
At any point on the great circle equidistant from the two poles of a globular lodestone (*magnetic equator*) the needle of the *versorium* was found to lie parallel to the surface of the stone, while at the poles it set itself perpendicular to the surface. Further, upon moving the needle about over the stone, Gilbert found that its inclination to the surface varied according to its distance from the poles in a manner which recalled the behaviour of the dipping-needle in different terrestrial latitudes (see Illustr. 171). He was thus led to conceive the Earth as a huge spherical lodestone of which the globular stone of his experiments was a miniature (and thence called a *terrella*, a miniature Earth). From the behaviour of the needle near the pole of the *terrella*, Gilbert concluded that the dip in the northern regions of the Earth would be greater than in London. This surmise was later confirmed by Hudson during a voyage of exploration in the Arctic regions of America. Hudson found, in fact, in 1608, that even in latitude 75° N. the dipping-needle assumed an almost vertical position. This result did not quite



Illustr. 170.—"Armed" Lodestones

correspond to the ideas of Gilbert, who supposed the magnetic and geographical poles to coincide, but it has been borne out by subsequent magnetic surveys.

The analogy between the Earth and his *terrella* led Gilbert to a mistaken explanation of the phenomenon of magnetic declination, or variation of the compass, which he develops in Book IV. Using an unevenly shaped *terrella*, he found that the direction of the *versorium* was affected by the projections and depressions on the surface. He accordingly supposed that, while the magnetic and geographical poles of the Earth coincide, the variation of the compass arises from the local irregularities of the Earth's surface, the needle being deflected towards continental masses and away from marine basins, since water is non-magnetic. Diversities in the composition of the Earth in different regions (e.g. the presence of magnetic iron deposits) also play their part, he supposed, in disturbing the needle. This explanation led Gilbert to suppose that the variation at any one place on the Earth would remain constant to all time, in the absence of any great geographical changes; and he gives a rough survey of the variation in the known parts of the world as it had been recorded by sailors, chiefly Portuguese. These scanty data tended to bear out his hypothesis, which, however, was overthrown when fuller information was obtained. In the meantime his work served to dispel the notion that the variation is simply related to the longitude.



Illustr. 171.—The Reaction of Small Magnets to a Terrella

Turning next, in Book V, to the phenomenon of dip, Gilbert studies in greater detail the manner in which this quantity varies in relation to magnetic latitude on the *terrella*, gives rules for determining latitude from observations of the dip, and describes an improved form of dipping-needle.

Gilbert supposed that, just as a magnet is surrounded by a sort of atmosphere through which its power is diffused, so the Earth's magnetic virtue may be imagined to extend into the surrounding space. "From about a magnetical body the virtue magnetical is poured out on every side around in an orbe" (II, 7). He was easily led on to the idea that the heavenly bodies (and the Sun and Moon in particular) are, like the Earth, endowed with magnetism. This notion was later taken up by Kepler and developed with a view to explaining the motions of the planets. In his concluding (sixth) book, Gilbert argues in favour of the Copernican hypothesis of the solar system. He justifies the Earth's motion on teleological grounds, and attributes it rather vaguely to magnetic virtue. "In order that the Earth may not perish in various ways, and be brought to confusion, she turns herself about by magnetic and primary virtue" (VI, 4). This portion of the book is mainly scholastic in tone.

Gilbert's theory of the nature of magnetism may best be considered in antithesis to his views on the cause of electrical attractions.

Up to the time of Gilbert, knowledge of electrical phenomena (which were of less immediate practical utility than magnetic) had scarcely progressed beyond the few facts described by classical writers; and there had been much confusion between magnetic and electrical effects. It was known that amber, and perhaps one or two other substances, when rubbed, acquire the power of attracting light bodies. Naturalists knew about the torpedo-fish, described by Aristotle, which stupefies its prey by the electric shocks which it administers. Sailors were acquainted with the "St. Elmo's Fire," and, of course, lightning was familiar and was the subject of much superstition. These several electrical manifestations were not definitely co-ordinated until the eighteenth century. It fell to Gilbert, however, to show that the property of amber is shared by numerous other substances, and so to establish the science of frictional electricity.

In one of the chapters of his book (II, 2) Gilbert describes his experiments on electrical attraction. He made himself a *versorium*, or electroscope, consisting of a metal pointer three or four digits long, turning easily about its centre upon a pointed support. He took each of the substances under investigation one by one and, having rubbed it, brought it up to the *versorium* and noted if the nearer end of the latter were attracted towards the substance. He found that many other substances besides amber produced a deflection of the pointer, and, in most cases, attracted all manner of other bodies. These attracting substances, or "electrics," included gems (such as diamond and sapphire), glass, sulphur, spar, crystals, resin, etc., as well as certain liquids. The metals were a notable exception, and formed the principal item in Gilbert's list of "non-electrics." Gilbert noticed that his experiments succeeded best when the air was dry. He thought that substances were attractive or non-attractive according as they were predominantly aqueous or earthy in their composition; but we know now that conducting substances, such as metals, must necessarily have given negative results when examined by Gilbert's method, since, when held in the hand and rubbed, they must have lost their charge as fast as it was excited. As his *versorium* was never insulated and charged, Gilbert missed discovering electrical repulsion.

Gilbert clearly discriminated for the first time between electrical and magnetic attractions, though the distinction which he drew between the agencies respectively at work was somewhat vague and metaphysical. "Electrical motions become strong from matter, but magnetic from form chiefly. . . . Electrical motion is a motion of aggregation of matter; magnetical motion is one of disposition and conformation. The globe of the Earth is aggregated and coheres by itself electrically. The globe of the Earth is directed and turned magnetically" (II, 2). Thus electricity binds the particles of a body together, while magnetism gives the body a determinate shape and a tendency to rotate about an axis having a definite orientation.

Gilbert's explanations of electrical and magnetic attractions were conceived on traditional lines. He supposed that amber and other electrics, when excited, exhaled subtle *effluvia* which united any light body in the vicinity with the excited substance, thus making one body out of two, so that they moved towards each other as parts of a whole. "All electrical attraction occurs through an intervening humour" (or fluid). According to Gilbert, the air plays an analogous part in determining the fall of heavy bodies towards the centre of the Earth. This conception of *effluvia* as carriers of electrical effects was retained and developed into a scientific theory by the physicists of the eighteenth century, who occupied themselves especially with the study of frictional electricity. On the other hand, Gilbert did not attempt a physical explanation of magnetic phenomena, but likened magnetism to a soul. He considered that the lodestone, together with the Earth as a whole and the heavenly bodies, is endowed with life (V, 12). Magnets emit no *effluvia* and exert no violence upon one another, but move towards one another spontaneously.

This explanation enabled Gilbert in some measure to surmount the difficulty, already presenting itself, of explaining the mutual actions of bodies separated from one another by empty space. Here again Gilbert was followed by Kepler. The lack of clear theoretical conceptions, however, does not impair the value of experimental results such as we owe in abundance to Gilbert.

BARLOW

A younger contemporary of Gilbert was William Barlow or Barlowe (d. 1625), who became Archdeacon of Salisbury. Barlow devoted much attention to investigations in magnetism, and he was the author of a work on the subject, *Magneticall Advertisements* (London, 1613, 1616, 1618). He introduced improved methods of magnetizing and of suspending compass-needles, and he distinguished between the magnetic properties of iron and steel. Barlow corresponded with Gilbert, but the relation of the two men in respect of their discoveries is somewhat obscure.

From 1600 to the early years of the nineteenth century magnetic and electrical science developed along separate lines; and we shall first consider the growth of knowledge and theory concerning magnetism.

Magnetism II. The Seventeenth Century

KIRCHER AND CABEO

Of less account than Gilbert's book is the voluminous work entitled *Magnes, sive de arte magnetica* (Rome, 1641), by the learned German Jesuit, Athanasius Kircher (1601–80). He was a professor at Würtzburg, and ranks with Porta, Schwenter, and other men even less inspired with the modern spirit of inquiry. He was not a physicist such as Gilbert and Galilei, but describes scientific marvels and popular toys at great length. These include a species of telegraphy with the aid of magnetic needles. It is worth noting, however, that he sought to define the strength of a magnet by means of the balance. The magnet was suspended from one pan of the balance and was counterpoised by weights placed in the other. A piece of iron was then brought into contact with the magnet and the additional counterpoise necessary to break this contact was noted. Much of Kircher's book is taken up by his schemes for healing diseases and wounds with the aid of magnetism. This mediaeval form of therapy had been the principal theme of van Helmont's *De Magnetica* of 1621. Kircher attributed many phenomena of the animal world, such as the flight of birds, to magnetic agency, and he devotes a separate section of his book to the "magnetism" of love. The work concludes with the reflection that God is all Nature's magnet (*totius naturae magnes*).

Another book on magnetism by a learned Jesuit of the same period is the *Philosophia Magnetica* (Ferrara, 1629) of Niccolo Cabeo, or Cabeus, who gave much attention to cases of magnetization of iron such as are now attributed to the inductive action of the Earth's field.

DESCARTES

Theories of the *nature* of magnetism current during the first half of the seventeenth century were vague and mystical, and generally attributed intelligence to the magnet. The first scientific theory of magnetism was that put forward by Descartes in his *Principia Philosophiae* (1644). This theory formed part of his general system of vortices described elsewhere.

Descartes explains how, from each pole of a cosmic vortex, there must stream in towards the centre large particles shaped like screws whose threads turn in opposite ways according as a particle comes from one pole of the vortex or from the other. The particles from one pole enter the central star of the vortex and pass through it by pores shaped like the nuts of screws, turned in the sense necessary to give a free onward passage to the particles as they rotate with the vortex. Arrived at the opposite surface of a star, the stream of particles meets another stream coming from the opposite pole, whereupon they pass externally round the star. As many as possible re-enter the star and repeat the circuit, while the remainder are shed abroad. The particles coming from the other pole behave in a similar manner, so that the star is the centre of two contrary circulations of particles. This state of affairs persists, to some extent, even when the star has degenerated into a planet (the Earth, for instance). The only portion of the planet, however, in which the pores remain open is the massive interior layer, which is largely composed of lodestone or iron. Lodestones thus allow the particles to pass through them with the least interference of any substance, and they are orientated into the most advantageous positions for this purpose by the momentum of the streams of particles. Each lodestone, moreover, becomes the centre of a miniature circulation of particles whose course can be mapped out with the aid of iron filings. These particles tend also to enter any adjacent lodestone, and so, by driving away the air between them, to make the two stones move together. The two kinds of pores in iron, unlike those in lodestones, can easily be made to interchange their properties, the polarity of an iron magnet being thus readily reversible.

On these lines Descartes succeeded in explaining practically all the magnetic phenomena known in his time, and his theory, though full of arbitrary assumptions, was mechanically intelligible, and in some sense anticipated the modern conception of magnetic induction.

Descartes' ideas were taken up and expounded by his disciple Jacques Rohault (1671). They were extended to cover electrical phenomena, and continued to hold the field, with certain modifications, throughout the seventeenth century and the greater part of the eighteenth. It was, in fact, in this branch of science (which was largely neglected by Newton) that Descartes' authority lasted longest.

NEWTON

No serious attempt was made during the seventeenth century to ascertain the quantitative laws of magnetic force, which were not elucidated until the close of the eighteenth century. Newton, however, in his *Principia* (III, 6), mentions some rough observations which led him to the conclusion that the force of a magnet varied nearly as the inverse cube of the distance.

Terrestrial Magnetism

We turn next to the development of terrestrial magnetics in the seventeenth century.

COMPASS VARIATIONS

The first important advance upon Gilbert's work was the discovery that the variation of the compass at any place generally changes in course of time. Even in the sixteenth century the necessity for taking account of such changes seems to have been recognized by the Flemish compass-makers. For there is some evidence that, in the construction of their instruments, they allowed for a variation of 11¼° E. at the end of the fifteenth century, and for a variation of 6° E. at the end of the sixteenth century (see N. H. de V. Heathcote's article in *Science Progress*, No. 105, July 1932). The first explicit admission of the existence of such changes, however, was the result of a series of determinations of the variation in London. William Borough measured this quantity at Limehouse in 1580. Gunter, observing at the same place in 1622, obtained a result 5 degrees less than that of Borough, but does not seem to have drawn any conclusion. A similar diminution was noticed at Whitehall, which led Henry Gellibrand (1597–1637) and some friends, in 1634, to repeat Gunter's observation with his original compass-needle. The result showed a further diminution, and Gellibrand concluded that "the variation is accompanied with a variation" (see H. Gellibrand, *A Discourse Mathematical on the Variation of the Magneticall Needle, together with Its admirable Diminution lately discovered*, London, 1635; edited by G. Hellmann in his *Neudrucke*, No. 9). Gellibrand, however, adhered to Gilbert's explanation of the variation as due to the Earth's surface irregularities. This view was soon afterwards abandoned under the influence of Descartes' teachings. Descartes explained the variation of the compass as due to the disturbing action of magnetic iron deposits in the neighbourhood of the needle. Changes in the variation he attributed to the transport of iron from place to place and to the corruption of old iron deposits and the formation of new ones.

Diurnal and seasonal fluctuations in the variation of the compass were discovered during the eighteenth century.

Further observation cast doubt upon Gilbert's assumption that the Earth's magnetic poles coincided with its geographical poles, for it was found that the isoclinics (lines through places having equal dips) intersected the parallels of latitude. The question arose whether the *isoclinics* might not be sufficiently regular and permanent to serve for the determination of longitude in accordance with the principle already explained in connection with *isogonics*.

Henry Bond, a London "Teacher of Navigation," worked out a method on these lines in his book *The Longitude Found* (1676). He conveniently assumed that there were two magnetic poles, distinct from the geographical poles and situated at diametrically opposite points of a magnetic sphere closely surrounding the Earth. The dip at any place depended merely upon the distance from these poles, so that the isoclinics were small circles of the sphere, constantly inclined to the parallels of latitude. In order to account for the secular changes in the magnetic elements, Bond simply assumed that the magnetic poles fell behind the Earth somewhat in its diurnal motion, and so slowly described circles about the geographical poles. Bond regarded his system as divinely inspired, and treated any criticism as blasphemy. Nevertheless, it was severely handled by one Peter Blackborrow, who, in his counterblast, *The Longitude not Found* (1678), attacked Bond's arbitrary and fictitious assumptions. Bond drew up a table on the basis of his theory, predicting the values of the variation in London during the succeeding half-century, but this proved of little value. His work is of interest, however, for its influence upon Halley's speculations on terrestrial magnetism.

HALLEY

Halley showed but little interest in questions on the nature of magnetism, but sought to construct an hypothetical model which should account for the observed values of the variation of the compass and for the slow changes therein.

In the first of his two principal papers on the subject (*Phil. Trans.*, 1683), Halley drew up a synopsis of the more recent and trustworthy determinations of the variations in different parts of the world, giving the date when each was made. These data enabled him to prove the insufficiency of Bond's theory. He further pointed out that the variation, in general, changes slowly and continuously as we traverse the Earth's surface, the needle being deflected the same way over huge tracts of land or sea. This told against Descartes' theory that variation arises from masses of magnetic material in the immediate neighbourhood of the observer; though Halley admitted that local disturbances of the compass might arise in this way. But the data told equally against Gilbert's theory, for on the coast of Brazil the needle was deflected eastward, away from the continent and towards the ocean. Halley's paper accordingly concludes with a new hypothesis—that the Earth is a magnet with four poles, two in each hemisphere, and that these are in slow relative motion, as required to account for slow changes in the magnetic elements.

On such a view it was no longer possible to regard the Earth as an ordinary magnet, and in a later paper (*Phil. Trans.*, 1692) Halley put forward the hypothesis that the Earth was made up of an outer magnetic shell with two poles, and an inner magnetic nucleus, concentric with the shell and possessing two poles of its own. He supposed that the two magnetic axes were inclined to each other and to the Earth's axis of rotation, and that the space between the shell and the nucleus was filled with a fluid medium which he thought might be luminous so as to permit of life in the interior of the Earth. Relative motions of the poles were easily accounted for by supposing the shell and the nucleus to have slightly different periods of diurnal rotation. If this simple scheme were insufficient, it might be convenient to regard the Earth as a nest of concentric magnetic shells with independent axes and differing periods of rotation—a system which recalls the planetary spheres of Eudoxus, and foreshadows the modern Fourier series.

Halley appealed to sailors and others to record and send in as many observations as possible of the variation in different parts of the world in order that a complete theory might be framed. He was himself destined in large measure to supply this need, for in the course of his later voyages of exploration in the Atlantic (1698–1700) he collected many magnetic data. These he embodied in a chart, or, as appears from the researches of L. A. Bauer, in two different charts published between 1701 and 1705 (*Terrestrial Magnetism*, Vols. I and XVIII). They were constructed on Mercator's projection, then rather a novelty, and showed how the lines of equal variation were found to be distributed over the ocean for the year 1700. A number of revised editions of Halley's world-chart appeared during the eighteenth century, as well as other charts by independent investigators.

Halley was the first to recognize that there is a connection between the *aurora borealis* and the Earth's magnetism. He suggested, following Descartes, that the Earth must be the centre of circulating magnetic *effluvia* entering it at one pole and leaving it at the other. These *effluvia*, he supposed, might, under certain unknown conditions, become luminous, as electrified bodies sometimes do in the dark. He was inclined to identify these luminous *effluvia* with the fluid medium enclosed in the Earth between the magnetic shell and nucleus, which he thought might escape through pores in the Earth's crust. The prevalence of auroral displays in the extreme north might then, he thought, be referred to the comparative thinness of the crust in those regions consequent upon the Earth's oblate form.

Electricity in the Seventeenth Century

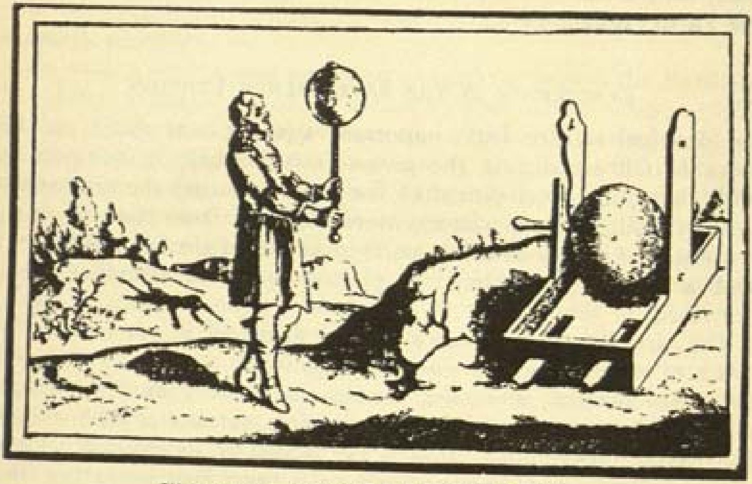
In electrical science little important advance was made on the work of Gilbert during the seventeenth century, in comparison with the rapid development of the subject during the eighteenth century. Gilbert's experiments were repeated from time to time, a number of additions being made to his list of electric substances; and a few isolated electrical phenomena were examined and described.

The *Accademia del Cimento* arranged electrical substances in the order of their attracting power when rubbed, amber heading the list. They noticed, moreover, that excited amber loses its charge when held near a flame; and they made a number of other minor discoveries. Newton, about 1675, performed an experiment with a piece of glass which he supported on a brass ring just above the level of the table. In the interspace of about an eighth of an inch between the table and the glass some fragments of paper were confined. Upon rubbing the glass vigorously, Newton noticed that the paper was set in agitated motion, being frequently attracted to the surface of the glass *opposite* to that which had been rubbed, and as often repelled. This experiment was repeated by the members of the Royal Society under Newton's instructions. In 1675, again, Jean Picard, the French astronomer, observed a luminous appearance in the vacuum of a barometer which he was carrying away from the Observatory at Paris—a phenomenon which later formed the starting-point of Hauksbee's electrical researches.

Perhaps the most significant discovery of the century following Gilbert was that of electrical repulsion. This effect seems first to have been noticed incidentally by Cabeus, who, in his *Philosophia Magnetica* (1629), describes how filings attracted by excited amber sometimes recoiled to a distance of several inches after making contact (*op. cit.*, II, 21). This phenomenon was first clearly recognized and described, however, by Otto von Guericke, the inventor of the air-pump, who also constructed the earliest mechanical contrivance for generating electrical charges.

GUERICKE

Guericke describes his frictional electrical machine with his other experiments, in Book IV of his *Experimenta Nova (ut vocantur) Magdeburgica de Vacuo Spatio* (1672), from which Illustr. 172 is taken. Guericke filled a glass globe, as large as a child's head, with molten



Illustr. 172.—Guericke's Electrical Machine

sulphur. When this had cooled, he broke the glass and mounted the sulphur sphere so obtained on an iron axle which rested on two supports, so that the sphere could be set in rotation. The dry hand was then applied to the sulphur and served as a rubber, but there was no conductor. The excited sphere attracted paper, feathers, and other light objects, and carried them round with it—a process to which Guericke likened the similar action of the Earth upon objects upon its surface. Drops of water brought into the neighbourhood of the sphere were agitated, while luminosity and a crackling sound were noticed when the finger was brought near.

It was while working with this machine that Guericke noticed the repulsion of similarly electrified bodies. He observed that a body which had been first attracted and then repelled from the sulphur sphere was attracted by other bodies, and that it was again attracted to the sulphur after having come into contact with the finger or with the ground, or after having been brought near a flame. Thus a feather placed between the electrified sphere and the floor jumped up and down from one to the other. Guericke also proved that an electric charge could travel to the extremity of a linen thread, and he further noticed that bodies became electrified even when merely brought near to the rubbed sulphur sphere. He was thus a pioneer in the discovery of electrical conduction and induction. Unfortunately, Guericke's work in this field did not attract the popular attention which his pneumatic experiments received, and even those who ought to have appreciated his discoveries allowed them to be forgotten.

Guericke's book, however, inspired Robert Boyle to perform a number of experiments in electricity and magnetism, though he did not reach any results of major importance. Boyle speculated extensively about the nature of electricity. Theories on this subject current in the seventeenth century usually attributed electrical effects to the action of *effluvia* or Cartesian vortices. Boyle, for instance, supposed a glutinous *effluvium* to emanate from a charged body and to return thither carrying light objects along with it.

(See P. F. Mottelay, *Bibliographical History of Electricity and Magnetism*, 1922; E. Hoppe, *Geschichte der Elektrizität*, Leipzig, 1884; also the list of books on p. 274.)

CHAPTER XIV

METEOROLOGY

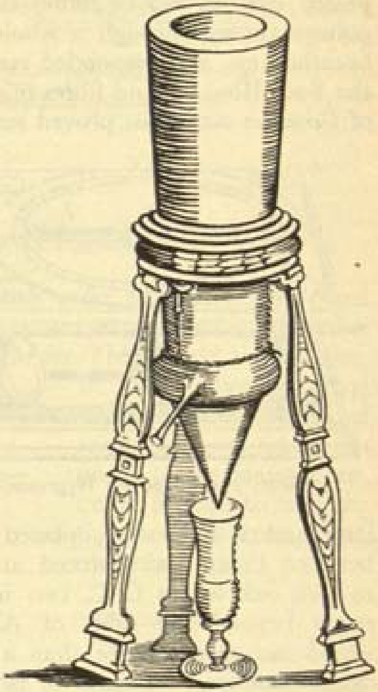
THE development of Physics and Mechanics in the seventeenth century led to a radical transformation of Meteorology. The rise of inductive methods of studying natural phenomena, and the invention of the thermometer, barometer, and other meteorological instruments, opened the way towards an exact study of the atmosphere in place of the astrological predictions or mere weather-lore which had been accepted as the basis for forecasts in the Middle Ages. Aristotle's *Meteorologica*, which had been a standard textbook at the Universities, yielded place to such works as Descartes' *Météores* (1637), which, though based upon an almost equally fictitious scheme of the universe, nevertheless did much to establish meteorology as a branch of physics.

METEOROLOGICAL INSTRUMENTS

Like other branches of science, meteorology depended for its progress to a large extent on the invention of suitable instruments for the measurement and record of the phenomena with which it is concerned. The most important meteorological instruments are undoubtedly the thermometer and the barometer. These instruments, however, are of such wide importance in physical science that they have already been dealt with in the chapter on Scientific Instruments (Chapter V). Here we shall confine ourselves to the description of the other instruments which are more specifically devised for the study of the conditions of the weather. These instruments include the hygroscope, the wind-gauge, the rain-gauge, and the weather-clock.

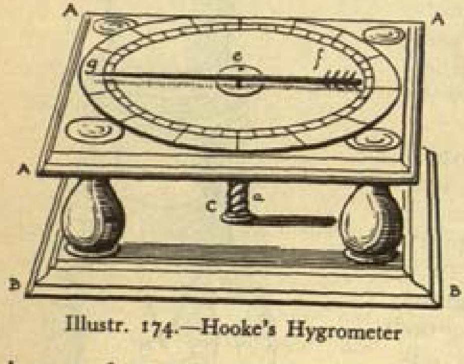
HYGROSCOPES

The seventeenth century saw the invention of numerous hygroscopes embodying most of the principles upon which such instruments can be constructed. The earliest hygrometer appears to have been made by the Accademia del Cimento, which, as we have already seen, also busied itself with the improvement of thermometers. This hygrometer consisted of a hollow cone of cork with an outer cover of tin. To the bottom of the cork cone a glass cone was attched (Illustr. 173). When the instrument was filled with ice, the moisture from the air was deposited on the glass cone, and ran into the measuring vessel. By comparing the quantities of water condensed, in a fixed period, the relative humidities of different places, or of the same place at different times, could be determined. In another hygrometer, described by Amontons in 1688, and later perfected by Deluc, the contraction or expansion of a small sphere of wood or leather, consequent upon changes in the humidity of the air, caused the rise or fall of a fluid in a tube issuing from it. Molyneux made a simple hygroscope by suspending from a whipcord a metal ball having a horizontal pointer which traversed a graduated scale as the cord wound and unwound with variations in the humidity, thus working on a similar principle to the popular "weather-house" or "Jocky and Jenny" of to-day.



Illustr. 173.—Hygroscope made by the Accademia del Cimento

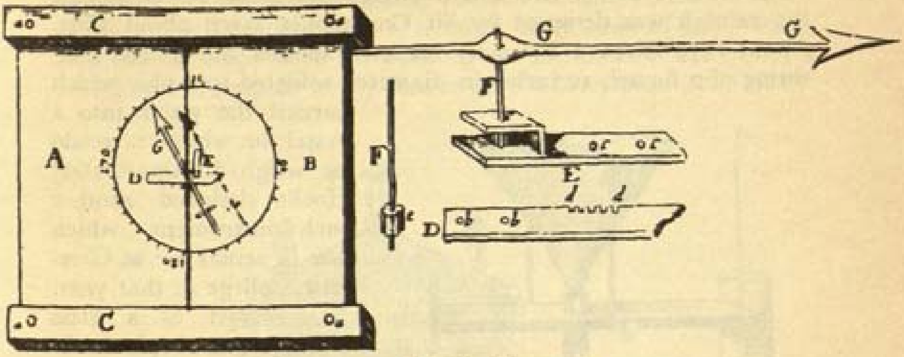
Hooke constructed a hygroscope out of the beard of a wild oat (*Micrographia*, Observation XXVII, p. 147, and Sprat's *History of the Royal Society*, p. 173). The "beard" is the bristle growing out of the husk covering the grain of the wild oat. It was noted that when the grain was ripe, this bristle was bent at one end through nearly a right angle, and that if it was then wetted the bent end would gradually turn round in relation to the rest of the bristle. Hooke, in his *Method for making a History of the Weather* (Sprat, *op. cit.*, p. 173), attributes to Emanuel Magnan the use of this property in the construction of a hygroscope for measuring the moisture of the air. Hooke suggests making the instrument in the form of a box with an ivory lid and basket-work sides, or preferably of an ivory plate merely supported on pillars. In this way the air would have free access to the beard, one end of which was fixed at C (Illustr. 174) to the base of the apparatus, while the other end passed up through the ivory plate and carried a light pointer *fg* attached to the beard at *e*, and travelling over a graduated dial. To increase the sensitiveness of the apparatus, a number of beards could be used, attached end to end. The pointer indicated changes in the humidity of the air by moving gradually over the scale as the bent end of the beard moved round. In order that account might be taken of complete revolutions of the pointer, Hooke suggested that a pin be attached to the lower side of the pointer, and that a light toothed wheel be mounted on the upper plate in such a way that the pin would move this wheel a tooth forward or backward every time the pointer passed over it. Hooke found this instrument very sensitive. The pointer turned through a whole revolution when the beard was breathed on, and responded readily to the heat of the fire or of the Sun. Hooke found fibres of gut less satisfactory, but the beard of *Geranium moschatum* proved superior even to wild oat, and later he substituted the "cod of a vetch" (Gunther, *Early Science in Oxford*, VI, p. 269).



Illustr. 174.—Hooke's Hygrometer

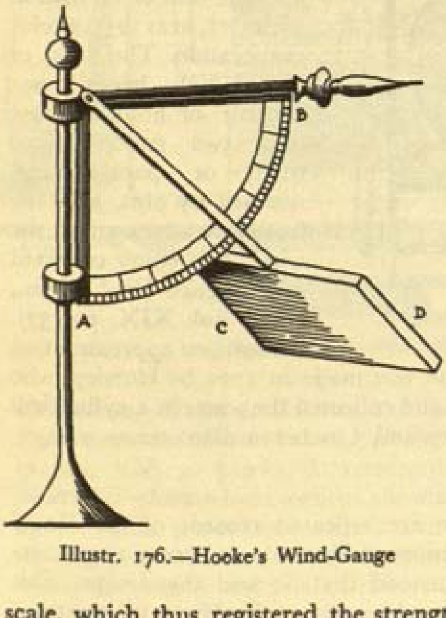
An ingenious form of hygroscope was described by a Dublin correspondent in a letter to Oldenburg (1676, *Phil. Trans.*, Vol. XI, No. 127). It consisted of two boards A, B (see Illustr. 175) of deal or poplar, about two feet long and a foot wide, placed side by side with a little space between them, and fastened at the four outer corners, *a*, *a*, *a*, *a*, to two oak ledges C, C, two inches wide, and long enough to reach beyond the sides of A, B. Assuming that the boards would not shrink more than a quarter of an inch even in the driest weather, a tongue of brass D, two or three inches long and a quarter of an inch wide, was fastened to the board A. The brass D had four equally spaced teeth, *d d*, near the free end which overlapped the board B, on which a pinion-wheel was mounted, by means of the fitting E, so that its teeth engaged those on the brass tongue. As the boards swelled or shrank with changes in the humidity of the atmosphere, the teeth *dd* turned the pinion-wheel, whose axle F carried a pointer GG round a circular scale, arbitrarily graduated to record "degrees of the drought or moisture of the air." With a possible shrinkage of about a fifth of an inch the pointer would move ten to twenty degrees in an hour or two. The inventor claimed that this hygroscope recorded rapidly the hygroscopic changes in the atmosphere, and was superior to those made with oat-beards.

The Dublin hygroscope, and several others, utilized the change of form which bodies undergo in moist air as an index to hygro-



Illustr. 175.—A Dublin Hygroscope

scopic changes. But there were also other hygroscopes invented which were based on the changes in weight of various substances when they absorbed moisture from the air. Thus Gould, for instance, in 1683, suggested that the increase in weight of sulphuric acid, when exposed to the atmosphere, might be used to measure the amount of atmospheric moisture.



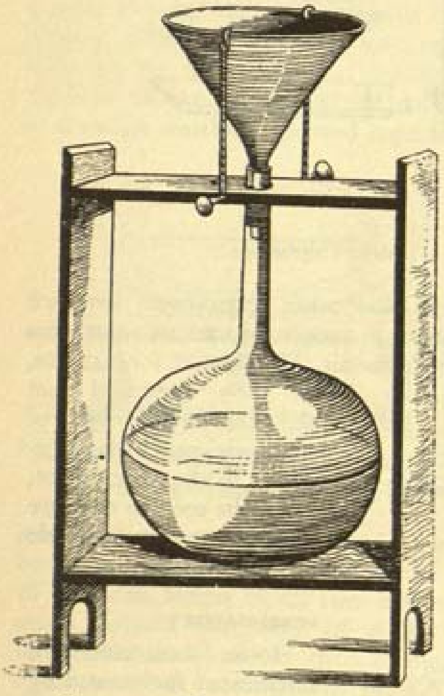
Illustr. 176.—Hooke's Wind-Gauge

WIND-GAUGE

Hooke constructed an instrument for measuring the strength as well as the direction of the wind. In this instrument (see Illustr. 176) a plate swung freely by an arm which moved over a graduated scale as the plate was blown aside by the wind. The stronger the wind the higher was the plate blown along the scale, which thus registered the strength of the wind. In principle this wind-gauge was essentially like the drogue used in a modern aerodrome.

RAIN-GAUGE

Rain gauges appear to have been in use in Korea as early as the fifteenth century. The earliest English instrument for measuring the rainfall was designed by Sir Christopher Wren about 1662. About 1677 Richard Towneley invented another rain-gauge, consisting of a funnel, 12 inches in diameter, soldered to a pipe which carried the water into a vessel in which it could be weighed. About 1695 Hooke designed another such instrument, which was in actual use at Gresham College in that year. It consisted of a glass funnel, about 11·4 inches in diameter (see Illustr. 177), mounted on a wooden frame, and leading into a larger flask, which had a narrow neck, 20 inches long and one-fifth of an inch in diameter, so as to minimize evaporation. The flask, or "large bolt head," was capable of holding more than two gallons. Two stays, or pack-threads, strained by pins, held the funnel steady against the wind. The water collected was weighed (*Phil. Trans.*, 1697, Vol. XIX, p. 357).

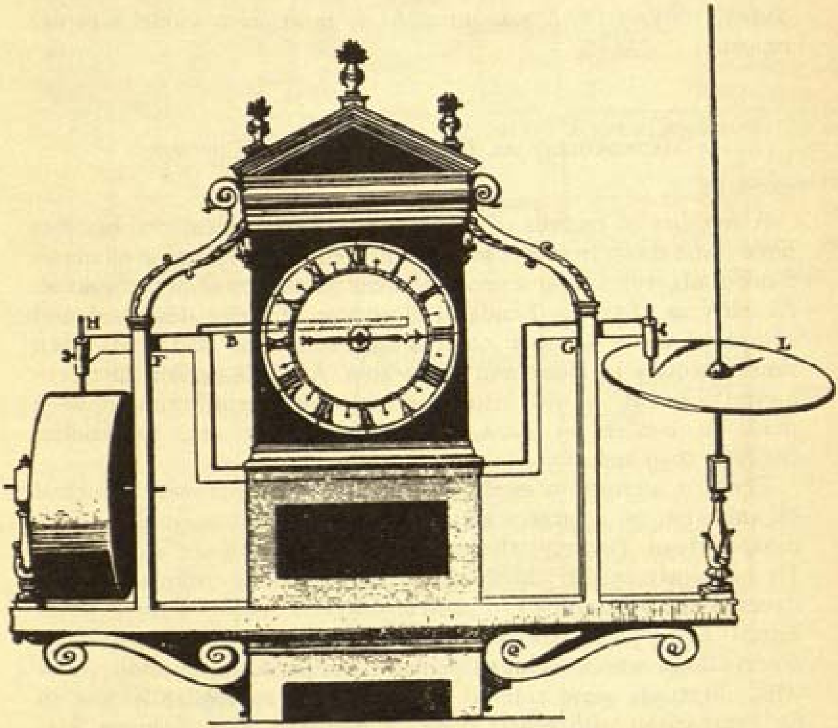


Illustr. 177.—Hooke's Rain-Gauge

The earliest approximation to the rain-gauges now in use was made in 1722 by Horsley, who used a funnel 30 inches wide, and collected the water in a cylindrical measuring glass 10 inches deep and 3 inches in diameter.

WEATHER-CLOCK

From 1664 onwards there are repeated records of the Royal Society's having given instructions to Hooke to construct a weather-clock, of which it was understood that he had the design. This was a development of some early attempts by Wren to construct such an instrument (Gunther, *Early Science in Oxford*, VI, p. 162). It was not until December 5, 1678, however, that Hooke produced part of the instrument, which was intended to measure and record the direction and strength of the wind, the temperature, pressure, and humidity of the atmosphere, and the rainfall. In the following January the President and a number of Fellows of the Society inspected and approved his work on the instrument, and nearly a year later (December 1679) Hooke gave a brief description of the



Illustr. 178.—Hooke's Weather-Clock

instrument, shown in Illustr. 178 (see W. Derham, *Philosophical Experiments and Observations of Hooke*, 1726, p. 41; and Gunther, *op. cit.*, VII, p. 519 f.). It consisted of two parts: (1) a strong pendulum-clock which, besides showing the time, turned a cylinder upon which paper was rolled, and operated a mechanism for making punches therein once every quarter of an hour; (2) instruments for measuring the phenomena enumerated above. These instruments (barometer, thermometer, hygroscope, rain-bucket, wind-vane, and wind-mill, whose revolutions were counted) moved punches, whose positions were periodically marked on a paper roll which was slowly wound on a cylinder. The mechanism, of which no detailed account seems to have been forthcoming, was sufficiently complicated, and it is not surprising that the instrument was soon reported to be in need of repairs.

Hooke also drew up "the Form of a Scheme" for recording the phenomena of the weather in an orderly way. The Table on p. 313 (*Phil. Trans.*, 1667, No. 24, p. 445; and Sprat's *History of the Royal Society*, 1667, p. 179) was intended to serve as a model weather report.

Meteorological Observations and Theories

RECORDS

A number of records of continuous meteorological observations have come down from the seventeenth century. The value of simultaneous observations at several different places was soon recognized. As early as 1637 the Landgraf Hermann of Hesse described and compared some weather observations which he had had taken simultaneously in Hesse and Pomerania. And the earliest meteorological observations with instruments, of which record remains, were made in concert at Paris, Clermont-Ferrand, and Stockholm, between 1649 and 1651.

The first attempt at establishing an international meteorological organization on a large scale was made by the Grand Duke Ferdinand II of Tuscany, the patron of the *Accademia del Cimento*. He had instruments (chiefly thermometers and hygrometers) constructed and sent abroad to chosen observers (many of them Jesuit priests) living at Paris, Warsaw, Innsbruck, and other places. Their observations, which included pressure, temperature, humidity, and wind direction, were entered on forms, and subsequently sent in for comparison with others made at Florence, Pisa, Bologna, etc. This activity ceased, however, with the closing of the *Accademia* in 1667. A subsequent scheme for founding such an organization in Germany resulted, in 1780, in the *Societas Meteorologica Palatina*.

In the meantime, observations of barometric pressure and weather conditions were made at Hanover, in 1678, and at Kiel, from 1679 to 1714, at the instigation of Leibniz. The observations were intended to test the capacity of the barometer for foretelling the weather, and also partly for the sake of Mariotte, who wished to compare observations taken throughout France with others made simultaneously in Germany. The barometers used by the German observers were of Hooke's type (wheel-barometers), and their scales were inscribed with weather indications. Mariotte's own

The *Form* of a *Scheme.*

Which at one view represents to the Eye Observations of the Weather, for a whole Month, may be such, as follows.

Days of the Moneth, and Place of the Sun	*Remarkable hours.*	*Age and Sign of the Moon at Noon.*	*The Quarters of the Wind, and its strength.*	*The Faces or visible appearances of the Sky.*	*The Notablest Effects*	*General Deductions.*
June 14	4	27	W - - - - 2	Clearblue, but yellowish in the *N E.*	A great Dew	These are to be made after the side is filled with Observations, as From the last Quarter of the Moon to the Change, the weather was very temperate, but for the Season, cold; the Wind pretty constant between *N.* and *W.*
	8		- - - - - - 3	Clouded toward the *South.*		
♊	12	♉ 9. 46	- - - - - - 3½		Thunder far to the S.	
	4	*Perigeum*	- - - - - -	Checkered blue.		
12.46′	8		WSW 1		A very great Tyde.	
	12		- - - - - -			&c.
15	8	28	N W 3	A clear sky all day, but a little check-er'd about 4 *P. M.* At Sun-set red and hazy.	Not by much so big a Tyde as yesterday. A great Thunder-Showre from the *N.*	
♊	4		4			
	6	♉ 24.51	N 2			
13.40′	12		1			
16	10	*New Moon at* 7. 25. *A. M.* ♊ 10.8	S 1	Overcast and very lowring, &c.	No dew upon the ground, but very much upon Marble-stones, &c.	
♊			&c.			
14.57		&c.				
&c.						

Illustr. 179.

Hooke's Form for a Weather-Report

special services to meteorology lay chiefly in the quantitative study of rainfall and its comparison with river drainage.

Johann Kanold of Breslau collected periodic observations from all over Germany and from other places, including London, and published them in a quarterly journal (*Breslauer Sammlung*) between 1717 and 1726. The value of these observations was impaired, however, through the instruments employed not being standardized.

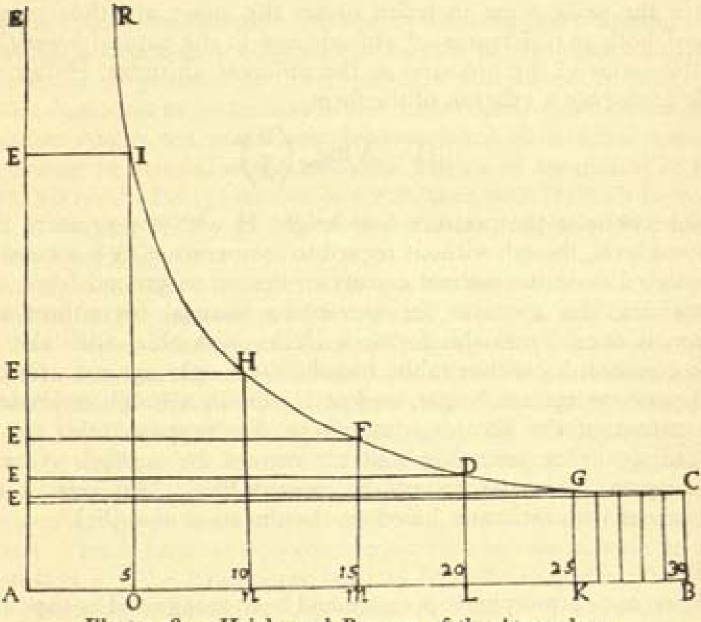
James Jurin, Secretary of the Royal Society, inserted in the *Philosophical Transactions* for 1723 (Vol. XXXII, No. 379) an invitation for meteorological observations to be sent in annually to the Society. Instructions were included as to the manner in which such observations should be made, and thereafter an increasing number of correspondents sent in observations as directed. (See G. Hellmann, *Beiträge zur Geschichte der Meteorologie*, Berlin, 1914, etc.)

HEIGHT OF THE ATMOSPHERE

Boyle's formulation of the law connecting the volume and pressure of a quantity of gas gave rise to a series of attempts to ascertain to what height the atmosphere extends above the Earth's surface, and how atmospheric pressure varies with the altitude.

Hooke, who had much to do with the experimental establishment of Boyle's Law, discussed the problem in his *Micrographia* (1665). He considers a vertical column of the atmosphere and divides it into 1,000 layers all containing equal quantities of air. He calculates from the density of the air at ground level that each of these layers must exert the same pressure as a 35-foot layer of air having that density, in order that the barometer should stand at its normal height. From Boyle's Law he calculates the thickness of each layer in order of ascent from the Earth up to the 999th layer. He does not, however, attempt to sum these thicknesses, and he recognizes that the 1,000th layer must be of infinite thickness. "Since we cannot yet find the *plus ultra*, beyond which the Air will not expand itself, we cannot determine the height of the Air" (*op. cit.*, p. 228). Hooke mentions having found the pressure at the top of St. Paul's steeple sensibly lighter than at the foot.

Mariotte, in his *Discours de la nature de l'air*, divides the height of the atmosphere into 4,032 parts corresponding to as many layers of equal weights, each of them represented by one-twelfth of a line of the normal barometric height of 28 inches. From experiments of his own he concludes that the thickness of the lowest of these layers is 5 feet. Hence, he argues, the thickness of the 2,016th layer above the Earth (under half the pressure at ground level) will be 10 feet. Mariotte knew that the intervening layers would increase in geometrical progression, but he assumes for simplicity that their average thickness is the arithmetic mean of 5 feet and 10 feet—i.e. $7\frac{1}{2}$ feet—giving the height of the lower half of the atmosphere as $7\frac{1}{2} \times 2{,}016$, or 15,120 feet. He similarly calculated the thickness of half the superior half of the atmosphere, again obtaining 15,120 feet. This process can, of course, be carried on indefinitely. Twelve successive applications of it give a height of about 35 miles. Mariotte adopted this as a lower limit to the height of the atmosphere, as



Illustr. 180.—Height and Pressure of the Atmosphere

he had no evidence that air could be expanded beyond the degree of rarefaction which it would have at this altitude.

Halley dealt more successfully with this problem in 1686 in a contribution to the *Philosophical Transactions* (Vol. XVI, No. 181). The basis of his method is the analogy between Boyle's Law connecting the pressure and the "expansion," or volume, of a quantity of gas, and the law connecting the co-ordinates of a point upon an hyperbola referred to its asymptotes. In the gas-law, volume varies inversely as pressure; and in the rectangular hyperbola RHDC (Illustr. 180) the ordinate varies inversely as the abscissa, i.e. AO : AN :: NH : OI. Hence, if AO, AN, AM represent pressures of a given quantity of air on a certain scale, OI, NH, and MF will represent the corresponding volumes on a certain scale. Halley shows, from the properties of the curve, that the difference between the altitudes to which correspond respectively the pressures AM and AL is proportional to the area MFDL bounded by the pressure-axis, the curve, and the ordinates through M and L. He further proves that

$$(\text{area MFDL}) : (\text{area NHFM}) = \log \frac{\text{AL}}{\text{AM}} : \log \frac{\text{AM}}{\text{AN}}.$$

Since the same areas included under the curve are thus proportional both to differences of altitude and to the natural logarithms of the ratios of the pressures at the different altitudes, Halley was able to deduce a relation of the form

$$\text{H} = \text{A} \log_{10} \left(\frac{\text{B}}{b}\right)$$

which connects the pressure b at height H with the pressure B at ground level, though without regard to temperature. A is a constant calculated from the normal density of the air at ground level, and containing the modulus for converting natural logarithms into common ones. From this formula Halley was able, with the aid of a common logarithm table, to tabulate height against pressure, and pressure against height, and so to obtain a rough estimate of the extent of the Earth's atmosphere. He supposed this not to exceed 45 miles, assuming that air cannot be rarefied to much above 3,000 times its volume at ground level. This was in fair agreement with estimates based on the duration of twilight.

WINDS

When once atmospheric pressure had been recognized as the cause of numerous physical phenomena, it became usual to attribute movements of the atmosphere to disturbances of the equilibrium governing this pressure. Torricelli appears to have been the first to attempt to explain air-currents on this physical principle. In one of his *Lezioni Accademiche* (Florence, 1715), he assumes that between regions of more rarefied air and other regions of denser air equalization takes place by means of a current which we perceive as a wind. He takes as an example of this equalization the cold wind which blows out through the doors of large churches on warm spring days—a phenomenon which occurs with especial frequency in Italy. "The air in large buildings," so his explanation runs, "is at this time appreciably cooler and heavier than the air in their neighbourhood. Therefore it flows out at the doors, as water would do if it had been confined in a building and then an opening had been suddenly made in the side" (Torricelli, *op. cit.*, p. 50).

At the instigation of Mariotte, simultaneous barometric observations were made at Paris, Dijon, Loches, and other places in France; and he tried to procure similar data from Germany for purposes of comparison. In his *Discours de la nature de l'air* he tries to correlate the indications of the barometer with the weather and the direction of the wind, and to explain the correlation with the aid of the rather crude *mechanical* hypotheses customary at that time.

Halley took considerable interest in contemporary attempts to establish a connection between the heights of mountains and the differences of pressure at their summits and bases. In 1697 he visited Snowdon to make barometric observations there on his own account. About ten years later Johann Jakob Scheuchzer used a barometer to determine the unknown heights of mountains in the Alps, his results being reduced in accordance with Halley's formula (see F. Cajori, *History of Determinations of the Heights of Mountains*, in *Isis*, Vol. XII).

Halley concludes his paper with speculations, similar to those of Mariotte, on the connection between wind, weather, and barometric pressure. Most of the speculations of the period on this subject were of little value, the relation of winds to the pressure-distribution being misunderstood. Thus Halley supposed high barometric pressure to be due to an accumulation of air set up by the confluence of two contrary winds, while low pressure is due to the partial exhaustion consequent upon the divergence of two air-currents. Rain goes with low pressure; and fair weather with cold, dense winds from north or east. This is because aqueous vapour falls as rain as soon as the lower layers of the atmosphere cease to be sufficiently dense to support it, on the Archimedean principle.

Having assigned to winds such an important rôle in determining weather conditions, it was natural that Halley should go on to inquire into their distribution and causation. Accordingly, his next contribution to the *Philosophical Transactions*, a paper on "Trade-Winds and Monsoons" (Vol. XVI, No. 183), presents a survey of the winds prevailing in the three great oceans, and is illustrated by a wind-chart. The survey marked an advance on that of Varenius (*Geographia Generalis*, Amstelodami, 1650), then accepted as the best; while the chart was actually the first meteorological chart to be produced. The essential features of the tropical wind-belt—the equatorial calms and the north-easterly and south-easterly trade-winds—are correctly described and depicted; while the seasonal fluctuations consequent upon the periodic changes in the Sun's declination are also noted in the text. Halley's account of Atlantic conditions is full, and rich in personal reminiscence. Of the Indian

Ocean he naturally knew less; and for the Pacific he was mainly dependent upon the vague reports of Spanish sailors.

Halley's theory of trade-winds owed something to previous seventeenth-century writers on the subject, and pointed the way to the accepted explanation provided by George Hadley in the eighteenth century. Halley attributes these phenomena to "the Action of the Sun's Beams upon the Air and Water, as he passes every day over the Oceans, considered together with the Nature of the Soyl, and Situation of the adjoyning Continents: I say therefore, first, that according to the Laws of Staticks, the Air, which is less rarified or expanded by heat, and consequently more ponderous, must have a Motion towards those parts thereof, which are more rarified, and less ponderous, to bring it to an Aequilibrium; and secondly, that the presence of the Sun continually shifting to the Westwards, that part towards which the Air tends, by reason of the Rarifaction made by his greatest Meridian Heat, is with him carried Westward, and consequently the tendency of the whole Body of the lower Air is that way. Thus a general Easterly Wind is formed, which being impressed upon all the Air of a vast Ocean, the parts impel one the other, and so keep moving till the next return of the Sun whereby so much of the Motion as was lost, is again restored, and thus the Easterly wind is made perpetual." That is, a trade-wind is the resultant of

(1) A convectional displacement of the air at ground level *towards* the heat-equator; and
(2) A westward flow of air following the subsolar point *along* the heat-equator.

This accounts for the trade-winds being generally inclined both to the meridians and to the parallels of latitude; while irregularities in their distribution are to be referred to the interposition of land-forms.

In connecting trade-winds with the heat of the Sun, Halley was following the ideas of Francis Bacon, who tentatively attributed the tropical *briza* to the expansion of the air due to solar heat (*Historia Naturalis et Experimentalis de Ventis*, 1638); and still more the ideas of Varenius, who supposed that the Sun, having rarefied the air in the torrid zone, thrusts it from east to west as he travels in that direction (Varenius, *op. cit.*, Cap. XXI). Halley's explanation, however, marks an advance on that of Galilei, who believed that the Earth's surface irregularities carry the lower layers of the atmosphere round in the daily rotation, except on the tropical oceans, where there are no such irregularities and where the Earth's eastward motion is swiftest, and where, consequently, a perpetual east wind blows (*Two Chief Systems*, Dialogue IV). But Mariotte came nearer the modern explanation than did Halley, when he attributed the *westerly* direction of the variable winds to the fact that the Earth's surface is moving eastward more rapidly at the equator than in higher latitudes, so that winds blowing northward from the tropics are deflected eastwards (*Traité du Mouvement des Eaux*).

Halley recognized that every current of air is only part of a complete circulation, so that "the North East Trade Wind below, will be attended with a South Westerly above, and the South Easterly with a North West Wind above." And this principle led him straight to his explanation of monsoons. "To the Northward of the Indian Ocean there is every where Land within the usual limit [of the trade-winds] of the latitude of 30°, viz. Arabia, Persia, India, etc., which . . . are subject to unsufferable heats when the Sun is to the North, passing nearly Vertical; but yet are temperate enough when the Sun is removed towards the other Tropick; because of a ridge of Mountains at some distance within the Land, said to be frequently in Winter covered with Snow, over which the Air, as it passes, must needs be much chilled. Hence it comes to pass, that the Air coming according to the general Rule, out of the N.E. in the Indian Seas, is sometimes hotter, sometimes colder, than that which by this Circulation is returned out of the S.W., and by consequence, sometimes the under Current or Wind is from the N.E., sometimes from the S.W." Halley thus regards monsoons as modifications of the local trade-winds rather than as independent convectional effects of the continents. But his explanation marks an advance upon prevailing views.

Hooke, too, speculated on the cause of trade-winds (*Posthumous Works*, 1705). He supposed that owing to the Earth's rotation the atmosphere exerted less pressure at the equator than in higher latitudes, tending rather to recede into space. Hence the natural tendency of air to move from regions of higher pressure to regions of lower pressure would cause winds to blow at ground level towards the equator, from which they returned poleward at a higher level, thus maintaining a constant circulation of air.

The writings of William Dampier, the buccaneer, extended the knowledge of the wind-distribution, but did not advance theory.

George Hadley, in his classic paper of 1735 on the trade-winds (*Phil. Trans.*, Vol. XXXIX, No. 437), agreed that the Sun, by rarefying the air at the equator, was the original cause of the trade-winds, but held that "the perpetual Motion of the Air towards the West, cannot be derived merely from the Action of the Sun upon it." His explanation is that "the Air, as it moves from the Tropicks towards the Equator, having a less Velocity than the Parts of the Earth it arrives at, will have a relative Motion contrary to that of the diurnal Motion of the Earth in those Parts, which being combined with the Motion towards the Equator, a north-east Wind will be produced on this Side of the Equator, and a south-east on the other," the winds being due east at the equator "by reason of the Concourse of both Currents from the North and South." Hadley explains the westerly winds in temperate latitudes on the same principle. Later developments in this realm are associated with the name of Buys-Ballot.

EVAPORATION

In 1687 Halley published the first of several papers dealing quantitatively with the evaporation which occurs at the surfaces of seas and lakes. The series opens with an account of an experiment designed to measure the rate of evaporation from water at summer heat, the numerical result being then applied to the economy of the Mediterranean Sea, including the Black Sea (*Phil. Trans.*, Vol. XVI, No. 189).

"We took a Pan of [salt] Water," Halley writes, "about 4 inches deep and $7\frac{9}{16}$ inches diameter, in which we placed a Thermometer, and by means of a Pan of Coals, we brought the Water to the same degree of heat which is observed to be that of the Air in our hottest Summers; the Thermometer nicely showing it. This done, we affixed the Pan of Water, with the Thermometer in it, to one end of the Beam of the Scales, and exactly counterpoised it with weights in the other Scale; and by the application or removal of the Pan of Coals, we found it very easie to maintain the Water in the same degree of Heat precisely."

From the dimensions of the vessel and the observed loss of weight suffered by the water, Halley calculated that the surface was lowered by evaporation at the rate of one-tenth of an inch in twelve hours. Hence, assuming the Mediterranean to be maintained by the Sun at summer heat for twelve hours a day, and so to yield each day one-tenth of an inch in vapour, Halley calculated its daily loss of water, which came to 5,280,000,000 tons.

How is this loss made good? Partly from the inflow of tributary rivers, whose contribution Halley estimates somewhat crudely as follows: He assumes that the nine important tributaries of the Mediterranean system each supply ten times as much water as the Thames. Observations of the breadth, depth, and rate of flow of the Thames at Kingston gave that river's daily output as 20,300,000 tons, and ninety times this quantity would yet be only about one-third of the daily loss of the Mediterranean. Part of the remaining two-thirds, Halley concludes, must be returned by dews at night and the balance supplied by the strong current flowing in at Gibraltar.

The above investigation may have been suggested to Halley by Perrault's and Mariotte's numerical comparison of the output of the Seine with the rainfall in its drainage area. (For Perrault see Chapter XVI, pp. 316 f.) In his *Traité du Mouvement des Eaux*, Mariotte contends that rainfall is sufficient to maintain rivers and streams in flow, and to prove his point he performs a calculation which is of some interest, though his data could not now be regarded as accurate. Observations with a rain-gauge on a roof at Dijon showed an annual rainfall of not less than 15 inches. Mariotte calculated that this would give a rainfall of 714,150,000,000 cubic feet of water over the basin of the Seine. He next estimated the cross-section of the Seine at the Pont-Royal, and also its rate of flow, which he obtained by noting the speed of a stick floating on the surface, and by making allowance for the slower rate of the under layers. He arrived at 105,120,000,000 cubic feet as the Seine's annual outflow, or less than one-sixth of what was supplied by rain. Hence, he argued, enough water would be available to supply the river even if one-third of the rainfall evaporated immediately after falling and one-half remained in the ground.

In the second paper of his series (*Phil. Trans.*, Vol. XVII, No. 192), Halley goes on to consider how the vapours drawn up from the sea eventually return thither; and his theory of the origin of springs once again recalls the ideas of Mariotte. But while the French physicist had believed springs to be maintained solely by intermittent rains, Halley ascribes them to the continual precipitation of vapours on high mountain ridges, where the chilled air was unable to contain so much vapour—an hypothesis suggested by the heavy dewfall which had inconvenienced him at St. Helena. Upon precipitation, the vapours "gleeting down by the Crannies of the stone; and part of the Vapour entering into the Caverns of the Hills, the Water thereof gathers as in an Alembick into the Basons of stone it finds, which being once filled, all the overplus of Water that comes thither runs over by the lowest place, and breaking out by the sides of the Hills, forms single Springs." These springs, uniting into rivers, bear the water once more back to the sea.

A further investigation of evaporation, carried out at Gresham College by Hunt under Halley's instructions, is described in the *Philosophical Transactions* for 1694 (Vol. XVIII, No. 212). Its object was to prove that Sun and wind must play an important part in the vaporization of water exposed to their action. The rate of evaporation from a screened and sheltered water surface at room temperature was found to be only a fraction of that observed in the previously described determination of 1687, and quite inadequate, in Halley's opinion, to account for the measured rainfall. The residual evaporation required to balance the rainfall was accordingly attributed to the action of the excluded factors, namely, the Sun's action in "agitating" the water particles and the wind's action in removing the vapour as it rises from the surface.

The last of this series of studies on evaporation appeared much later, in 1715, and is of great scientific interest (*Phil. Trans.*, Vol. XXIX, No. 344). The paper treats primarily of the economy of seas and lakes which have no outlet (e.g. the Caspian Sea, the Dead Sea, Lake Titicaca, etc.), and shows that their waters must swell until the loss by surface evaporation just balances the inflow. Remarking that such lakes are always *salt*, and that the ocean is, in some sense, a lake with no outlet, Halley unfolds his celebrated scheme for deducing the age of the Earth from the salinity of the ocean.

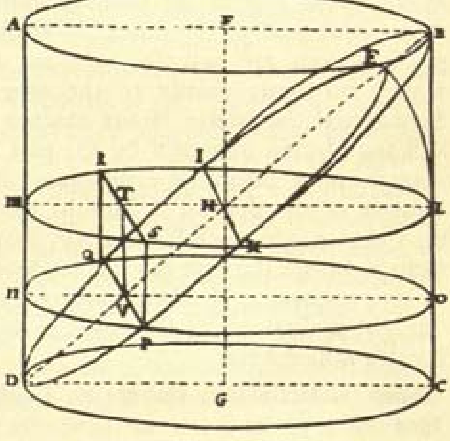
His method depends upon the consideration that, while the rivers are daily carrying dissolved salt to the ocean, the vapour exhaled from the ocean consists entirely of fresh water. Hence the salinity of the seas must be steadily increasing. (These ideas were in marked contrast to the accepted views of Varenius according to whom the salt in the ocean comes from submerged salt-rocks, the inflow of rivers tending only to *freshen* the water.—*Geographia Generalis*, 1650, Cap. XV). Now, if the salinity of the ocean could be ascertained at two epochs separated by several centuries, and the increase during that interval noted, then a proportion sum would give the length of time during which the rivers must have been contributing salt to the ocean. This method, however, is subject to the uncertainty (which Halley recognizes) that the oceans may have contained an original supply of salt when first they were formed. We nowadays have to admit the even more serious objection that the rate of supply of river salt may not have been constant throughout the past.

Halley was aware, moreover, that he could not hope to put his proposal into effect himself, for the increase in the sea's salinity must be very slow, and there were no ancient records of its value with which to compare contemporary measurements. With his customary foresight for future generations, he writes: "I recommend it therefore to the Society, as opportunity shall offer, to procure the Experiments to be made of the present degree of Saltness of the Ocean, and of as many of these Lakes as can be come at, that they may stand upon Record for the benefit of future ages."

Although there were then no data for a numerical estimate, it was clear enough that the application of this method would allot a respectable antiquity to the Earth. Hence Halley, whose religious opinions were already suspect, had to be on his guard against ecclesiastical censure. He therefore represents himself as anxious to confute the Lucretian doctrine (just then finding some favour) that the Earth has existed *for ever*, by proving that its age must lie within a finite limit. He further argues that the formation of *man* six or seven thousand years ago has no bearing on the possible age of the *Earth*. For man was created last of all on the sixth day; and the "days" in question could not have been natural days, seeing that the Sun was not created until the fourth "day."

DISTRIBUTION OF SOLAR RADIATION

While studying the part played by the daily warmth of the Sun in maintaining the circulation of aqueous vapour, Halley, in 1693, devised a method of calculating the proportions in which that warmth is shared among the various latitudes of the Earth at each season of the year (*Phil. Trans.*, Vol. XVII, No. 203). He knew that, apart from atmospheric interference, any place exposed to sunshine receives heat at a rate directly proportional to the sine of the Sun's altitude at the time of observation. The Sun's altitude, however, undergoes continuous change, in general, throughout the day; and Halley's method of summing its variable heating effect is equivalent to taking a time-integral of the sines of its altitude from rising to setting: "the time of the continuance of the Sun's shining being taken for a Basis, and the Sines of the Sun's Altitudes erected thereon as Perpendiculars, and a Curve drawn through the Extremities of those Perpendiculars, the Area comprehended shall be proportionate to the Collection of the Heat of all the Beams of the Sun in that space of time."



Illustr. 181.—Distribution of Solar Radiation

To construct such a curve and to measure its area, for any given values of the latitude and of the Sun's declination, constituted a difficult problem. Halley succceeded in proving, by an involved geometrical process, that the area required is analogous to that of the curved surface (QBPO in Illustr. 181) intercepted between a right section (NPOQ) and an oblique section (DPBQ) of a right circular cylinder ABCD. Using certain theorems of Archimedes, it is possible to evaluate this area (in terms of PQ, BC, BO, and the arc QOP); and it provides a measure of the Sun's heating effect when BO is put equal to the sine of the Sun's meridian altitude and the arc QOP equal to his diurnal arc above the horizon, for the given values of the latitude and declination.

Halley was thus able to construct a table giving the proportion of the diurnal heat received in every tenth degree of latitude, at the equinox and at each solstice. Thus, taking the equinoctial heat collected at the equator as 20,000, the heat in latitude 50° N. is found to be 12,855 at an equinox; 22,991 at the summer solstice; 3,798 at the winter solstice. We also obtain the somewhat unexpected result that (neglecting atmospheric factors) the heat collected at the north pole at the summer solstice appreciably exceeds the equinoctial heat at the equator—25,055 against 20,000. That the polar regions are actually so much colder than the equatorial Halley attributes chiefly to the long polar night: "Under the Equinoctial the twelve Hours absence of the Sun does very little still the Motion impress'd by the past Action of his Rays wherein Heat consists, before he arise again: But under the Pole the long absence of the Sun for 6 Months wherein the extremity of Cold does obtain, has so chill'd the Air, that it is as it were frozen, and cannot, before the Sun has got far towards it, be any way sensible of his presence. . . ." Halley misses the important point that polar sunlight is dimmed by traversing a thicker absorptive layer of the Earth's atmosphere.

(See G. Hellmann, *Beiträge zur Geschichte der Meteorologie*, Berlin, 1914, etc.)

CHAPTER XV

CHEMISTRY

AT the beginning of the modern period chemical investigation followed three main tendencies. First, there was still prevalent the alchemical search for the philosopher's stone or some other means for transmuting base metals into gold. Secondly, there was the tendency to turn chemical knowledge to medicinal uses. This movement, known as iatro-chemistry or medicinal chemistry, was not altogether dissociated from the alchemists' search for the elixir of life and for a panacea, which were respectively to prolong life indefinitely and serve as a cure for all its ills. Moreover, the ancients already had made various attempts in this direction. But the main impetus to iatro-chemistry had been given by Paracelsus (1493–1541), and some of the ablest chemists at the dawn of the new age were followers of Paracelsus. A third tendency was that intimately associated with the mining industry. The practical needs of this important industry, coupled with the great opportunities which it afforded for close observation and experiment, had led from early times to the accumulation of considerable knowledge of metals and their treatment. The men engaged in this kind of work, however, were not given to expressing their views in books, least of all in the kind of speculative treatises which were likely to appeal to the learned people of the time. The introduction of printing, however, made a difference, and gradually a number of books made their appearance in which the practical knowledge of the miners found more or less adequate expression. This movement found its most systematic expression in the writings of Georgius Agricola, otherwise Bauer (1494–1555), just as the iatro-chemical movement found its fullest expression in the writings of Johann Baptista van Helmont (1577–1644). There was a certain amount of rivalry between the three tendencies just indicated, and there was no lack of strong language in their mutual criticisms, but most of their followers shared more or less the ideas developed by the alchemists.

IATRO-CHEMISTRY IN THE SEVENTEENTH CENTURY

LIBAVIUS

Among the iatro-chemists of this period Andreas Libavius, otherwise Libau (? 1540–1616), was probably the most deserving. He was born at Halle, in Germany, studied medicine as well as history and philosophy, and eventually became Director of the Grammar School at Coburg. He is chiefly noted as the author of the first real text-book of chemistry. It was published in 1597 under the title *Alchemia*, and contains a full discussion of the chemical theories of the time. In his account of current theories Libavius showed considerable independence and sanity of judgment, neither accepting new views merely because they were new nor rejecting them merely because they were not old. In fact, he does not appear to have held any strong convictions on chemical theory, but just recorded the Aristotelian (or rather pre-Aristotelian) view of the four elements theory (earth, water, air, fire), Jabir's sulphur-mercury theory of the composition of metals, and Paracelsus' view of the three principles, or elements, namely sulphur, mercury, and salt. But Libavius was not merely a compiler or critic. He made a number of original contributions to chemical knowledge. He discovered stannic chloride ($SnCl_4$), which was consequently long known as *spiritus fumans Libavii*. He described a compound of tartar and calcined antimony now known as tartrated antimony. The solution, which he prepared by leading the vapours of burning sulphur into water, he named "acid spirit of sulphur," and identified it with the acid obtained by distilling green vitriol or by heating sulphur with *aqua fortis* (nitric acid). He also prepared sugar-candy (hydrated sugar crystals). He described how spirits of wine could be obtained from grain, fruit, etc., by fermentation and distillation. He gave a method for the analysis of mineral waters, namely by evaporating the water and comparing the weight of the saline residue with the weight of the water which had been evaporated. He taught a simple and quick way of determining whether a water is mineral at all, that is, whether it contains metallic, alkaline, and earthy salts. A weighed white cloth is steeped in the water to be tested, then allowed to dry in the sun, and weighed again. If it has increased in weight and shows spots, then the water must have contained fixed mineral substances. Libavius also made artificial gems by colouring glass with metallic oxides, showed that fluor-spar was a flux for metals and their oxides. He was also the first to prepare ammonium sulphate, and to record the blue colour produced in ammonia by copper.

VAN HELMONT

Johann Baptista van Helmont was born in Brussels in 1577. A wealthy nobleman by birth, he preferred hard work in the chemical laboratory to the splendours of Court life. He studied classics in the University of Louvain, visited London in 1604–5, and then devoted himself to the medical service of the poor. It was in this way that he came under the influence of the medicinal chemistry of Paracelsus, whom he greatly surpassed. Van Helmont's greatest service to chemistry consisted in having been the first to show scientifically the material character of gases and their variety. Thereby he became the forerunner of the pneumatic chemists of the eighteenth century. The term "gas" was actually introduced by him. (He derived it from the Greek *chaos*, an expression which Paracelsus had applied to air.) But it was not till the time of Lavoisier that the term came into real vogue, most chemists till then being content to use the word "air."

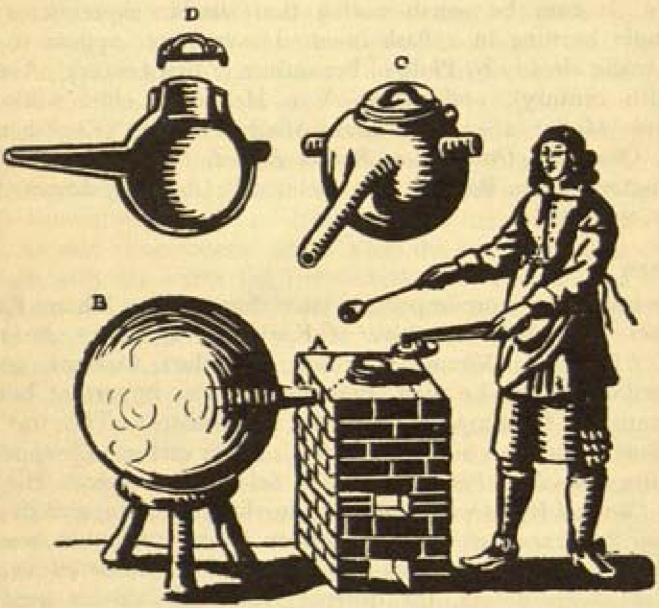
Van Helmont recognized the existence of many different gases. This was certainly an advance on the previous tendency to regard air as the only substance of its kind. But owing to the absence of any means of collecting gases for suitable experimental examination he could only classify them in a rough kind of way based mainly on their more obvious physical properties. Thus he enumerated the following kinds of gases: wild or unrestrainable gas, windy gas (air), fat gas, dry or sublimed gas, smoky or endemical gas, etc. He also observed that fat gas (obtained from the large intestine and by the fermentation of animal excrement) is inflammable, whereas wild gas extinguishes a flame. Moreover, he pointed out that gas obtained from the firing of charcoal, from the fermentation of vegetable juices, from the action of distilled vinegar on the shells of certain fish (what would now be described as the action of acetic acid on carbonates), from intestinal putrefaction (as in the digestion of food), from certain mineral waters, from mines, and from certain caverns (like the Grotto del Cane near Naples) are all the same kind of gas, namely what he called wild or unrestrainable gas (*spiritus silvester*), and which consisted mostly of what is now known as carbon dioxide. But he cannot be regarded as the discoverer of carbon dioxide, for he had no method of identifying it, and he actually included under wild gas the gas obtained from the solution of silver in *aqua fortis* (which really yields nitric oxide), from the burning of sulphur (which yields sulphur dioxide), etc. All that he meant by wild gas was a gas that "cannot be constrained by vessels, nor reduced to visible body." In the course of his experiments van Helmont noticed that his vessels were often shattered when gas was released in them even in the cold. He had more than a mere suspicion that the gas was the cause of the breakages, and he even suggested that the destructive action of gunpowder was due to the gases which it produced. (See *Oriatrike*, p. 106.)

Some of van Helmont's experiments induced him to embrace the view that "all salt, clay, and indeed all tangible bodies are really and materially the product of water only, and may be reduced again to water by nature or art." One of these experiments consisted in distilling some oak-wood, which yielded a colourless liquid like water. From this he concluded that even the gas obtained from the burning of charcoal was in the last resort really composed of water. To confirm this hypothesis he tried the reverse experiment of showing how water is transformed into wood. For this purpose he carried out an historic experiment, which had been suggested, if not actually made, by Nicolas de Cusa in the fifteenth century. This is van Helmont's account of the experiment. "I took an earthen vessel, and put in it two-hundred pounds of earth that had been dried in a furnace. I moistened the earth with water and planted therein the trunk or stem of a willow-tree weighing five pounds. At the end of five years the tree which had grown up weighed one-hundred-and-sixty-nine pounds and about three ounces. I had moistened the earthen vessel, whenever necessary, with rain-water or with distilled water . . . and I had covered the mouth of the vessel with an iron plate covered with tin lest the dust flying about should mingle with the earth [in the vessel]. . . . I computed not the weight of the leaves that fell off in the four autumns. Eventually I dried the earth in the vessel, and there were found the same two-hundred pounds, less about two ounces. Therefore one-hundred-and-sixty-four pounds of wood, bark, and roots had been formed out of the water alone." In further support of his views van Helmont pointed to the fossils of shells to be found on what had long been dry land. These regions must have been water when the shell-fish lived in them, but had been transformed into earth since then. Van Helmont, however, did not hold the view (usually associated with Thales) that water is the origin of *all* things. He regarded air as entirely different in character, not derivable from water, nor convertible into it. Water, he admitted, can of course be converted into vapour; but this is merely vapour, that is, water the atoms of which are rarefied, and which are at once condensed by the action of cold so as to resume their former state. (See *Oriatrike*, pp. 52, 109.)

Some of van Helmont's conclusions, as already remarked, would have been very different if he had the necessary apparatus for collecting and examining gases. Yet one of his experiments nearly placed the required apparatus within his reach, and may actually have suggested the subsequent invention of the pneumatic trough and its accessories. The experiment in question was described by him in the following terms. Place a lighted candle at the bottom of a basin; pour water into the basin to a depth of two or three inches; cover the candle, one end of which is above the water, with an inverted glass receiver. Presently you will see that the water is, as if by a kind of suction, raised in the receiver and takes the place of the diminished air, and that the flame is extinguished. The only conclusion he drew from this experiment was that it is possible to create a vacuum, but that it is filled immediately by a material substance. It did not occur to van Helmont that something might have been taken from the air in this experiment, any more than it had occurred to him that the tree (in the experiment described above) might have derived something from the air in which it had grown. It may be worth noting that similar experiments with a candle burning in a flask inverted over water appear to have been made already by Philo of Byzantium (? first century), Averroes (twelfth century), and others. Van Helmont's chief works are *Opuscula Medica* (1644) and *Ortus Medicinæ* (1648) (English trans. by J. Chandler, *Oriatrike, or Physick Refined*, 1662). See also J. R. Partington, "Joan Baptista van Helmont" (*Annals of Science*, 1936, vol. 1, 359–384).

GLAUBER

The last of the more important iatro-chemists was Johann Rudolf Glauber (1604–68). A native of Karlstadt, Germany, he spent most of his life in Vienna, Salzburg, Frankfurt, Cologne, and in Amsterdam, where he died. Perhaps his most important book is that entitled *A Description of the Art of Distillation*. This was first published in German in 1648. In 1651 a Latin version of it appeared with the title *New Philosophical* (i.e. Scientific) *Furnaces*. His collected *Chemical Works* were published in 1658, in Latin, and in 1689 in English (translated by Christopher Packe). Glauber was an expert metallurgist and assayer. He described in detail various methods of distillation, the different kinds of furnaces used for the purpose, and the divers uses to which the products distilled might be put. In his theoretical speculations he compromised between Paracelsus and van Helmont by accepting three principles or elements but substituting water for mercury in the list of Paracelsus. "The principles of vegetables," he wrote, "are water, salt, and sulphur, from which the metals also are derived." He was, however, essentially a practical experimenter who was more concerned with the utility of chemical discoveries than with speculative theories. His fame is popularly associated with the so-called "Glauber's salt," which he himself had named "the wonderful salt." When he was about twenty-one he had an attack of fever while at Vienna. He was advised to take the waters of a certain well in Neustadt. He did so, and got rid of his fever. Later on he analysed a sample of the water, and observed the formation of crystals very like those produced by dissolving and crystallizing the residue left in the retort after the preparation of what he called "spirit of salt" (also "muriatic acid," or briny acid) from vitriol and common salt. Glauber's account of the preparation of "spirit of salt" is worth quoting, if only in virtue of his description of the apparatus used. "You take common cooking salt and mix some vitriol or alum with it. The mixture is placed on glowing coals. The issuing spirit is condensed in a receiver (see Illustr. 182). Should anyone say that



Illustr. 182.—Glauber's Distillation Furnace (1)

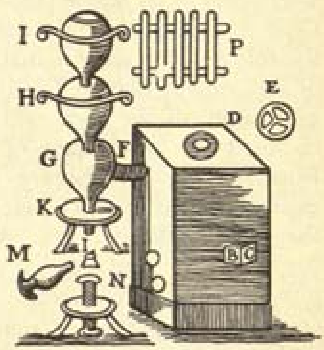
A is a Furnace containing an iron Still connected to a Receiver, B, outside. C shows the external Form of the Still, and D shows what it looks like inside.

this spirit of salt is not pure because the spirit of the vitriol or of the alum has mixed with it, I answer that it is not so. For I have frequently put vitriol alone and alum alone into the furnace, but no spirit issued, because the spirit of vitriol or of alum does not rise but is burnt." (*Works*, Packe's Tr., p. 4.)

In connection with the method just described and illustrated, it is interesting to point out that Glauber subsequently improved the process by heating the mixture in a glass vessel, and was thus the first to prepare "spirits of salt" in a high degree of purity.

Glauber recommended his wonderful salt as "a splendid medicine for internal and external use." He advised the domestic use of spirits of salt as a substitute for vinegar, and said that it softens meat and greatly improves the taste of meat and poultry. He also found that the liquid obtained by similarly distilling a mixture of salt and saltpetre with oil of vitriol could be used like *aqua regia* for dissolving gold. Next, he discovered that by gently heating saltpetre and oil of vitriol in a glass retort placed in a bath of sand over a furnace, a specially pure form of *aqua fortis* could be obtained. In this way Glauber was probably the first chemist to be familiar with the three mineral acids and *aqua regia*, which constitute the four great reagents of inorganic chemistry.

Glauber also gave some prominence to the idea of chemical affinity and, which is much more remarkable, to what is now known as metathesis or double decomposition. For instance, in his account of the reaction which occurs when corrosive sublimate ("sublimed mercury") is heated with sulphide of antimony, he remarks that the "corrosive spirits" (or "spirits of salt" in this case) forsake the sublimed mercury and associate themselves with the antimony, and thus form butter of antimony, while the sulphur of the antimony is joined to the quicksilver and yields cinnabar (see Glauber's *Works*, translated by C. Packe, 1689, p.7 f.).



Illustr. 183.—Glauber's Distillation Furnace (2)

D is a Furnace to which are attached Receivers, supported on a Stool, K, and various Devices for accelerating Condensation. P, iron Bars of the Furnace Grate.

Glauber earned his living by the sale of secret medicinal preparations. His favourite prescriptions included a universal medicine, or panacea, obtained by heating the calx of antimony with tartar; a secret sal ammoniac (which was actually ammonium sulphate, and not the chloride) prepared from oil of vitriol and spirits of hartshorn; and solutions of gold or iron or mercury or antimony with muriatic acid or *aqua regia*.

In his book called *The Prosperity of Germany*, Glauber showed his interest in problems of political economy, with special reference to the industrial and commercial aspects of chemistry. He drew attention to the great mineral wealth of Germany, but protested against the export of it in the form of raw materials from which other countries manufactured goods which they then resold to Germany. Among Glauber's own contributions to the chemical industries of his fatherland must be included not only the various medicaments already mentioned, but also his discovery of a new process for colouring artificial gems by means of gold, copper, manganese, and other metals; his discovery that organic matter can be dyed black by means of a solution of silver in *aqua fortis* (silver nitrate), and many other discoveries.

REY

Jean Rey, the author of *Essays de Jean Rey, docteur en medecine, sur la Recherche de la cause pour laquelle l'Estain et le Plomb augmentent de poids quand on les calcine* (Bazas, 1630; reprint, Paris, 1777; Alembic Club Reprints, No. 11), was one of the first to draw attention to the rôle of air in the calcination of metals. We know almost nothing of Rey except that he lived about 1630; but in his *Essays* he gave expression to an idea which long afterwards, under Lavoisier, changed the whole face of chemistry. Rey states that the Sieur Brun, Master Apothecary in Bergerac, had asked him for an explanation of the fact that tin and lead increased in weight when they were calcined. Accordingly, Rey "devoted several hours to the question," and allowed the *Essays* to "slip from his hands." To provide the Master Apothecary with an answer to his query, Rey composed a preface, twenty-eight essays, and a conclusion in "efforts to seek the truth in so arduous a question."

First of all, Rey found that he had to depart from views generally held, and to recognize that all things in nature were heavy. This was opposed to the prevailing theory that earth and water, being heavy, took up the lower regions of the world on account of that heaviness, whereas air and fire, being light, rose upwards. To Rey all substances were heavy, and there was nothing light in Nature; each of the four elements occupied its place on account of its heaviness; the upward movements of air and fire being due to their displacement by the heavier elements, earth and water. All moved naturally downwards, the heaviest to the lowest place, and the least heavy were forced to the highest place by the downward motion of the others. In these matters, he argues, the balance has caused deception and weights must be examined "by the reason." "I . . . affirm," he wrote, "that the examination of weights which is made by the balance differs greatly from that which is made by the reason. The latter is only employed by the judicious; the former can be practised by the veriest clown. The latter is always exact; the former is seldom without deception. The latter is attached to no circumstance of place; the former is commonly exercised only in the air, and occasionally, with difficulty, in water. It is from this that the error I have combated (that air is without weight) draws an argument which may dazzle feeble eyes, though not clear-seeing ones. For, balancing air in air itself, and finding no weight in it, they believed that it had none. But let them balance water (which they believe to be heavy) in water itself, and they will find no weight in it either: the fact being that no element shows weight when weighed in itself" (*Essays*, pp. 17, 18, Alembic ed.).

Air, therefore, according to Rey, has weight; but as his design is to show that the increase of weight of tin and lead on calcination is due to admixed air and as the calces of these metals are tested by the balance and weighed *in air*, he seeks to show that this admixed air is heavier than ordinary air—if it were not heavier it would show no weight in air. He accordingly argues that air can be made heavy by addition of heavier foreign matter, by compression, and by separation of its less heavy parts. With regard to this last method, he argues that fire makes water denser by the separation of its less heavy parts; and therefore it has the same effect on air. If air be heated, its less heavy parts will be driven off and its heavier parts will remain; and thus air becomes denser on heating, and will show this increase in density when weighed on a balance in ordinary air.

Rey is now almost, but not quite, ready to give his answer. "I comprehend already," he says, "that to elude the force of so many reasons and experiments" (which experiments he certainly did not make), "I shall be told that the examples I have brought forward can indeed be verified in our gross and impure air, but that it would be otherwise with pure air, if there were such in nature. And assuredly I wish for nothing better to dispose me to a song of triumph. For does anyone believe that I think the sieur Brun and the others who have performed the augmentation in question, procured some purer air, by letters of exchange, from beyond the bounds of nature?" (*op. cit.*, p. 34).

After a short essay showing how, by suitable methods, air may be made to decrease in weight, Rey gives his "Formal response to the question, why Tin and Lead increase in weight when they are calcined." He says: "Now I have made the preparations, nay, laid the foundations, for my answer to the question of the sieur Brun, which is, that having placed two pounds six ounces of fine English tin in an iron vessel and heated it strongly on an open furnace for the space of six hours with continual agitation and without adding anything to it, he recovered two pounds thirteen ounces of a white calx; which filled him at first with amazement, and with a desire to know whence the seven ounces of surplus had come. . . . To this question, then, I respond and sustain proudly, resting on the foundations already laid, 'That this increase in weight comes from the air, which in the vessel has been rendered denser, heavier, and in some measure adhesive, by the vehement and long-continued heat of the furnace: which air mixes with the calx (frequent agitation aiding) and becomes attached to its most minute particles: not otherwise than water makes heavier sand which you throw into it and agitate, by moistening it and adhering to the smallest of its grains.' I fancy there are many who would have been alarmed by the sole mention of this response, if I had given it at the beginning, who will now willingly receive it, being as it were tamed and rendered tractable by the evident truth of the preceding Essays" (*op. cit.*, pp. 36, 37). And Rey is careful to point out that the gain in weight covers losses due to vapours and exhalations and to the increase in volume of the tin on calcification.

After disposing of various objections and describing how he carried out an experiment (probably his only experiment on this problem), at his son's ironworks, where he calcined tin on a hot iron ingot and found it to increase in weight, Rey states: "By a single experiment all opinions contrary to mine are entirely destroyed" (*op. cit.*, p. 49). This experiment he copied from Hamerus Poppius, who in his *Basilica Antimonii* (1618) describes how he took a weighed quantity of antimony, calcined it, by means of the Sun's rays, with a burning mirror, on a marble slab, and found an increase in weight. Here, says Rey, there is no question of loss of celestial heat, consumption of aerated particles of the metal, addition of soot or parts of the containing vessel, admixture with the vapours or volatile salts of charcoal, or humidity. "Let now," he adds, "all the greatest minds in the world be fused into one mind, and let this great mind strain every nerve beyond its power; let him seek diligently on the Earth and in the Heavens, let him search every nook and cranny of nature: he will only find the cause of this augmentation in the air when the Sun's rays heat it, and render it dense and heavy, so that it then mixes with the calx as the antimony on calcination crumbles and becomes adherent in its minutest particles. And this confirms entirely the truth of my belief in the augmentation of lead and tin: which can have no other cause than the admixture of condensed air, there being no difference between the increase of weight in these two metals and in antimony, only that in the last case the air is condensed by the solar rays, and in the former by the heat of the common fire" (*op. cit.*, p. 51).

Finally Rey says that the calx does not increase in weight indefinitely because, as in other phenomena, such as the mixing of a solid with a liquid, "Nature in her inscrutable wisdom has here set limits which she never oversteps." The calx becomes saturated with the condensed air and it can then take up no more. And, as for calces that do not increase in weight, they are produced from substances that contain much exhalable matter or they suffer great increase of volume, and in neither of these cases is the loss of weight made good by the addition of the condensed air. This brings Rey to the question of lead. Sieur Brun had observed a loss of weight in the case of lead—"by one ounce in the pound." But, says Rey, others (Cardan, Scaliger, and Cesalpinus) have observed an increase. The explanation of the different results obtained is, according to Rey, quite simple. "Some lead is more pure than others, either because it comes so from the ores or because it has been previously melted. Those named above have formed augmentation with the pure lead: the sieur Brun a diminution with the other" (*ibid.*, p. 54).

Rey's work passed unnoticed, and it only became known when Bayen directed attention to it, in 1775, when Lavoisier had begun to experiment on this problem.

(See J. M. Stillman, *The Story of Early Chemistry*, 1924.)

The Beginning of Chemical Science

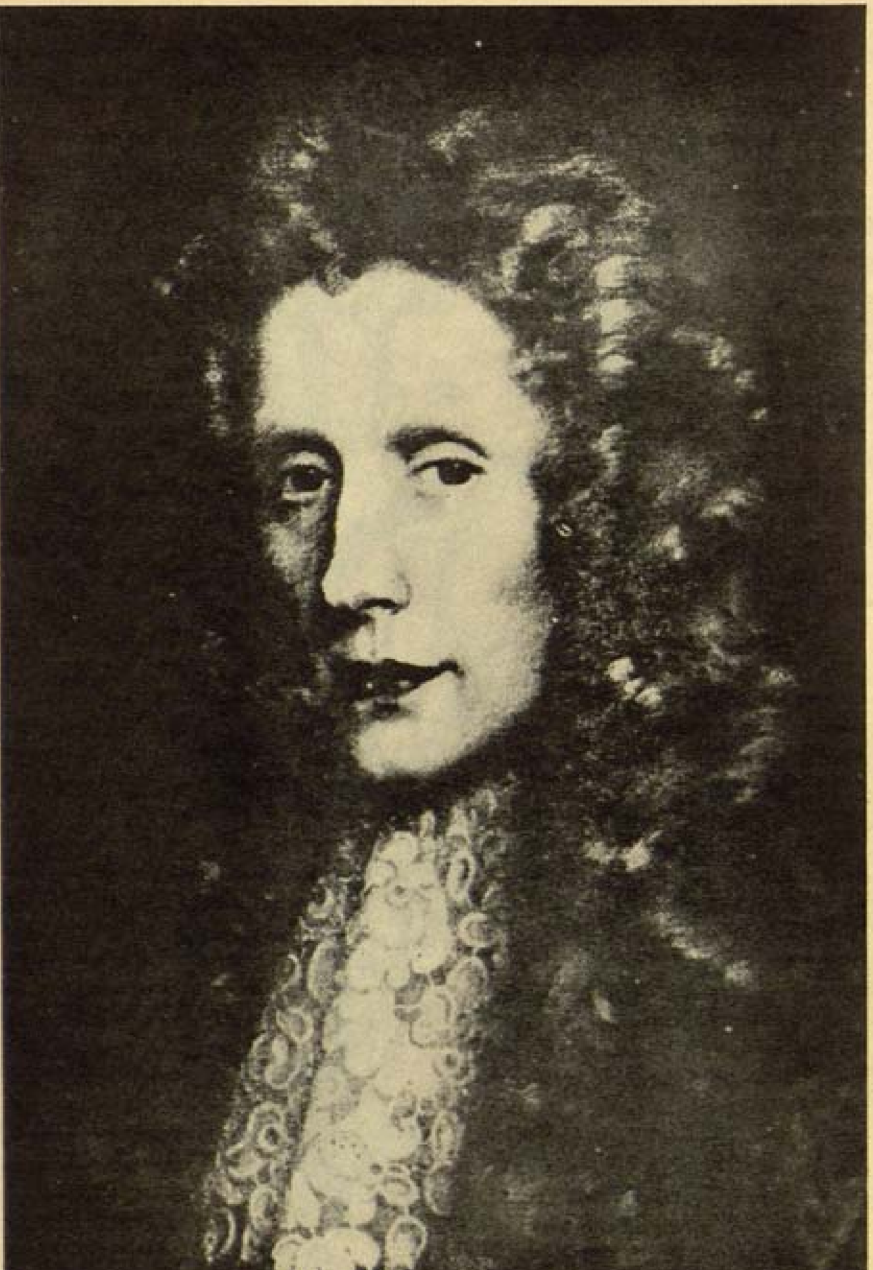
The activities of the alchemists, metallurgists, and iatro-chemists had certainly led to results of various kinds which could be turned to scientific account. But the aim of these investigators was practical rather than scientific, and their experiments accordingly were what Francis Bacon called "fructiferous" rather than "luminiferous," that is, directed to the production of utilities (real or imaginary) rather than to the promotion of a scientific understanding of chemical phenomena. Now, important as observation and experiment are for science, they are not sufficient in themselves. Science needs also illuminating ideas for the guidance and understanding of the facts observed. If the new age protested, and protested rightly, that ideas without adequate experimental data are empty fancies, it was equally necessary to realize that mere experimentation without adequate ideas is blind. The case is, in fact, worse than that. No empiric is content to dispense with ideas altogether; and in the absence of clear concepts, and of a sufficient grasp of scientific method, the empiric simply indulges in wild fancies, and ends in sheer mystification. After Galilei's researches had set up models of scientific investigation in mechanics it was time for a critical scrutiny to be made of the conceptions and preconceptions current among the chemists of the time, and to show the way for the proper conduct and interpretation of chemical experiments. This complex task was undertaken mainly by Boyle and Hooke, who may be credited with having laid the foundations of chemical science.

Broadly speaking, the speculative ideas current in the chemistry of the time may be summarized as follows. The so-called Aristotelians, Peripatetics, or Hermetic philosophers maintained that all bodies were composed of the four elements, earth, water, air, fire. On the other hand, the followers of Paracelsus, the so-called Spagyrists or "chymists" simply, held that salt, sulphur, and mercury were the hypostatical principles, or constituents, of all things. Various compromises between the two lists of elements were adopted. Some adopted both lists in their entirety; others accepted the three "principles" of Paracelsus coupled with two out of the other four elements (say, water and earth); yet others selected just one or two from each list. The attempt to make shift with such a small number of kinds of entities led to all sorts of dubious devices, notably that of giving the same name to ever so many different kinds of things. The radical defect was confusion about what was really to be understood by an "element" or "principle." Intimately connected with this confusion was the almost universal belief in fire as the unfailing instrument of analysis capable of resolving all mixed or compound bodies into the elementary substances which compose them. And to crown this mass of confused notions, explanations were made not only in terms of occult qualities, but also in terms of substantial forms—the "forms" of things being conceived more or less as souls or spirits, after the manner of the Stoic distortion of the views of Aristotle, whose differentiation between matter and form was identified by the Stoics with that between body and soul. Quasi-animistic explanations of this kind only tended to confirm the general conception of chemistry as a kind of magic. Such, briefly, were some of the ideas which Boyle attempted to rectify.

BOYLE

The Honourable Robert Boyle (1627–91) was the seventh son of the Earl of Cork. He was born in Ireland, studied at Eton, and then travelled on the Continent. He was in Italy in the year in which Galilei died and Newton was born (1642). He returned to England in 1644, just about the time when the Philosophical or "Invisible" College was started; and when this College became the Royal Society in 1662 he was one of its first and most influential members. He had come under the influence of Francis Bacon's ideas about science, and of the mechanical (or anti-animistic) philosophy which had received considerable impetus on the continent through the writings of Galilei and Descartes. He was devoted to experimentation, yet appreciated also the importance of ideas, so long at least as these did not clash with his religious prejudices, and he could at times speak contemptuously of mere experimenters as "sooty empirics." Fortunately for Boyle and for Chemistry, his experimental researches had no obvious bearing on Christian theology, and so

Jan Baptista Van Helmont



Robert Boyle

he could pursue them in a genuinely scientific spirit, which did much to sweep away the cobwebs which smothered the chemistry of his day. Of his numerous books the most important one is *The Sceptical Chymist*, first published in 1661, though it was written and privately circulated some years before.

Boyle begins by insisting that chemistry should be pursued, not as an empirical art for the making of precious metals or of useful medicaments, but as a science, a branch of natural philosophy. "Finding the generality of those addicted to chymistry to have had scarce any view but to the preparation of medicines, or the improving of metals, I was tempted to consider the art not as a physician, or an alchymist, but as a philosopher" (Preliminary Discourse to Boyle's *Works*, ed. 1725, by P. Shaw, Vol. I, p. xxvii). As a branch of philosophy or science chemistry is concerned mainly with the theoretical explanation of phenomena rather than with their practical exploitation. And such explanation was seriously thwarted by the obscure jargon in which chemical writers of the time wrapt their views and assumptions as if to shield themselves from detection and attack. This smoke screen had to be dispersed so that all that was hostile to science might be seen and repulsed. Boyle accordingly proceeded to deal critically with the various unwarranted assumptions indicated above, and his criticism was always supported by reference to actual experiments.

First he attacked the current assumption that all things are composed of three or four or some other small number of elements. This assumption appeared to him as unwarranted as if somebody "seeing a great book written in cypher, whereof he were acquainted with but three letters, should undertake to decipher the whole piece" with those few letters. "The Book of Nature" may need many more than three or four elements wherewith to decipher it. Boyle exposed the current confusion in the use of the term "element" (or "principle," etc.), and formulated his own conception of it as follows: "I . . . must not look upon any body as a true principle or element, but as yet compounded, which is not perfectly homogeneous, but is further resoluble into any number of distinct substances, how small soever. . . . I now mean by elements . . . certain primitive and simple or perfectly unmingled bodies, which not being made of any other bodies, or of one another, are the ingredients of which all those called perfectly mixed bodies are immediately compounded, and into which they are ultimately resolved." The formulation of this conception was itself a valuable service to chemical science, though the fullest use of the concept was not made until Lavoisier based on it his list of chemical elements. However, having cleared up the notion of an element, Boyle warned his readers against mere fancies and vague possibilities. The business of the scientific chemist is "to consider, not of how many elements it is possible that nature may compound mixed bodies, but (at least as far as the ordinary experiments of chymists will inform us) of how many she doth make them up." Boyle has no difficulty in showing that not all substances can be compounded out of, or resolved into, either the four elements or the three principles, and he offers to defray the cost of an experiment which will decompose gold into salt, sulphur, and mercury. Again, from some bodies more than three or four distinct substances can be obtained. Blood, for example, yields phlegm, spirit, oil, salt, and earth. The four elements of the Peripatetics and the three principles of the Spagyrists are far too few to account for even a tenth of the phenomena known. Their inadequacy is concealed by the application of the same name to many different substances. Thus, for instance, the term "salt" is applied to such different things as fixed vegetable salts, volatile animal salts, etc., though (as Boyle shrewdly notes) they differ visibly in their (crystal) forms. Similarly, the term sulphur is applied to different substances, some of which float on water while others sink in it. And so on. (*Scep. Chym.*, pp. 236, 350.)

Boyle next attacks the radical error on which the above errors are based, namely the contemporary misconception of the action of fire in chemical experiments. It was commonly assumed that fire is the universal instrument of analysis, and that it only separates all the elements which pre-exist in the heated substance. Boyle contends that this triple assumption is unwarranted, and he supports his contention with experimental evidence. In the first place, fire (or heat) does not analyse all mixed bodies. Glass, for instance, cannot be analysed by means of fire, although it is known to be composed of the salt and the earth remaining in the ashes of a burnt plant. Even when fire does separate a mixed body into various parts, these parts are not necessarily elements. For the results obtained when the same material is treated by fire are different according as the fire is applied in the form of combustion or of distillation. Wood, for instance, yields soot and ashes when burnt, but gives oil, spirits, vinegar, water, and charcoal when distilled. The effect of fire is really different in different circumstances. Thus coal burnt in the open air is calcined to ashes, but is not calcined at all if heated in a closed vessel, even if kept red-hot in a vehement fire. Similarly, sulphur if burnt in the open air gives fumes which yield an acid liquor, but if heated in a closed vessel it sublimes to flowers of sulphur, which may be melted into the original kind of sulphur. Fire may, in fact, compound the parts after a new manner, or even incorporate new ingredients (instead of separating the old ones), and so produce what did not pre-exist in the original mixed body. Thus in experiments with plants, like van Helmont's above-mentioned experiment with a tree, it would seem that the various things obtained from the plant by distillation, namely salt, spirit, earth, and oil, may be produced out of water, in which they could not have been pre-existent, any more than the glass produced by heating the ashes of a burnt plant can be said to have been pre-existent in the plant.

While making the criticisms just outlined Boyle had occasion to clear up yet another important chemical concept, namely that of "composition." In a mere "mixture" each constituent retains its properties and can be separated from the rest; in a "compound mass" each constituent loses its characteristic properties and is more difficult to separate from the rest. Sugar of lead, for instance, is composed of litharge and vinegar, but is sweet in taste, unlike either of its components.

In the course of his attack on the view of fire as the universal instrument of analysis Boyle repeatedly pointed out that various mixed substances which could not be separated by means of heat could be analysed easily by other means, such as *aqua fortis*, a solution of salt of tartar, etc.

Elsewhere, in his study of colours, Boyle made an observation of considerable chemical importance, when he found that syrup of violets, which is blue, turned red on addition of acid, and green on addition of alkali. He suggested that these changes might be used to determine the nature of bodies "chymically prepared" and the changes "that nature and time produce" in them.

Boyle's interest in the nature and action of fire nearly led him to the discovery of oxygen. He observed that various burning bodies, such as candles, coals, sulphur, when placed in the receiver of his air-pump were extinguished when the receiver was exhausted. He also noted that the flame of a lamp, though fed with oil, went out even in an unexhausted receiver, and that all but a very small part of the air was left in the receiver after the extinction of the flame. It seemed, therefore, that only some *part* of the air was necessary for combustion. He concluded, accordingly, that "there may be dispersed through the rest of the atmosphere some odd substance, either of a solar, astral, or other foreign nature, on account whereof the air is so necessary to the subsistence of flame." These experiments on combustion in an unexhausted receiver, however, misled Boyle to underestimate the proportion of this "odd substance" in the air. For he noted that the "spring" of the air in the unexhausted receiver was undiminished after combustion. To Boyle, therefore, the air seemed practically unchanged. His method of experiment was such that it could not reveal the true cause of the undiminished "spring," namely the fact that the gaseous products of the combustion had maintained the volume of the original air. His experiments on the calcination of metals did not help to enlighten him on the matter, because he explained their increase in weight as due to their absorption of particles of heat or fire from the furnace (*Fire and Flame weighed in the Balance*, 1673). Anyway, Boyle had arrived at the conclusion that some part of the air was necessary for combustion. This conclusion and the experiments on which it was based were described in his *New Experiments Physico-mechanical, touching the spring of the Air, and its Effects* (published in 1660) and in *Hidden Qualities of the Air* (1674).

In the former treatise Boyle also discussed problems of respiration. His experiments tended to show that the life of an animal is as dependent on some part of the air as is the flame of a lamp. He quotes the view of Paracelsus who had compared the relation of air to the lungs with that of foods to the stomach; the lungs digest and consume part of the air and expire the rest as a kind of excrement. "We may suppose" (Boyle continues) "that there is in the Air a little vital Quintessence . . . which serves to the refreshment and restauration of our vital Spirits, for which use the grosser and incomparably greater part of the Air being unserviceable, it need not seem strange that an Animal stands in need of almost incessantly drawing in fresh Air." His only criticism of this view of Paracelsus is that it should have been verified experimentally—a defect which Boyle tried to remedy. He showed that animals placed in a receiver, from which the air was then pumped out, died very quickly. An eel in such an exhausted receiver turned on its back, but revived when air was readmitted. Even in an unexhausted receiver animals only live a comparatively short time if the air in it is not renewed. Fishes, whose gills function just like lungs, die in ponds that are frozen over and so exclude new supplies of air. Boyle also considered the case of the embryo or foetus and the absence of direct respiration on its part. The absence of direct respiration appeared to be made good by a supply of the mother's arterial blood, containing a rich supply of "vital quintessence" drawn from the air by the lungs of the mother. Thus the conception of a vital substance or quintessence contained in the air and helping to sustain animal life through respiration and mingling with the animal's blood brought Boyle once again to the verge of the discovery of oxygen. But Boyle appears to have been too cautious to venture on the identification of the part of the air used up in combustion with the part used up in respiration, or even to suggest the chemical properties of either part. The problems of combustion and respiration were carried a stage nearer their solution by Boyle's contemporaries, and co-workers at the Royal Society, Robert Hooke and Richard Lower.

HOOKE

Robert Hooke was born at Freshwater, Isle of Wight, in 1635. From about 1655 he was employed as research assistant by Robert Boyle, whom he helped with the construction of the air-pump, and in his numerous experiments. In 1662 he was appointed Curator of Experiments to the Royal Society, of which he was elected a Fellow in the following year. From 1662 until his death in 1703 Hooke carried out innumerable experiments at the Royal Society. He was a very skilful experimenter and a most versatile thinker, but apparently lacking in sufficient perseverance to carry through completely many of the thoughts which occurred to his fertile brain. The nature of his post at the Royal Society, and the free and easy exchange of ideas at the meetings of this Society, have inevitably made it difficult in many cases to ascertain who was really the first author of various suggestions which were first ventilated at these meetings. It is quite possible that Hooke did not always get due credit for his share in a number of scientific discoveries or inventions. Be that as it may, his share in the researches into the nature of combustion and respiration seems clear and creditable. Presumably he had some share in the experiments made by Boyle and described in his *New Experiments Physico-mechanical*, of 1660, of which some account has been given above. Hooke, however, advanced beyond Boyle by identifying the part of the air used up in combustion with the part used up in respiration, and by describing this part of the air as nitrous in character. The main facts relating to Hooke's contributions to the study of combustion and respiration are recorded in Birch's *History of the Royal Society*, and in Hooke's *Micrographia* (1665), and especially in his *Lectures* (1681–2). They may be summarized as follows.

In 1664 Hooke made experiments which showed that when enclosed in a receiver containing compressed air a lamp would continue to burn, and a bird or a mouse would continue to live, much longer than when the air in the receiver was at ordinary pressure. In 1663 Christopher Wren appears to have thrown out the suggestion that air contains "nitrous fumes" which sustain animal life; and he actually tried to invent something for fumigating sick rooms with nitrous fumes. The idea seems to have been taken up promptly by Hooke, who extended it also to explain combustion. At the end of 1664 (O.S.) Hooke made an experiment which was intended to show that flame or fire is the dissolution of combustible bodies brought about by a "nitrous substance inherent and mixt with the air." In the *Micrographia* this "nitrous substance" is described as "like, if not the very same, with that which is fixt in salt-petre" (ed. 1665, p. 103; Alembic Club Reprint, No. 5, p. 44). In a later volume Hooke assimilated respiration to combustion in the statement that animals "live no longer than they have a constant supply of fresh air to breathe, and, as it were, blow the fire of life; for so soon as that supply is wanting the fire goes out, and the animal dies" (*Of Light*, May 1681, in R. Waller's ed. of the *Posthumous Works* of R. Hooke, 1705, p. 111). On the other hand, he showed that animals can be preserved alive by blowing through their lungs with bellows, even without the usual breathing motions of the lungs. As already remarked, the essential part for combustion and respiration, according to Hooke, is the volatile nitrous substance which is contained in the air. That the part of the air involved is nitrous seemed evident to Hooke from the fact that mixtures containing nitre can burn even without air. "This" (he says) "is obvious in compositions made with salt of nitre and other combustible substances, as in gunpowder and the like, which will actually burn without the help of air . . . under water; nay, in an exhausted receiver . . . though where this nitrous part is wanting no combustion . . . will be produced, be the heat never so great" (*ibid.*, *Discourse of the Nature of Comets*, 1682, p. 169).

In 1665 Hooke showed experimentally that plants need air for their growth. He had sown some lettuce-seed upon earth in the open air; and at the same time upon other earth in a glass receiver which was afterwards exhausted of air; the seed exposed to the air was grown up an inch and an half high within eight days; but in the exhausted receiver not at all (T. Birch: *History of the Royal Society*, 1756–7, Vol. II, pp. 54, 56).

In 1667 and 1668 Hooke studied experimentally the entry of air into the blood of animals and the effects produced thereby. In the presence of Richard Lower he made experiments to ascertain whether an embryo in the womb lives by its own or by its mother's respiration. The experiments seemed to show that the blood of the embryo is ventilated by the help of its mother, and that there is a "continual and necessary communication of the blood of the dam with that of the foetus" (T. Birch: *History of the Royal Society*, 1756–7, Vol. II, p. 233). Other experiments showed "that blood, though of a dark blackish colour, would, when exposed to the air, become presently very florid, and that florid surface being taken off, and the adjacent part exposed again, would acquire the like floridness; and that therefore it might be worth observing by experiment whether the blood, when it passes from the right ventricle of the heart into the left, coming out of the lungs, it hath not the tincture of floridness, before it enters into the great artery; which if it should have, it would be an argument, that some mixture of air in the blood in the lungs might give that floridness" (*ibid.*, p. 274). These and connected problems were investigated more thoroughly by the above-mentioned Richard Lower, who availed himself, however, of Hooke's method of producing artificial respiration.

LOWER

Richard Lower (1632–91) was born in Cornwall, studied medicine at Oxford, was elected a Fellow of the Royal Society (in 1667), and eventually became the most noted London physician of his time. He was the first to carry out successfully the operation of transferring blood from one animal into another; and consequently dreamed dreams of what might be achieved "by exchanging the blood of old and young, sick and healthy, hot and cold, fierce and fearful, tame and wild animals." Here, however, the chief interest lies in his valuable work to explain the significance of respiration in animals. So far Boyle had shown the necessity of air for animal life, and Hooke had shown that animals can be kept alive without the breathing movements of their lungs, if air is blown through these by means of bellows. Hooke also suggested that the florid colour of arterial blood might be due to "some mixture of air in the blood." It was at this point that Lower took up the problem experimentally and brought it nearer solution. His experiments and conclusions are described in his *Treatise on the Heart*, published in 1669. The difference in colour between (florid) arterial blood and (dark) venous blood was well known even to the ancients. Sometimes the blood in the arteries was regarded as different in kind from that in the veins; sometimes it was thought that the blood changed colour somehow during its passage through the heart. Lower showed that the blood in the arteries and the veins is the same, and that the change in colour from dark to florid is due to the action of the air in the lungs. First he showed experimentally that the difference in the colour of the blood in the right ventricle of the heart and in the left ventricle of the heart has nothing to do with the heart. The blood in the right ventricle of a suffocated dog is as dark as that in the left ventricle; while the blood in the left ventricle is as florid as that in the right ventricle is normally, if the lungs are so perforated and inflated that the air has access to the left ventricle. This showed that the "production of the scarlet colour must be attributed wholly to the lungs," or rather "to the particles of air insinuating themselves into the blood" (*Tractatus de Corde*, 1669, p. 166). This is confirmed by the already familiar fact that venous blood when shaken with air becomes scarlet. In the meantime Lower had also adopted the Paracelsan idea of the composite nature of air, Hooke's (or possibly his own) assimilation of combustion and respiration, and Wren's and Hooke's identification of the "nitrous" part of the air as mainly involved in both combustion and respiration. Accordingly, Lower regarded respiration as a process by which the "nitrous spirit" (a term reminiscent of Wren's "nitrous fumes") of the air enters the lungs, saturates the blood, and so gives it its florid colour. But "after the air has again for the most part escaped into the structure of the body . . . and has transpired through its pores . . . the venous blood, deprived of it, immediately appears darker and blacker" (*ibid.*, p. 170). Whether Lower had any clear idea of the nature of the "nitrous spirit" of the air seems very doubtful, for, in the same passage in which he speaks of this nitro-aerial spirit, he also speaks of the "nitrous spirit of snow" passing through dishes of delicacies, and cooling summer wine. (See *De Corde*, facsimile edn. with English trans. by K. J. Franklin, Oxford, 1932.)

MAYOW

John Mayow (1641–79) was born in Cornwall, studied law and medicine at Oxford, and was eventually elected a Fellow of the Royal Society. In 1668 or 1669 he published two small treatises *On Respiration* and *On Rickets*. In 1674 he published *Five Medico-Physical Treatises* containing the one *On Rickets*, an improved version of the one *On Respiration*, and new essays *On Sal Nitrum and Nitro-aerial Spirit*, *On the Respiration of the Foetus*, and *On Muscular Motion* (complete English translation published by the Alembic Club, 1907).

Mayow knew Lower personally, and was familiar with the experimental work done by Boyle and Hooke as well as Lower. In his account of nitro-aerial spirit and of respiration he brought together all the ideas that had come to light concerning combustion, fermentation, and respiration as the result of the researches of Boyle, Hooke and Lower, as outlined above. It is probable that Mayow also carried out some experiments on his own account, and he was clever enough to have some ideas of his own, even if he was in some ways as uncertain or obscure as were some of his older and abler contemporaries. His *Treatises* did not elicit much praise, or even approval, among his contemporaries. Oldenburg informed Boyle that "Some very learned and knowing men speak very slightly of the *Five Treatises* of J. M." The main reason for this, no doubt, was that these treatises as a whole did little more than describe the results already arrived at by older and better known contemporaries. But a century later, after the discovery of oxygen by Priestley and Lavoisier, matters took a new turn. Mayow was "discovered" as the man who had anticipated Priestley and Lavoisier by a century. At least one writer put him on the same pedestal with Francis Bacon and Newton; others have compared him with Aristarchus, the neglected forerunner of Copernicus, and so on. This kind of beatification of Mayow had become a well-established tradition, when, in 1931, T. S. Patterson's researches (published in *Isis*, Vol. XV) showed up the errors of the traditional estimate of Mayow. Now there is some danger perhaps that Mayow's services may be underestimated.

The three treatises in which Mayow deals with combustion, fermentation, and respiration have the merit that they bring together in a comparatively concise and orderly manner the scattered researches of a number of different people. He was not always clear himself. For instance, he frequently speaks of nitro-aerial "spirit," yet seems to regard it as composed of solid particles which float in the air, rather than as a gaseous component of air. But then Boyle, Hooke, and Lower also are not always clear and definite, and they were certainly more long-winded than Mayow, whose account is at least a useful summary, interspersed with some original suggestions or comments, and illustrated by helpful pictures of the experiments described. In fact, a summary of Mayow's account may serve here the purpose of a brief survey of the results reached, at the period under review, concerning the chemistry of combustion and respiration.

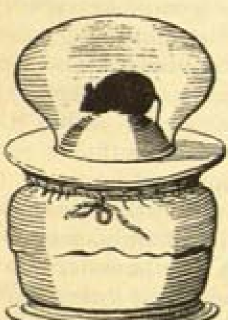
The air is impregnated with a salt of a nitro-saline nature, or a vital, igneous, and highly fermentative spirit. Nitre itself consists of an extremely acid salt and of an alkali; or of purely saline volatile salt taking the place of the alkali. The volatile part of the nitre comes from the air, while the fixed part is derived from the Earth. Experiments on combustion have shown that such "nitro-aerial particles" are indispensable to the production of fire, and that they are only a part of the air. Nitre itself contains such particles, for when mixed with sulphur (as in gunpowder, for instance) it burns in an exhausted receiver, in which other fires would go out immediately. Again, comparing antimony calcined by a burning glass and antimony treated with spirit of nitre poured on it and drawn off again, the result is found to be the same, and is due to the action of the nitro-aerial particles in both cases. Moreover, the increase in weight shown by antimony calcined by the Sun's rays must be due to the nitro-aerial particles fixed in it during calcination, as no other cause of the increase in weight can be imagined. (It is true that, in 1630, Jean Rey had suggested that the increased weight of calcined metals is due to the air; but he did not regard the air as a factor in calcination, but as absorbed by the metal *after* calcination.)

Fermentation and respiration are also due to the action of nitro-aerial particles. In respiration these particles are taken from the air by the lungs and passed into the blood. That is why the air expired from the lungs is lighter and less in volume than the air which had been inhaled. These particles play the leading rôle in the life and movement of animals and plants. In the case of an embryo the absence of respiration is made good by a supply of the mother's arterial blood, which is rich in nitro-aerial particles. Animal heat is also the result of the union of nitro-aerial particles with combustible particles in the blood; and the increased heat resulting from violent exercise is due to the extra intake of nitro-aerial particles with the increased respiration. The scarlet colour of arterial blood is likewise due to the action of these nitro-aerial particles. The essential factor in respiration is not the movement of the chest or of the lungs, but the supply of nitro-aerial particles from the air.

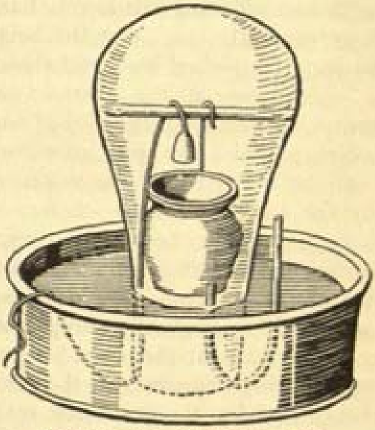
Mention may also be made of a few other matters which do credit to Mayow as a chemist. He showed by means of roughly quantitative experiments that what we call hydrogen and nitric oxide follow approximately the law of inverse relation of pressure and volume (Boyle's Law). He was also responsible for the experiments which showed that when nitric oxide is mixed with air the mixture shows a contraction to the extent of a quarter of the volume of the air. Moreover, Mayow had a good insight into the nature of the exchanges between salts and acids. Thus, to quote but one or two of his many examples of chemical exchanges, when describing the distillation of sal ammoniac with salt of tartar he explains that the acid part of the former coagulates with the fixed salt of tartar, "but the volatile salt, of which it also in part consists, ascends of the same nature as before. And the reason of this is that the acid spirit of salt is capable of entering into closer union with any fixed salt, than it is with the volatile salt, so that it immediately leaves the volatile salts that it may be combined more intimately with the fixed salt. But if oil of vitriol is united with salt of tartar, they can scarcely be separated from each other. And yet this is not because these salts have mutually destroyed each other, but because there is nothing in nature with which either of them can unite more firmly than they do with each other" (*Alembic Club Reprints*, No. 17, p. 161).

We may conclude this account of Mayow by citing two of his experiments with the accompanying illustrations. The second experiment here described is especially noteworthy because it was the same kind of experiment, though more elaborate, which, about a century later, led to the invention of the instrument now known as the eudiometer, for measuring the "goodness" of the air, that is the amount of oxygen contained in it.

(1) "Let a moistened bladder be stretched over the circular orifice of a vessel and tied to it just as the skin of a drum is stretched; then let a small bell-jar in which a little animal, say a mouse, has been put, be accurately applied to the said bladder by placing a weight upon the jar lest the animal inside should upset it. [See Illustr. 188] . . . It will in a short time be seen that the jar is firmly fixed to the bladder; and the bladder also, in the place where it lies under the jar, is forced upwards into the cavity of the glass



Illustr. 188.—Experiment with a Mouse



Illustr. 189.—Elasticity of Air

just as if the jar had been applied with a flame enclosed in it. . . . If the jar be grasped by the hand and raised, the bladder, along with the vessel, will still adhere firmly to it unless the vessel is very heavy . . . and from this it is clear that the elastic power of the air enclosed in the aforesaid jar has been diminished by the breathing of the animal, so that it is no longer able to resist the pressure of the surrounding air. . . . I have ascertained from experiments with various animals that the air is reduced in volume by about one-fourteenth by the breathing of the animals" (*On Sal Nitrum*, Alembic Club Edition, p. 72 f.).

(2) "Let a rod equal in length to the diameter of a glass bell-jar at its widest part be put inside it, and placed transversely and drawn downwards till both ends of the rod lean upon the sides of the glass and are supported by them [as in Illustr. 189]. Next let an earthenware vessel, glazed inside and capable of holding about four fluid ounces, be hung from the transverse rod by an iron hook attached

to it, and let it be about half-filled with spirit of nitre. Further, let some small pieces of iron, tied together into a bundle and suspended by means of a string from the rod, be made to hang directly over the vessel (the string moreover ought to be of such a length that its other end may reach to the mouth of the glass and hang outside, in the manner shown in the illustration). These arrangements made, the mouth of the inverted bell-jar should be sunk in water about five finger-breadths, yet so that the water within the bell-jar may be at the same level as the water outside, as may be done by means of a syphon. . . . Then let the water outside be drawn off until it is lower than the water inside by about three finger-breadths. . . . Let the height of the water within be noted by papers attached here and there to the outer surface of the glass. . . . And now let the aforesaid small pieces of iron be lowered by means of the string, the end of which hangs outside, into the vessel which contains the spirit of nitre. . . . A very intense action will soon be excited and the water within will at once be depressed by the vapours thence arising. . . . When the water within has been depressed about three finger-breadths by the vapours produced, let the pieces of iron be lifted out of the vessel by means of the aforesaid string. This done, after a short time you will see the water within gradually rising, and in the course of an hour or two you will see it far above the height first marked. For the water which was quickly depressed by the aforesaid vapours about three finger-breadths *below* the point first marked, now rises some three finger-breadths more or less *above* it; so that about a fourth part of the space in the glass which was previously occupied by air is now occupied by the water rising within, and indeed the water which has risen in this way in the glass will not, even after a long time, fall to the original mark. So that clearly we must conclude that the air contained in the glass has its elastic force diminished by about one-fourth part, in consequence of the said action produced by the spirit of nitre encountering the iron" (*ibid.*, pp. 94–6).

THE DISCOVERY OF PHOSPHORUS

Before concluding this chapter something must be said about the discovery of phosphorus. Sometime about 1670 (the dates given vary from 1667 to 1674) Brand, an alchemist and quack physician of Hamburg, appears to have prepared a phosphorus from urine that was unlike other known phosphori in that it glowed in the dark without preliminary exposure to light. By some means or other, Krafft, a chemist of Dresden, obtained possession of the secret of Brand's preparation, and crossed to England, where he exhibited the product before Charles II in 1677. Among those who saw the substance was Boyle; and, after the manner of the times, in an exchange of secrets Krafft hinted that the substance essential to the preparation of this astonishing new product and scientific curiosity "was somewhat that belonged to the body of man" (Boyle: *The Aerial Noctiluca*, 1680, p. 12; *Works*, ed. 1772, IV, p. 382). About 1680 Boyle discovered how to make this phosphorus, publishing his studies in *The Aerial Noctiluca* (1680) and *The Icy Noctiluca* (1681–2). In these tracts he described the various facts that he had discovered relating to the glowing of phosphorus, namely, that contact with the air was necessary for the production of the glow, that the glow was produced in solutions of phosphorus in some oils but not in others, that water which had been in contact with phosphorus and its fumes yielded on evaporation a liquid [phosphorus acid] from which flashes of light and small explosions were produced on heating, that after long exposure to the air phosphorus emitted a strong smell [due to the production of ozone] which differed from that of the "smoke" [fumes] simultaneously given off, and that one part by weight of phosphorus, dissolved in spirit of wine, still exhibited the glow when over 600,000 times its weight of water had been added to this solution. Thus Boyle had at this time discovered all the important facts now known with regard to the glowing of phosphorus. His method of preparation was as follows: A large quantity of human urine was evaporated to the consistency of a thick syrup, incorporated with about three times its weight of sand and heated strongly in a retort, the phosphorus given off being collected under water in a receiver luted to the retort (*The Aerial Noctiluca*, 1680, pp. 105 f.). The carbon necessary for this process would be provided by the decomposition of organic matter present in the urine.

Probably Brand, Krafft, and Kunckel (about 1678) prepared phosphorus by the same method; but Boyle holds the chief place in the history of the study of phosphorus, as he discovered all the important facts relating to its glowing, and was the first to describe, though not to discover, a method of preparing it. The yield by this process was, however, slight, and phosphorus commanded a price of six guineas an ounce (Boyle's *Works*, ed. P. Shaw, 1725, III, p. 210 footnote). Indeed, it remained expensive until Scheele devised the method of preparing it from bone-ash in 1777 (*Collected Papers*, trans. L. Dobbin, 1931, pp. 312–13). But it was extensively studied by those who could afford to buy it. On account of its origins, its glow was thought to have some connection with the "flame of life," and its discovery intensified the importance already attached to urinous products such as the crystalline substance, sodium ammonium hydrogen phosphate, obtained by the decomposition of urine, and long known, on account of this, as microcosmic salt.

(See J. R. Partington, *A Short History of Chemistry*, London, 1948.)