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To

MARGOT

" But that which relies on calculation and measurement will be the best element in the soul?"

" Of course."

" Then that which opposes it will be one of the beggarly elements in us?"

" Inevitably."

PLATO, *Republic*.



Christian Huygens

CHRISTIAN HUYGENS

AND

THE DEVELOPMENT OF SCIENCE IN THE SEVENTEENTH CENTURY

By

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LONDON
EDWARD ARNOLD & CO.

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First published 1947
Reprinted 1950

Printed in Great Britain by
Billing and Sons Ltd., Guildford and Esher

PREFACE

THERE can be no doubt that Christian Huygens was one of the greatest scientific geniuses of all time. A man who transformed the telescope from being a toy into a powerful instrument of investigation, and this as a consequence of profound optical researches; who discovered Saturn's ring and the satellite Titan; who drew attention to the Nebula in Orion; who studied the problem of gravity in a quantitative manner, arriving at correct ideas about the effects of centrifugal force and the shape of the earth; who, in the great work *Horologium Oscillatorium*, founded the dynamics of systems and cleared up the whole subject of the compound pendulum and the tautochrone; who solved the outstanding problems concerned with collision of elastic bodies and out of much intractable work developed the general notion of energy and work; who is rightly regarded as the founder of the wave theory in light, and thus of physical optics—such a man deserves memory with the names of Galileo and Newton, and only the accidents of history have so far prevented this. It might be argued that Robert Hooke, who like Huygens was influenced by Descartes's teachings, is of equal stature and showed as much inventive genius and intuition. In the extraordinary range of their activities there is some similarity. The overwhelming difference lies in the fact that Huygens was a great mathematician and exponent of the quantitative method, whereas Hooke could never get beyond the first phase of a piece of work: that which led to the need of exact measurement and the discovery of mathematical relations.

Having made this claim for Huygens, it is natural to ask how he compares with Newton. It is a question which arises from time to time in these pages, and one to which there is no epigrammatic answer. In some senses it was Huygens's greatest misfortune to grow up under the powerful influence of Descartes, who was a great friend of his father, a frequent visitor to the

family, and master of at least one disciple who taught Huygens at the university. From Descartes too many of Huygens's hypotheses were taken, so much so that he might stand as the exact opposite of Newton, whose objection *hypotheses non fingo* he did so much to call forth. Looking at Huygens in this way it is easy to dismiss him as a "Cartesian", one whose ideas were largely superseded after the publication of Newton's *Principia*. But this would be a serious mistake. If he could not so brilliantly see the way to extend the sphere of natural law to the heavens, perceiving that the task of science is not to disclose a mechanism so much as to arrive at laws, he nevertheless did important work to prepare men of science for this modern attitude. If Newton owed nothing to Huygens, and he certainly owed exceedingly little, it is very probable that he was indebted in another way, for it may well have been the feeling of dissatisfaction with the position men like Huygens were reaching that drove Newton to make the new "instauration" Bacon had looked for—a renovation of natural philosophy. The progress of scientific explanation may then be seen to be a process of *leaving out* redundant elements, of emancipation from imaginary qualities, until one arrives at the really successful procedure of abstraction.

But Huygens was in all other senses an astonishingly modern thinker, and he had the disposition which sets out to face things as they are which marks the man of science as much as does the possession of specialized knowledge. As a scientific researcher he was the first of a new profession, and this permanent occupation with science brought its own characteristic attitude of mind. Colbert, Louis XIV's energetic and shrewd minister, subsidized scientific investigation for the first time in history when he obtained pensions for Huygens and the other scientists who formed the nucleus of the Académie Royale des Sciences. Of course Colbert had his eyes on commercial as much as on intellectual advances. Considering his jealousy of Dutch commerce Colbert was indeed fortunate to get as an ally of French power the most noted Dutch scientist of the age. It has indeed been a puzzling feature of Huygens's life that, having embraced French manners, delighting in the freedom that his position at the Bibliothèque du Roi gave him, he could even so remain in Paris in his official position during the years when Louis waged war on the Netherlands, determined if he could to break the

newly found independence of the Dutch for ever. More than this, Huygens in 1673 dedicated his *magnum opus*, the *Horologium Oscillatorium*, to his royal patron.

Huygens possessed a delicate constitution and was, it should be admitted, not of the stuff of which fighters are made. Like Pascal he suffered from frequent illness, like Spinoza there was a certain effeminacy about him. Again it may be argued that war in those days never concerned more than the limited class of professional soldier. If Huygens had quitted Paris the best he could have done towards the defence of Holland would have been to work in a diplomatic capacity as did his brother Constantin, or decipher codes as did the mathematician John Wallis in the civil war in England, or be killed like any ordinary soldier as was Gascoigne at Marston Moor. Men of the professional class in those days were not expected to give up their activities, and there can be little doubt that Huygens's scientific work benefited greatly from his life in Paris. At home at Voorburg, near the Hague, he lived the life of a recluse with only this stimulus of his correspondence with Paris and London. The investigation of the physical world appeared to Huygens and to nearly all men of science to be something of such moment that all events in the social and political world were merely so many inconvenient interruptions. That this was so in the seventeenth century is evident from the way in which the meetings which led to the foundation of the Royal Society were carried on during the civil war. From the very beginning the men of science felt themselves to be advancing the real causes of humanity and this longer view may well have been the one at which Huygens looked when, in 1672, he decided to remain in Paris.

From birth Christian Huygens grew up under the strongest French influences. In Paris, as a young man, he evidently imbibed the fashionable ideas which in religion tended to agnosticism and in morals pointed the way to greater freedom. His seriousness forbade the lax immorality then current. "The *beaux esprits* believed in God merely as a matter of courtesy and for reasons of state", wrote Garasse, but while this might be true, the men of science, especially in England, kept their religion. Huygens stands in contrast with the other great scientists of his time and in closer resemblance with some of the encyclopædists of the following century in that he turned away from Calvinism as from Catholicism.

In spite of the evident growth of power politics and the existence of widespread corruption, the period of Huygens's life was one of extraordinary optimism among the men of science. Science was the last activity to come from the humanistic impulse and it was to its devotees the most dazzling discovery to be attributed to man's freedom. " . . . this is the age ", wrote Henry Power, " wherein all men's souls are in a kind of fermentation, and the spirit of wisdom and learning begins to mount and free itself from those drossie and terrene Impediments where-with it hath been so long clogg'd . . . This is the age wherein (methinks) philosophy comes in with a Spring-tide . . . " The greater minds of the period were less confident over the inevitability of progress; Huygens in particular was especially cautious when asked to compare his own times with the age of Pericles. But a cruder spirit came to be associated with the men of science of the eighteenth and nineteenth centuries.

At least one modern writer has, however, condemned the " false modern emphasis " on the bold confidence and rebellious energy of the Renaissance, and has pointed out the amount of pessimism in English seventeenth century literature. On the Continent the inroads on religious belief seem in fact to have been more rapid than they were in England. Bishop Sprat, writing a defence of the Royal Society in 1667, believed that science would never undermine the socially acceptable beliefs of his time. He never dreamed that a "Universal Inquiry into things hitherto unquestioned " could have the unending consequences it has in fact produced. He obviously supposed, as did Descartes, though not Huygens, that the complete scientific account of the universe would in its essentials require the labours of only one or two generations of gifted intelligences. In these labours the experimentalists would, thought Sprat, have always before their eyes the " Beauty, Contrivance and Order of God's works ". As the Master of Trinity College has written, " God was to be praised by studying the plan of His creation, but no further attempt was to be made to fit the findings of science into the scheme of theology, as the schoolmen of old had striven so long and painfully to do." This was Newton's position, it was Boyle's, but it was not Huygens's. He alone among the men of science of his day found the temper of scientific enquiry alien to orthodox religious faith. As it was, both in France and England there were divines who supported the plan of organized enquiry

in natural philosophy. Liberal minded abbés and protestant bishops gave their approval to scientific research for the glory of God and the service of man. Occasionally the former tended to lose their nerve and clung then for stability to Descartes's "system".

The particular feature of Huygens's thought which was at the same time his strength and weakness was his concern for particular problems and his distrust of all speculative generalizations. This distrust he no doubt acquired during his study of Descartes's writings. His attitude towards Spinoza's ideas may be explained in this way, for it would otherwise have been expected that these ideas would have gained his sympathy, for Spinoza, of all great religious writers, has come nearest to expressing the scientific attitude to the world. But Huygens distrusted the system which Spinoza elaborated and no doubt for the reason that Spinoza sought to apply the Cartesian method. This method, Huygens saw, is unable to give us an understanding of nature, and he himself made great contributions to the new scientific method. The fact that he personally despised Spinoza seems to be explained by a sense of superiority which rested on social rather than on intellectual considerations. It was not, of course, a democratic age.

In his scientific work Huygens was the greatest mechanist of the seventeenth century. He combined Galileo's mathematical treatment of phenomena with Descartes's vision of the ultimate design of nature. Beginning as an ardent Cartesian who sought to correct the more glaring errors of the system, he ended as one of its sharpest critics. The development in the seventeenth century of Dynamics and Astronomy beyond the stage of geometrical description demanded new inductive principles of correlation; the ideas of mass, weight, momentum, force, and work were finally clarified in Huygens's treatment of the phenomena of impact, centripetal force and the first dynamical system ever studied—the compound pendulum. In Astronomy Huygens explained the appearances of Saturn, until 1656 the greatest anomaly of the Copernican system. His eminence as an observer was due to the high quality of his telescopes and this, in part, resulted from his thorough theoretical researches on the problems first attacked by Kepler, Snell and Descartes. It is well known that physical optics practically took its rise from Huygens's *Traité de la Lumière*.

The growth of interest in the history of science may be considered to be in a sense an outcome of the increasing specialization of science itself. So much is commonly ignored in concentrating on the discipline of science, that for education in the broader sense, when once the demands of life are allowed to supersede merely professional ones, something more is needed. Many have felt that the history of science may provide an important humanistic element. A protest may here be made against the practice of inserting historical notes in scientific textbooks without regard for the conflict of old and modern ideas. Once a subject has become well developed, the logical and not the historical method is to be desired, for so much of the earlier science can be properly understood only with a really adequate study. The great scientist of the past tends soon to appear a distant and indeed thoroughly dead sort of figure. The modern scientist to a large extent accepts his reputation on trust and has little time and often less inclination to read the original work. How many now read Galileo's *Discourses* or Newton's *Principia*? It is otherwise in art and in literature. If science is to become a more widely accepted means of education (in the sense of a form of culture) there is need of more works on its history. A modern estimate can do much to revivify the past and present these great men, its creators, in a clearer light.

As an account in English of the work of Huygens this study is to be regarded as only a beginning. The *Oeuvres Complètes de Christiaan Huygens*, published by the Societé Hollandaise des Sciences, comprise more than twenty magnificent volumes and contain all the material for a definitive work; beside them the present book must appear almost insignificant. It is greatly to be hoped that before long a large work on this great subject will be written by a scholar of the requisite stature. Writing as one whose training has been principally in chemistry the author has met with many difficulties in Huygens's works. It need hardly be said that they are properly to be studied by a mathematician, while the subject as a whole requires a combination of historian and Latin scholar, physicist and philosopher, which it must be rare to achieve.

A.E.B.

CLIFTON, 1947

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PART I

THE LIFE OF CHRISTIAN HUYGENS

I

CHRISTIAN HUYGENS has been a strangely neglected figure—apart from the study he has rightly received in his native Holland. A man of the greatest scientific genius without any doubt, he was one in whom great sagacity and mathematical power went side by side with a feeling for elegance and form in the interpretation we make of Nature, so much so, that it is without surprise that we find he was devoted to music and the arts and was by no means the type of narrow research worker that later scientific studies did for a time produce, and still produce in some measure. Huygens was a professional scientist in an age when the boundaries of Science were scarcely drawn, and his interest lies as much in his general outlook as in his specialized studies.

Huygens had not the religious feeling of a Spinoza or the sensitivity of a Pascal, nor was he a philosopher of the stature of Descartes or a mathematician of the rank of Leibnitz. In an age when the human mind was making great marches into the territory of natural philosophy, Huygens's energies were thrown now into the study of applied mathematics, now into optical researches or astronomy; and he managed somehow to pursue the most strikingly original researches in several subjects quite simultaneously, so that in his note-books matters of the most varied kind jostle one another in profusion, and a very large volume indeed would be needed to do justice to his labours. What is of chief significance to-day can be reduced to much smaller limits, and the reader who wishes for more must go to the great volumes published by the Société Hollandaise des Sciences under the auspices of the Dutch Government.

Here we are concerned rather to look back for a space on that interesting period in Europe between the death of Galileo in

1642 and the rise to fame of Newton, a period in which Huygens, in fact, stood unchallenged as the greatest man of science of the age.

It has been remarked[1] that "In 1600 the educated Englishman's mind and world were more than half medieval; by 1660 they were more than half modern". And this remark need not have been limited to Englishmen. On the Continent also, about the middle of the century, a certain profoundly important change was becoming visible. It was, perhaps, in the years following 1670 that the break-away from authoritative teachings —of Descartes as of the schoolmen—became the feature of the really important scientific theories. Galileo and Huygens both struggled to make use of teaching they received in their youth, and both failed; they were each forced in some degree to rely on their own powers. Indeed, underneath all successful scientific work there lies a great deal of experiment in failure.

One must read Dante, or toil over Thomas Aquinas, to get a picture of the universe as it was conceived by educated men in the Middle Ages. The sheer verbalism of all argument about the world repels and astonishes the modern reader, but there was an undeniable attractiveness in the notion of a Cosmos: the "idea of a hierarchically-ordered finite world structure", a world in which all was made for man and consequently one in which clear and simple reasons existed why things are as they are. What we see as an appeal to objectivity must then have seemed to some to be pure obstinacy and blindness, for what the men of science really abolished was not so much an over-rational world structure as the appeal to feeling in the making of explanations. The new studies offered at first no more satisfaction than that which could be found in the agreement of theory with measurement. Nevertheless, scientific explanations did not get a reputation for their "inhuman" quality until the eighteenth century, when many physical theorems were generalized in abstract mathematical form.

Early in the century Descartes worked out an ingenious and even æsthetically satisfying system which welded natural science on to the structure of a philosophical theory about the nature of matter and of space, and some reference to this system must be made in later pages. The chief point about Descartes's teaching,

[1] Douglas Bush: *English Literature in the Earlier Seventeenth Century*, (1945).

if it were accepted, was that experiment and observation could soon be dispensed with and the human mind could rest satisfied with the knowledge it could gain through a rationale worked out by philosophers. So seductive was his reasoning, and so persuasive the arrangement of the arguments, that both in France and England there were soon many ardent Cartesians who were distinguished by the ease with which they accounted (in a general way) for natural phenomena. Since space was supposed to be full of a "subtle matter" and this moved around each planet in a kind of vortex, it was easy to imagine various effects as resulting from the properties of this medium. And Huygens was himself for many years a Cartesian. The essays produced by Descartes were a flirtation with the mathematical treatment of observations begun by Galileo, only they went far further and cast the human mind in great voyages of imagination—further, in fact, than it was yet ready to go. It is always an interesting question, therefore, how Huygens came to be a strong critic of Cartesianism, and on the other hand, why he rejected Newton's treatment of gravitation and even at the end of his life had not thrown overboard the whole Cartesian apparatus. Of all the events in Huygens's life when one would give much to know what happened, there is an occasion of which one gets only a faint glimpse: Huygens and Newton getting into a stage coach at seven o'clock on a July morning in 1689, to go from Cambridge to London. Huygens was then sixty and his zeal and lively curiosity were unabated; Newton was forty-seven, and everywhere acclaimed as the author of the magnificent *Principia*—though it had to be confessed that only a handful of men really knew what it was all about. Huygens had left Holland in poor health in order to see Newton and to visit old friends among the English men of science. But all that can safely be connected with this meeting is the fact that Newton subsequently produced a further study of the Cartesian vortices and, on the other hand, Huygens began to object to Leibnitz's use of them. As the coach rolled on its way to London, it may be that Huygens was turning over in his mind the final objections to any further developments of Descartes's ideas. His own work had led far in this direction and the end of it all seemed to be that Descartes's ventures in physics had been pure romance, "un beau roman de physique" as Leibnitz himself concluded. Or is such conjecture too dangerous? Huygens, with his only moderate English and

his weakness for a picturable sort of explanation may have made little of a taciturn Newton; he recorded nothing of interest from the meeting.

II

Unlike Newton, Christian Huygens came of a family which had already shown genius. His father, Constantin, was extraordinarily brilliant; a poet, student of natural philosophy, classical scholar and diplomat, he typified the conception of culture at its best at the beginning of the century. As secretary to the Prince of Orange, Frederick Henry, he must be considered important in the guiding of the country through difficult times. In this, however, his own father, an earlier Christian, stood as an example, for he had been secretary to William the Silent in the eventful years after 1578. This Christian was a native of the Southern Low Countries, while his wife, Susanna Hoefnagel, was of Antwerp, though at the time of her marriage a protestant refugee from Amsterdam. The two sons, Maurice and Constantin, were born in troubled times, the latter on September 4th, 1596, at the Hague.

The last quarter of the sixteenth century saw the independance of the seven northern provinces of the Netherlands regained after an eighty years' struggle with Spanish power. In the South, Spain and Catholicism continued to dominate; in the North, religious and political liberation occurred together and there grew up a deep mistrust of all hierarchial powers; even the doctrines of Luther were rejected because they acknowledged the authority of the State in religion. A new Calvinist commonwealth now existed, and its rise has been described in the pages of Motley's *Rise of the Dutch Republic*. The assassination of William the Silent, in 1584, came after he had accomplished his great task for he had, as Motley says, "planted a free commonwealth under the very battery of the Inquisition in defiance of the most powerful empire existing".

It is interesting to look back at these important events which came close to the life of the Huygens family. When Maurice of Nassau was engaged in defeating the Spaniards in the open field, Constantin Huygens, father of the scientist, was receiving a careful and thorough education as a boy. This Huygens showed quite a distinction in mathematical work but all the influences

of his life were in the direction of the courtier and diplomat. He was often at the court of Louise de Coligny, the widow of William the Silent, and he accordingly spoke French from boyhood. He completed a course of Law at Leyden University and then was introduced at twenty-one to the life of diplomacy. This Huygens became by far the most well-known member of the family up to the mid-seventeenth century. His all-round culture has been mentioned, and he did in fact become known all over Holland, and in England, as a latinist and poet, as an amateur of music and painting, and as a student of philosophy. He was, besides, a close friend of Descartes and, at length, best known of the leaders of contemporary thought in the Republic: "no Dutchman commanded a more European culture; no Dutchman was more thoroughly Dutch". After their first meeting, Descartes wrote of him " . . . despite what I heard of him, I could not believe that a single mind could occupy itself with so many things and acquit itself so well of them all".

Christian Huygens's father, then, was a man of outstanding ability and brilliance and he was very well known in England. He studied at Oxford for a time and became an intimate friend of John Donne. He played the lute at the court of James I, and in 1622 received an English knighthood. Nor was this brilliance a mere glitter, the effect produced by a versatile and fashionable courtier. Constantin Huygens corresponded for years with Descartes, with Mersenne, the great intermediary of men of science of that time, with Diodati, a friend of Galileo, and with many well-known mathematicians, notably Schooten the elder. In his MSS. have been found notes on Euclid's propositions and records of his study of optics. When Golius succeeded Snell at Leyden, Constantin Huygens recommended him to apply himself to optics. "The consequences of the law of refraction [formulated by Snell in 1621] have not been sufficiently studied by anyone," he wrote. He himself is said to have attempted to grind lenses to the forms proposed by Descartes—the surfaces being of elliptical or hyperbolic section instead of spherical. Descartes had concluded that such lenses would be free from spherical aberration but Huygens (or the skilled mechanic employed by him) found the work impossible with the ordinary tools then used. His indirect influence in scientific work was undoubtedly of greater significance: without his encouragement Descartes might never have published his *Dioptrique*. The

philosopher was induced to overcome his well-known hesitancy only through the efforts of Constantin Huygens and Mersenne.

This versatile man of letters and diplomat in 1627 married his cousin, Susanna van Baerle, daughter of a wealthy merchant of Amsterdam and by all accounts an intelligent and cultivated woman. The children of this marriage, which must interest geneticists, were Constantin (1628), Christian (1629), Louis and Philip, the last of whom died young, and Susanna. In 1637, after only ten years of married life, the mother of this family herself died. Another cousin took over the care of the family, which removed to a newly built country house at Voorburg, close to the Hague. Here, when he had recovered from the death of his wife, Constantin received officers of the French army, French diplomats and men of letters. Here Descartes himself made occasional visits and remarked on the prowess of young Christian in mathematics, a study in which he complained he saw no great progress.

Descartes spent a good deal of time in Holland and did much of his more important work in the quiet of the country. Even in Holland, however, he did not feel sufficiently secure to bring out his treatise *Le Monde* and it was not until 1637 that his *Discourse on Method* appeared. But it is easy to imagine the great influence of Descartes on the intellectual family at Voorburg in those years just succeeding the publication of the famous *Discourse.* The work itself shows the appeal of Descartes's mode of argument and, to a generation who read and sympathized with Campanella's *Defence of Galileo,* it must have seemed that Descartes was indeed the apostle of intellectual freedom. Campanella's tract, composed in a Neapolitan dungeon in 1616, was printed at Frankfurt in 1622, and during the next thirty years it was widely read by educated men all over Europe. Its courageous stand for freedom of enquiry and for the truth of the Copernican theory was a source of inspiration. For it is clear that a generation which could revere Galileo did so because their minds were already partly prepared by earlier critics of Aristotle: Benedetti, Stevinus and others. In Campanella there was a vigour and boldness which recalled Giordano Bruno. Bruno and Campanella held that there are an infinite number of worlds, and if in Descartes's writings this doctrine as well as that of Copernicus was taught with great caution there can be no doubt

that this was through circumspection. Descartes was a cautious man but very probably in conversation he was bolder.

Constantin Huygens was extremely proud of his two eldest sons, who early showed intellectual brilliance. They were taught at home by a private tutor until Christian was sixteen. This education included singing, playing the lute, and the composition of Latin verses. Like Newton, as a young boy Christian loved drawing and the making of mechanical models on which he spent much labour and ingenuity. So much so that his tutor felt misgivings; such practical work was after all an inferior and even a dubious sort of occupation for a young man of family and position. From the beginning, however, Christian showed promise of great skill in geometry while his brother, Constantin, excelled rather in literary compositions. Descartes was much impressed with some very early work of Christian's and he saw that great things might be expected from this rather serious boy with the rather pale face and the large dark eyes. Christian was rather delicate and by nature gentle, and his sensitivity seemed almost feminine to his father, who seems to have been fortunate in possessing an unflagging and exuberant vigour, quite different in character from his son.

Characteristically enough, the first experiments of the youthful Christian were in mathematics, and this is typical of him, for he rarely ventured publications on other than abstract and somewhat theoretical subjects. But the influence of a cultured and enlightened society remained with him, and his interests, early determined, lasted unchanged all his life.

In 1645, when he was sixteen, Christian and his brother entered the University of Leyden. Here they studied Mathematics as well as Law, the younger Schooten, a protégé of Descartes's, then being professor. Schooten was an able mathematician and Christian acquired the reputation of being his best pupil. Mathematics was a subject which included what we would now call mechanics and, for example, in Stevin's *Hypomnemata,* a work in six volumes, there are discussions of centres of gravity, levers, simple machines and hydrostatics. Christian's father was clear about the supreme importance of mathematical training. In 1644 Descartes had published his *Principia,* a bold attempt to reduce all the changes of Nature to mechanistic processes and he, it was well known, exalted the study of the subject. Radical changes were taking place in men's

ideas and during his time at Leyden Christian lived in an atmosphere of intellectual ferment. The ideas of Descartes were hotly contested by the Aristotelians and to such an extent that in 1646 and 1647 the university almost became a battlefield. Unfortunately, there are only scanty records of Huygens's reactions to these experiences. Regarding Descartes's *Principia* he many years later remembered the deep impression it made on him. "It seemed to me when I first read this book, the *Principia*, the first time," he wrote, "that everything in the world became clearer and I was sure that when I found some difficulty that it was my fault that I did not understand this thought. I was then only fifteen or sixteen years old."

Descartes's ideas were strongly represented in Holland. Renier, one of his disciples, taught Cartesian philosophy at Leyden for a time and later went to Utrecht. Here he had great influence and was followed by Regius, one of his own pupils. Aristotelian philosophy was associated with the Jesuits and nowhere more than in northern Holland was their influence more strongly resisted. Nevertheless, even in Holland, freedom of thought was not absolute and only a few years previously the Aristotelians had scored notable victories by arousing suspicion as to the religious consequences of Descartes's teachings. Cartesianism owed its wide appeal to the need felt for a new celestial mechanics after the acceptance of the ideas of Copernicus. This, apparently, Descartes's theory supplied. Moreover, Aristotle's outlook in natural science was in the main teleological. It was felt that if the guiding principle of teleology were abandoned some way of expressing the determinism of events must be found. On this point Descartes's analysis proved less sound but his system as a whole was ingenious and even æsthetically satisfying.

In 1647, after two years at Leyden, Christian Huygens joined his brother at the College at Breda. This college, founded by Frederick Henry, seems to have achieved a temporary fame but it came to an end during the century. Descartes seems to have taken some interest in the place and certainly the forces of Aristotelianism were there unable to challenge the new philosophy. John Pell, an Englishman, taught mathematics and was a man of quite high reputation. It was fortunate that, after Schooten, Huygens had so able a teacher.

As soon as Huygens's period at Breda was completed he made a number of journeys, first going to Denmark in the company of

the Count of Nassau-Siegen and later, with Constantin, to Frisia, Spa and Rome. When in Denmark it was a great disappointment to him that the weather made it impossible to reach Stockholm for Descartes was then living at the court of Queen Christina.

Travel and a thorough education were, however, not the only elements which made up the pattern of Christian's early years. Most important, perhaps, of all was the correspondence he took up with Père Mersenne, who was next in importance to Descartes among his father's acquaintance in the centre of the learned world. Duhem has described Mersenne as a man of insatiable curiosity and the exuberant imagination of the artist. He was at this time the great intermediary for scientific communications between the chief centres of experiment. He popularized much of Galileo's work and did much to thrash out those fundamental notions on which seventeenth century mechanics was based. Men like Descartes, Gassendi, Fermat and Pascal met together at the cell of the Minorite father in Paris and this group has been described as the origin of the Académie Royale des Sciences. Mersenne was indeed a remarkable man, for he retained the esteem of both Church and the scientific world; ". . . he did not believe all his religion," Pineau wrote to Rivet, "he was one of those who are glad enough to see church service done . . . he dared not often repeat his breviary for fear of spoiling his good Latin." He was not himself a great originator. Pascal possessed for mathematical and scientific work all the qualities which Mersennet lacked: a profound penetration, logical rigour, critical acuity, but Mersenne saw clearly which problems then mattered most and Huygens was indebted to him for many of the subjects of his early researches.

Aristotle, whose mechanics was the weakest part of his natural science, had supposed that heavy bodies fall towards the centre of the earth because this is their "natural" place. The heavier a body is, the faster it moves towards the earth. If it were to fall through a hole passing through the centre of the earth it would come to a standstill on reaching the centre. As early as 1585 Benedetti had protested against this. He saw, in a general way, that the inertia of the mass would carry it past the midpoint and that it would in fact oscillate after the manner of a pendulum bob. Stevin, with greater certainty than in the case of Galileo, is known to have experimented by dropping large and small weights simultaneously and showing that they reached the

ground together. Galileo made a more thorough examination of naturally accelerated motion and calculated the distances travelled in successive seconds by a freely falling body. Mersenne, in an early letter to Huygens, questioned if in fact the mass did not in some way determine the limit of the velocity which could be imparted. Huygens explained that his objections were all based on observations of air resistance and gave such an able exposition of what is now termed Newton's first law of motion that Mersenne gave him ungrudging praise: "I assure you that I think so highly of your demonstration concerning falling bodies that I believe Galileo would have been delighted to have you as his follower." Mersenne went on to set Huygens the problem of finding the form taken up by a rope hanging from its two ends which are fixed at the same height and some distance apart. Huygens did not solve this mathematical problem until he recurred to it late in life but he studied the disposition of weights along the rope which would give it a parabolic form. He also became interested in Mersenne's famous problem of determining the centre of percussion of suspended bodies. This most important problem was given its first general solution by Huygens many years later.

Young Huygens was delighted with these letters, which he received " with joy and avidity ". His father noted with approval the penetration with which young Christian, then only seventeen, tackled problems then exercising the world's foremost men of science. In December 1646 Christian wrote that he was occupied with problems of centres of gravity and with modern demonstrations of some of Archimedes' propositions on the sphere and cylinder—a remark which illuminates the nature of his early training—" but nothing yet concerning centres of percussion of which you recently wrote. However, I shall not fail to do all that I can to find the demonstration although, up to the present, it seems to me to surpass my ability . . ." Mersenne acknowledged that he also could not see how a single rule could satisfy the variety of figures for which the centre of percussion (or of oscillation) was required. The problem was that of finding a formula which would make it possible to calculate for any suspended body the length of the simple pendulum which would have the same period of oscillation. An experimental solution could, of course, be found but this was not acceptable as an answer. It is at first sight surprising that a great deal of interest should be

aroused by so academic a problem. This was because the problem was one concerning a dynamical system (as opposed to a single mass) and it was obvious that a new approach was needed. Problems of this sort led to the development of the calculus by Newton and Leibnitz. Huygens, however, obtained a solution in advance of either of them, although it was without the use of their modern methods.

Some interesting matters are discussed in the correspondence of Mersenne and the elder Constantin Huygens. Christian, because of his precociousness, is sometimes referred to as a modern " Archimedes ". Mersenne wrote about the new work of the young Pascal, then twenty-five, the problem of the nature of the vacuum, the development of the telescope and the most recent astronomical observations. There was a widely accepted belief that a true vacuum is contrary to nature and this made it very necessary to explain the well-known experimental results obtained by Viviani and Torricelli in 1643.

The followers of Descartes were in obvious difficulties because Descartes rejected the atomic doctrine and with it the notion of a void. Since Gassendi was reviving, at least in part, the atomic doctrine of Epicurus and considered it a profound philosophical necessity that a vacuum should be possible, this apparently recondite and academic matter aroused vigorous controversy. Galileo attributed the more or less constant height of the barometer to an equilibrium between the weight of the column of liquid and an attractive " force " acting upwards. This force was, of course, quite an illusion. Torricelli and Viviani verified that the relative heights of liquid which could be supported in a barometer tube varied inversely with the densities and in 1644 Torricelli really gave the correct explanation based on the pressure of the atmosphere. Four years later Pascal's explanation of the behaviour of the barometer was put forward, after the experiment carried out for him on the Puy-de-Dôme in September 1648, but his views were by no means universally accepted. Quite a literature was produced in disproof of the existence of the vacuum and even Constantin Huygens, who confessed himself most anxious to " penetrate all the mystery ", found the new explanation very contrary to his inclinations, which were all for Cartesianism.

It is important to realize the fascination which all but a few critical spirits found in Descartes's natural philosophy (see p. 109). In a sense, of course, Cartesianism was anti-scientific. At a time

when the trend of natural philosophy was in the direction of empiricism, Descartes emphasized the great limitations of the empirical method. While he scorned scholastic logic he considered that mere empiricism was futile and that his discovery of analytical geometry illustrated the true method by which physical problems of all kinds involving motion in space could be attacked. He believed that the way was open to reduce all phenomena to the terms of geometrical description. From the postulates of space and motion, without any assumptions as to the innate properties of matter, he hoped, by successive applications of his intuitive method to isolated problems, to build up an account of all the phenomena of the Cosmos. The *a priorism* of Descartes's method is thus anti-scientific. On the other hand it must be remembered that Aristotelian science was concerned with logical rather than spatial relations. Descartes, on the other hand, has been well described by the remark that he was "the author and prophet of mechanism". With Galileo he asserted the belief that the laws of Nature are both simple and open to discovery. The danger for a youthful student such as Huygens was that Descartes paid too little regard for what have been called "stubborn and irreducible facts" and that he strayed too far from the path of scientific work in undertaking to heal the schism between the natural and the revealed.

In the famous vortex theory of the *Principia Philosophiæ* (1644) Descartes supposed all space to be filled with a "subtle matter" which moved with the planets in their paths. He made brilliant play with this medium and used it to work out plausible explanations of gravity and magnetism as well as the action of the barometer. Light was treated as an action or as an inclination to move, possessed by the particles of the subtle matter. From this explanation, comparable with the idea of pressure in a liquid or of impact amongst particles in motion, Descartes attempted to derive the laws of reflection and refraction. The same spirit was shown in the mechanical explanations offered in the *Meteors*: atmospheric phenomena and the rainbow were given explanations based on known—or partly known—scientific principles. In the *Principia Philosophiæ* Descartes dealt, among other things, with the nature of matter and the general laws of motion. The whole of this "system" rested on insecure foundations and there was a temptation to ignore small but "stubborn and irreducible facts" which did not fit in. Since a perfect vacuum was

something to which Descartes denied existence the Torricellian space in a barometer tube was supposed to be filled with the Cartesian subtle matter which, like the ether of the nineteenth century scientists, penetrated almost everywhere. A crucial experiment was performed in which a sealed and empty bladder was placed in an evacuated tube. The fact that the bladder expanded was explained by Huygens as due to a small amount of residual air: Roberval, an original and controversial writer, also considered the experiment disproved rather than supported the Cartesian theory.

Mersenne died in September 1648, but his influence on Huygens had been important. Although he was no great physicist or mathematician he stimulated the criticism of ideas; he was, for example, strongly opposed to Descartes's well-known treatment of animals as automata. In the years between 1648 and 1657 Huygens, from being a youthful admirer of Descartes's philosophy, became more and more critical. He wrote frequently to his old teacher Schooten and to the mathematician Slusius about Descartes's demonstrably false laws of impact between elastic bodies. The laws, he wrote, did not agree with any experiments and the fifth law conflicted with the second. Before 1656 he had completed his own important work on the subject (see p. 109) but some twelve years elapsed before he communicated his conclusions to contemporary men of science. The complete treatise, *De Motu Corporum ex Percussione*, was not published during his lifetime.

The first published work of Huygens came out, however, as early as 1651, when he was only twenty-two. This was his *Cyclometriæ*, a treatise written to show up the fallacies of the mathematician Gregory de St. Vincent committed in a book of 1647 where Gregory had claimed to have developed no less than four different ways of "squaring the circle". The task of replying to Huygens's serious objections was left to certain pupils, and notably to Ainscom. The result was considerable prestige for Huygens, for he was seen to have proved his case. The larger work, *De Circuli Magnitudine Inventa*, which appeared in 1654, it is safe to say, assured him of a place amongst the leading mathematicians of the day. He was hailed as the reborn Vieta and compared with Pappus and Apollonius, two giants of classical Greek geometry. The comparison was, in fact, not inept. In the years following 1652 Huygens spent a lot of time on re-

ducing to algebraic analysis problems which Archimedes, Nichomedes, and other Greek mathematicians had been able to solve only through geometry. Without these early studies it may be doubted if Huygens could have succeeded in the great problems he was later to tackle.

Before the death of Mersenne Christian had hopes of going to Paris in the company of his father, but the idea was postponed. In 1649 came the first of two revolts by the nobility against the rule of Anne of Austria and Mazarin—during the minority of Louis XIV. Until 1653 the situation continued to be uncertain; twice Mazarin was a fugitive, Anne was hunted from Paris and the monarchy was in jeopardy. The rebellious nobility were in league with Spain and the times were not propitious for the Huygens' visit. Not until 1655 was the long-projected visit made. The intervening five years were spent chiefly on Huygens's early researches, interrupted only by another journey to Denmark and some time spent in the Low Countries. Huygens's important work on telescope construction dates from these days. The first telescopes were made in Holland early in the century, but they were very imperfect and it is remarkable that observations such as those of Galileo were ever made with such instruments. The task of improving the telescope occupied Huygens throughout his life and in this he was encouraged by his father and had, from time to time, the skilful collaboration of his elder brother. By means of his own telescopes Huygens made his important observations on Saturn and a copy of a letter written at this time bears two rough sketches, one of Jupiter and the other of Saturn showing appendages. The contents of the letter do not relate to these matters and the date of the drawings is uncertain. The discovery of Saturn's ring cannot be put earlier than February or March 1656. Before he went to Paris in July 1655, however, Huygens had made the interesting discovery of a satellite of Saturn. The study of this anomalous planet whose irregular contour was such a mystery was continued by his brother Constantin in his absence.

The fertility of Huygens's mind at this period was truly astonishing and it can be matched only by comparison with Newton. Fundamental research in pure and applied mathematics, optical studies including important work on the theory of lens systems, the invention of an improved eye-piece for the astronomical telescope, and to crown the practical side of his

work, the discovery of Titan, all belong to this period of his life. Yet there was a curious weakness in this energetic mind, a flaw implanted perhaps by Descartes's brilliant philosophizing. For the man of science who was himself a few months later to discover Saturn's ring seems to have concluded that, with the discovery of six planets and six satellites, the human mind had reached the limits of the solar system. This preoccupation with numbers reminds one of Kepler and shows how persistent were old currents of thought.

III

In 1655 Louis XIV was only seventeen and France continued to be governed by Mazarin. The second "Fronde" was at an end but this disorder and scramble for power left its results, abortive though it had been. The conditions were precisely those which could not but impress Louis with the need of becoming master and his reign from 1661 onwards was characterized by the greatest absolutism.

For a son of a noted Dutch diplomat and man of letters young Huygens remained extraordinarily aloof from the turmoil of events. His habits soon became those of a scholar and at twenty-six there was a marked vein of seriousness in his pursuits. During his five months in Paris, however, he revelled in the opportunities of pursuing the arts as well as the sciences. Music, the drama, and the society of intellectual and artistic people made life in the capital extremely interesting. At the country house of Conrart, the protestant secretary of the French Academy, he met Jean Chapelain, a mediocre but popular poet and a man of cultivated tastes, and Marie Perriquet, an attractive young woman who seems to have shown interest and some ability in scientific problems. The comic dramatist Scarron, the astronomer Boulliau and the philosopher Gassendi were also among his new acquaintance.

Gassendi was at this time an old man. It is not clear how much Huygens could have been directly influenced by the philosopher on the occasions when they met, but his indirect influence on Huygens and several other men of science was considerable. Gassendi was at that time an important, perhaps the most important, opponent to Cartesian teachings. His objections, moreover, reflected the influence of Galileo: logical deductions for

him were useful only so long as they did not conflict with the physical facts. For Descartes mathematical and logical deductions could be valid irrespective of verification from experience. The fact that this appears to us an impossible and irresponsible attitude must be attributed to the influence of Gassendi, Huygens and Leibnitz as well as of Newton in the history of thought. Even in a more detailed way, however, Gassendi had an important influence on Huygens. He held an atomic theory which was later developed by Boyle and with this went a belief in scientific materialism. To Descartes's "cogito ergo sum" he objected that existence might be inferred from any other action besides thinking. Unfortunately Gassendi's name has become linked, not only with the revival of atomic doctrines, but also with the doctrine of mechanism. As a matter of fact Gassendi did not take up this extreme position. The atoms of bodies, he held, were not eternal or unproduced or moving of their own accord—a problem which he seems to have viewed with the same perplexity as we feel now for the rotation of nebulæ. God, for Gassendi, was the creator and first cause, He was over and above the physical world.

From 1653 up to his death in October 1655 Gassendi lived at the house of Habert de Montmor, a wealthy amateur of the sciences, who gathered together at his house, 7 rue Vieille du Temple, many who had formerly met at the cell of Mersenne. This "Montmorian Academy" was an important forerunner of the Académie Royale des Sciences. As in London and in Florence, an informal gathering of men, free from the "systems" of the universities, committed to no philosophy save that of enquiry, founded the modern organization of scientific work. Besides enquiring into new phenomena something was done to conserve the past. Gassendi wrote a life of Tycho Brahe and Copernicus. It is worth noting that he preferred the cosmology of the former to that of the latter. His pupil, the poet Chapelain, took a keen interest in the progress of scientific studies and formed a strong friendship with young Huygens. Chapelain knew little science or mathematics but his zeal was great and he assisted Sorbière, permanent secretary to the Montmorian Academy, to draw up its rules and maintain its foreign correspondence. Chapelain's letters to Huygens, after the latter's return to the Hague, show that he kept his master's atomic doctrines to the fore and conscientiously maintained a critical attitude towards Cartesianism.

After Gassendi's death in 1655, in fact, his place in Chapelain's life was taken by the newcomer Huygens. Unfortunately, the death of Gassendi was the beginning of a series of troubles for Montmor. First he lost his child and then his wife fell ill; finally, when she was recovering, his sister died. These misfortunes brought to an end, temporarily, the meetings held at his house. There may have been other reasons, for Gassendi's assistant, de la Poterie, and Huygens disliked each other, and Pierre Petit and Thévenot were hostile to Sorbière. Numerous petty squabbles occurred and marred the work of the "Academy" for a period. Nor were other societies in Paris more successful. Thévenot later started discussions at his own house (in 1663) but these ended in 1664 because of the expense, part of which was incurred by keeping the mathematician, Bernard Frenicle de Bessy, and the anatomist, Steno, at his house. Groups supported for a time by Henri Justel or the Abbé Bourdelot suffered no better. It was through experience of this sort, as will be seen, that leading amateurs of science were brought to the conclusion that the Government should be responsible for the maintenance of a permanent academy.

At the time when Huygens first visited the Montmor group there was, as usual, little contact with the Sorbonne. Some of the university teachers attended meetings, but perhaps as much out of suspicion as out of sympathy. The ecclesiastical authority of the university colleges felt itself challenged by Cartesianism, and Gassendi's views were no more popular. Nevertheless, a rationalist sect existed within the Church and this was not wholly opposed to new ideas in natural philosophy. Among its supporters the writings of Du Vair and Descartes were increasingly popular. But the support of neither the Jesuits nor their opponents the Jansenists was of any value for the cause of science.

Huygens enjoyed the meetings of the Montmor group and was anxious to prolong his stay in Paris. From the mathematicians he learned of problems on probability which occupied Fermat and Pascal about this time. It is of interest that his own little treatise on the subject, written after returning to Holland, became a classic. Before he left France he told Boulliau and Chapelain of his discovery of Saturn's satellite and the latter urged on him the importance of publishing the details. Huygens obviously wished to settle the problem of the ring first; he was

pleased at the discovery that already his telescopes were as good as any that were to be had in Paris.

IV

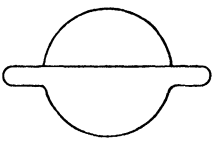
In 1610 Galileo made a number of important telescopic observations. In January he found that Jupiter had four satellites; in July he made out the appearance of Saturn as consisting of "three spheres which almost touch each other, which never change their relative positions, and are arranged in a row along the zodiac so that the middle sphere is three times as large as the others". In the same year he distinguished separate stars in the Milky Way and saw the phases of Venus. His work left the Copernican theory in a much stronger position but certain unresolved doubts still remained. This "tri-spherical" form of Saturn for example was something completely anomalous. Were these outer spheres a peculiar type of moon? On the Copernican theory it appeared probable that other planets besides Jupiter would be found to possess satellites.

This is where Huygens's work commenced. At the age of twenty-six he made a search for satellites of Venus and Mars, but in vain. Turning his own twelve-foot telescope on Saturn, it appeared to him much as it did to Galileo. The nature of the lateral bodies or appendages could not be distinguished. Leaving this problem on one side, however, Huygens at eight o'clock in the evening of March 25th, 1655, noticed a small star very near the line passing through the planet and its appendages or "anses". His suspicions that this would prove to be a satellite were strengthened during the following days, for the position of the star altered. After a few weeks Huygens decided that the period of the satellite (Titan) was sixteen days and four hours.

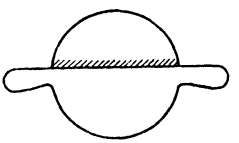
This discovery, as has been mentioned, was made before Huygens went to Paris in July. No doubt several of Montmor's group discussed with him the puzzling problem left by Galileo. Hevelius, the noted astromer of Danzig, confirmed the planet's peculiarities, his telescope being not much better than that of Galileo. Huygens recognized that everything depended on improving the instrument. The diverging eyepiece of the Galilean telescope restricts the field of view and Scheiner, about 1630, successfully made the first instrument having two or three convex lenses. Telescopes of this type have a better field of view but are more

subject to the defect of chromatic aberration, a matter not then understood. Huygens went to the lens maker Mocchi while he was in Paris and learnt all he could from him. When he returned to the Hague he worked continuously on lenses, trying amongst other things to produce a hyperbolic or elliptical surface, but both proved too difficult. He succeeded, however, in building a larger telescope having twice the magnifying power of his twelve-foot instrument and this enabled him to study Saturn more closely.

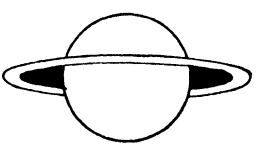
In the winter months of 1655–56 great progress was made towards solving the problem. Instead of the "tri-spherical" form he was able to distinguish a sort of band passing across the middle of the planet and drew it in the form:



A slightly later drawing of Saturn showed it in the form:



It is difficult to imagine what his conjectures were at this point. A new twenty-three foot telescope with the best lenses he could make was assembled with all speed and this was in use after February 19th, 1656. With this instrument the planet appeared much more distinctly and he at last made a drawing of it showing it surrounded by a ring:



The drawings he made show that he was struggling to gain a clearer image and only by degrees became certain about what he could see (cf. p. 194).

During an interval between June and October the planet was

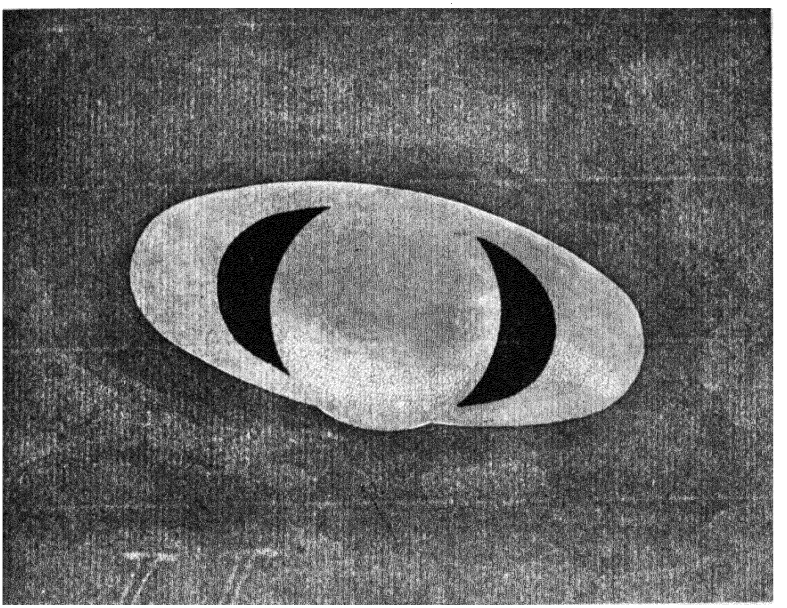
not clearly visible but Huygens already felt confident that his observations had only one interpretation: Saturn is surrounded by a thin ring of matter slightly inclined to the ecliptic. This idea was concealed in an anagram published in his *De Saturni luna observatio nova,* which came out in the spring of 1656. When disentangled the anagram reads "Annulo cingitur, tenui, plano, nusquam cohaerente, ad eclipticam inclinato," viz.: "It is encircled by a ring, thin, plane, nowhere attached, inclined to the ecliptic." From his correspondence it is clear that he was confident of his conclusions as early as February of that year.

The use of anagrams was common in those days. Huygens adopted the device so as to give an opportunity for other astronomers to bring their own discoveries to the light of day "so that it may not be said that another has borrowed from us, or we from him". The method was superseded with the growth of scientific periodicals. In this instance, Roberval, Hevelius and one Hodierna all came forward with their own announcements. Hevelius alone produced anything of importance. His *Dissertatio de Natura Saturna Facie* (1656) contained in fact a complete theory for the observed periodicity in the phases of Saturn. He cannot have seen the planet clearly for he supposed it to be ellipsoidal and not spherical in form; also there were two appendages physically attached to its surface.

Roberval put forward the theory that Saturn is surrounded by a "torrid" zone. From this equatorial zone "exhalations" were ejected and these were supposed to be transparent except when present in great quantities. The periodicity in the phases was ignored. Even more remarkable was Hodierna's account of Saturn. His theory that the planet had the form of an egg or plum having two dark patches deserved careful verification, Huygens caustically remarked. Certainly such an appearance called for study by a better telescope than one of a magnification of five!

Boulliau was unable to see the satellite Titan and this made Huygens suspect the quality of his telescope and for this reason to trouble little about Boulliau's criticism of the ring theory. It mattered rather more when Wallis, the English mathematician, wrote to say that the English had forestalled him. This, however, proved to be a practical joke by Wallis, who otherwise is known only for the seriousness of his pursuits.[1] However, the details

[1] See notes on Wallis and others (p. 212).



Saturn—Reproduced from Huygens's MS.

of the ring continued to give Huygens, as he said, " no little trouble ". The difficulty was to fix the interval between the phases and calculate the future appearance of the planet. At the end of 1657 Huygens was able to inform Boulliau of the confirmation of his theory. " On the 17th of December I saw Saturn with my big telescope for the first time after it had passed the sun and was delighted to find it exactly in the form I had predicted according to my hypothesis." He went on to say that the ring appeared a good deal larger since its last occultation " so that now the sky can be seen through it ".

At the crowded assembly of Montmor's circle Chapelain presented a detailed account of Huygens's studies of Saturn. The planet was in all other respects normal: it traversed an orbit around the sun and its axis of rotation was almost parallel with that of the earth. The axis was always perpendicular to the plane of the equatorial ring. The solid and permanent nature of the ring could be clearly perceived. Twice in thirty years—the sidereal period of the orbit—the ring appeared to vanish since it was viewed edge on. The company was a distinguished one and general praise was forthcoming for the young astronomer's discovery. Even Roberval paid him a generous tribute and retracted an earlier suggestion that Huygens was indebted to him for his ideas. He still maintained, however, that his own theory was to be preferred. Huygens wrote that the ring was without doubt a great novelty and one to which " in the rest of the universe there appears to be no parallel ". In June 1659 his book *Systema Saturnium* appeared.

Copies of the little treatise were sent to Paris and to Prince Leopold de Medici, to whom it was dedicated. The prince was a great supporter of science and founder of the Accademia del Cimento. Influenced by Boulliau, who sent him criticisms of the hypothesis, however, Leopold hesitated to express his opinion of Huygens's work and it was only after a considerable delay that he acknowledged its importance. Of the noted astronomers of the day, Hevelius, Boulliau and Riccioli did not accept Huygens's account of Saturn's ring. To this day no-one seems to have recognized the importance of Huygens's theory that the ring would be stable under uniform gravitational attraction—assuming mechanical resistance to fracture. He did not state that the gravitational force kept the ring in rotational stability but he did suggest that Saturn's *gravity extended to the ring.*

It was as a comment on Copernicanism that Huygens intended his book to be read. The nature of gravity was, he insisted, the same for all the planets. A stationary ring of uniform thickness would, then, be in equilibrium. No further evidence could well be expected. There are other interesting matters in the work, but these will be discussed later. What concerns us here is that the severest attacks which were made on the *Systema Saturnium* were made for religious and not scientific reasons. It is curious that after a period of tolerance the Catholic Church became bitterly opposed to Copernicanism in the seventeenth century. In 1615 the Holy Congregation had declared all books of Copernican doctrine to be condemned and prohibited. Chapelain certainly expected trouble and wrote that it was surprising that the hypothesis of the movement of the earth was allowed to pass in Holland. Père Honori Fabri, a Jesuit, and an astronomer, Eustachio Divinis, were foremost in their antagonism to Huygens. These critics found it necessary to impugn not the arguments advanced by Huygens but his very observations. This drew a sharp reply from Huygens in his tract *Antidivinis,* but the controversy dragged on until Huygens was well established in Paris in 1666 and will need a further account later. The name of Fabri is obscure enough now. Nevertheless, under the name of his friend and pupil, Mousnier, appeared one or two interesting attempts to develop mechanics. The trouble was that Fabri possessed the outlook of an Aristotelian, for he wished to deduce the mathematical laws of dynamics from principles of natural philosophy. So deeply rooted was this habit of mind at this time that considerable feeling was frequently roused by ideas which rested on an entirely different attitude. In France the authorities of the theological college of Paris University tried to get decrees issued in defence of Aristotle's philosophy and against the new heresies as late as 1671. This ridiculous situation was treated to a sarcastic burlesque by the playwright Boileau, who thereby did much to wreck the scheme. Later, a more eclectic outlook existed; Cartesianism entered the Sorbonne itself.

V

Between 1655 and 1660 Huygens spent much time on the invention of an accurate pendulum clock. The significance of

this invention in the history of science is that it marked the new interest taken in time as a dimension. We shall see that Huygens effectually began the study of dynamics. One reason why the history of mechanics up to his time was really the study of statics was undoubtedly the tendency resulting from the neo-Platonic revival of the sixteenth century which coincided with the decline in Aristotelianism. This tendency was to reduce most physical problems to geometry. But the absence of accurate time measurement was undoubtedly another reason. Galileo, it will be remembered, used a water clock in his experiments on acceleration over the inclined plane.

Very probably it was in the first place Huygens's early enthusiasm for astronomy which led him to tackle the problem of the pendulum clock. Balance clocks existed, of course, from much earlier times, probably from the thirteenth century, but they were crude and unreliable machines. Tycho Brahe used one in conjunction with his mural quadrant and corrected for its errors by comparison with the sun. The measurement of the time of passage of a star across the meridian could be used to replace the measurement of its meridian altitude, this being a more difficult measurement and rendered uncertain through the absence of reliable corrections for the atmospheric refraction. Also, as a member of a seafaring nation, Huygens could not have failed to know that an accurate clock would afford the simplest method of determining longitudes at sea. This question seems to have interested him more *after* he had made his first clock.

His first publication, *Horologium*, a short treatise describing the application of the pendulum to the escapement, appeared in 1658 but the invention was known to his friends some two years earlier. Unfortunately a controversy arose through the claim made by Leopold de Medici that the priority for the invention belonged to Galileo. Roberval and a Paris clockmaker, Thuret, also claimed that they had anticipated Huygens. The whole history of the pendulum clock has in fact been obscured by various energetic contestants.

In 1598 the King of Spain offered a prize of one thousand crowns for a means of finding longitudes at sea and this was followed by an offer of ten thousand florins by the States General of the Netherlands. Now Galileo is said to have discovered the approximate isochronism of a simple pendulum in 1581. He himself, in 1636, offered to the States General a method of determin-

ing longitudes based on the telescopic observation of the occultations of the moons of Jupiter. It was proposed to publish an almanack of the eclipses of these moons and to use a "numeratore del tempo" to measure the time intervals. This instrument, from all accounts, was merely a simple pendulum maintained swinging by hand and fitted with a completely impracticable mechanism for counting the swings. Admiral Reael's committee did well to reject the "invention". It is quite possible that in 1637 Galileo came across Leonardo da Vinci's drawing for a clock regulated by a pendulum. It was in this year that da Vinci's manuscripts were given to the Biblioteca Ambrosiana at Milan by Galeas Arconati and the donor is known to have been at pains to bring his treasure to the notice of contemporary men of science. Galileo in this year became blind after a long period in which his eyes were diseased but he had around him Viviani, Torricelli and his son Vincenzio. Viviani, writing to Leopold in 1659, described *from memory* how Galileo discussed with his son the construction of a pendulum clock. The date given was 1641. Whether Vincenzio ever completed its construction is not known. It is certain that Huygens knew nothing about the design until after the publication of his *Horologium* in 1658. A copy of this was sent to Leopold de Medici, who replied guardedly, pointing out that Galileo had had the same idea.

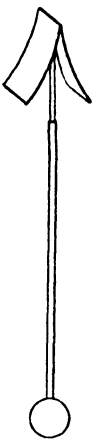
As against the theory that Galileo was indebted in any way to da Vinci it needs to be mentioned that the design commonly attributed to Galileo differs somewhat from that shown (though rather imperfectly) in da Vinci's note-books. Huygens's design differs from that of Galileo and was, in fact, closer in principle to that of da Vinci.

It was in the records of the Accademia del Cimento of 1662 (published in 1667) that the implication of Huygens's plagiarism was really blazoned abroad. Here it was stated that Vincenzio had put his father's design into practice in 1649. No details were given and the illustration simply showed a drum-shaped clock mounted horizontally on a vertical pedestal. From the underneath side of the drum hung something resembling a simple pendulum, the mode of attachment of which had to be imagined. It is obvious that a simple pendulum would be useless, since it is impossible to give an impulse to the thread and presumably a thin iron rod was intended. Nevertheless, Matteo Campani stated that he saw the clock constructed by Vincenzio

—"an antique and rusty machine not at all complete"—and Leopold's letters certainly suggest that such a clock was in existence, though whether it conformed to the diagram is not known. The clock was never forthcoming and it has generally been supposed that Viviani pressed the whole case for Galileo out of a desire to honour his master. Nevertheless, the evidence does seem to indicate that Galileo did precede Huygens in achieving the successful application of the pendulum to the escapement, but that the complete clock was constructed is exceedingly doubtful. Probably da Vinci was the first to have the idea and still more probably Huygens was the first to carry it through to fruition. In a later chapter it will be shown how astonishingly thorough in every particular Huygens's work was; so completely did he clear up the theoretical and practical problems that he is in a real sense the father of modern time-measurement. Samuel Coster, his clock-maker at the Hague, made a large number of clocks to his design and these were the first to be commercially available.

As has been mentioned, Huygens probably interested himself in the longitude problem after he had made his first clock. He may have read the work *Nieuwe Geographische Onderwijsinge* in Dutch by Metius (1614) which pointed out that it was only the irregularity of balance clocks which prevented them from supplying a means of finding longitudes at sea, but in any case the relation between local time, standard time and longitude was well known. The pendulum clock was a far better instrument but it was very easily disturbed. Huygens consistently underestimated this problem of the movement of the ship. He thought that a pendulum whose period was independent of the amplitude of swing would enable this difficulty to be overcome. The idea was ingenious and led to the discovery that the period of oscillation of a cycloidal pendulum is independent of the amplitude, but the practical value of the discovery was strictly limited. A great deal of time was occupied by the investigation of this problem, conducted as the research was by elementary and tedious mathematical methods. In practice a cycloidal pendulum may be constructed by allowing a simple pendulum to swing between two curved metal plates along which the thread curves itself on each half of its swing. In applying the idea to the clock pendulum Huygens used a short ribbon attached to a rigid pendulum. These plates or "cheeks" were first tried in

1657 or during the last days of 1656. Towards the end of 1659 Huygens showed that theoretically they should themselves possess the form of cycloid arcs. He was immensely pleased with this discovery and ranked the geometrical part of the work above all the rest.



About this time (1659) more detailed accounts of Galileo's escapement became available. Models of this escapement have since been made and it cannot be said to work satisfactorily. As Huygens pointed out at the time this escapement imparts a very uneven movement to the pendulum. His own clock remained therefore the only one in this field.

Continued efforts by other inventors, including the clock-maker Thuret, to profit from the invention drove him to the unwelcome decision that he should take out a patent or "privèlege" to protect his rights. Much delay occurred before the French "privèlege" was issued, but thereafter Huygens's priority was recognized and he made some profit from the construction of clocks to his design.

The story of the clock needs to be told with reference to certain of Huygens's mathematical researches. His first essay in this field had dealt (1651) with some fallacious work by Gregory de Saint Vincent on the rectification (or measurement) of certain curved lines. Huygens became interested in the rectification of curved lines known as conics and in the age-long problem of the rectification of the circle. When Boulliau sent him some problems by Pascal on the curve known as the cycloid in 1658, therefore, the subject was by no means a new one. These problems, to which Pascal had already obtained solutions, and which he set for the interest or exasperation of other mathematicians, were known as the "Dettonville" problems, this being the pseudonym under which they were issued. Huygens succeeded in solving some of the necessary preliminary problems but found the main ones so difficult that he declared himself unconvinced that they had ever been solved. When he later came across a rectification of the cycloid by Christopher Wren he expressed his admiration. It was, he commented, the first curved line known to be rectified, and he wondered if it were the only one which could be rectified. There was some correspondence between Huygens and Pascal on the "Dettonville" problems. Pascal praised Huygens's

Fig. 1.

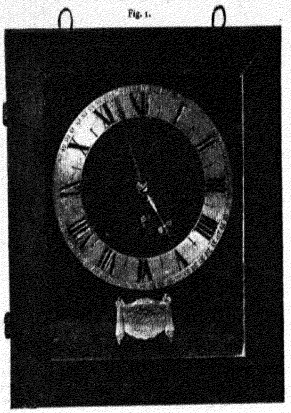


Fig. 2.

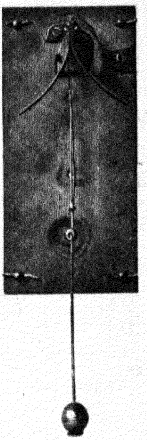


Fig. 3.

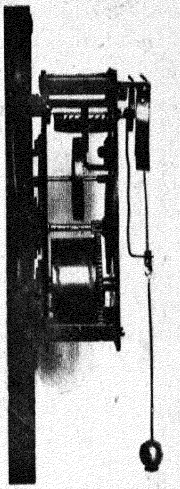


Fig. 4.

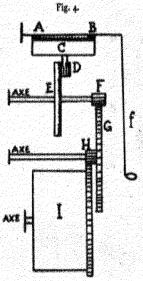


Fig. 5.



Fig. 6.



One of Coster's Clocks

pendulum clock very highly, but such was Huygens's esteem for Pascal as a mathematician that he deprecated such mechanical inventions. There was, he remarked, little science or subtlety in such things. About this time Pascal's adherence to the Jansenist sect was somewhat weakened but his periods of religious preoccupation invariably interrupted his most interesting work—and his most promising friendships. So it was in his relations with Huygens. The latter was eager to collaborate but closer relations were frustrated.

It is clear that at some time between September 1659 and January 1660 Huygens discovered the theoretical form of the small plates or "cheeks" for his corrected pendulum. These dates may be fixed by an examination of his correspondence. From this it seems that he did not at first use the metal plates except for clocks in which a large swing was employed. Later, taking the view that a marine clock would benefit from having a pendulum swinging through a large arc, he felt that it was imperative to discover the theoretical form of the restraining plates. His success in this problem gave him the pleasure of a mathematician with a pretty solution. He announced that the second edition of his *Horologium* would contain "a fine invention which I added to the clock a little while ago". It appears probable that, although Pascal's problems were of a very different character, the interest of the cycloid led Huygens to make his investigations.

The improved clock was in use towards the end of 1669 and was adopted after that date as being the best time measurer then made. Earlier astronomers, notably the Landgrave of Hesse (who used balance clocks made by Byrgius in the sixteenth century), Tycho Brahe, and later, Hevelius and Mouton, recognized the importance of time measurements, but Roemer and Flamsteed, late in the seventeenth century, were really the first to use the clock systematically. Delambre, in his great *Histoire de l'Astronomie Moderne*, states that Huygens "started the great revolution" in practical astronomy by the invention of the pendulum clock.

Curiously enough the cycloidal pendulum, in spite of its elegance, did not have a very long life. It was recognized by Huygens himself that small circular arcs were equally accurate and in his model of 1658 he was able to restrict the size of swing. After the application of the anchor escapement by a London

clock-maker, Clement, in 1680, few cycloidal pendulum clocks appear to have been made. The anchor escapement causes the pendulum to describe small arcs of constant amplitude and this made the cycloidal pendulum—for all but marine clocks—superfluous.

As for the marine clock or chronometer, although Huygens constantly considered himself near to success, it proved eventually to be a failure. The pendulum seemed for many years to be the only means of controlling the going of the clock with sufficient accuracy. Huygens accordingly tried various forms of suspension and various forms of pendulum, all designed to withstand the movement of the ship, but none proved to be a practical proposition. Such was the commercial rivalry of the various East India Companies, however, that he was encouraged to persevere, and persevere he did up to the last year of his life. How near he came to success will be described later.

Meanwhile the astronomers obtained their longitudes by Galileo's method of observing the recurrent eclipses of the satellites of Jupiter. Cassini, working at Paris, drew up the first tables for the observation of these satellites and, with Richer, in consequence of this work was able to make the first modern estimate of the distance of Mars. But astronomical methods were clearly unsuited to the determination of longitudes at sea.

Huygens's great essay, *Horologium Oscillatorium,* on the construction of the clock and all the relevant propositions on the cycloidal pendulum and on the centre of oscillation did not appear until 1673. Already, however, he had made notes for the work and even wrote to Chapelain in September 1660: "The treatise on the clock has been finished a long time but there is no means of having it printed before my journey . . . " This refers to an extended edition of the original *Horologium* which included a treatment of the cycloid; much was yet to be added before the work reached its final form. In October Huygens left the Hague for Paris.

VI

In considering the encouragement given to literature and the arts in France during the seventeenth century, the credit belongs almost as much to Mazarin as it does to his successor Colbert.

Mazarin it was who gave pensions to many of the great writers who made this the golden age of French literature. Molière, Balzac, Descartes, Pascal, Racine, Corneille, Boileau and others benefited from Mazarin's patronage and his example was followed by Colbert after 1661.

Nevertheless, the establishment of the Académie Royale des Sciences would never have been achieved if the men of science had waited for Colbert. Up to 1663 what progress there was in any regular pursuit of science was made by men who were associated with one or other of the amateur societies, Montmor's and Thévenot's being much the most important. But in this year there was a rather defeatist air about the correspondence on the subject of a permanent academy and Sorbière summarized the difficulties in an account sent to Colbert. There had to be appeals by Sorbière, Thévenot and the Abbé d'Aubignac, however, before any impression was made. Finally Auzout publicly appealed to the king's pride (and vanity) and after the Peace of the Pyrenees things began to look more hopeful.

The scientific societies did indeed develop under difficult conditions. In England, for example, where the Royal Society was taking shape, there was in progress a stern struggle between king and parliament, a deep religious dissension and in the commercial sphere a rivalry with the Dutch which had become acute. In Holland, on the other hand, it was understood that the chief political problem of the time was the neutralizing of the growing power of Louis XIV. Civilization was passing through a critical period and internal dissension as much as external danger made the times, one would have thought, unpropitious in most countries of Europe for the growth of societies with the calm interests of natural science as their pursuit. But perhaps these interests were all the greater attraction; as Sprat wrote afterwards, the members of the Royal Society wished simply for " the satisfaction of breathing a freer air, and of conversing in quiet with one another, without being engaged in the passions and madness of that dismal age ". Needless to say their work was not always taken seriously and both in Paris and London there were scoffers who doubted the worth if they did not mistrust the influence of " natural philosophy ". Pepys recorded that Charles II " mightily laughed at Gresham College for spending time only in weighing of ayre and doing nothing else since they sat ". But Colbert certainly saw that there was more to those pursuits than

met the eye. It may be doubted if Louis XIV, unaided, saw anything significant at all in what was going on.

In spite of the difficulties an extensive correspondence was carried on between the men of science. Paris was at first the chief centre of experiment, but London later rivalled and then surpassed it in activity. The meetings of the scientists seem to have been devoid of political motive; religious difference only rarely caused antipathy, national differences scarcely ever. As will be seen, Huygens spent many years in Paris under the patronage of Louis XIV, even though his family had a long association with the house of Orange and, after 1672, the young Prince of Orange headed the resistance to French invasion. It might have been thought that Huygens, a protestant Dutchman, would have been regarded as a spy but this was not the case. Not until 1683 did it become clear that, with Colbert's death, support for his continuance in Paris was gone. Colbert was succeeded by Louvois, and in 1685 the Revocation of the Edict of Nantes caused many protestants to leave the country. All this, however, lay in the future.

In 1658 Montmor charged Sorbière with the task of drawing up rules for the meetings of the assembly which were held regularly at his house. The keynote of the rules, in the form finally adopted, was the need of restricting " the vain exercise of the mind in useless subtleties ". Mere philosophizing, it was agreed, was profitless. Unfortunately the assembly did not appreciate how their aim should be achieved: without a programme of experimental work directed to the solving of selected problems too many of their meetings continued to degenerate into philosophical combats.

Huygens returned to this gathering of scientific amateurs in 1660 and was introduced by Chapelain on November 2nd. At Montmor's, he wrote to his brother, " there is a meeting every Tuesday where twenty or thirty illustrious men are found together. I never fail to go . . . I have also been occasionally to the house of M. Rohault, who expounds the philosophy of M. Descartes and does very fine experiments with good reasoning on them . . . " Rohault's meetings were held on Wednesdays and began about 1658. There is no doubt that he did a great deal to make science popular in Paris. Unauthorized editions of his lectures were published, so great was the popular interest. It is not surprising that Huygens approved of him, although no

association between them seems to have occurred. Rohault was enlightened and modern in his attitude but he was an expositor and lecturer rather than an original thinker. Where he abandoned Aristotle he followed Descartes.

At Montmor's house Huygens noted a room "full of beautiful paintings", a cabinet of curious inventions and mathematical instruments, and drawings by Albert Dürer. With the astronomers he discussed his work on Saturn and the problems of lens grinding; with the mathematicians, as he noted, "my theories of the superficies of conoids and spheroids and the new properties of the cycloid for pendulums"; and with the clockmakers and telescope-makers Huygens also passed interesting hours. He met Conrart, Roberval, de Carcavy, Pascal, Pierre Petit, Sorbière, Desargues and others. Some of these names will recur later.[1] He corresponded with Fermat and with his friend Boulliau, then staying with Hevelius at Dantzig. With Robert Moray, a prominent member of the London group of "scientists", he also began a correspondence. These men were all really amateurs and the title astronomer, in most cases, for example, simply indicates the kind of work for which a particular man showed especial interest and in which he spent his leisure time. Nevertheless, a man like Cassini, later invited to work at Paris, represents the new type of professional worker in that most of his time was in fact spent on genuine systematic work. Huygens also belongs to this class. Real specialization in the modern sense was quite unknown, of course. Like all the early scientific assemblies of the mid-seventeenth century the Montmorian society cast its net almost too wide. Huygens's diary records dissections of human bodies, the examination of machines for which perpetual motion was claimed, the making of lenses and telescopes and many other matters. Too much time, Huygens considered, was spent in arguments of a purely philosophical nature. He felt that a sterner discipline, a greater application, was needed than could come out of the performance of merely curious experiments and the holding of discussions.

Nevertheless it must have been an interesting and stimulating experience to have met so many natural philosophers, all of whom felt the common interest in the new study of nature. At the house of the Duc de Roannes, Huygens, in December 1660, met Pascal. Eight days later the Duke, with Pascal, visited

[1] See notes on Persons Mentioned, pp. 212-6.

Huygens at his lodgings in rue Sainte Marguerite and, wrote Huygens, " . . . we talked of the force of water rarefied in cannons and of flying; I showed them my telescopes." Pascal was at this time a sick man. The writer of *Provincial Letters* had, in fact, by this date retired more or less completely from the world. Less than a year later he was dead.

The publication at this time of tracts against Huygens's account of Saturn shows that the orthodox Jesuits were not prepared to ignore the author's Copernican doctrines. Père Fabri especially opposed what he called Huygens's " furtive insinuation " of the Copernican " error ". In his *Brevis Annotatio in Systema Saturnium Christiani Eugenii* (1660) he presented his own fantastic theory, although this work was published over the name of the astronomer Divinis. According to this theory the planet had two luminous bodies (lucidi) and three dark ones (obscuri) placed around it and the different relative positions of these bodies were the cause of its observed phases. This criticism drove Huygens to compose his *Brevis Assertio Systematis Saturnii* within the year. Hevelius is said to have been so favourable to this reply that he abandoned his own theory in favour of the theory of the ring. Leopold, to whom both Divinis and Huygens dedicated their publications, remained reserved. In 1661 he sent Huygens a further pamphlet by Divinis and Fabri but it did not seem to Huygens to deserve a reply. It is noteworthy that by January 1665 even Fabri recognized the truth of the ring theory, convinced at last by the excellent telescopes of Guiseppe Campani. Huygens was extremely pleased by this conversion of his critic. " No-one, in my opinion, could reasonably reproach me " he wrote, " for having adapted my account of Saturn to the system of Copernicus . . . the truth of the matter can only be explained by following Copernicus, and indeed our system of Saturn corroborates his own strongly."

VII

One of the reasons for Huygens's visit to London in 1661 was undoubtedly his desire to obtain information of the society of men of science then meeting at Gresham College, the society which by charter became in 1662 the Royal Society. He arrived in London in March—just before the coronation of Charles II and left for the Hague at the end of May.

The London which Huygens saw in 1661 was the London which was largely swept away by the Fire and he was not at all favourably impressed with the condition of the town. All, he found, was in marked contrast with Paris: the smoke from the furnaces of the brewers, soap boilers and dyers; and the stench of narrow alleys innocent of drainage and sanitation. Even Gresham College had been rendered malodorous. Monk's soldiers had for a year used it as a barracks and it was, Bishop Wren wrote to a member of the society, "in such a nasty condition, so defiled, and smells so infernal, that if you should now come to make use of your tube [telescope], it would be like Dives looking out of hell into heaven." This was in 1658 or the "fatal year 1659". With the "wonderful pacific year 1660" meetings of the "invisible college" as Boyle called it, recommenced. By 1661 the college was presumably cleaned up. Huygens, at any rate, had only admiration for the proceedings there. Brouncker, Moray, Oldenburg, Godard, Boyle, Wallis and many others were familiar figures at the meetings of the society and their activities seemed to him to surpass anything done in Paris. The observation of stars was done in the garden of Whitehall Palace and there Huygens tried his own telescope lenses, sent over by his brother Constantin. These proved to be better than the English. The Duke and Duchess of York came out to observe the Moon and Saturn.

Huygens's meeting with Wallis is of especial interest. This great mathematician showed in his *Mechanica sive de Motu* (1669–71) that he had much to contribute to mechanics. The historian Duhem has given the opinion that this work was "the most complete and the most systematic which had been written since the time of Stevin". In his work Wallis generalized the idea of force which up to his day was used only in connection with gravity. Huygens's English was at this time not at all good but he saw that it would be most valuable to keep in communication with Wallis, as indeed with others of the Gresham College group. This was the begining of a life-long association with the English men of science. It is striking that, in spite of the official position Huygens came to have in the Académie Royale des Sciences, in 1670, when he feared that he had only a short time to live, he made arrangements to entrust his papers not to the Paris society but to members of the Royal Society. Through Oldenburg, the indefatigable secretary of the Royal Society, Huy-

gens was fortunately able to remain in close contact with the progress of science in England.

There is good evidence that the outlook characteristic of the English men of science was less complicated by the considerations of *a priori* philosophies than that of the Paris group. Francis Bacon has, probably, always influenced literary men more than he has the men of science, but there is no denying his importance. Bacon was no scientist and his scientific "method" was the literary man's conception of science. He never advanced as far as Descartes into scientific studies. He foresaw, "he cast forth brilliant intuitions"; ridiculing Aristotle's natural philosophy he pointed to experiment and observations as the only means of discovering truth: "Nature to be commanded must be obeyed." With this spirit the English men of science, nevertheless, were thoroughly imbued. Their opposition to Hobbes illustrated their belief in empiricism. Hobbes's dictum "Experience concludeth nothing universally" appeared to them mere philosophic wind; his excursions into physical science the sort of thing against which their motto *Nullius in Verba* was later aimed. It has to be admitted, of course, that most of the English men of science were shocked by Hobbes's acceptance of the Epicurean philosophy. This appeared to them to be an approach to Nature which was neither scientific nor pious. Although he was not explicit about it, it was the more empirical attitude of the English men of science which impressed Huygens so favourably.

Before leaving London Huygens took part in a determination of the comparative sizes of the ring and globe of Saturn. He was pleased to find that his account of the planet was accepted with admiration. Huygens also saw a transit of Mercury from Long Acre, using one of Reeve's excellent telescopes.

After Huygens's return to the Hague his father and younger brother went on a diplomatic mission to Paris. The elder Constantin Huygens, a man of European repute, then made the acquaintance of some of his son's associates. Through him the Montmor group heard of Christian's latest experiments. The Dutch diplomat took the opportunity to present Louis XIV with one of his son's pendulum clocks. This gift was opportune, for it was at this time that Colbert was drawing up his schemes to excel all past achievements in making Paris the cultural capital of the world and Louis the pre-eminent monarch of the age.

Louis, of course, became surrounded by an almost ridiculous cult which sought to elevate him above everyday existence, but even this had its merits! The singling out of writers, poets and men of science was at any rate one of the better consequences. These were given rewards totalling many thousands of pounds and the inventor of the pendulum clock later came in for suitable appreciation. More important, however, was Huygens's subsequent invitation to Paris to assist in organizing a scientific society under royal patronage. There were delays in carrying out this project, which must be ranked high among Colbert's achievements, but in 1666 the societies which had been associated with the names of Montmor, Thévenot and others received this formal recognition of the importance of their work.

VIII

The early scientific societies exhibited an enthusiasm and universal interest which scarcely characterizes the professional societies into which they have developed. Specialization was virtually unknown and through Latin the members had a means of communication with foreign societies and with a learned world which had existed before the new studies had begun. It is not surprising that men like Huygens were acquainted with the works of some of the Greek writers, nor that it was the Greeks of the Alexandrian period that held the greatest attraction. Huygens was only following in the steps of Galileo when he studied the works of Archimedes, for they contained some of the fundamental ideas used in mathematics and in statics. But it was clear that new ideas of a fundamental kind were needed in mechanics and it was equally necessary to clear away many plausible suppositions which had no basis in fact.

Huygens saw clearly that such simple machines as the lever, the pulley, and the wheel and axle, although they gave a mechanical advantage, could in no way increase the energy available. Machines for flying, for propelling boats by means of springs connected with trains of gears, strengthened his conviction that their limitations resulted from a simple mathematical identity of some kind. But it was many years before this idea could be expressed satisfactorily. Desargues seems to have tried to evolve a proof that perpetual motion is impossible but, proof or no proof, the impossibility was accepted as axiomatic within

the realm of mechanics by Huygens. The search for a form of perpetual motion did in mechanics the sort of work that in chemistry was produced by the search for the philosopher's stone. In 1659 a book entitled *Mechanica Hydraulico-Pneumatica*, by the Jesuit Schottus, reached Huygens. It was partly about perpetual motion but it also described Guericke's invention of the simple vacuum pump. In 1661, while in England, Huygens saw experiments performed at Gresham College using Boyle's pump, which was a great improvement on that of Guericke. After reading Boyle's book, *New Experiments Physico-Mechanicall touching the Spring of the Air* (1660) he had a copy of Boyle's pump constructed in November 1661. It is clear from his correspondence that he repeated many of Boyle's experiments, observing for himself the boiling of water under reduced pressure, the absence of propagation of sound and the expiration of small birds in a vacuum. An important original discovery made during this work was that of the tensile strength of liquids, an effect which at that time baffled explanation and which led Huygens to make far-reaching conclusions on the existence of a subtle fluid or ether which later came into his theory of light.

The question constantly in view behind all work with vacuum pumps was whether a complete vacuum could really exist. Many scientists felt, with Hobbes, that empty space is " an imaginary space indeed ". A fundamental experiment was to fill a tube with water and invert it so that the open end was under water in an open vessel and then to place the apparatus in the receiver of the air pump. When the pressure was reduced the liquid fell inside the tube and with continued pumping was brought down to the level of the water in the vessel. With rather more difficulty the same result was obtained, approximately, using mercury in place of water. These results agreed well with Pascal's explanation of the barometer. However, the appearance of small air bubbles in water which was placed in the receiver of the pump raised serious doubts, for the descent of the water and mercury might be attributed to the dilatation of these bubbles and not to the evacuation of the upper space in the tube. Huygens, much to his astonishment, found that if air-free water was used no descent occurred. If a very small bubble of air was introduced the descent took place. Boyle's law, however, showed that the magnitude of the effect was far too great to be accounted for on the dilatation theory. Huygens's observation

was confirmed in England, and, at Boyle's suggestion, the effect was obtained without the use of a pump at all. Long barometer tubes of mercury were inverted and were found by Brouncker to give the effect if air bubbles were carefully excluded. A column of mercury 75 inches long failed to descend unless a minute air bubble was present and then the level fell to the normal position of about 30 inches.

For several years no explanation of this effect satisfied Huygens. But in 1668 he concluded that there must be a subtle fluid capable of penetrating glass where the contact of the liquid is not complete and that the height of the barometer is due to the combined pressures of this fluid and air. Wallis pointed out that if the subtle fluid were capable of penetrating glass it would penetrate the Torricellian space also. It is surprising if Huygens did not see the force of this criticism. Although he saw an analogy with the cohesion of two wet glass plates he missed the true explanation, which is that films of moisture (such as exist on mercury and glass) have considerable tensile strength. In his *Traité de la Lumiere,* written in 1678 and published in 1690, much was made of this ethereal fluid in explaining refraction. In fact, very great importance must be attached to Huygens's experiments with the vacuum pump, for his conclusions profoundly affected his whole outlook. He became an admirer of Boyle, whom he supported against the criticism of Hobbes and Linus. The former, he saw, contributed nothing to natural philosophy; of the obscure ideas of Linus (Francis Hall) he thought just as little. Boyle's *Skeptical Chymist* was published in September 1661 and Oldenburg gave Huygens an account of its contents. Later Huygens received a copy of the book which he read with "great pleasure". "It contains an infinity of useful and remarkable things," he commented, "and in my opinion it is worth twenty of these other books which are continually printed on the matters of Philosophy and Chemistry. This *Carneades* certainly speaks very truly, reasons acutely, and without doubt shows the true way to discover the truth of things...." In 1662 Huygens heard of Boyle's famous experiments on the alteration of the volume of a gas with the pressure and he read Boyle's retort to Hobbes and Linus (*A Defence of the Doctrine touching the Spring and Weight of the Air*). The kinetic theory of the gaseous state which originates with this work was propounded by Hooke as well as by Boyle. Hooke spoke of his

theory as Epicurean after the Greek philosopher who, with Democritus, expounded an atomic doctrine. With the resuscitation of this doctrine in Europe Gassendi had a good deal to do. Hooke went into greater detail. The particles of air, he suggested, have "much the shape of a watch spring, or a coyle of wire" which, having rotatory motion, sweeps out a "potential sphere", the volume of which varies with the closeness of the adjacent particles. Huygens was somewhat uncertain if this theory was in accord with the fact that at high pressures air retains its fluidity.

If Huygens gained an interest in experiments employing the air pump from his visit to London in 1661, the English philosophers gained for their part just as much although in a different direction. For Huygens, by 1661, had discovered the use of a particular axiom in mechanics which enabled him to solve problems which Wren, Wallis and others found especially difficult. This axiom is a simple one: the centre of gravity of a system of bodies cannot rise as a result of any motion of the bodies under gravity. Experiments on the ballistic pendulum, carried out in Huygens's rooms in London, showed that he could by this means calculate the heights to which elastic pendulum bobs would ascend after collision. Another discovery which greatly intrigued English mathematicians was the theorem that oscillations of a body in cycloidal arc, occurring under gravity, are truly isochronous. At the time he was in London Huygens had not worked out a complete proof although his note-books show that the work was well advanced. Many mathematicians were consequently attracted to the problem in the hope of being first to provide a proof. Brouncker and Auzout both failed, the former ignominiously, in attempting this problem, which is difficult by the old geometrical methods but simple when treated by means of the differential calculus.

All of this work, which was of first-rate importance, was held up because of Huygens's attempts to construct a successful marine clock—a task which was obstructed more by the limitations of the artisan's resources than by theoretical difficulties. Alexander Bruce, Earl of Kincardine, then living at the Hague, collaborated with Huygens in this work. In January 1663 Bruce crossed to England with two pendulum clocks suspended from ball and socket anchorages in the ceiling of his cabin. The weather was so rough that one clock fell from its suspension and

the other stopped also. In April of this year two similar clocks were taken on a voyage to Lisbon by Captain Holmes. One clock went fairly regularly, and the report on its behaviour is preserved in the British Museum. A filibustering expedition in 1664 to the west coast of Africa and Guinea gave Holmes another opportunity. On one occasion the clocks proved more accurate than the method of dead reckoning in use. Huygens was optimistic as a result of this report and quoted it in his *Horologium Oscillatorium* of 1673. In 1665 war broke out between Holland and Britain and this ended English collaboration.

Huygens rather resembles Hooke in the variety of his scientific interests, but he was far more thorough than his English contemporary and was besides a more " mathematical head ". Besides his work on the marine clock and in mechanics, his work on telescopes and the theory of optics was kept up. It is worth noticing, this dual activity—experimental and mathematical. Huygens used it—to obtain a guiding idea rather than a quantitative result. Very great obstacles then lay in the way of exact quantitative work except in Astronomy. Huygens thus knew experimentally what *order* of aperture was needed, in a telescope of given length and magnifying power, to produce a clear and sufficiently bright image. He saw that the building of longer and yet longer telescopes required improved methods of lens grinding so as to secure sufficient aperture. It also raised problems of a purely structural kind. Wooden tubes, suitably braced, were used for telescopes of about 20 or 30 feet; for great lengths Huygens proposed using two short tubes, one at the objective and one at the eyepiece and with rings placed along the intervening space. This method was tried in Paris, but it was found to be very difficult to align the two lenses—as can be imagined. These " aerial telescopes " gave high magnification but poor definition. Many astronomers accordingly experimented on the grinding of lenses to forms suggested by Descartes. Many abortive attempts were made before the idea was abandoned. Huygens made some use of a machine for grinding lenses but it was not possible to make really large lenses by any method then known. His superior knowledge enabled him to see that the eyepiece could in certain ways be made to compensate for the defects of the objective. In 1662 he spoke of using two oculars instead of one as a " new manner " of en-

larging the field of view. The date of his well-known eyepiece is, however, not quite certain, but it probably was not invented much before 1662 and it may have been as late at 1666.

Huygens's theory of Saturn's phases was at this time so widely accepted, and was confirmed by observation as improved telescopes came more and more into use, that only more detailed matters remained to be settled. It was questioned, for example, if the periods of the phases agreed with the theory that the ring remained at a constant inclination to the ecliptic. Huygens showed how the phases could be calculated and succeeded in converting most of his critics. Wren wrote that " when . . . the Hypothesis of Huygens was sent over in writing, I confesse I was so fond of the neatness of it and the naturall simplicity of the contrivance, agreeing so well with the Physicall causes of the heavenly bodies that I loved the invention beyond my owne . . . " And the accumulation of observations bore gradual witness to the success of Huygens's work.

Wren's hypothesis is now forgotten. He and Neile, in 1658, tried to reproduce the appearance of Saturn by fitting an elliptical " corona " to the planetary globe, meeting it at two places. They suggested that this corona rotated with the planet once during its revolution round the sun, on an axis coinciding with the plane of revolution.

As a result of his work in astronomy and in mechanics Huygens's reputation was already high. Moreover, his scientific temper was in accord with that of the best spirits of his age. " I notice," he wrote to Boulliau (who was an ardent Pythagorean) on receiving a copy of his treatise on light " that in many places you dispute the opinions of Aristotle. That is always worth doing." His opposition to the Aristotelianism of the schools, his disregard of the Catholic opposition to Copernicanism, and the steadfastness of his belief in the new mathematical method brought him the esteem of modern spirits among his contemporaries. It was, then, natural that in France, where Colbert was working to raise the achievements of art and learning above that of previous ages, Huygens should be considered as one of the more brilliant among notable foreigners who might be invited to take up a residence in Paris.

Just as we know little about Newton which is not a description of his mental quality, so it is with Huygens. His correspondence speaks a mind of great intellectual power and clarity,

and the singular absence of violence and prejudice in his comments on men and things is but a necessary concomitant of this mentality. Nevertheless, he was a very human creature and one can sense that the parental authority at times aroused irritation just as at other times the elderly Constantin's desire to show off his son caused amusement. The trouble was that old Huygens's attitude to his sons did not change as they grew up into men, and when he was forced to treat them no longer as children he regarded them as young diplomats who might conveniently do him services in different parts of Europe. Diplomacy was, however, not much in Christian's line, and while he affected French elegance and a seriousness of bearing, at the same time he was impatient with those who were tedious and self-important.

IX

The vicissitudes which both the French and English societies experienced before receiving official support, and even after, were such that they might well have died in infancy. Meetings at Montmor's were discontinued in 1661, but in 1662 the society held meetings at the house of the Marquis de Sourdis and when Huygens made another short visit to Paris in 1663 the society had regained much of its former activity. After the foundation by charter of the Royal Society in 1662 it was inevitable that in Paris, where meetings had been held at Mersenne's and elsewhere as early as 1650, the idea of a similar institution should be discussed. Sorbière, ignoring or ignorant of the early history of the Royal Society, considered that the early Paris societies had in fact led the way. But however this may be, he and Huygens seem to have been on a semi-official errand when they came to London in 1663 to study the organization of the new Royal Society. Writing to Boyle of Huygens's introduction to the Royal Society, Oldenburg said "we had no ordinary meeting; there were no less than foure strangers, two French and two Dutch gentlemen: ye French were, Monsieur de Sorbière and Monsieur Monconis; ye Dutch, both the Zulichems,[1] Father and Son, all foure inquisitive after you." Huygens evinced some surprise that no particular qualifications appeared to be necessary for election to the Royal Society at this time. Christian accompanied his

[1] Christian Huygens held the title of seigneur de Zulichem (in the province of Gueldre) up to the death of his father. He then inherited the title of seigneur de Zeelhem.

father on a diplomatic mission on this occasion and was still in London when the news of his award from Louis XIV was made public. This necessitated a return to Paris.

During this short stay in England, however, he had occasion to see further into the character and customs of his hosts. Through his father's connections in this country he dined a good deal with the great, and met many personalities outside the scientific circle at Gresham College. Huygens and his brother Constantin both dabbled in art, and perhaps his chief interest on this occasion lay in his visits to the studio of Sir Peter Lely from whom he obtained a recipe for making pastels.

Soon after his return James Gregory arrived from England with some correspondence from Moray. Moray wrote of this young man that he had " a present to make you of a book of which he is the author, which he calls *Optica Promota* . . . " He suggested that Huygens should give his opinion of the work and its author but Huygens left no record that he did this. The work was interesting in that it contained a description of a reflecting telescope—some eight years before Newton's invention.

After wintering in Paris, Huygens returned to the Hague (1664) bent on the pursuit of more fundamental researches than could be carried out in Paris. Not until 1666, when he became an official member of the newly formed Académie Royale des Sciences, did he return. Although, therefore, Huygens's work for the new academy was very important and the prestige he conferred on it was especially advantageous, it fell to others to complete the details of the organization. A wealthy amateur named Thévenot gave hospitality to the society at this time and did a good deal of the preliminary organization. It was he who, doubtless with Colbert's knowledge, approached Huygens in November 1664 with a suggestion that he should become a member of the reconstituted society. As will be seen, the offer finally carried with it an official position in Paris with facilities for scientific work.

Soon after his return to Holland, Huygens set about obtaining patents protecting his design of a pendulum clock for use at sea for determining longitudes. The news of this move not unnaturally aroused a good deal of excitement. The commercial value of a reliable method of finding longitudes at sea would be enormous and several others were after the prize. The members of the Royal Society, who knew of previous trials with marine

clocks, were frankly sceptical about the use of a pendulum clock. As a result of full discussion, the society, with the national interest in view, resolved to investigate other methods. Of these some sort of spring-regulated clock appeared to be the most promising. It is not surprising, therefore, that Hooke, a most fertile experimenter, should have taken up the question of the isochronism of the oscillations of a loaded spring. In August 1665 Huygens heard of Hooke's successful experiments and his confidence that a spring-regulated clock would be the solution of the problem.

Huygens returned Hooke's scepticism. So long ago as 1660, he remarked, the Duc de Roannes had tried the idea but without success. Temperature changes, he considered, would have a serious effect on the going of such a clock and sufficient accuracy would be impossible. Hooke, he concluded, spoke too confidently about this "as also of many other things". Nevertheless, Huygens tried a spring-regulated clock in November 1665, but was hindered through the great delicacy of workmanship required. Brouncker, in England, found that Hooke's spring-driven spring-regulated clock was not so accurate as a pendulum clock. The plague interrupted scientific work in London and Hooke's *Potentia Restitutiva,* on the properties of springs, did not appear until 1678. Huygens had to leave the Hague and retire into the country for a time.

There, at Voorburg, he returned to his work on the compound pendulum, in particular the problem of determining the centre of oscillation. Lacking a general method, he proceeded to study the problem inductively, starting with several simple examples. It was not long before, discarding the erroneous work of Descartes, he arrived at some "quite pleasant propositions". The work aroused great interest in England and its technical nature will be explained later.

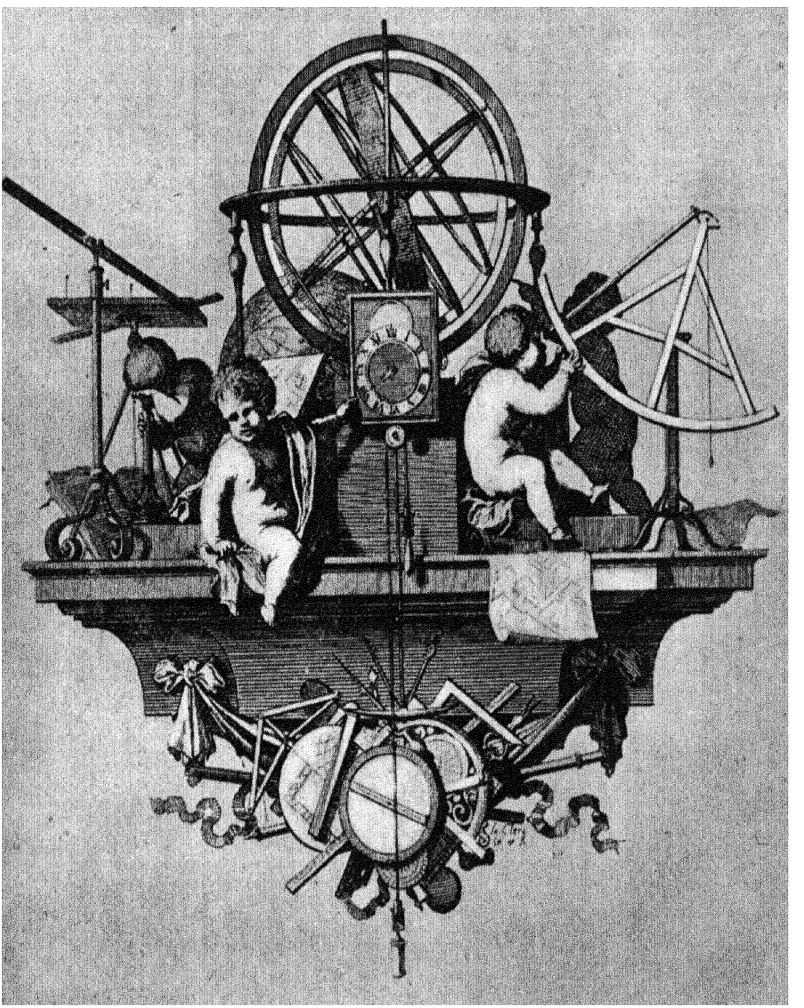
It should be mentioned that by this time (1665) there were two scientific journals of repute for the publication of new work. The publication of the Royal Society, *Philosophical Transactions,* was begun by the secretary, Oldenburg, on his own initiative in March 1665; in Paris the *Journal des Savants* was started, also as a private venture, by de Sallo in January of the same year. De Sallo's privilege was withdrawn after about a year because of his denunciation at Rome, but the Abbé Gallois restarted the journal in January 1666. Neither the *Journal* nor the *Transactions* had

the form of modern scientific periodicals: little original work was published and they were more of the nature of reports. The second number of the *Transactions* bore an account of some observations by Guiseppe Campani on Saturn's ring. These were of interest since Campani claimed to have distinguished the shadow thrown on the planet by its ring, the remarkable thing really being that his telescope was sufficiently good for such detail to be seen. It was said that Campani's lenses were ground and polished on a machine, but attempts so far made in this direction had been discouraging. Hooke published an account of a machine but it does not appear to have been well tested and he was castigated for publishing an account "upon a meer theory".

Cassini, using a telescope made by Campani, observed a permanent mark upon the surface of Jupiter and from its return was able to give the period of revolution. This, Huygens affirmed, was "assuredly a very fine discovery". He himself succeeded in observing the shadow of one of the satellites of Jupiter on the surface of the planet as predicted by Cassini. He also spent some time studying a comet which made its appearance at the end of 1664. As will be explained later, the paths of the comets, so far as these were known, were proving a great difficulty for Descartes's cosmology. Huygens was primarily interested in them as they concerned the Copernican theory. He was at first sceptical about the idea that they recur at long intervals of time. It is interesting that Horrox's defence of the Copernican theory, written about 1635 and resuscitated by members of the Royal Society, came to Huygens's notice through his correspondence with Moray. Horrox, although he died at the age of twenty-two, is generally agreed to have made his mark as an astronomer of a very high order.

The largest telescopes used at this time were of the type now known as Huygens's aerial telescopes, but it is not clear that he originated the idea or wished to claim it as his own. Auzout used an aerial telescope and devised his own method of aligning the lenses; in England the suggestion was widely attributed to Wren. An invention of greater importance and one to which Huygens made an interesting contribution was the micrometer eyepiece. It began to be realized that telescopes could be used for the determination of small quantities which were completely beyond the scope of ordinary instruments used up to that time

PLATE III



Huygens's Clock as the Centre Feature of a design showing Scientific Apparatus of 1671

for quantitative work. The measurement of small angular separations, for example, required the use of a very large quadrant, but these large instruments became distorted under their own weight. Gascoigne first hit on the idea of using two fine hairs close together and situated in the focal plane of the objective as a means of converting the telescope to quantitative measurements. Auzout and Huygens did some measurements of planetary diameters in 1664 and 1665, but Huygens's micrometer was a thin plate of metal in the form of a trapezium. It was inserted between the two lenses of his eyepiece where the real image was formed. The plate could be moved until the disc of the planet was just obscured. In this way, as early as December 1659, he obtained a good result for the diameter of Mars. Shortly after Huygens went to Paris in 1666 a micrometer consisting of moveable hairs was used. The modern form of micrometer was invented by Auzout and Picard. Curiously enough it was Picard who saw the value of the pendulum clock in astronomy rather than Huygens. Delambre remarks that Huygens "started the great revolution" in practical astronomy by his invention of the pendulum clock but it was Picard who did most to introduce regular time observations at the Paris observatory. Using Huygens's pendulum clock he used the times of meridian transit of stars to determine their differences in right ascension.

Huygens was not, in fact, a regular observer. His contribution to astronomy lay rather through his work on optics, which had throughout a practical bias: the invention of his eyepiece and the study of conditions under which spherical aberrations may be reduced. In this period just preceding his departure for Paris, Huygens became deeply interested in two works sent over from England: Hooke's *Micrographia* (1665) and Boyle's *Experiments and Considerations touching Colours* (1664). Hooke, indeed, was at his best in descriptive and experimental work in which mathematics was not required. The hypotheses which he and Boyle advanced regarding the nature of light and the cause of colours were extremely stimulating and aroused Huygens to the desire to carry out experiments on the subject. From these days some of his important work in physical optics may be dated. He was convinced that before the phenomena of colour could be explained it would be essential to understand the mechanism of refraction. This, he considered, Hooke and Boyle had omitted to study sufficiently. His own note-books show that he calculated

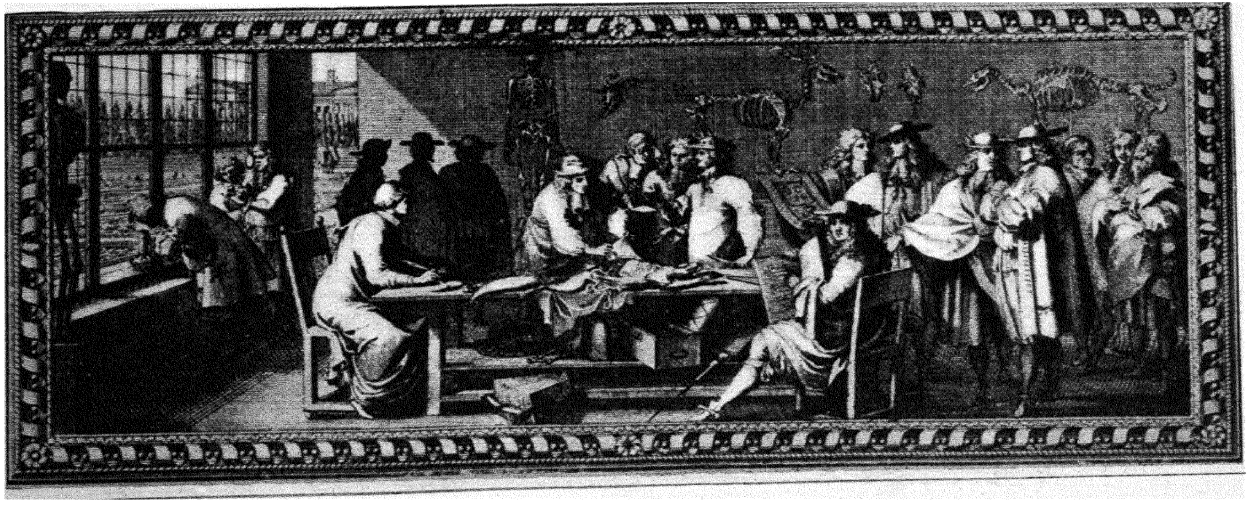
the order of thickness of the air film involved in the production of colours by interference in the so-called Newton's rings experiment (November 1665). Boyle, while admitting that he knew of this experiment, wisely declined to be drawn into a discussion of its explanation.

In his views on the nature of light Huygens always showed a greater dependence on Descartes than in the rest of his work. This bias may explain his first scornful reception of Fermat's least-time principle, for Fermat was, of course, the great critic of Descartes's work in optics. His principle—that a ray of light follows that path for which the time of transmission is less than for any alternative path—had also an Aristotelian flavour, or so it seemed to Huygens. He declared he found no satisfaction in the idea and considered it was a "pitiable axiom". Nevertheless, he repeated Fermat's calculation of indices on this "obviously precarious" principle and, while retaining doubts as to its validity, began to be convinced more and more that the refractive index of a medium is in fact given by the ratio of the velocities of light in air and in the medium. It was necessary to suppose, with Fermat, that light has a finite velocity, whereas Descartes staked his scientific reputation, as he said, on the belief that its velocity is infinite. Roemer's famous calculations of 1676–7 were thus extremely important, for they showed that Fermat and Huygens were correct.

X

In the meantime, as has been mentioned, the men of science in Paris had not found it easy to get the project of a permanent academy of science properly launched. The intimations Huygens received of a position in such an academy were not for a time followed by any concrete offer. Nevertheless, his name was kept in front of Colbert. Moray wrote to Oldenburg in 1665 that "Colbert intends to sett up a Society lyke ours and make Huygens Director of the designe," but during this year Huygens began to feel far from confident about the statements which reached him from Chapelain. He bombarded Carcavy with anxious letters and his feelings had to be assuaged with a variety of excuses. No doubt official delays occurred and accommodation had to be found. Huygens was, however, more concerned over the amount of his salary, clearly through anxiety to live in

PLATE IV



Members of the Académie Royale des Sciences

the style to which his upbringing and habits had accustomed him. When he arrived in Paris in 1666, it was to find that no plans for the new society had been drawn up: The official founding of the Académie Royale des Sciences, on June 1st, meant at first nothing by way of financial aid. It was simply agreed that Auzout, Roberval, Carcavy, Frenicle, Picard, Buot and Huygens should be the nucleus. But, from a letter [1] written by Montmor to Alessandro Segni, the secretary of the Accademia del Crusca, it looks as if it was from the first intended that Huygens should have a prominent place. Then, too, writing to Hevelius in 1667, Boulliau said: "Chief of all is the renowned Christian Huygens . . . Next are Roberval . . . Auzout . . . "

Huygens became a close friend of the Colberts and served on occasion as the minister's scientific adviser. Meetings were held at first in Colbert's library and the first co-operative undertaking, an observation of a lunar eclipse, June 16, 1666, was planned to take place at his house. Unfortunately, cloudy weather made observations impossible. But two weeks later the same group, Huygens, Carcavy, Roberval, Auzout, Frenicle and Buot met to observe a solar eclipse. Once more visibility was poor and results were disappointing. Micrometer measurements giving the relative diameters of the sun, moon and planets were, however, made.

The appearance of the members at these early gatherings of the Académie Royale has been excellently recorded in the work of the skilful engraver Le Clerc. One of this artist's pictures, for example, shows an informal meeting of members and may well represent an hour spent in desultory discussion before the giving of an address. It has been suggested that the figure holding a lens and standing in the window is that of Huygens. When we look at the plate showing a meeting attended by Louis XIV (facing p. 60) it does not appear that Huygens was included. This is the opinion of E. C. Watson,[2] who points out that Huygens was away from Paris through illness early in 1671.

In August 1666 Huygens took over apartments at the Bibliothèque du Roi after the headquarters of the Académie had been transferred there. On December 22 the society gathered officially for the first time to hear from Carcavy the decision of the king to protect the new institution.

[1] A. J. George. *Annals of Science*, III, 372.
[2] E. C. Watson, *Osiris*, VII, 556.

Adherents of Descartes's philosophy, men, that is, who gave out their belief in vortices of subtle matter and who did not accept atoms or the existence of a vacuum, were not conspicuous in the make-up of the Académie. Roberval was a noted critic of Descartes; later the Académie included Mariotte, who also was dubious about Cartesian theories. Frenicle resembled Mariotte in being prepared to accept resemblances between facts without feeling obliged to attribute them prematurely to a single cause. Huygens alone referred to the doctrines of Descartes's *Principia* when called on for explanations of such phenomena as gravity, and he and Charles Perrault for a time exerted a slight influence in favour of Descartes. In time, it is clear, Huygens became distinctly aware of the failure of such an experimentalist as Rohault to maintain his Cartesian explanations without disingenuousness, and his work shows a progressive decline in his adherence to the great "system". Yet he long remained in two minds and it only required some ignorant criticism of the great philosopher to rouse him to his defence. It is surprising too, to see how closely the form of some of his work (for example, that on impact) resembled that of Descartes[1].

Unfortunately this period of Huygens's work in Paris was twice interrupted by serious illness, necessitating a return to his native air. One gains the impression that his position was thereby weakened in some degree, for his absences were prolonged. When he left for yet a third time to regain his health it was never to return. His protector Colbert died soon after and profound changes in the political situation militated against his regaining a position which all along had aroused a certain envy. Huygens's last years were consequently spent in comparative retirement. Mach says that Huygens "shares with Galileo a noble, unsurpassable and complete uprightness" and this is a true estimate. The manner in which his years at Paris came to an end can only be deplored. Huygens's scientific work throughout illustrates a readiness to make his personal reputation always subservient to larger interests. *Nil actum reputans, si quid superesset agendum* was, according to the historian Poggendorff, his adopted device.

The presence of Huygens in Paris throughout the onslaughts of the French armies against the Dutch Republic is the fact which historians find hardest to explain. To a large extent wars reflect nothing of individual feeling towards members of another

[1] Cf. Mouy. *Le Développement de la Physique Cartésienne* 1646–1712 (p. 197).

PLATE V



Louis XIV at a Meeting of the Académie

nation and in those days the instruments of propaganda necessary for whipping up appropriate hatreds did not exist. Nevertheless, in the shifting scene of the wars of Holland, now against England with France as ally, now against France, then against both, and later with England as an ally, the opposition to Louis XIV really remained the one permanent feature. Louis was bent on destroying the Dutch Republic and, with the treacherous help of Charles II, it looked in 1672 as if he would succeed. Huygens could have secured a position of eminence under the Prince of Orange at this time but he had a deep repugnance for political activity and remained in Paris, suspected by some but protected throughout by the minister Colbert. It is not surprising, therefore, that he came in for some criticism by his fellow-countrymen. This criticism was brought to a head in 1673 by the eulogistic dedication of his great *Horologium Oscillatorium* to Louis XIV. The explanation of these facts seems to be that, once having yielded to the cordial friendship of his associates at the Académie Royale and having decided to endure the war, he had to pursue a difficult and always depressing course. The dedication may be regarded as a piece of political wisdom, as justifying his continued patronage, in fact. As a friend of the Dutch ambassador van Beunigen, who during the short war of 1667-8 was suspected of a plot against Louis, it would have been easy for him to come under suspicion as a spy. For Huygens was by no means remote from the world of affairs. He was very well known at court and had many influential friends. After the rise of Louvois and the death of Colbert none of these things mattered; the feelings of Louis towards the house of Orange can scarcely be said to have improved after 1678.

But in 1666 Huygens was indisputably the one who chiefly guided the affairs of the Académie Royale des Sciences. Profiting from his knowledge of the Royal Society, Huygens emphasized in Paris the importance of Bacon's teaching. "Experiment and observation," he wrote, "provide the only way of arriving at the knowledge of the causes of all that one sees in Nature." This attitude is the more striking when one reflects that Descartes had so long been his model. The point here is that while both Bacon and Descartes distrusted formal logic, Descartes scorned empiricism while Bacon apprehended its power. It is not clear that Huygens realized the shortcomings of Bacon's "method". The great omission in Bacon's scheme of research was the recog-

nition that measurements provide the key to the understanding of phenomena. Bacon ranged himself with Aristotle in saying *classify* when he should have said *measure*.

The attention paid to Chemistry should, in Huygens's view, be restricted to essential problems. He appears to have recognized that the old alchemy was decadent and that the beginnings of a true science lay in the work of Boyle and others. Problems such as that of combustion were obviously worth the closest study. Huygens was interested in Hooke's experiments at the Royal Society which, he held, agreed with the "bizarre hypothesis" of an "aerial saltpetre". This hypothesis of an active constituent in the air he considered was "not ill conceived" but he tended towards Moray's empiricism. "We others," wrote the latter, "look for the truth of existence and the nature of things as belongs to the true philosophy".

There can be no doubt of the influence of the London group on Huygens's views of the functions of the new Academy. This influence abroad was recognized by the English themselves, who were fully conscious of the unique importance of their work. "I hope our Society will in time ferment all Europe at least," wrote Oldenburg to Boyle. "Let envy snarl," he wrote, when the new societies excited opposition, "it cannot stop the wheels of active philosophy in no part of the known world."

The Académie at first made astronomy its special study, encouraged, no doubt, by the occurrence of a partial eclipse of the sun in 1666. Huygens noted with dissatisfaction the paucity of astronomical observations in earlier years and this was to be remedied. New observatories were in the course of construction at Greenwich and Paris; Hevelius at Danzig had for some years applied himself to completing Tycho Brahe's observations and had, in 1661, made with Boulliau careful observations of a solar eclipse. In 1666 more was expected from eclipse observations, namely, to rectify the motion of the earth and the moon and to determine differences of meridian on the earth. In the course of the work, several telescopes were compared and micrometers were used for obtaining the relative diameters of the moon and sun.

In continuing his work on lenses at Paris, Huygens was hindered by the poor quality of the French glass, which was inferior to the Venetian. The material showed veins or striæ and tended to extrude salts on cooling. Lens-making brought Huygens into contact with the work of Spinoza, who had then a greater reputa-

tion as a lens-grinder than as a philosopher. Lenses were ground by hand in a hollow form or mould in which abrasives of increasing fineness were successively used. It was the impossibility of grinding any but small lenses in this way that put a limit to the power of telescopes. The appearance of colours in the image was considered by Huygens to be connected with the inclination of the lens surfaces. The error in this was recognized by Newton some years later but in the meantime a great deal of work was expended in the attempt to make lenses of other than spherical curvature.

Astronomy in Paris gained very greatly by the arrival of Cassini in 1669. His first observations were made at the new observatory in 1671. Here he continued his striking work on the rotation of certain of the planets. Huygens had observed the rotation of Mars in 1659 but, true to his device, had not considered the results sufficiently good for publication. Cassini was rewarded by the discovery of four satellites of Saturn and the division in Saturn's ring which is now known by his name.

Huygens was not a competitor with Cassini for the honours of new astronomical discoveries. After 1666 his interests lay more in the direction of terrestrial mechanics and, as the sequel shows, this preference was sound. "I am now starting experiments on circular motion," he told his brother in 1667. A few years later, when Richer's expedition to Cayenne returned to Paris it brought back interesting evidence which bore on the question of the earth's gravity, but the effect of circular motion *of a medium* was the question which at this time interested Huygens, for it was through this that he hoped for an explanation. In taking up the effects of rotation, Huygens was, from one standpoint, returning to work which he had put away ten years before. By 1659, it is thought, he had arrived at the expression $\frac{v^2}{r}$ for the acceleration towards the centre in the case of a body describing a circular path. In 1669, however, he chose to address the Académie Royale not on this but on an elaborate theory employing the vortex of subtle matter as the cause of gravity. Any easy convictions we may have that Huygens had by this time rid himself of Cartesian influences must be profoundly shaken by a perusal of this discussion. The opposition with which his theory was greeted by Mariotte and Roberval on this occasion may have been highly beneficial, for the criticisms they made

were entirely justified. It may be mentioned in passing that Huygens at this time believed that circular motion is a fundamental form. Uniform rectilinear motion, he saw, had no effects on events which normally occur in an apparently stationary environment. Circular motion, however, introduced new effects. It was only after the appearance of Newton's *Principia* that Huygens retracted this statement of the absolute nature of circular motion. He then, more consistently, took a firm stand on the relative nature of all motion—and against the idea of any absolute space.

Mariotte was a French priest who joined the Académie Royale in the year of its foundation and thereafter played an important part. He must be reckoned among the lesser lights who at this time were attempting to make the important next step beyond the mechanics of Galileo. His *Traité de la percussion ou choc des corps* (1677) shows that he and Huygens were working on similar problems. When Oldenburg approached Huygens in 1668 with a request that he should contribute to the Royal Society some work on mechanics he replied by sending some work on impact. This, afterwards published in the posthumous *Tractatus de Motu Corporum ex Percussione* (1703), is really a study of various applications of the law of conservation of momentum. There can be no doubt that Newton profited from the work on impact which was carried out by Huygens, Mariotte, Wallis and Wren. The formulation of his relation between rate of change of momentum and external impressed force completed in a magnificent way this contemporary work. In regard to centrifugal force Huygens forestalled Newton by many years. " What Mr. Huygens has published since about centrifugal force I suppose he had before me," wrote Newton with some chagrin.

The immediate result of the correspondence with Oldenburg was that Huygens learnt that Wren and Wallis had both communicated papers on the subject of impact and momentum and at practically the same time as his own. More instances were to come in which Huygens felt himself to have been unfairly forestalled in publication and in some cases he gave vent to severe criticisms which were by no means justified. Oldenburg showed great fairness and removed much of this feeling of resentment, but the outbreak of some acrimonious correspondence over some mathematical work by James Gregory shows that Huygens had become unduly nervous for his reputation. When Mercator put

forward a method of determining longitude by means of a pendulum clock he roundly condemned his intrusion. It was fortunate that when Barrow's *Lectiones Opticæ* came out in 1669 it was evident that the work did not overlap with Huygens's prolonged researches in optics. Huygens was surprisingly slow to learn the consequences of his own attitude towards publication.

It was not customary in those days to isolate a particular problem and to study it exclusively for a considerable time. The seventeenth century men of science were nearly all capable of turning their attention to a wide range of subjects and they frequently were engaged on a variety of topics. Huygens indeed must be considered one of the most versatile men of the age, for he excelled Hooke in the quantitive nature of his work while at the same time he showed as wide a range of activity. Hooke's *Micrographia* stimulated Huygens at this time to attack the problems of constructing microscopes, employing the theoretical advances he had achieved in his work on the telescope. Spinoza was interested in similar problems. Galileo was described as having constructed " an *occhiale* which magnifies . . . so that one sees a fly as large as a hen ". This was a compound microscope. Hooke improved the instrument as regards its mounting and the illumination used. Optical improvements were seriously needed. Huygens's own microscopic observations will be mentioned later; they belong to the years after his translation of Leeuwenhoek's work into French in 1677 or 1678.

As has been mentioned, Huygens's health was never robust. From early youth he was from time to time subject to a certain kind of debility, later accompanied by severe headaches. The illness of 1670 brought about his complete prostration in Paris and he clearly believed himself to be at the point of death. In these circumstances he concluded that he should bequeath his more important unpublished work in mechanics to someone capable of appreciating its importance and he decided to send it to London in the hands of Francis Vernon, secretary to the English ambassador. This action is sufficiently interesting in view of Huygens's official position in Paris for Vernon's account to be given at length. In a letter to Oldenburg he described Huygens's condition " . . . I saw the condition hee was in which was none of the most lively, that his weaknesse & palenesse did sufficiently declare how great a destruction his sicknesse had wrought in his health and vigour & that though all was bad, which I saw, yet

there was something worse which the eye could not perceive nor sense discover, which was a great dejection in his vital spirits, an incredible want of sleep, which neither hee, nor those who counceld and assisted him in his sicknesse knew how to remedie & that hee did not know what the end of these things would bee, butt his fancy was ready to suggest the worst . . . " This mood Vernon did his best to dispel. He accepted his mission to England should the worst befall. Then, he wrote, " hee fell into a discourse concerning the Royal Society in England wich hee said was an assembly of the Choicest Witts in Christendome & of the finest Parts: hee said hee chose rather to depositt those little labours of his which God had blest and those pledges which to him were dearest of anything in this world, in their hands sooner than in any else. Sooner then of those into whose Society hee was here incorporated & from whom hee had received all demonstrations of a most affectionate civilitie because hee judged the Seat of Science to bee fixed there & that the members of it did embrace & promote Philosophy not for interest, not through ambition or a vanity of excelling others not through fancy or a variable curiosity, butt out of naturall principles of generosity, inclination to Learning & a sincere Respect and love for the truth. . . . Whereas hee said hee did foresee the dissolution of this academie because it was mixt with tinctures of Envy because it was supported upon suppositions of proffitt because it wholly depended upon the Humour of a Prince & the favour of a minister, either of wich coming toe relent in their Passions the whole frame & Project of their assembly cometh to Perdition."

It is clear that so early as 1670 differences had arisen between Huygens and certain members of the Paris Academy. This fact will be of interest later when the circumstances of the rupture of his official connection are considered.

Huygens's illness lasted in acute form for several weeks during which great anxiety was felt by his friends in Paris and London. In June there were signs of recovery and three months later the convalescent was able to return to the Hague. In October he resumed correspondence with Oldenburg.

XI

Apart from this winter in Holland, 1670-71, the five years from 1670 to 1675 were spent by Huygens in Paris. And they were stirring years in the scientific world. Huygens as chief of the

Paris Académie was at the centre of things on the Continent, while he was well informed of what passed in England. In 1671 Picard's *Mésure de la Terre* came out, a work of interest from the technical aspect as well as for a general discussion of current theories concerning the shape of the earth. Picard, for example, was well aware in 1671 that the length of a seconds pendulum was different at London, Lyons and Boulogne, but while he admitted that the results might be in conformity with the rotation of the earth, he did not think there was sufficient confirmation of the results, as yet, to justify any conclusion. Very probably he was acquainted with the notion of centrifugal force through his relationship with Huygens, for the latter had arrived at his important theorems as early as 1659. In 1672 news came from England of Newton's work on the solar spectrum; from Holland in the same year came interesting mathematical work by Slusius on the drawing of tangents to curves. 1673 was the year of Huygens's *Horologium Oscillatorium,* his magnum opus. In 1674 Hooke issued a work giving his views on evidence for the motion of the earth.

During this period Huygens worked with Denis Papin on the use of gunpowder as a source of useful energy, and, more important, with Leibnitz at Mathematics. In 1675 Leibnitz brought out his *calculus differentialis.* And to these busy years belong also the invention of the spiral-spring regulator and balance wheel which are essential parts of the watch and chronometer.

Yet they were not altogether happy years for Huygens. After the invasion of the Low Countries, by the armies of Louis XIV in 1672, he may frequently have asked himself why he had returned to Paris in 1671, and immersed himself in work as the only outlet for his despair at his situation. Huygens felt keenly the wrong done to his country and it required much tact and consideration from his friends in Paris to preserve the calm relationship in which he had been accustomed to live. Huygens followed the course of the war with anxiety, but it must be remembered that at forty-three he was by upbringing and experience almost as much a citizen of the French capital as he was of Holland. Paris was indeed the centre of the cultivated world and the prospects for the man of science who should be cut off from the activity of one or other of the two flourishing societies would be poor indeed.

Huygens seems to have worked even when quite ill; certainly he was struggling to regain his old activity in October 1670 when he received some interesting work on mechanics from Wallis. Yet he agreed that his recovery (" for which I thank God ") was too recent for him to do other than attempt very little.

We shall see that Huygens's work in theoretical optics, for example, rivals that in mechanics, but in 1670 he considered the latter subject more important. Though overlooked nowadays, the ideas he contributed to mechanics were as fundamental as his contributions to physical optics. His *Horologium Oscillatorium* was practically completed and had grown from a treatise on the construction and regulation of the clock to a work on centres of oscillation, the tautochrone, the theory of evolutes and centrifugal force. As an examination of this treatise will show, it contains a great deal which was made more explicit in Newton's *Principia* (1687) although discovered by Huygens independently. The law known as Newton's First Law was known and used by Galileo and Huygens; Huygens, in addition, must have employed the Second Law in arriving at his propositions on centrifugal force as early as 1659. He also saw the necessity of distinguishing between mass and weight at about the same time (see p. 119). The greater merit of Newton's work, in fact, was that he gave a clearer presentation of these ideas and made them more useful by means of simple mathematical relations.

When the *Horologium Oscillatorium* came out in 1673, after Huygens's return to Paris, it showed the extent to which his thought had developed. The work was singularly free from Cartesian influences. Huygens himself hoped that it would be in direct line with the great work of Galileo and his hopes were not disappointed. Newton wrote to Oldenburg of his " great satisfaction " with the work and said he found it " full of very subtile and usefull speculations very worthy of ye Author ". Newton especially admired Huygens's mathematical style and considered him the " most elegant writer of modern times ". This remark starts some interesting reflections. Newton regretted that he had not applied himself to geometry before proceeding to algebraic analysis. It was Huygens's predominantly geometrical methods, employed in the *Horologium Oscillatorium,* which aroused his admiration. At this time Newton was well advanced in his work on fluxions and, as we know, Leibnitz took up similar

problems after 1672. The central idea of the differential calculus owes a great deal to the study of motion, for this study introduced the notion of a continuously varying quantity. Huygens's work in this connection was of the greatest importance for, as Leibnitz admitted, it was Huygens who had dispelled the mystery attaching to the study of motion.

The two mathematicians, Huygens and Leibnitz, met in Paris in 1672 and Leibnitz became a regular visitor at the Bibliothèque du Roi. Under Huygens's guidance Leibnitz's ideas developed rapidly, for up to this date, as he himself admitted, he had been only an amateur in such studies. In 1674 Huygens was able to present to the Académie Royale Leibnitz's first paper on the differential calculus. Whether Huygens gave Leibnitz an inkling of Newton's work on fluxions will always remain an interesting speculation. Newton's own ideas date from about 1665 or 1666 and there is no doubt that after 1669 these were well known to his friends in England. Wallis especially must have known about them. Huygens himself was not happy in the use of analytical methods. He was, in Newton's words, " the most just imitator of the ancients " and it is a striking fact that the classical geometrical method was used by Newton himself in writing the *Principia*. This fact, which has always troubled historians in some degree, must be explained by the prestige of Huygens at this time and the fact that proofs by the newer methods were not everywhere accepted. The ideas of both the *Principia* and the *Horologium Oscillatorium* were later cast in analytical form by the mathematicians of the eighteenth century.

The *Horologium Oscillatorium* made a great impression on contemporary men of science. The propositions on centrifugal force, given at the end, were of course important in the development of planetary theory, and the conical pendulum interested those who, like Hooke, concerned themselves with the problem of time measurement. It is certain that Huygens employed the conical pendulum in clocks in 1659 and again in 1667, when he had more fully investigated the laws of motion involved. Controverting Hooke's claims to the invention of such a clock, he pointed out, what Hooke certainly did not know, that the conical pendulum should be so designed that all revolutions of the bob describe horizontal circles in the surface of a paraboloid of revolution with the axis vertical. Only then would all revolutions be isochronous.

Huygens showed considerable dislike for quarrels such as the one of 1674 in which he became involved with Hooke and others. In 1675 he had yet another such experience. This was over his invention of the first successful spring regulator for the clock. Huygens's design of a spiral spring combined with a balance wheel is the one which is still used in watches and its distinction lies in the fact that the centre of gravity of the oscillating part bears a fixed relation to the stationary parts. This meant that the influence of gravity was eliminated. A quarrel with the clockmaker Thuret and the Abbé de Hautefeuille was settled by the Académie Royale on these grounds in Huygens's favour, for Hautefeuille used a straight spring and not a spiral. Hooke was, however, a more tedious antagonist in connection with the same invention and he was made more bitter by the support given by some members of the Royal Society for Huygens's priority. "Zulichem's spring not worth a farthing," he wrote in his diary. When Huygens, for a quiet life, gave Oldenburg the rights to the English patent, he drew from Hooke a full and free expression of his long dislike of the secretary. Oldenburg, he averred, was Huygens's spy. "Saw the Lying Dog Oldenburg's *Transactions*," he noted eight months later, "Resolved to quit all employment and to seek my health." Oldenburg, he said angrily, was a "trafficker in intelligence". He would hear nothing favourable about Huygens's marine clocks. "Seamen knew their way already to any Port," he somewhat childishly stated. Altogether the complaints of Hooke appeared in print over a considerable period but they hold little interest now.

A more famous result of Huygens's correspondence with the English scientists during his years in Paris was that the French became aware in 1672 for the first time of the work of Newton. On January 11 of that year Oldenburg wrote to Huygens of the "invention of a new sort of telescope by Monsieur Isaac Newton, Professor of Mathematics at Cambridge". His next letter gave a full description and this was utilized by Huygens for an account published in the *Journal des Savants* of the following month. It should be mentioned that Gregory's design for a reflecting telescope was never put into practice and the new invention was based on his ideas. Huygens found it "beautiful and ingenious" and he thanked Oldenburg for the news of "the marvellous telescope of Monsieur Newton". The problem of making the concave mirror, though difficult, did not appear to be insuperable.

He supposed that Newton had come to despair, as he had himself, of overcoming spherical aberration but did not refer to the fact that it was this consideration which had led Gregory to his idea nine years before. Up to this date Huygens had not heard of Newton's experiments on the composition of white light and certainly underestimated the defect of chromaticism.

It is consequently surprising, after this initial enthusiasm, to find that Huygens soon abandoned the reflecting telescope. The trials which he himself immediately carried out proved disappointing owing to the imperfect polish given to the mirror. Newton's first telescope was hardly more than a model and when the construction of larger telescopes was attempted in England, the most expert glass worker in London, Cox, found the task of grinding the mirrors too difficult. Reflecting telescopes of a large and useful size were in fact not made for more than fifty years. Huygens found that metal mirrors were unsuitable since the polish was unequal to that given to glass and it was not permanent. He found himself compelled to return to the refracting telescope but realizing that a new difficulty beside that of spherical aberration now required to be overcome.

For in March 1672 Oldenburg sent Huygens a copy of the *Philosophical Transactions* in which, he said, Huygens would find "a new theory of Monsieur Newton (the inventor of the cata-dioptric telescope) touching light and colours: where he maintains that light is not uniform but a mixture of rays of different refrangibility, as you will see fully in the same discourse. . . " This copy of the *Transactions* contained, of course, an account of Newton's famous experiment on the spectrum. The Royal Society, on receipt of this, Newton's first published scientific paper, sent it to Huygens as the one whose opinions would carry the most weight. Huygens's reply was that the conclusions drawn and the theory put forward seemed "very ingenious". But, he went on, "it must be seen if it is compatible with all the experiments". Three months later he wrote that he considered the compound nature of white light had been proved by Newton's *experimentum crucis,* in which it was shown that the separate coloured rays emerging from the prism could not be further decompounded. Huygens went on, however, to make observations which disappoint the modern reader almost as much as they disappointed the young Newton. He questioned if it would not suffice to base an explanation "on the nature of move-

ment" for the two colours yellow and blue only. Until the essential difference of these two colours was understood "he [Newton] will not have taught us what the nature and difference of colours consists of, but only this accident (which assuredly is very considerable) of their different refrangibility". Failing to see the distinction between an impression of colour and the different rays of the spectrum, Huygens suggested that Newton would find that yellow and blue would be sufficient to produce white light. The other colours he regarded as "degrees of yellow and blue more or less deep".

These criticisms were an easy prey to Newton who, far from rushing prematurely into publication, had kept the work by him for at least five years. Oldenburg warned Huygens that Newton, then thirty, was not a man who spoke lightly about anything he advanced. Newton flatly denied that all colours could be "derived out of the Yellow and Blew . . . none of all those colours which I defined to be Original" could be so obtained. "Nor is it easier," he insisted, "to frame an *Hypothesis* by assuming only two Original colours rather than an indefinit variety; unless it be easier to suppose, that there are but two figures, sizes and degrees of velocity or force of the Æthereal corpuscles or pulses, rather than indefinit variety; which certainly would be a harsh supposition." It would be indeed, he remarked, "a very puzzling *phænomenon*", ". . . But to examine how Colors may be explained *hypothetically* is besides my purpose. I never intended to shew, wherein consists the Nature and Difference of colors, but only to shew, that *de facto* they are Original and Immutable qualities of the Rays which exhibit them; and to leave it to others to explicate by Mechanical *Hypotheses* the Nature and Difference of those qualities: which I take to be no difficult matter". It was unimportant if two colours in the spectrum could be combined to give an appearance of white. Such light was different in its physical nature from ordinary white light and could not be resolved by the prism into more than the two components. Clearly Newton was deeply disappointed, at the outset of his career, to receive so little appreciation of the true nature of this work from one so eminent as Huygens. He wished, he said, in future "to be no further solicitous about matters of Philosophy". His rather summary answers to Huygens's remarks disposed at the same time of certain criticisms put forward by Hooke. For once Huygens and Hooke were in

alliance, both opposed to what was essentially a new attitude to scientific problems; both, moreover, found the new facts difficult to reconcile with their respective wave or pulse theories of light. This first occasion for the comparison of Huygens and Newton raises, it will be seen, a question on which these two men of science consistently differed. This was the place of hypothesis in scientific method, a subject for which a later chapter must be reserved.

It is unfortunate that the two greatest scientists of this period did not achieve harmony in their attitude to problems of common interest. For their divergence was not particularly fruitful although it extended from optics into the realm of mechanics. Huygens greatly admired the *Principia* after its appearance in 1687 and he met Newton in 1689. By this time, however, the difference of outlook had become too ingrained and Huygens at sixty had become less amenable to new persuasions. And yet this difference is certainly not so great as some writers have suggested. One biographer of Newton, Louis Trenchard More, considers that men like Hooke and Huygens relied on an inward sentiment of knowledge and in opposing Newton " were merely opposing theory by hypothesis ". Whatever the significance of this distinction, this is a question which can be dealt with only after a careful survey of Huygens's work as a whole. It will be seen that it is a profound mistake to treat Huygens as merely another Cartesian, for all his life he vacillated between the Cartesian view that the objects of scientific calculation are products of thought and the materialism which regards them as external realities. Not only is there much to say in Huygens's favour with regard to the status of scientific concepts, but in methodology also Huygens perceived as well as Newton the end of scientific investigation. " I do not believe we know anything with complete certainty," he wrote to Perrault, " but everything probably and to different degrees of probability . . . as 100,000 to 1 as in geometrical demonstrations." The latter he considered were in a category by themselves. " In the matter of Physics there are no certain demonstrations and one can only know causes through the effects in making suppositions founded on experiments or known phenomena and trying afterwards if other effects agree with these same suppositions." These remarks should make clear the difference between Huygens and Descartes. For Descartes the intuitional method, to which More refers, did undoubtedly

take precedence over the experimental. For Huygens, a life-long experimentalist, there was no way to final certainty.

In the period we are considering Huygens continued his studies in optics but it is difficult to estimate his progress. This work began as early as 1652. By 1653 he had completed a first *Tractatus de refractione et telescopiis* of 108 pages. This was subsequently amplified but remained unpublished. As Huygens found, Cavalieri had independently obtained some of his results and published them in the *Exercitationes Geometricæ Sex*. The only other previous writers of originality were Alhazen (11th century) and Kepler, whose *Paralipomena* was published in 1604 and *Dioptrice* in 1611. In England the most important work was done by Barrow and by Halley. The latter drew attention to the advantages of algebraic formulæ; up to this time the relations used in optics were expressed in the most cumbersome geometrical form. Many of the problems to which Alhazen had given prominence were definitely geometrical in character and these continued to be studied. A new interest was injected into these somewhat academic studies by Bartholinus's discovery of double refraction in 1669. This was described in a short Latin treatise, *Experimenta Crystalli Islandici Disdiaclastici* . . . , which was widely read. Huygens obtained a specimen of Iceland Spar and a considerable part of his *Traité de la Lumière*, completed in 1678 (published 1690), deals with experiments he then carried out. Huygens had developed a pulse theory of light and the effort he made to reconcile his theory with the new and peculiar optical properties of Iceland Spar was a masterly one. Curiously enough there is little reference to this work in his correspondence.

About this time Huygens had as his assistant Denis Papin, a Frenchman who later worked with Boyle in England. With Papin, Huygens in 1673 experimented on gunpowder as a source of mechanical energy. There is a possibility that Huygens had considered some kind of atmospheric engine as early as 1660 when he talked with Pascal about "the force of water rarefied in cannons". In these experiments of 1673 we can see the forerunner of Papin's atmospheric engine, which did in fact employ steam in place of gunpowder. Papin was later, through the interest of the Landgrave of Hesse, appointed professor at the university of Marbourg and it was here that he developed the atmospheric engine which gave Newcomen his clue.

In exchange for Papin, as one might say, Oldenburg sent over

to Huygens the wealthy young amateur Walter von Tschirnhaus, a friend of Spinoza and Leibnitz. He belonged to a class which had early supported the new scientific societies but he was exceptional in that his interest in science did not quickly flag and in that he made himself something more than a mere dilettante. Through Tschirnhaus Huygens undoubtedly learned more of Spinoza's philosophical ideas but he showed himself little interested in them. Unlike many of the seventeenth century men of science Huygens did not occupy himself with philosophical or theological questions and neither he nor Leibnitz seems fully to have grasped the nature of Spinoza's thought.

XII

Early in 1676 Huygens was again ill. There is no doubt that the illness of 1670 had recurred and this time he showed greater caution in meeting the danger. In March 1676 he set out to return home to the Hague while he was yet able, but the journey was slow and very uncomfortable. To his brother he confessed his doubts whether he would return to a life in Paris which seemed to be injurious, and even when he had recovered, a year later, he procrastinated under the pretext of uncertain health. Colbert gave permission for his remaining at the Hague for the winter of 1677–78 and the return journey to Paris took place in June 1678.

During these two years at home he pushed on with his researches. To these years belongs a great deal of work on the double refraction of Iceland Spar and the development of his wave theory of light. On November 22, 1676, Roemer read a paper to the Académie Royale in which he gave the first calculation of the velocity of light. Huygens was immediately interested on receiving a copy of the paper and an interesting correspondence with Roemer was begun. The assumption that light travelled with a finite speed was fundamental in Huygens's work and on this assumption, he wrote to Colbert, he had "demonstrated the properties of refraction and, a little while since, that of Iceland crystal which is no small marvel of nature nor one which it is easy to understand". It was consequently gratifying that this assumption should receive confirmation and that the speed of light should be approximately known. There was some similarity of outlook between Roemer and Huygens for Roemer supposed, like Huygens, that the passage of light through crys-

tals (thrown into prominence by the Iceland Spar phenomena) was analogous to the transmission of impulses through contiguous spheres. The explanation of double refraction along these lines would, he considered, establish the truth of the theory of light which for some years had been associated with Huygens's name.

It is well known that Huygens was led to his theory of transmission of light through his work on impact. The transmission of longitudinal, compressional, vibrations through perfectly elastic spheres seemed to him to have applications in light, since crystals and other transparent media might be supposed to be composed of assemblages of atoms. And even though he was unable to suppose the atoms of the elements were the actual medium—since all substances are not transparent—he found a mechanism which reduced light to a form of motion and brought it within the treatment of the " true Philosophy ". In this philosophy " one conceives the causes of all material effects in terms of mechanical motions. This, in my opinion, we must necessarily do, or else renounce all hopes of ever comprehending anything in Physics." This quotation is taken from the beginning of Huygens's *Traité de la Lumière*. To explain the transmission of light through the Torricellian space and all manner of transparent substances, however, some pervading subtle medium was assumed. We must remember that Huygens was led to conclude that such a medium existed from his vacuum experiments. The ether was, however, not a continuous medium but was composed of very light particles in contact. These, on impact with the heavy vibrating atoms of incandescent bodies, transmitted their vibrations in all directions according to the laws of impact. The elasticity of air, Huygens thought, " seems to show " that it is made up of particles which are " agitated very rapidly in the ethereal matter composed of much smaller parts ". It was important that slight impulses travelled as fast as strong ones, a fact which was readily explained by applying Hooke's law of elasticity to the particles of ether. Also, individual wavelets by themselves were too weak to produce effects of light, which only arose when the wavelets combined to form a wavefront according to the well-known Huygens construction now given in all text-books on light.

Huygens's theory is better described as a pulse theory rather than a wave theory but in the *Traité* he made the remark that

the vibratory motion " . . . is successive and . . . spreads as sound does, by spherical surfaces and waves ". Hooke developed a wave theory also—largely in relation to his observations of colours produced by thin films. The difference between his ideas and those of Huygens was mainly that Hooke did not consider the formation of a wave-front by the innumerable individual wavelets.

Not all scientists, however, were prepared to accept Roemer's estimate of the velocity of light. Descartes had been so convinced of the instantaneous transmission of light that he unwisely said he would stake all his system of philosophy on its truth. Unlike Huygens there were many who remained under his spell. In the *Traité*, therefore, Huygens went to some pains to demonstrate the error of Descartes's reasoning. Cassini was opposed to Roemer's explanation of the apparent advance and retardation of the occultations of Jupiter's satellite, but mainly because only the innermost satellite had been studied. When the Académie had to decide on the dispute which arose over the work they came to the conclusion that Roemer was right; as he explained, the occultations of the outer satellites were less frequent and less sharply observable for obvious reasons. While the method he had put forward was the best one available for finding the velocity of light, he hoped that surface marks on Jupiter would prove of use, and later in the year observation of a spot on the planet gave the period of revolution of the planet on its axis. Observation of this spot could then be relied on in place of occultations for measurement of the velocity of light. Clearly Roemer was a man of the same outlook and ingenuity as Huygens. At the time when Huygens was at the Hague, Roemer was attempting to determine what effect the motion of the earth should have on the apparent positions of the heavenly bodies when this motion was transverse to the direction of the light rays. It scarcely matters that Roemer conceived the problem in terms of the Cartesian vortices; the point was that the circular motion of the terrestrial vortex should produce an apparent curvature of the path of light. In its modern form the problem was propounded and explained by Bradley, who discovered the effect of " aberration " in 1728.

Huygens made the journey back to Paris in the middle of the summer of 1678. With him went Nicholas Hartsoeker, later known as a maker of lenses. Once more Huygens settled down to

his old occupations. The period to which we have come was, unhappily, one during which he was not for long well. He was ill again in 1679 and although he recovered he was compelled again to return to the Hague in 1681. From this last convalescence he never returned to Paris. We are consequently faced with the fact that this was Huygens's last stay in Paris, and one which was seriously interrupted by illness.

Curiously enough, in view of the swift reverse which was in store, Huygens's prestige seems never to have been higher than it was at this time. It is clear that he was widely regarded as in a real sense the head of the Académie Royale des Sciences, the position of which seemed even more assured than that of the Royal Society at this time. The Royal Society in fact suffered from the political upheavals of the time and from the defection of some of its members. 1678 was the year of the Popish Plot, which, according to Titus Oates, aimed at the conquest of the kingdom by the Jesuits. As late as November it was held that "there hath been and still is a damnable and hellish plot, contrived and carried on by popish recusants, for the assassinating and murdering of the King and rooting out and destroying the Protestant religion". With the death of Oldenburg in this year, Huygens's relations with the society were practically at an end. The Académie Royale had on the other hand increased in vigour after its slow development in the first few years. Thanks to Colbert, Huygens and Auzout had been able to equip the Académie with all the laboratory and astronomical apparatus required and each year saw improvements in scientific technique. The society had begun to undertake enterprises such as the expedition of Richer to Cayenne in 1672 and this had led to important information concerning the shape of the earth.

Nevertheless, in France as in England there were jealous opponents of the new learning. The universities always feared an undermining of their authority if the scientific societies became too strong or too serious in their tasks, the Jesuits wished to have a monopoly of the new knowledge, and there were some who stirred the popular mind against investigations which seemed to a less and less degree to aim at the production of new inventions or the amelioration of life. In France, Paul Pelisson, who was writing a history of Louis's reign, gave Huygens space to deal with current criticisms and to enlighten the public on the aims and work of the Académie.

In this review Huygens limited himself to the particular field in which he was an authority. He began by alluding to the need of astronomical studies and the great importance of the new observatory. The use of pendulum clocks and improved telescopes had made possible observations incomparably more exact and more easy than they were in the time of Tycho Brahe. The discovery of new stars, the confirmation of the ring of Saturn and the discovery of its satellites, a full study of the moon's surface and the description of comets and sunspots were among modern achievements. The discovery and measurement of the velocity of light were adduced as a consequence of such observations. Nor was such work without practical value: the occultations of Jupiter's satellites provided a method of determining longitudes, a problem for which the pendulum clocks might soon provide a better answer. The great appeal of the work of the Académie, however, lay in the steady expansion of man's knowledge and understanding of the world. Expeditions had been sent to Cayenne and to Hveen; more exact star catalogues and ephemerides were to be prepared so that the theories of the universe might accord more exactly with observation. The Earth itself had been made the object of scientific measurement. Geometry had been applied " in the study of causes in the field of Physics, it being accepted by almost all philosophers of today that the figure and movement of the corpuscles of which everything is composed are alone responsible for all the wonderful effects which we see in nature ". This is really a statement of the new mechanistic philosophy to which the physical researches of the men of science had led. The world had come more and more to be regarded as a perfect machine and, says Burtt[1], "first in Huygens and (in a more philosophical form) in Leibnitz we have this opinion unequivocally proclaimed ". Huygens clearly feared that this summary might seem to be written from the standpoint of the vortex theory of Descartes and he went on: " which opinion having been greatly supported through the philosophy of Descartes, they nevertheless adhere neither to his sentiments nor to those of any other philosopher in order to gain authority ". Descartes, he pointed out, was mistaken in many things through lack of experiment and in particular he had sacrificed the accurate definitions of concepts which Galileo had begun to clarify. Truer ideas were now held regarding motion and force and momentum, the nature of

[1] E. Burtt: *The Metaphysical Foundations of Modern Science* (1932).

meteors and other celestial phenomena, and the nature and effects of light. Microscopes, telescopes, the air pump and many other machines had been brought into use and had extended man's knowledge and led to the advancement of the sciences.

Nevertheless, while Huygens was an enthusiast for the new world of the seventeenth century science, he displayed caution in comparing his own age with that of classical antiquity. His friend Charles Perrault frankly regarded the seventeenth century as superior to all other ages and considered Huygens himself an example of this superiority. To such praise and to that of the younger Fermat, who compared him with Descartes, he returned a modest reply. "I am one of those who have profited from the wisdom of that great man," he wrote.

That Huygens was not everywhere so popular and that there were factions in the Académie at this time can scarcely be doubted. The eminence of Huygens, in whose honour a medal was struck in 1679, was not agreeable to Cassini and de la Hire and the latter is known to have led an opposition to the entry of all foreigners—and especially the friends of Huygens—into the academy. In France just as in England religious differences were being exploited for political ends. The greatest division within the Académie Royale seems to have resulted not from nationalistic or religious partisanship but arose between members who, like the original Montmorians, were eminently followers of Descartes, and those who, like Huygens and Mariotte, showed an increasing scepticism towards the Cartesian system. In these circumstances Huygens seems to have felt more affinity with men of science who were not involved in the dispute and with Leibnitz in particular there grew up an interesting correspondence.

It will be remembered that Leibnitz had studied mathematics with Huygens in 1672. During the subsequent years the German mathematician had pursued his researches along new paths. In 1676 he had been in correspondence with Newton about methods of expansion in series. Newton mentioned his binomial theorem and the method of fluxions but did not describe the latter, although he added some illustrations of its use. By 1675 Leibnitz was employing his own form of differential calculus but was unable to involve Newton in discussing anything which might arouse controversy. Newton's method of fluxions was, in fact, not published until 1693. In correspondence with Huygens, Leibnitz claimed to have developed the calculus into a method by

means of which he had successfully treated a variety of problems. Huygens, however, would not abandon geometry for the differential calculus and never gained any facility in its use. Leibnitz wrote at length also about the subject of symbolic logic, of which he was an originator, but his ideas were not appreciated by Huygens or anyone else at that time and they were not taken up until the following century. Leibnitz was anxious to secure nomination to the Académie Royale as a foreign member but this Huygens seems to have been unable to obtain. Not until 1700 was Leibnitz—and in the same year Newton—admitted as a foreign member. It was in 1700 that Leibnitz organized the Berlin Academy of Sciences.

At the Académie Royale Huygens's chief activity at this time was the presentation of his work in geometrical optics in a series of lectures lasting from May to August. The whole summer of 1679 was spent in editing the work of many years before and with special problems concerning Iceland spar. Much of the work on the optical properties of conic sections which comes at the end of the *Traitè de la Lumiére* was completed about this time. Fermat's least-time principle, also, he succeeded in deducing for refraction on the assumption that light travels more slowly in glass or water than in air. As for Descartes, both Huygens and Leibnitz had scant regard for the greater part of his work in this field. His "pretence of a demonstration" of the laws of refraction was replaced by the well-known treatment which employs Huygens's secondary wavelets. Huygens's work in optics may in fact be regarded as standing in relation to previous studies by Kepler, Snell, Descartes, and Fermat much as Newton's mechanics stands in relation to the mechanics of Galileo and Huygens: Huygens achieved the same union of the physical and mathematical aspects of the subject. His mastery of geometry of course equipped him in a unique way for this task. The subject of colour was, however, left on one side; Huygens seems always to have held that a mathematical explanation of this was not possible. Nevertheless, he appreciated the practical outcome of Newton's work in this subject: the discovery of chromatic aberration of lenses showed, he saw, that this effect may be no less important in telescopes than spherical aberration. It followed that the search for the achromatic lens might be more profitable than the attempt to obtain lenses with non-spherical surfaces had been. He would probably have had to

admit by this time that the idea of the reflecting telescope was by no means "extravagant".

The interest aroused in microscopic work by Hooke's *Micrographia* and the translation from Dutch into French of Leeuwenhoek's work by Huygens about 1677 led many at this time to take up such studies. The whole world of infusoria awaited discovery. The imperfections of the compound microscope were still considerable, however, and Leeuwenhoek, as is well known, preferred to use a single lens of short focal length in his observations, which perhaps included the discovery of bacteria. Huygens used very small lenses of glass, some of which he made hollow and filled with alcohol. Locke, who was in Paris in 1678, wrote to Boyle of the "extraordinary goodness" of Huygens's microscopes. In devising a mount for his very small lenses Huygens introduced a method of altering the intensity of illumination of the object. Later, in 1692, he introduced dark ground illumination. These were the contributions of a practical microscopist. After 1676, in fact, Huygens was very interested in making observations of infusoria in rain water.

Ill-health no doubt accounts for a diminution of the mathematical and more abstract studies of Huygens after 1680. He left Paris at the end of the summer of this year for a short stay at Viry, where the country air restored him for a time. He returned to the capital in time to take part in observations of a comet and as a recreation started the construction of a planetary machine which would reproduce by means of clockwork the relative motions in the solar system. Early in 1681 he was again ill, but not until September was his return to Holland practicable.

XIII

The convalescence after this last illness was slow. Letters arrived bearing the good wishes of men of science in Paris and London. Even de la Hire, only recently elected but before long a prominent member of the Académie, sent the good wishes of "all the company". It is evident from his letter that de la Hire hoped for the position which Huygens had left at least temporarily vacant; there is a strong presumption that he did in fact work hard to prevent Huygens from having much opportunity to return to Paris. Huygens for his part was at first in no hurry to leave Holland even when, in 1682, he had practically

recovered. Later in the year the Dutch East Indies Company showed interest in the latest pattern of marine clock and this was a further incentive to remain. By taking up the summer of 1683, the work on the new marine clock really decided his future, for Colbert died in September of this year and without his patron's support the opposition to Huygens's return began to be really formidable. Not only this but the political horizon was dark with the uncertainties caused by the renewal of Louis's activities abroad. By 1686 the situation in Europe was not unlike that of 1672. The Revocation of the Edict of Nantes (1685) roused all Protestant countries. To Holland fled a large number of exiled Frenchmen. It was a time of rapidly diminishing freedom in France and Huygens's experience well illustrates the fact. For when he renewed his application to return the appeal fell on deaf ears. Whether anti-Protestant feeling was the sole reason is not altogether clear. Huygens's friend Roemer left Paris a few months earlier, and it was four years later that the Edict was repealed. Quite as much as anti-Protestantism, probably, personal jealousies spoiled the work of the Académie; the years after 1681 seem indeed to have been years of retrogression.

It is a striking illustration of the hostility which the Cartesians had come to feel for Huygens at this time that the Abbé Catelan should, nine years after the publication of the *Horologium Oscillatorium*, attack the fundamental principles employed by Huygens in his treatment of the compound pendulum. There is nothing of scientific interest in Catelan's criticisms and they were designed to discredit Huygens's work in the eyes of those who were comparatively ignorant of mathematics and mechanics. The mathematician James Bernoulli came forward to champion Huygens's ideas in 1684.

In the middle of 1684 Huygens was visited by Thomas Molyneux, a contemporary and acquaintance of Flamsteed and Hooke. Molyneux wrote to his brother that he was received "extraordinarily civilly". Huygens, he said, "beyond my expectations talked to me in my own language, and pretty well". He was shown Huygens's planetary machine which he decided was "nothing more than an ingenious curiosity" for, he said, "I asked him could he by help of it exactly determine an eclipse, and I observed that he could not give me a positive answer, as being loath to confess the imperfections of his contrivance to me that seemed to admire it so much as I did". Huygens had in fact

come to the end of his great period of scientific activity, but some profound ideas were yet to be developed. The news of the death of Picard (1682), who had done notable work at the Paris observatory, caused Huygens to ponder the uncertainty of life and to think of publishing the "many good and useful things which I have written or found, to complete which I desire only peace and the continuance of my health". As events turned out his retirement held more of solitariness than he desired. The death of his father at a great age in 1687, and the departure of his brother Constantin for England in 1688, when William of Orange became King of England, left him alone in the isolated residence at Voorburg in the summer. The winters he spent at the Hague. In his letters he lamented the absence of any with whom he could discuss scientific topics. Owing to financial worries he began to consider the possibility of securing a position as counsellor to William III, but this only embarrassed the King, who perceived that Huygens had "higher ideas than to loiter with administrators".

The idea of a position in England seems to have occurred to Huygens after a short visit to this country in 1689. He was in London from June to August of this year, but only brief records remain. He met Flamsteed at Greenwich and attended a meeting of the Royal Society at Gresham College. In company with Fatio de Duillier, a Swiss mathematician, he met Newton for the first time. Little is known about this or of another occasion when, in July, Huygens, de Duillier, and Newton travelled from Cambridge together on the occasion of Newton's application for the position of Provost of King's College. Huygens also met Boyle on several occasions and witnessed some chemical experiments. He left London with many regrets for the isolation in which he then lived.

It would, of course, be extremely interesting to know what discussions took place between Newton and Huygens on the occasions when they met. It is clear that in mechanics the two scientists held certain divergent views, notably on the subject of the conservation of energy and on the existence of absolute space and time. For Huygens, after reading the *Principia*, became strongly critical not only of Newton's postulate of universal gravity but also of his belief in the existence of absolute space and motion. He had early perceived that a body, moving uniformly in a straight line with respect to one observer, might be

accelerated with respect to another. And while he first made a distinction in favour of the absolute nature of motion in a circle which was accompanied by the existence of a centrifugal force, he abandoned this also after reading the *Principia*. This was in contrast with Newton's acceptance of an absolute space and time according to which all motion possessed an absolute character. Whether they discussed such differences of view and whether they compared notes on the subject of resisted motion and other matters in which they were both interested at this time is not known.

Over the question of the cause of gravitational attraction there was, of course, a complete divergence of view between Newton and Huygens, for while the latter speculated for some time on the subject it was one for which Newton felt no interest. Huygens went out of his way to expound his views in such a way that they would not give Newton any offence. He seems to have been a little nervous about Newton's reception of yet another hypothesis. It may be remarked here that Huygens's theory grew out of his work on the nature of light and was an attempt to explain gravity as due to the action of an ether or fluid matter which, owing to rotation, seeks to travel away from the centre and thus, as he thought, forces slower moving bodies together. Even at this time, however, the difficulties of such a theory were becoming clearer. A fluid which could permeate matter could scarcely exert a reaction on it and de Duillier, who had gone to England for the purpose of studying Newton's works, pointed out to Huygens that the absence of any apparent resistance to the motion of planets and comets argued that the ether must be excessively attenuated. As is now known, however, Newton was not so thoroughly opposed to the ether theory as was generally supposed. Although he condemned the idea (as expressed by Hooke) in 1675, he returned to the question in the "Queries" to his *Optics*.

The inverse-square law of gravitational force posed great difficulties for Huygens's mechanistic theories. It was, he said, "a new and very remarkable property of gravity of which it was very necessary to search out the reason". He could not see that the cause could be given on the principles of mechanics or of the rules of motion. The view that gravity was an inherent property of matter, he said, "takes us very far from the principles of mathematics or mechanics". Leibnitz also was

against Newton's view of gravity as innate. If it was to be regarded as a "law of God who brings about this effect without using any intelligible means, then it is a senseless occult property, which is so very occult that it can never be cleared up, even though a Spirit, not to say God himself, were endeavouring to explain it," he wrote to Hartsoeker.

The importance of Newton's work was not, however, lost on Huygens, who perceived that the *Principia* utterly destroyed the Cartesian vortices. Writing to Leibnitz about the elliptical orbits of the planets Huygens said he would like to know if he could continue to hold to Descartes's vortices after reading the *Principia* since these "in my view are superfluous if one accepts the system of Mr. Newton in which the movement of the planets is explained by the gravity towards the Sun and the *vis centrifuga* which balances it . . . ".

The extreme Cartesian view of gravity was expounded (1690) by Régis in a book on Richer's observations at Cayenne. The explanations put forward were closely similar to the ideas expressed by Huygens in 1669 on the occasion of the discussion at the Académie Royale. Régis made no mention of Newton in his book. In 1690 Huygens felt a good deal of uncertainty and wavered between his original ideas and the view expressed in his letter to Leibnitz. The appearance of the tract on the cause of gravity at the end of the *Traité de la Lumière*, published in this year, cannot be held to represent Huygens's final views, about which more will be said later. In England the effect of the *Principia* was more profound. Fatio de Duillier said that some of the Royal Society were "extremely prepossessed" in the book's favour and reproached those who were not under its spell as being too Cartesian. "They . . . have led me to understand that after the meditations of their author all Physics has been much changed" he wrote to Huygens. There can be no doubt that on the Continent the criticisms made by Huygens and Leibnitz strengthened the position of the Cartesian philosophy for a good many more years. And yet Huygens's own work was, at its best, as opposed as Newton's to the Cartesian frame of mind and he did a great deal to dispose of the errors of Descartes's physical ideas. The last five years of Huygens's life were in fact to be years of crisis for the Cartesian philosophy. Leibnitz and Huygens would have developed an alternative analysis which freed itself from Descartes's errors while at the

same time rejecting Newton's conception of matter, time and space but this project was never carried through. In the event their effect was to delay the acceptance of Newton's work while at the same time weakening the supports on which Descartes's physical teaching rested.

In the meantime the more orthodox Cartesians were driven to great lengths to show that the new results of scientific research were fundamentally in accord with Descartes's ideas. Even the differential calculus was claimed by Catelan to be derivable from Descartes's geometry. The faulty treatise which he produced to support his view led to a dispute with the mathematician de l'Hôpital. The latter, regarding Huygens as a natural ally, gave violent support to the latter's mechanics, also criticized by Catelan. This somewhat embarrassed Huygens, who was by no means sure of some of de l'Hôpital's ideas on this subject. For de l'Hôpital tried to obtain some sort of proof of the principle that the centre of gravity of a system of connected bodies cannot rise under the sole action of gravity. Huygens preferred to regard this principle as self-evident. Pascal and Torricelli, he pointed out, had used the same idea though limiting it to statics.

Of a different character was Huygens's correspondence with Pierre Bayle about this time. This famous French sceptic was appointed professor of philosophy at Rotterdam in 1681, so that he arrived in Holland in the year that Huygens returned from Paris. Under the conditions of Catholic intolerance the intellectual ferment, once concentrated in Paris, was becoming diffused into the freer but less educated provinces and into Holland. In 1684 Bayle started a periodical entitled *Nouvelles de la république de lettres*, the first number of which he sent to Huygens. The latter became interested in Bayle's aims and received him at his house, where he enlightened him on the subject of scientific studies. His correspondence with Bayle came to an end, however, after the philosopher was condemned as an atheist in 1693. Bayle's view was that religious dogma is of its nature irrational and that there is no merit in believing that which is merely consonant with reason. This outlook of *credo quia absurdum* was one which could not appeal strongly to Huygens.

Rather more interesting was Huygens's correspondence with Pierre Daniel Huet, another sceptic whose avowed purpose was

to discredit reason in the realm of religious belief. Both Huet and Bayle actually influenced thought in the opposite direction to that which was intended, that is, towards scepticism. Huet in addition was strongly opposed to the rationalism of Descartes and strongly in favour of empiricism. In October 1689 he sent Huygens a copy of his *Censures* on the Cartesian philosophy. In reply Huygens said that he also had meted out rough treatment to Descartes, and that he hoped that his scientific work had replaced Descartes's doctrines with truer ideas. He agreed with Huet that while Descartes had overthrown the older philosophies he had borrowed from them their dogmatic spirit. He had had the ambition to be the author of a new philosophy and in his haste had been led to maintain ideas even against disproof. To Huygens this philosophy appeared as the successor of Aristotelianism. Nevertheless, when Martin van Helden, a Cartesian and professor of mathematics at Louvain, was threatened with imprisonment for his criticisms of scholastic philosophy, Huygens assisted him so that he should not become "a martyr to Cartesianism". He did not feel very strongly over this matter, for the battle of experimental science against the *a priori* philosophies seemed to him to be won. "It seems to me" Leibnitz was able to say, "that the Cartesians have very much declined and that they have not too many able men".

XIV

In 1685 Huygens was still negotiating over his return to Paris and there were endless letters, many of them unanswered, sent off from Holland. Nevertheless, it is not really certain that he wanted to go back, and he may well have been deterred by knowledge of the changed conditions at the Académie. Up to 1688 he stayed on at the Hague, and in the spring of that year he settled at Hofwijk, a property in the neighbourhood of the city which had belonged to his father. After his father's death in March 1687, the house was lent to Christian by his brother Constantin, who left with William III on his memorable expedition to England in the following year.

In these last years (1685–95) Leibnitz was solicitous about Huygens's unpublished works and recommended him to conserve his strength, for, he wrote, "I do not know anyone who could replace you". Huygens's old age was a lonely one and he

was troubled with ill-health, but, he replied, "I see that one becomes accustomed to all these things". He was not quite forgotten, for Charles Perrault drew public attention to the great scientist's work in his *Parallèle des Anciens et des Modernes* and when Varignon was about to publish a book on mathematics he felt that he must take the opportunity, as he said, of paying homage to "the greatest mathematician of our age".

The pattern of Huygens's life remained much the same right up to the end. He continued to work on the improvement of lenses, on the spring-regulated clock and the marine clock, and the writing of his last work, the *Cosmotheoros*. Undaunted by the unpromising performance of the various marine clocks he had constructed since 1663 he continued, with characteristic patience, to labour at this still urgent problem of the marine chronometer. In 1685 he went himself on a short trial (the only one) on the Zuyder Zee. In 1686 and 1690, clocks fitted with bifilar pendulums were sent in charge of the captains. All these trials were unsuccessful. The failure of the bifilar pendulum was the greatest disappointment, for work on this type of clock dates as far back as 1673 at least, since it was described in the *Horologium Oscillatorium*. After 1690 Huygens experimented with a new type of regulator and reverted to the spring drive which he had tried at the beginning and then abandoned. The new clock went well in laboratory trials and in 1694 Huygens hoped that the Dutch East Indies Company would take it up. He died before anything further could be done.

The fact that Huygens could not accept the chief conclusions of Newton's *Principia* is the most interesting fact that comes out of his correspondence at this time. Five years after its appearance he wrote of Newton, "I esteem his understanding and subtlety highly, but I consider that they have been put to ill use in the greater part of this work, where the author studies things of little use or when he builds on the improbable principle of attraction." The idea of universal gravitation "appears to me absurd" he wrote. Yet he felt compelled to admit that Newton's explanation of comets was incomparably better than anything imagined by Descartes. It was difficult to see how comets could cut across the vortices imagined by Descartes, or to explain the eccentricity of the planetary orbits and the real accelerations and retardations of the planets in their orbits except on the lines laid down by Newton. Over the shape of the Earth, also, Huygens

was in accord with Newton. He did not deny that if the gravitation of the planets towards the sun were taken as inversely proportional to the square of their distances " this, with the centrifugal virtue gives the Eccentric Elliptics of Kepler ". But he and Leibnitz, far from feeling that this reduced the solar system to order, felt that it raised an insistent question of how gravitation arose. Leibnitz thought he could perceive an analogy with the intensity of light which, as a simple geometrical deduction, also obeyed the inverse-square law. Rays of attraction might be imagined which caused bodies to descend if their centrifugal force diminished. These rays were dismissed by Huygens, however, as incompatible with his theory of a circulating medium. It almost seemed as if a return might be made to Kepler's identification of gravity with a kind of magnetic attraction. Leibnitz, at least, inclined not a little to this view. Both he and Huygens insisted on attributing the effects of gravity to the medium which they believed pervaded the universe. Consequently they were both interested in the study of motion in a resisting medium, for they no doubt perceived that this was the Achilles' heel of their system. If the medium had mechanical properties exhibiting themselves as gravitational force, magnetic force and in other ways, what influence must it have on the orbital motions of the planets and on terrestrial motions? Newton's *Principia* had dealt with this problem and much of the work was deliberately aimed at the overthrow of the Cartesian vortices. Huygens considered Newton's treatment to be not without fault but he agreed with him as against Leibnitz over the definition of resistance, " for you," he wrote " call the resistance the velocity lost or the loss of velocity caused by the medium . . . For Mr. Newton and myself, however, the resistance is the pressure of the medium against the surface of the moving body . . . " It is really astonishing to us now that Huygens did not see that Newton's study of resisted motion completely disproved the vortex theory, but we must remember that the elastic fluid theory was held in the nineteenth century under even greater difficulties. Furthermore, a comparison of Huygens and Leibnitz at this date leads to a decision in favour of Huygens's notions. In 1692 Leibnitz still supported vortices, while accepting Kepler's laws; Huygens had at least got to the point of seeing the overwhelming force of the quantitative work of Newton even while he rebelled against innate gravity. As he finally left them, the

vortices were considerably attenuated affairs, suitable only for popular exposition of the sort found in the *Cosmotheoros*. Leibnitz, on the other hand, converted the subtle matter of Descartes into a production of his own: the *materia ambiens*.

A good deal of what we should term pure mathematics crops up in Huygens's and Leibnitz's letters. Leibnitz took up several of the problems studied by Huygens and gave them new form. "My design has been" he wrote, "to give a little trouble to these good Cartesians who, through having read the Elements of Bartholin or Malebranche, believe they can do all in Analysis." There followed a series of letters in which Leibnitz gave Huygens an account of the differential calculus and its uses. He was able to investigate the properties of a curve like the cycloid, he said, from a purely analytical treatment and without any recourse to the figure. In regard to the calculus, Leibnitz was not a clear expositor. It is clear that from one aspect the new method was regarded not so much as a development of pure mathematics as an instrument for physical research. The union of mathematics with experiment is what Sir William Dampier has called the "new mathematical method". For Huygens, as for Galileo and indeed for Newton, experiment had not achieved the position it later held in certain branches of science. From a comparatively few observations, by the aid of "geometry" one could advance far into new realms, a fact which is well illustrated by Huygens's work on impact and on the compound pendulum. "It must be admitted" wrote Huygens, "that geometry is not made for all sorts of minds."

From being sceptical Huygens soon became envious of the *calculus differentialis*. Finding Liebnitz's accounts rather obscure he wished that either he or Bernoulli could be there to assist him. Some collaboration did indeed spring up with Fatio de Duillier, and Huygens's note-books contain many pages of working on the new lines. The great change of outlook was a difficult one for the great geometer and he did not attain facility in the use of the calculus. The new calculus, Leibnitz emphasized, gave its results by a kind of analysis without any effort of the imagination, "and it gives us over Archimedes all the advantages which Vieta and Descartes have given us over Apollonius".

Fatio's work on the calculus is important in the history of the subject, for it was through him that the dispute between the

followers of Newton and Leibnitz sprang up. More's *Isaac Newton* (1934) gives a good account of the episode. Fatio seems to have become resentful of Leibnitz's rather superior criticism of his work, which was of an undistinguished nature, and it is considered that Fatio smarted under a sense of grievance. After returning from England, where he had been in contact with Newton, Fatio wrote to Huygens, saying that priority for the invention of the differential calculus certainly belonged to Newton. He suggested that Leibnitz's ideas were in fact obtained from Newton's letters which went back to 1676 and 1677. The publication of these, he hinted, would embarrass Leibnitz. Actually, these dates were beside the point since both mathematicians seem to have used the calculus much earlier. Leibnitz, at any rate, affected to be unimpressed by news of Newton's advances and hinted to Huygens that he had done a number of things of which Newton knew nothing. Huygens's part in the quarrel which ensued was small but noteworthy for it was through him that Leibnitz first learned of the charges made by de Duillier.

About this time an interesting comparison in mathematical methods was made through the study of the same problem by Leibnitz, Huygens and James Bernoulli. This problem was the one propounded by Mersenne many years before; to find the theoretical form of a chain suspended from its two ends which are at the same height, so that a curved line hangs between them. The publication of the results showed a fair agreement between the three mathematicians but showed up the advantages of the calculus, which was more and more applied, not only to new problems but also to others already solved by classical methods. Huygens was not altogether pleased by some of the new methods, notably that adopted by James Bernoulli in solving the problem of the centre of oscillation. But the truth is that the form of Huygens's work in mathematics had always been somewhat reactionary and was fast becoming obsolete.

There is a faint echo of the Royal Society and the pleasant days spent in England in some of Huygens's last correspondence. Constantin, his brother, in 1691 presented a telescope objective having a focal length of 122 feet to the society. This was by no means the best achieved by Huygens. During these years he made one of 210 feet focal length. The 122 foot lens was a fine objective, however, and Hooke was entrusted with the erection

of an aerial telescope to accommodate it. The lens was marked by Constantin so that it should afterwards be readily identified—Constantin apparently not finding Hooke any more trustworthy on personal acquaintance than he had anticipated. This marking has made possible the identification of the objective and examination of its quality in recent times. The figuring and centring of the surfaces is described as "astonishingly efficient," but the quality of the glass is "hopelessly bad". From Constantin, Huygens heard of the death of Robert Boyle. "Mr. Boyle is dead as you know already without doubt" he wrote to Leibnitz, and added "It seems strange that he built nothing on all the experiments with which his books are filled; but the thing is difficult, and I never thought him capable of as great an application as is necessary to establish the true principles." Since Huygens had often expressed his admiration of Boyle this comment may be taken to indicate more esteem of the difficulties of Chemistry than depreciation of the father of that science.

The other prevailing practical interest, besides the clock, to which Huygens gave attention during these last years was the telescope. In 1684 he published his *Astroscopia Compendiaria.* This contained an account of tubeless telescopes and this may explain how it is that Huygens has been supposed to be the originator of this difficult and ultimately unsatisfactory technique. He was compelled to reconsider his attitude to the reflecting telescope through the apparently insuperable obstacles raised by aberration—chromatic and spherical. Yet he still preferred the refracting telescope because metal mirrors took such a poor polish and the grinding of glass mirrors was extremely difficult. Moreover, it was at that time very difficult to silver the upper surface and silvering the back surface meant that a second image was formed by partial reflection at the top surface so that a double image was formed. Even if the radii of curvature of the two surfaces were such that the two images were coincident the difficulty remained that the images could not be of the same size. Huygens's conclusion that in practice the weaker image would not be distinguished by the eye if the images were coincident led him to renewed experiments but the work does not seem to have progressed very far.

To these last years belong the remainder of his work on his *Dioptrica,* a paper on harmonics, his continued studies in mathematics—partly in collaboration with David Gregory, who visited

him in 1693—and the clarification of certain points in his atomic theory. Huygens was a strong supporter of the atomic doctrine which was later to provide the basis of a scientific development of Chemistry, but he is not usually numbered among those who developed the atomic theory simply because his researches led him in the direction of Physics. He continued to oppose Newton over the doctrine of universal gravitation and added to this his difference from the English scientist over the corpuscular theory of light then gaining wider acceptance. The extreme rarity and speed of the corpuscles and the absence of any obvious way of explaining colour on this hypothesis were Huygens's main objections to the theory which was for a century to overshadow his own elegant treatment of the propagation of light. Huygens however, was not inclined to seek controversy. In 1694 he had occasion to correct Renau, an engineer to Louis XIV, over his connotation of force, a matter which was too important to overlook even in his declining health.

Huygens himself recognized that the illness which had dogged him since his return from Paris had become much more threatening. In March 1695, Huygens felt it necessary to call his lawyer and make the final corrections to his will. The following month he became worse and from then until July, pain and sleeplessness spared him hardly at all. He lived in dread of losing his reason and his days were filled with a deep despair. Constantin visited him for a few days at the end of May. Neither he nor the Calvinist pastor who was at last brought against Huygens's wishes could give him comfort. Against the doctrines of personal immortality and the exhortations of the Reformed Church he maintained an obstinate scepticism. Huygens died facing the problem of individual and personal immortality as "a problematical question," his attitude in this contrasting with that of other seventeenth century scientists, especially Boyle, Pascal and Newton.

Nevertheless, when the end came during the afternoon of July 9th, the failing spirit, weakened by suffering, may have found some serenity. The author of *Cosmotheoros*, it has been well remarked, revealed himself, in the face of problems less abstract than those of mathematics and mechanics, as sensitive to the deep realities of life and the profound aspirations of the human spirit. But he could not forget that all experience may be subjected to the scrutiny of a dispassionate mind, and dispas-

sionate and remote Huygens always seems to have been. He lacked perhaps, a sense of the mystical and preferred to look on life—and death—with the vision of one to whom all things appeared capable of an ultimate rational explanation. For him as for Mersenne, the technique and method of the sciences were exalted because they freed men's minds from error. But for Huygens there was no "two-fold truth". Truth for faith and truth for reason were, for him, one.

The professional and serious interests of Huygens are the ones which are uppermost in his correspondence. Nevertheless, it would be a mistake to consider him as always having been nothing but a patient researcher. He was a man of wide culture and of acquaintance throughout Europe. The poetry and music of his own country, with which he was well conversant, were of a high order; its painting reached the summits marked by the names of Rembrandt, Franz Hals, Vermeer. In Paris, Huygens used frequently to visit noted musicians, among them the harpsichordist Chambonniere. He himself played the harpsichord. Nor was he averse to feminine society. One reads of his meetings with Marie Perriquet at the country house of the academist Conrart, with fashionable women at the lectures given by Rohault. Still more interesting is the fact that he was an occasional visitor to the salon of Madelleine de Scudery. Marianne Petit, daughter of one of Louis XIV's engineers, seems to have had especial charms for Huygens but she never married and their separation was due to her withdrawal from society when she entered a religious order. There were even scandals about Huygens during the war of 1672–8, for he paid frequent visits to Madame Buat, a grand-daughter of the Dutch poet Cats, but not possessed of the highest reputation for virtue. There were, too, some distant cousins whom he visited in Paris, and for the eldest of these there is no doubt he felt considerable attraction.

Huygens moved in an elegant and leisured society during his years in Paris and there are echoes of days at Viry, where the Perraults had a fine country house, as well as faint glimpses of an elegant Huygens visiting occasionally the salons of the city. The contrast of this varied life with the quiet of his days near the little village of Voorburg must have been marked. But in fact great changes were taking place and Catholic intolerance in France was undoing much that Colbert had been at pains to

build. After 1685, it was impossible for Huygens to think of returning to Paris. Holland remained what she had been early in the century, a refuge for free spirits, a country where new thoughts might still be brought into the world. But Huygens was past the days of his vigour and little that is new can be attributed to his later years. As an exceptionally placed observer he had seen a good deal, moving as he had done, in the diplomatic and learned worlds of his time. The political absolutism of the age must have contrasted strongly, one would have thought, with the new freedom of speculation which survived all repressive measures. But Huygens does not appear to have remarked on this. The doctrines of expediency, sovereign right, vital interest and the like have survived, and freedom of thought requires to be continually defended. That much is clear. But Huygens belonged really to the world of abstract thought and shrank from contact with political affairs. In his own world he was as Leibnitz and John Bernoulli agreed, " incomparable ". His loss, Leibnitz averred, was "inestimable" for he had equalled the achievements of Galileo and Descartes; helped by what they had done he had surpassed their discoveries: " In a word he was one of the greatest ornaments of this time."

Part II

HUYGENS'S SCIENTIFIC WORK

I

The State of Science in the First Half of the Seventeenth Century

The task of this chapter is not so much to describe actual achievements of the men of science as to explain how at this period some of them looked at Nature, and to indicate what particular problems they felt called upon to solve. Such a question bristles with difficulties. It may be that it is of the type which beckons on only the foolhardy. However that may be, it certainly must be admitted that the views expressed are necessarily of rather a personal character; they will invite, on some points at least, as much dissension as agreement.

Very probably there is a good deal to be said for the view that steady progress in the physical sciences had to wait until the seventeenth century because of the unsuitable manner of analyzing events which came from the study of Aristotle. Aristotle's interest, undoubtedly, lay rather in logical relations and his notion of change fitted in with the conception of development of form, of growth of living organisms, far better than it did with the phenomena of the inanimate world. Rejecting the notion of sheer physical determinism somewhat as Plato had done, he directed all attention to the purpose served by events and hardly any to the conditions which were invariable antecedents. The question *why* things happened as they did was always tempting and appealed to men as an outlet for temperament; it was so much less restrained and impersonal than the question *how* things are caused, and so long as men's minds were bemused with an elaborate hierarchical scheme the latter scarcely seemed to matter. For Aristotle held that change is purely a terrestrial affair, at least it must be limited to the sub-

lunary sphere, and within that sphere there were just a few natural motions through which the different elements could attain their proper level. The natural motion of "earth" downwards, or of "fire" upwards, were thus matters which invited no discussion. For "unnatural" motion, such as that of a lump of earth horizontally, a constant push must be applied. For motion in a circle, on the other hand, as in the case of the apparent motions of the heavenly bodies, no acceleration towards the centre was ever dreamed of; Aristotle had nothing but reverence for this form of motion for it was, he taught, the perfect form, and proper only to the celestial bodies.

Galileo's breaking away from scholastic doctrines must have come after a period of perplexity, for he no doubt tried out current explanations for what they were worth. From Roger Bacon to the Accademia del Cimento is a long distance in time and there was a considerable change in outlook. The difference was that the Accademia existed to learn about the world through experiment and to cast its explanations in a form which was no longer subservient to religion. But in such an intellectual climate as existed at the very beginning of the seventeenth century the only safe plan was to concentrate on the description of motion in geometrical terms and this, of course, Kepler was fortunate enough to do. Kepler stood little danger of the condemnation meted out to Galileo in spite of his almost equally great fame, because his work could be regarded as a presentation of Aristotle's formal cause in an elegant mathematical form. Mathematics, indeed, remained respectable, supported still by the prestige of Plato, and in the sixteenth century there was in some of the Italian centres of learning a renewed interest in the teachings of Pythagoras. From Pythagoras, Whitehead has remarked, "mathematics and mathematical physics took their rise. He discovered the importance of dealing with abstractions; and in particular directed attention to number as characterizing the periodicities of notes of music. The importance of the abstract idea of periodicity was thus present at the very beginning both of mathematics and of European philosophy." For Kepler "the cause of the observed effects is the mathematical beauty and harmony discoverable in them".

The history of this mathematical treatment of phenomena is usually somewhat briefly dismissed, as if it all resulted from the insight of Galileo and Newton—and all the rest were fools. But

Copernicus, it should be remembered, showed a fundamentally similar attitude much earlier, although neither he nor Kepler concerned themselves with the physics of their problems. By 1543, the date of his *De Revolutionibus Orbium Cœlestium*, he had analyzed linear simple harmonic motion as the product of two combined circular motions. This is the first record of a study of such a form of motion which Copernicus himself described as *pendentibus similes*—after the manner of suspended bodies. But, characteristically enough, circular motions were still thought of as raising no particular problems. Benedetti, however, in his *Disputationes* of 1585, showed an advance in that he adduced linear simple harmonic motion in controverting some of Aristotle's ideas about motion. Then, as against Aristotle's teaching that bodies fall towards the centre of the earth because that is their "natural" place, he asserted, though he did not prove, that if a weight were dropped through a hole passing through the centre of the earth, it would by no means come to a stop but would oscillate backwards and forwards in the manner of a pendulum bob. This interest in accelerated motion marked the beginning of the criticism of Aristotle which was to grow so much in intensity during the next hundred years. It may indeed be said that modern science begins with the study of dynamics and in particular with perplexities about simple harmonic motion.

Galileo, in his *Dialogue concerning the Two Chief Systems of the World* (1632), made considerable use of the simple pendulum in his study of naturally accelerated motion. He restated Benedetti's view of the subsequent motion of a body dropped through a hole passing through the centre of the earth. Dissatisfied with the formal cause which set such limitations to Kepler's work, he looked, in the case of accelerated motion, for what Newton later called an external impressed force. He even had some notion of the necessity of postulating action and reaction, for in writing of the simple pendulum he perceived that the thread, in so far as it possesses mass, must "hinder" the motion of the bob and that there was a problem—the problem of the compound pendulum—which remained to be solved. His study of the simple pendulum shows, however, a necessary amount of idealization, without which a relation between the period and the length would have been excessively hard to find. Galileo's work, in fact, marks the first successful union of experiment and mathematics,

for he brought together the criteria of the simplicity of natural laws and their accordance with experiment. The problems he bequeathed were as important as those he solved: the concepts of mass and momentum, and of force and work, and the law now known as Newton's Third Law were all apprehended in only a somewhat confused way at Galileo's death. Mersenne's *Harmonie Universelle* (1636) is not regarded as being of such moment as the writings of Galileo, to which Mersenne was in fact greatly indebted. He deserves credit, however, for being the first man in Europe to perceive Galileo's genius and to promulgate his ideas.

So far all that has been said has been concerned with the bright and positive side of the picture of the early seventeenth century, but there was a vast difference between the conditions which then ruled and those of today. Today we are accustomed to an intellectual demarcation of Science and Philosophy and to almost equally sharp boundaries between the various sciences. Then there was a chaotic muddle of "philosophies" and no tacit agreement on the fundamental concepts through which phenomena are ultimately explained. If we single out the great names it is possible to see the seventeenth century as a period of amazing clarification, but this it must rarely have appeared to be to the contemporary men of science. The alchemists, for example, were engaged in the most obscurantist practices, and if Science as a whole had depended on their progress in their "art" the period would have to be written off as a failure. The works of Paracelsus, Basil Valentine and Raymond Lully which continued to circulate, and the extensive forgotten literature produced by a host of enthusiastic and credulous amateurs, take us back to the worst periods of superstition and it is simply not true that the dawn of the seventeenth century brought a universal belief in the operation of physical law and a discredit of magic and mystic agents. There was this one brilliant triumph of mechanics and theoretical astronomy, but so acute a thinker as Descartes missed the true answer to the sceptical questions he set himself. By falling back on an intuitional method he was no doubt able to shorten his estimate of the labour required in this way to produce a complete science of inanimate nature, but this was in essence the same mistake as Aristotle's. Only those with a weakness for verbal and logical classification could follow Descartes willingly through his opening premises and voyage thence

through the world he constructed. There was, of course, the enormous difference that Descartes's universe was supposed to be governed throughout by physical laws, even though these happen not to be the ones which actually operate. But the idea was there, and the conception was one of the universal mechanical operation of law.

By comparison, the atomists who followed the teaching of Gassendi seem to be intellectually children, and the atomic doctrine, by 1650, was not noticeably an improvement on the original version of Lucretius. Its adoption by Hobbes, a friend of Gassendi, and the most uncompromising materialist of them all, was a misfortune, for Hobbes's reputation with the men of science became such that he had only to support a theory and it was damned. Much the most important contribution to atomic theory came in fact from Boyle, and we may see in certain brief essays in this style an embryonic form of the kinetic theory.

But the existence of so many principles of explanation, drawn from Aristotle, Pythagoras, Lucretius, Descartes and Galileo, and the inability of some men of science to distinguish what was fact from what was mere hypothesis, all this made for unnecessary obscurity. The discipline which characterized mathematics and astronomy had somehow to be introduced into a field which too easily rioted into thickets of explanatory hypothesis. Reference to authority became prohibited; an anti-intellectualist attitude had to be encouraged and we find that William Gilbert, for example, denounced the " vast ocean of books " produced by writers whose explanations exhibited merely verbal ingenuity. " Neither Greek arguments nor Greek words can assist in finding truth," he added. True, Francis Bacon wrote in a similar vein, but to a less extent than Gilbert was he able to explain the method which should be employed; indeed, he hindered progress by his persuasive suggestion that there was one particular method, and especially when he argued for classification of facts and omitted to consider measurement. Nevertheless, the men of science succeeded as much by practical ingenuity as by abstract reasoning in reducing their problems to some sort of order, and by the time the Paris and London societies came into existence some confidence was felt that natural philosophy could and should contribute in no small degree to the material welfare of men. That natural philosophy might profoundly alter men's beliefs about the world was not then thought likely, and it seems

that this modesty on the more intellectual aspect was genuine. The Cartesian scheme went as far as the educated man could desire in showing how the claims of Biblical cosmology and natural philosophy might be reconciled. And up to 1670 it does not appear that fundamental researches were planned with any expectation of a new conception of the universe, doubtful though Huygens and others might be of the validity of some of Descartes's theories when examined in detail. Quite a large proportion of the scientific work going on in the 1660's was concerned with practical things: mining, navigation, military science, textiles and so on. Following Bacon, the men of science seemed to believe that "the real and legitimate good of the sciences is the endowment of human life with new inventions and order".

There were good reasons for this practical bias, for many of the techniques of civilization had advanced relatively little over a period of centuries. With the expansion of commerce, for example, navigation particularly remained difficult owing to the lack of a reliable and practicable means of determining longitude at sea; the geographers and cartographers had in this respect also gained little since the twelfth century. Hence it was that when Galileo discovered Jupiter's moons quite as much interest was attached to their use as a source of standard time as to their cosmological significance, representing, as they appeared to do, a solar system *in parvo*. Not until 1668, however, were ephemerides for the moons published; then their publication by Cassini gained for the astronomer the recognition of an invitation to Paris. The secret of longitude determination could clearly mean a great deal for the national income, for ships had to struggle on their voyages to the East Indies by means of dead reckoning and such checks of longitude as could be made by observation of landmarks. Bond tried to use magnetic isogonals as an answer to the problem and his book *The Longitude Found* (1676) was considered to be important by Halley, though nothing came of the method in practice.

Astronomy remained the key to many of the problems which were felt to be urgent during the first half of the century and it was the science which was most advanced through the work of Tycho Brahe, Kepler, Galileo and others. The invention of logarithms by Napier in 1614 was timely for, in Laplace's words, it doubled the lifetime of astronomers—though Napier's labours

were greatly inspired by the aim of proving the Pope to be Antichrist. So soon as money could be obtained, the French decided, an observatory should be constructed at Paris. It must be remembered that as yet a convincing decision between the Ptolemaic, Tychonian, and Copernican systems had still to be given; even though the first was no longer seriously supported by men of science, it shared with Tycho Brahe's system the advantage that no observation of stellar parallax had ever been made. The supporters of Copernicus could attribute the absence of parallax only to the smallness of the effect and this depended on the dimensions of the solar system being very great.

When, accordingly, as a result of the energy of Auzout, Richer, Picard and others, an expedition was sent to Cayenne in order to make simultaneous observations of Mars there and at Paris, the size of the solar system was the primary question in view. Richer at Cayenne and Cassini at Paris took simultaneous measurements of the altitude of Mars—and in 1672 the planet was at its nearest to the earth. Knowing the base d of the triangle in the figure, and the base angles θ_1 and θ_2, the distance of the planet was easily computed. By a straightforward application of Kepler's laws of planetary motion all the required distances followed from a single determination of the distance of the two planets. For the sidereal times of the orbits were known and these were related with the mean diameters. Kepler's brilliant work on the orbit of Mars and this triumph of 1672 thus alike rested on the patient labour of Tycho Brahe and the whole was a sermon to the men of science on the power of quantitative mathematical relations. The conclusion that the sun's mean distance from the earth was 87,000,000 miles was of the right order. As J. W. Olmstead remarks in *Isis* (1942), writing of the expedition, "The great consequence of the expedition for contemporaries was, of course, the revelation of the tremendous

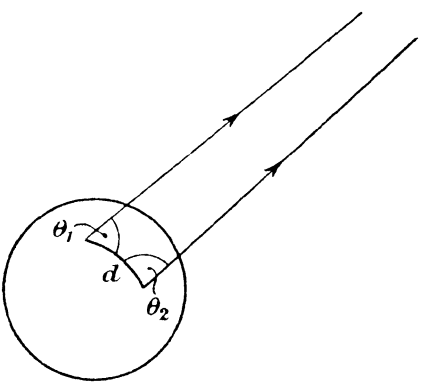


FIG. 1

dimensions of the solar system, as well as the prodigious size of the sun and some of the planets The disclosure, with some certainty, of the gigantic distances and masses involved was, for the general public, almost overpowering." The old Aristotelian conceptions of the universe might linger on in some minds but these were shattering blows and it was inevitable that educated men of the period should accommodate the increasing knowledge of Nature, up to 1672, with the only other plausible "system" in existence: that of Descartes.

It may be true that, as Professor Andrade has remarked in *Nature* (1942), "The Cartesian scheme was easy, pictorial, general: the Newtonian difficult, mathematical, precise." In such an opposition it may appear that Descartes's ideas were woolly and essentially unscientific. Nevertheless, it does not do to underestimate the amount of preparation which preceded the Newtonian synthesis or to despise investigations of men whose minds were, at least tentatively, inclined towards Descartes's theories. However deplorably qualitative his theorizing, Descartes inspired a generation at least with a vision of Nature as a sphere of universal law embracing planetary motion, rainbows, the properties of magnets and of lenses, all that could be explained by reference to underlying mechanisms. There was no limitation of the field of scientific investigations, and, moreover, the scientific explanations Descartes looked for were to possess an admirable economy of principles, the subtle matter and ether being trusted to account for a variety of phenomena. It was his desire for a pictorial mechanism which was the greatest weakness of Cartesianism, and if it had proved possible to reconcile his ideas with measurement and so formulate mathematical relationships, there is no doubt that Descartes would have done this. But he was willing to sacrifice the world of observation if need be, and he did not see that there was a radical difference between Galileo's idealization of his experimental studies and his own bold embarking on pure supposition.

All this became clear enough to Huygens in the course of his life; but the problems he took up can frequently be understood best by reference to Descartes's essays on the same subjects: the laws of impact, centrifugal force, the centre of oscillation, the behaviour of lenses, the nature of light and the cause of gravity, these are all topics discussed by Descartes and in a manner which

serves to introduce Huygens's own exact investigations. Professor Andrade makes an excellent remark when he says: "What Newton ignores is what Aristotle and Descartes tried to start with", and his summary of Newton's achievement cannot be questioned: "If we are asked to state in a sentence what was the main effect of Newton's work on the thought of his time, I think that the answer must be that it was to establish the power and universality of the methods of quantitative science." But ever since the time of D'Alembert's "Preliminary Discourse" to the *Encyclopédie* of 1751 there has been a tendency to admire Newton's magnificent work by depreciating the earlier work of the century, and especially everything which could be labelled Cartesian. It is important to realize what useful work was in fact done by the men of this period, and when we come to a consideration of Huygens's work much allowance has to be made for the late appearance of his works as publications. Otherwise the effect outside the Académie might have been far greater. "The evidence shows," wrote Professor Harcourt Brown,[1] "that Huygens took up the best elements of Cartesianism, adding to it along certain lines, rejecting some of it in other directions, and that he followed whatever seemed most fruitful of the suggestions received from parallel studies in England, Italy and elsewhere. He counts as one of a group whose efforts prepare the modern age of science, ushered in by way of the encyclopædism of the eighteenth century, whose urbane and moderate scepticism is a definite premonition of the century of Voltaire."

It must be said that where Huygens's views most foreshadow the eighteenth century was in his acceptance of complete physical determinism, even though he chose to cast this in rather rigid mechanistic form. This chapter may, therefore, end with a brief discussion of the mechanistic hypothesis of which Huygens was so strong an exponent. The influence of Gassendi may be seen from time to time, even though Descartes's system left little scope for the development of Gassendi's atomic doctrines and the two philosophers were in some senses regarded as being in direct opposition. But Gassendi seems to have strengthened in Huygens a leaning towards the thoroughly mechanistic view of Nature. Kepler had supposed that magnetic attraction resulted from a "mutual affection"; Gassendi considered it was a purely physical force. For him light was a material substance; there

[1] *Annals of Science*, 1936.

were indeed atoms of light and heat and for the propagation of sound—an example of a doctrine carried to extremes. Such views were opposed to Descartes's conceptions, for the latter held that space or " extension " is not infinitely divisible. Both considered that the universe should be regarded as a machine, although in the last resort Gassendi seems not to have thought it regulated itself. It was a scientific form of this teaching which came out of Huygens's mechanical studies, although he himself preferred to observe a separation between science and philosophy. Certainly, so far as they went, Huygens's mechanical explanations showed several radical differences from those associated with the name of Newton. In particular, Huygens found great difficulty in making universal gravitation a part of his scheme, preferring rather to regard matter as itself quite inert and incapable of any action except through collision with other matter. This difficulty meant in practice that he remained a Cartesian. In 1646 Descartes had ridiculed the " absurd belief " of Roberval that there could be mutual gravitation between lumps of matter. It would mean, he said, that a particle of matter was possessed of a soul and endowed with consciousness, so that it could know what happened across space and could in some occult manner exert its own influence there. For a Cartesian the really urgent problem was rather to discover the laws governing the transfer of momentum on impact and this, as we have seen, was one of Huygens's earliest studies.

It seems quite certain, therefore, that some of Huygens's rather curious conceptions come from Descartes. To the reader of Descartes's *Principia Philosophiæ*, for example, there would appear to be no incongruity in treating light and mechanics as related subjects. Descartes, and following him, Huygens, thought to find the explanation of gravity and light in the properties of media which would enable bodies to act on each other across intervening space. Huygens perhaps did for a time seek to bring in that mathematical and scientific form of Descartes's theories which Paul Mouy supposed that he actually created [1]. But in fact, while he was at the very beginning of his work, Huygens saw that there was exceedingly little truth in many of Descartes's scientific " principles ". On the other hand, he could not adopt Newton's attitude, which left the question of pictorial mechanism out of consideration to a very large extent.

[1] *Le Développement de la Physique Cartésienne* (1934).

As has been mentioned, Huygens considered that the conception of a gravitational force, innate and inherent in matter, appeared to be absurd and one which "takes us very far from the principles of Mathematics or Mechanics". As an enthusiast for a kinetic theory of all change he could allow matter no properties save that of inertia. It has to be remembered that he considered he had obtained direct evidence for the existence of a subtle matter. But for Cartesianism as a philosophic system he had little patience and he accordingly had several clashes with some of the Jesuits and doctrinaire Cartesians. When Kircher adopted the cosmology of Tycho Brahe, Huygens criticized him for what appeared to be nothing but timidity. "We others", he asserted, are "without fear".

In a sense then, Huygens's support of some of Descartes's ideas up to so late a date in the seventeenth century must be described as an experiment in failure. Very probably it was an experiment that had to be made; its failure showed clearly the great limitations of the extension of the notions of sense in the field of theory. It may even have been the feeling of dissatisfaction with the position men like Huygens were reaching that drove Newton to make his great renovation of natural philosophy. The progress of scientific explanation may then be seen as a process of leaving out of redundant elements until one arrives at the really successful procedure of abstraction. If so, this was a return to the methods of Galileo.

The practical value of much of Huygens's laboratory work needs no stressing, nor does his interest in the clock, telescope, microscope, and other experimental and observational aids need explanation. The experiments of Galileo and the Florentine academicians were done with the simplest of apparatus. But new discoveries, it was realized, often attended the use of new apparatus, a fact which was well illustrated by the invention of the vacuum pump. The rate of progress was for some time in direct relation with the experimental resources available. Here theory and practice interlock. It was not possible to improve the telescope, to invent a new eyepiece, without a quantitative study of the defects of the ordinary lens. Problems of theoretical interest led to the construction of the vacuum pump; experiments with this instrument led in their turn to new theoretical speculations regarding the nature of air, and, in Huygens's case, the cause of gravity. One can scarcely estimate the extent to which

new and improved instruments assisted the great scientific advances of the seventeenth century. The telescope, microscope, barometer, vacuum pump, pendulum clock, micrometer eyepiece, and other instruments might be mentioned here. Without them rapid progress would have been unthinkable.

II

Work on Collision between Elastic Bodies

The Cartesian mechanism was governed by the principles of inertia and of the conservation of *motion*, the laws of impact, the theory that solidity is derived from a condition of rest, and a kinetic theory of fluids. Independent of Galileo in most respects, Descartes expressed the Newtonian First Law quite clearly: by itself a body continues in its state of rest or of uniform motion in a straight line; curvilinear motion is constrained. We know that Galileo also stated this law. Descartes went further than Galileo, however, for he considered that rotational motion should generate an outward, centrifugal force. In his vortex theory this centrifugal force was balanced by the pressure of neighbouring vortices, also tending to expand. Each vortex had, in fact, a centre which was comparatively empty of matter and consequently matter leaving the ecliptic of one vortex was drawn in at the poles of a neighbouring vortex. Descartes thought to explain magnetism and sunspots by this means. Two other phenomena arose in consequence of the vortices: the centripetal attraction on matter resulting from the emptiness of the central part of a vortex produced the illusion of gravity, and the centrifugal pressure of particles of the subtle matter was the origin of light. Descartes never really answered the criticism that gravity acts along the radius and not along lines perpendicular to the plane of rotation and there were other difficulties for his view. In the first place he did not consider that the rotating subtle matter penetrated the earth. Yet the weight of an object may be shown not to vanish beneath the earth's crust. Also Descartes did not consider weight as proportional to the quantity of matter and he supposed that liquids, through the greater internal motion of their particles, must be inherently lighter than solids. Huygens saw these weaknesses. Nevertheless, he followed Descartes on the subject of gravity to a greater extent than in other topics. He even calculated how fast the subtle matter must move in its vortex to produce the known effects. There are other parallels, too, in Huygens's theory of the transmission of light. Nevertheless, in the pages which follow it will be seen that Huygens

remained outside the main stream of Cartesian thought which is to be traced in the writings of Rénier, Regius, Rohault, Régis, Fontenelle and Malebranche. On the other hand, he was not in spirit so deeply opposed to Cartesianism as Leibnitz, whose view of the *Principia Philosophiæ* was that it was " un beau roman de Physique ".

In this work Descartes supposed that motion could only be transferred from one body to another by direct or indirect impact. His assumption that the total amount of what he called the " quantity of motion " in the world is constant was not unreasonable: it conceals the germ of the idea of energy which may be traced in the writings of Stevin, Da Vinci and perhaps in those of Jordanus Nemorarius. But Descartes made a distinction between the speed of a body and its " determination " or direction which led him to treat these as separate entities. In his third law of motion Descartes stated that if a moving body, travelling in a straight line, meets another body with less " force " to continue its movement than the second one possesses to resist it, it will lose its determination without losing its motion. If it has more " force " it moves the second body and loses as much of its motion as the second one absorbs. Descartes's seven rules of impact were all corollaries of this law and so important were these in his system that Mouy has remarked: " Les règles du choc sont bien les règles du mouvement et la mécanique cartesienne est une théorie de la percussion." It was unfortunate, therefore, that all the rules were wrong when compared with experimental results. Descartes appears to have known this and to have replied in effect: the rules are true for perfectly hard bodies moving *in vacuo*. In experimental conditions the bodies are not perfectly hard and move in a fluid. The parts of this fluid " corrupt " more or less the bodies immersed in it owing to the motion of the fluid particles against the particles of the body.

During the year 1652, when he was only twenty-three, Huygens became convinced of the errors of Descartes's treatment of motion and impact. Correspondence with Schooten, Gutschoven and Slusius in the period of 1652–7 shows that he was occupied in substituting new principles for those referred to above. The fact that Huygens's work[1] was not published as a whole in his lifetime is certainly not to be attributed to his reverence for Des-

[1] It was entitled *De Motu ex Percussione*.

cartes but to his own difficulties over further dynamical problems. Actually the work was completed before 1656, for in that year Huygens undertook the recasting of his material in classical form. Some twelve years, therefore, elapsed between Huygens's completion of the treatise and the communication of his results to contemporary men of science (see p. 64).

Huygens at first limited himself to the study of direct collision between two equal bodies. He began with two hypotheses. The first was Galileo's principle of inertia, also stated by Descartes; the second stated a principle of symmetry. The latter was that if two exactly similar hard bodies collide with equal speeds in the same straight line they are reflected back with their speeds unchanged. He uttered a warning that this was true in respect of the system of co-ordinates used in specifying the position of the bodies at any instant and that the system might be in a state of uniform motion. Velocities relative to the system of co-ordinates or reference frame he saw must be treated as vectors —as forces were already treated. His problems were of course concerned with momenta, but this fact is a little obscured by his geometrical method of treatment—in which the velocities alone

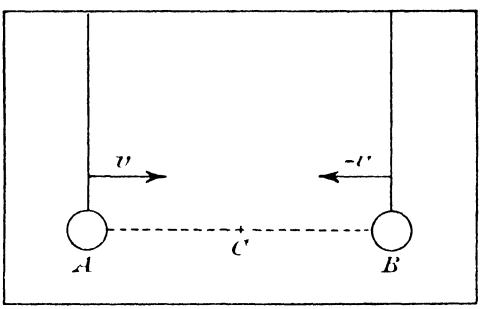


FIG. 2

are represented. The bodies were supposed to be smooth spheres of a perfectly elastic material. They were imagined as suspended by vertical strings held by a man standing in a boat. The boat—or system of co-ordinates—could be given a uniform velocity to the right or to the left so that the velocities of the spheres, as judged by a stationary observer on the bank, were in effect transposed to a different system or reference frame.

Thus, if the spheres have velocities v and $-v$ (Fig. 2) and the reference frame moves with a velocity $-v$ the first sphere is made

to appear stationary and the second to approach with a velocity of $-2v$. After impact, with respect to the frame, the velocities are $-v$ and v, but with respect to the stationary observer they appear to be $-2v$ and 0. Thus, when one body is stationary and is struck by a moving body of equal mass, the momentum is transferred. In the case where the body A has a velocity v_1 and B has a velocity $-v_2$ where $v_1 > v_2$, Huygens supposed the reference frame to be given a velocity $\frac{v_1 + v_2}{2}$ so that, to the stationary observer, the velocities appear to be equal and opposite. It followed that the real velocities are interchanged.

To progress in problems where the masses are unequal Huygens found it necessary to assume as self-evident that when a larger body meets a smaller one which is at rest, the smaller one is moved and the larger one loses some of its motion. Here he was engaged in clearing away some of Descartes's errors, some of which arose through his failure to treat velocity consistently as a vector quantity. In March 1669, in the *Journal des Savants*, Huygens gave a correct statement of the law of conservation of momentum in the form: "There always remains the same quantity of motion towards the same side after the quantity of contrary motion has been subtracted." Another form of law is given in Rule 7 of the *De Motu*: " . . . the common centre of gravity of the bodies advances always equally towards the same side in a straight line before and after impact." That is, the total momentum along the line of centres is unchanged. It followed directly from this that the relative speeds of approach and separation are equal.

Much the most interesting part of the *De Motu* is found in the proposition on the collision of bodies when the velocities are inversely proportional to the masses.[1] "When two bodies, whose speeds are inversely proportional to their masses, meet from opposite sides, each rebounds with the same speed with which it approached."

Here Huygens employed the important principle, referred to in Part I, that the centre of gravity of a system of bodies cannot ascend through any motion of the bodies under gravity. His proof consisted in showing that if the resultant speeds were other than equal and opposite to the original speeds, the centre

[1] Proposition 8. Huygens did not make a distinction between mass and weight in this work. The first use of a distinct term for mass comes in his work on centrifugal force (see p. 117).

of gravity of the bodies could be raised above its original height. This, following an idea found in the writings of Stevin and Torricelli, he assumed to be impossible, since it involved a possibility of *perpetuum mobile*. In the figure the mass m_A possesses the velocity AC, while the mass m_B possesses the velocity BC. These are related:

$$\frac{m_A}{m_B} = \frac{BC}{AC}$$

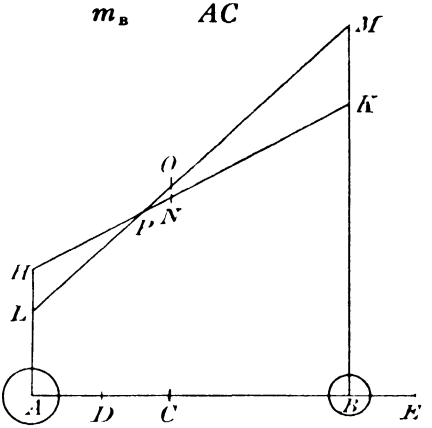


FIG. 3

After impact, according to the theorem, the velocity of m_A will be represented by CA and that of m_B by CB. Using a *reductio ad absurdum*, Huygens first assumed that the acquired velocities were CD and CE, the only necessary restriction here being that $AC + BC = CD + CE$ (ignoring signs). Supposing that the initial velocities were acquired by vertical descent under gravity, Huygens took the required heights to be HA and KB respectively. These were related through Galileo's equation $v^2 = 2gs$ (Fig. 3).

Thus:
$$\frac{HA}{KB} = \frac{AC^2}{CB^2}$$

Starting with the velocities CD and CE the heights attained would be AL and BM such that

$$\frac{AL}{HA} = \frac{CD^2}{AC^2}.$$

and
$$\frac{BM}{KB} = \frac{CE^2}{CB^2}.$$

When first elevated m_A and m_B have their common centre of gravity at N. After being brought to the positions L and M, the centre of gravity is moved to O for

$$m_A.AC = m_B.CB$$

and

$$\frac{HN}{NK} = \frac{AC}{CB} = \frac{LO}{OM}.$$

The remainder of the proof then consists of a simple demonstration that O is higher than N not only for this case but also for the case when m_A rebounds with more speed than before and for the possibility that it is brought to rest.

It will be noticed that the idea of converting a horizontal velocity into an ascent involves first the relation $v^2 = 2gh$ and second the idea of energy conserved, i.e., $mv^2 = 2mgh$. No doubt it was the use of this relation which enabled Huygens to solve problems on the ballistic pendulum when in England in 1661. It marks a distinct advance on Descartes's approach to the law of conservation of energy, since the latter had no use for any form of potential energy. This piece of work is accordingly of some importance in the history of the concept of energy.

After this point Huygens was able to give the solution of the most general case of impact between perfectly elastic bodies moving along the line of centres. The masses being in general unequal, Huygens employed his original device and gave the reference frame a velocity such that the velocities of the bodies bore an apparent relation inversely proportional to the masses. His geometrical method prevented him from expressing the result in a generalized form. The most general law embodied in the work on impact is the law of conservation of kinetic energy for linear motion in the horizontal plane. This is stated in a proposition showing that $m_A v^2_A + m_B v^2_B =$ constant, both before and after a collision. The difficulty of dealing with vector quantities is overcome by squaring the magnitudes. How Huygens hit on the quantity mv^2 is not quite clear, but his use of it appears to date from about 1652 and he considered this, the *efficacy* or *vis viva* of a moving body, to be a more fundamental quantity than its quantity of motion or, as we should now say, its momentum. Leibnitz adopted the same point of view, but neither he nor Huygens saw how the *vis viva* was related to the quantity of motion.

The remainder of Huygens's *De Motu Corporum ex Percus-*

sione deals with the effect of placing hard bodies between the two extreme bodies in motion so that the impact is transmitted. In the simplest case a hard sphere is brought to rest on meeting the end sphere in a row and the sphere at the other extreme end is set in motion with the velocity originally possessed by the first (Fig. 4.) Huygens experimented with glass balls suspended like pendulum bobs or rolling in a horizontal

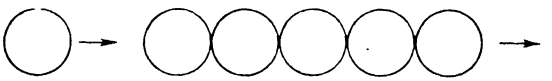


FIG. 4

groove. It is well known that he found in the study suggestions for a theory of the propagation of light. He showed in the present connection that a single interposed mass must be the mean proportional between the two extreme masses for the velocity attained by a third mass to be a maximum. Thus, at first, m_A has a velocity v_A while $v_B = 0$ and $v_C = 0$. It may be shown that if

$$m_B = \sqrt{m_A m_C},$$

then v_C is a maximum for any given values of m_A and m_C. The proof is straightforward, although tedious by Huygens's method.

As has been mentioned, others besides Huygens brought out work on the subject of impact. Wallis, in November 1668, read a paper to the Royal Society in which he also employed the Cartesian quantity of motion but dealt with inelastic bodies. In 1671 he published results for elastic impact, including the formula

$$u = \frac{mv \pm m_1 v_1}{m + m_1} \quad \text{for inelastic bodies,}$$

where v and v_1 are the initial velocities and u the common velocity after collision. In December 1668 Wren also gave some empirical rules which resembled some of Huygens's results, although arrived at independently.

For a general theory of impact, clearly systematizing contemporary work, one has to turn to Newton's *Principia*, where the subject is summarized in the introductory Scholium to Book I. After the three laws of motion Newton gave a correct statement of the law of conservation of momentum and stated as a corollary Huygens's "law" that the common centre of gravity of two or more bodies is unaffected by the actions of the bodies

among themselves. So slowly did the Newtonian system displace the Cartesian, however, that in the English edition of Rohault's *System of Natural Philosophy*, which came out in 1723, Descartes's original treatment of motion and impact is closely followed and we read, for example, on page 78: "When a Body moves any particular way, the Disposition that it has to move that way rather than any other is what we call its Determination." Gravesande's *Mathematical Elements of Natural Philosophy* (1721) was more up to date and contained a summary of Huygens's work on impact without, however, an acknowledgement of the source. This work was dedicated to Newton and is an interesting guide to the scientific heritage of the seventeenth century as it was passed on to the eighteenth century reader.

III

Centrifugal Force

The fact that Huygens early turned his attention to the study of circular motion is only another example of the influence of Descartes. In his *Principia* Descartes attempted to analyze the motion of a stone placed in a sling. He saw that although the stone tended to continue its motion along the tangent, this was prevented by the tension in the thread. The problem was to find the magnitude of this tension and this Descartes failed to do. Huygens completed his study of circular motion about 1659 and published the more important theorems at the end of his *Horologium Oscillatorium*. The treatise, *De Vi Centrifuga,* containing the proofs of these theorems and other matter, was published posthumously in 1703. By this time others, including Keill, Savilian Professor of Astronomy at Oxford, had become impatient for the proofs of the theorems and had supplied many of them. Newton arrived at the fundamental formula for the acceleration of a particle describing a circle independently of Huygens.

Huygens began his treatise with a statement of Galileo's conclusions concerning descent under gravity. "Gravity," he said, "is the tendency to fall" and the tension in the thread from which a heavy body is suspended results from this tendency. A constant acceleration must mean that "the spaces traversed in different times by bodies starting from rest are to each other as the squares of the times". The tension considered in the case of the sling is therefore a real force, as real as a force which produces an acceleration in the direction of the existing motion. Indeed, the measure of the force is supplied by the acceleration—and, he should have added, the mass moved. There is then an acceleration towards the centre in the case of circular motion.

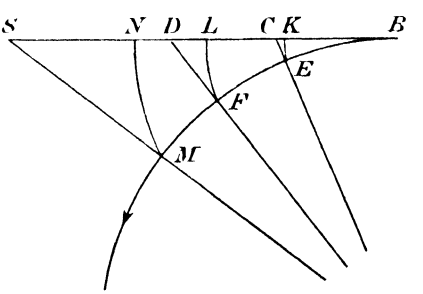


FIG. 5

In the figure (Fig. 5), Huygens considered the successive points reached by a particle liberated at some point of its circular path and free to travel along the tangent.

These, at successive equal intervals of time, would be *K, L, N* in the figure, the distances *BK, BL, BN* being equal to the lengths of the arcs *BE, BF, BM* respectively. If the intervals of time are small, *BK, BL, BN* approximate to *BC, BD, BS* respectively; *C, D, S* being points on the tangent where this is cut by the radii through *E, F,* and *M*. *EC, FD,* and *MS* are then the distances separating the two paths at successive instants. Without explaining his reasoning further Huygens then stated that these distances approach as a series, the series of squares 1, 4, 9, 16, etc., and thus resemble the successive distances fallen under gravity. The argument is somewhat reminiscent of Galileo's study of a projectile, which, starting horizontally, under the acceleration of gravity describes a parabola (*Discourses on Two New Sciences,* Fourth Day), but he may have got at the result as we should now by considering a parallelogram of velocities[1]. As a mathematician he was rather more attracted by the problem of the nature of the curves *EK, FL,* and *MN,* which he saw were the evolutes traced out by the movement of a stretched thread wrapped along the arc *BE, BF, BM,* etc., but he reverted to the physical aspect of the subject. The distances traversed (*EC, FD, MS*) in successive instants being approximately in the ratio of a series of squares, he wrote, "this tendency of which we have spoken is absolutely similar to that with which heavy bodies suspended by a thread try to descend. Whence we concluded also that the centrifugal forces of unequal bodies moved around equal circumferences with the same speed are among themselves as the weights or solid quantities"—*inter se sicut mobilium gravites, seu quantitates solides.* Professor Crew (*The Rise of*

[1] $a.dt = v\theta$
$\theta = \omega dt$ and $\omega = v/r$
$\therefore a = v^2/r$. Under gravity $a = v^2/2s$.

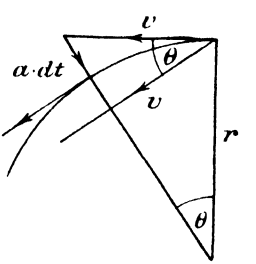


FIG. 6

Modern Physics), has remarked that this is probably the earliest suggestion of a distinction between mass and weight.

From this original beginning some simple conclusions follow immediately. The centrifugal force varies directly with the radius of the circle if the angular velocity and mass are constant (Fig. 7), it varies as the square of the tangential velocity at any instant (Fig. 8) and inversely as the radius (Fig. 9):

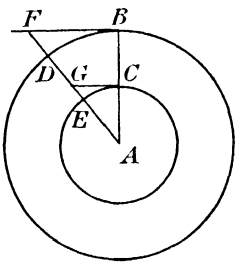


FIG. 7

(*a*) For equal angular velocities:

$$\frac{F_1}{F_2} = \frac{DF}{EG} = \frac{BF}{CG} = \frac{BA}{CA}.$$

i.e. the centrifugal force varies with the radius (prop. 1).

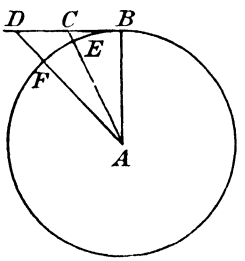


FIG. 8

(*b*)
$$\frac{v_1}{v_2} = \frac{BE}{BF}$$

$$\frac{F_1}{F_2} = \frac{CE}{DF}$$

and, by his original relation,

$$\frac{CE}{DF} = \frac{CB^2}{DB^2} = \frac{v_1^2}{v_2^2}. \quad \text{(prop. 2).}$$

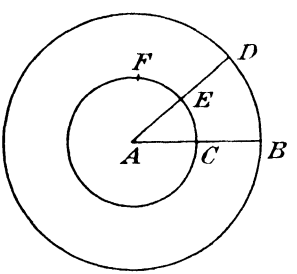


FIG. 9

(*c*) Equal masses have equal linear velocities: $BD = CF = v$.

Let $CE = v'$

$$\frac{v'}{v} = \frac{CE}{BD} = \frac{AC}{AB}$$

But $\frac{F_2}{F'} = \frac{BA}{AC}$ (prop. 1)

and $\frac{F'}{F_1} = \frac{AC^2}{AB^2}$ (prop. 2)

$$\therefore \frac{F_2}{F_1} = \frac{BA}{AC}\,\frac{AC^2}{AB^2} = \frac{AC}{AB}.$$

There does not seem to be much necessity to quote the other propositions on ordinary circular motion since they follow simply from the above. There are, however, some interesting studies of the conical pendulum: problems which may have been a reason for Huygens's taking up the whole subject. As has been mentioned earlier he made use of the conical pendulum in some of his clocks.

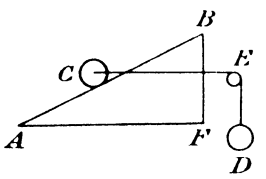


FIG. 10

This section commences with the statement of two simple theorems or lemmata on the equilibration of smooth bodies on inclined planes by means of tensions in *horizontal* threads. Thus in Fig. 10 (lemma 1) the weight D must be to the weight C as the perpendicular BF is to the base FA. This relation is not proved; Huygens says, justifiably, that it is one which is well known. It is used immediately in an interesting theorem on the revolution of a body on the inside of a paraboloid of revolution; all revolutions of a body travelling in horizontal circles on the surface of the paraboloid are completed in equal times irrespective of the amplitude of the circle. This theorem states the theoretical conditions to be fulfilled by a conical pendulum used in a clock. The point about Huygens's proof of his theorem is that, true to his conception of centrifugal force as equivalent to any other kind of force, he substituted it for the tension in the thread of the above lemma. At any point on the paraboloid, then, the centrifugal force must be that which will maintain the body against the force of gravity along the tangent. As he showed:

$$\frac{F}{mg} = \frac{HG}{GF} = \frac{HK}{KL}$$

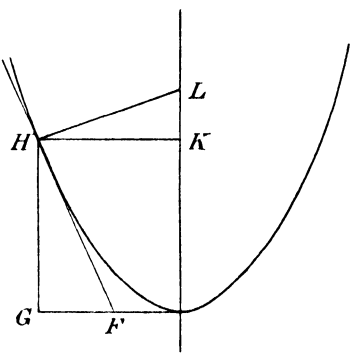


FIG. 11

But, by a property of the parabola, KL is constant (Fig. 11). Thus, for any two positions of a body on the paraboloid:

$$\frac{F_1}{F_2} = \frac{H_1K_1}{H_2K_2} = \frac{r_1}{r_2}$$

and thus the times will be equal (proposition 1).

In subsequent propositions, Huygens derived all the simple

relations which might be expected: the various factors of vertical height, length, inclination to the vertical and angular velocity all being considered separately. Just as he nowhere used the formula $\frac{mv^2}{r}$ in this form so he did not give the formula $2\pi\sqrt{\frac{l}{g}}\cos\alpha$ for the period of the conical pendulum. The work is consequently somewhat tedious for the modern reader. Near the end Huygens showed that when the angle with the vertical made by the thread of a conical pendulum is $2^\circ\ 54'$ the period is equal to the time of vertical fall from a height equal to the length of the pendulum. The work is completed by a consideration of circular motion in a vertical plane. Problems which are still common in modern text-books are solved here for the first time. Huygens gave also the necessary conditions for Galileo's experiment with the intercepted pendulum (Fig. 12). This was an instructive experiment since it illustrated the relation $gh = v^2/2$ and drew attention to the fact that the particular path makes no difference to the height achieved. This is sufficiently explained by the figure.

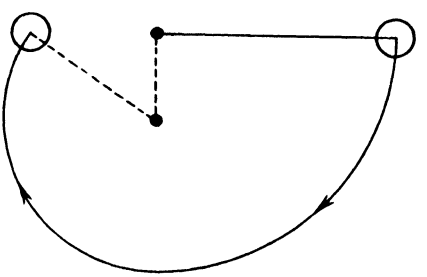


FIG. 12

As has been mentioned, Huygens followed the old Aristotelian view that circular motion is a distinct form. This accorded also with Galileo's view, for although he himself discovered parabolic motion he did not include it with the other two fundamental motions. It seems as if both Galileo and Huygens supposed the elliptical paths of the planets to be reducible to simpler components. At any rate Huygens for a long time reserved a special place for circular motion, considering it alone to be absolute and not merely relative in nature. After the appearance of Newton's *Principia,* however, he came out against the

idea of any absolute motion and opposed the idea of absolute space or time. In 1694, he criticized Leibnitz for holding to the old idea of absolute motion, but rejected the suggestion that he owed his point of view to Newton's *Principia*. Mariotte also he considered was in error in attempting to distinguish the *vitesses propres* of bodies. This, of course, raised the question of how we may know when bodies are relatively at rest. Huygens answered this in two ways: the bodies must be free to move and yet retain their relative positions with respect to each other and to their background; alternatively, by connecting the bodies by threads, one can dispense with the background, for if rotational motion exists, tensions will be set up in the threads.

The effect of the diurnal rotation on the shape of the earth was, of course, a problem which Huygens could not ignore. Kepler, in his *Epitome Astronomiæ Copernicæ* (1609), seems first to have proposed that the centrifugal force must be equilibrated by an "attractive virtue" and Huygens followed this lead in 1666. He calculated how much slower a pendulum clock would go in a latitude of 45° when compared with its rate of going at the poles. Supposing the earth to be perfectly spherical for the purpose of the calculation this amounts to a comparison of oscillation in a field of g units as against one $g' = g - F\cos\alpha$ where F is the centrifugal force acting on unit mass at a latitude $\alpha°$. Clearly we may put

$$F = \frac{4\pi^2 r^2}{T^2}$$

where $r = R\cos\alpha$ and R is the radius of the earth, T the period of the diurnal rotation. Substituting the values for R and T, for a latitude of 0° the centrifugal force is a maximum and works out to $\frac{1}{289}g$ approximately. This agrees with Huygens's estimate that the rate of rotation would have to be seventeen times as fast for the centrifugal force to equal gravity at the equator, but his estimate for the slowing down of the clock does not agree with a modern estimate. His attempt to deduce the form of the earth was, however, interesting since it laid down useful principles for the theory of the equilibrium of fluids. Huygens laid down the principle that a mass of fluid is at rest only when its surface is at each point perpendicular to the resultant force acting at that point. Later surveys carried out in

the eighteenth century confirmed his general conclusions, but not the extent of flattening at the poles which he had calculated.

For a time Cassini opposed the views taken by both Huygens and Newton as to the form of the earth resulting from rotation. A cartoon of the period is reproduced in Professor Cajori's edition[1] of Newton's *Principia*, in which the rival pictures are shown (Fig. 13).

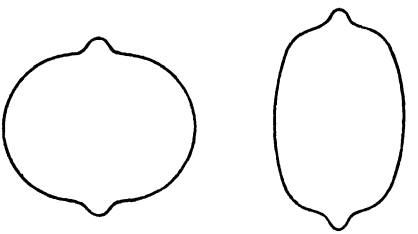


FIG. 13

The investigation by which Maupertuis later disproved Cassini's conclusions gained for him the title of the "earth-flattener". This was, however, complimentary when compared with the other epithets he received from Voltaire.

An account of the dispute which broke out between the Newtonians and Cartesians on the shape of the earth was given by d'Alembert in *L'Encyclopédie* of 1751–65 (Vol. VI).

[1] *The Mathematical Principles of Natural Philosophy*, a reprint of the English translation by Motte (1729) with some additional notes.

Statics.

In some early studies on the distribution of forces in threads which support a number of masses, Huygens used a fundamental principle which was closely related with the one he used so much in Dynamics. This stated that, for a system of bodies connected by threads and in equilibrium, a very small displacement (compatible with the connections) cannot cause the elevation of the centre of gravity of the system. This idea, a variant of the idea of virtual work, had been used by Torricelli and Pascal, but Huygens made it the basis of a general method. He used it, for example, in an independent proof of Jordanus's theorem on the inclined plane (see below), and it was, of course, a form of this principle which was employed so successfully in his study of the centre of oscillation. To some extent Huygens followed the lead of Galileo's *Discourses* but Stevin's *Beghinselen der Wieghconst* (1586) and *Hypomnemata Mathematica* (1608) were also important and supplied him with some of his early ideas. It is well known that Stevin regarded the impossibility of perpetual motion to be a principle on which a treatment of equilibrium under gravity could be based. As will be seen, Huygens's work carried this idea further and in effect introduced the idea of the conservation of energy in mechanical systems (see p. 154).

In the *Discourses on Two New Sciences* (Second Day), Galileo had given a "proof" of Archimedes' theorem on the simple lever and had considered also the conditions under which a uniform beam is fractured by bending. Both of these problems were considered by Huygens in some early studies dating from about 1662. The defect of Archimedes' work on the lever was that in effect he had to assume the very law he set out to "prove". Stevin, and later Galileo, shortened the "proof" but neither, in Mach's opinion[1], escaped the original difficulty; both employed "the doctrine of the centre of gravity in its most general form, which is itself nothing else than the doctrine of the lever in its most general form". Huygens did the same. His exercise on the subject is of interest since he employed a device

[1] E. Mach. *The Science of Mechanics* (trans McCormack, 1907).

which was virtually the taking of moments about an axis which was chosen arbitrarily. Wallis, alone with Huygens, shares the honour of introducing the idea of a moment into mechanics. In a letter to Huygens dated January 1, 1659, Wallis spoke of the *momenta* of the elements of a surface about a certain axis as forming a series in which each term was a weight multiplied by a distance. He used the idea in finding the centre of gravity of solids of various forms. As has been mentioned, Huygens met Wallis in London in 1661 (see p. 45), and it seems very probable that he was indebted to him for the germ of some of his ideas. Much the same sort of problem as the determination of centres of gravity was met by Huygens in evaluating the quantity Σmr for the compound pendulum. Such important ideas were, then, in use long before they became part of the published work on Mechanics. Varignon's *Projet d'une Nouvelle Mécanique* (1687) was actually the first book to contain the modern idea of moments.

In some manuscript notes on the fractures of beams Huygens showed an approach to the modern idea of work done. Under gravity the centre of gravity of a system will descend as far as possible, that is, the work done will be a maximum. Huygens used the term *descensus grævitatis* in this connection. When the system is in equilibrium the first principle, stating that the centre of gravity does not rise, is to be applied.

The problem of demonstrating the equilibrium of two smooth weights on inclined planes and connected by a thread for the conditions

$$\frac{m_1}{m_2} = \frac{AB}{BC}$$

was solved by the writer of the work *Jordani Opusculum de Ponderositate*–a thirteenth century work—and it was given an elegant solution by Stevin, who simply applied the principle that perpetual motion is impossible. The independent solution given by Huygens is therefore not referred to in modern works. Nevertheless, it shows clearly the usefulness of his fundamental axiom that the centre of gravity of a system of weights does not rise through any motion of the weights under gravity.

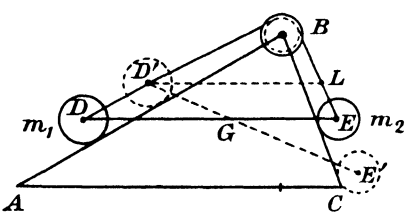


FIG. 14

In the figure (Fig. 14) m_1 and m_2 are related as shown above and are considered to undergo a small displacement. It is required to show that the centre of gravity G is not disturbed. The point G in the horizontal line DE must of course be such that $m_1.DG = m_2.EG$.

Taking $DD' = EE'$ and $D'L$ parallel with DE we have

$$\frac{BD}{BE} = \frac{BA}{BC} = \frac{m_1}{m_2} = \frac{EG}{GD}$$

and

$$\frac{BD}{BE} = \frac{BD'}{BL} = \frac{DD'}{EL} = \frac{EE'}{EL}.$$

Hence

$$\frac{EE'}{EL} = \frac{E'G}{D'G}$$

so that G is also the centre of gravity of m_1 and m_2 in their new positions.

In the history of statics, Wallis, by publishing his *Mechanica sive de Motu* in 1669, with successive parts in 1670 and 1671, contributed a systematic treatment which must be ranked above Huygens's work in importance. Huygens, as has been noted, was more interested in dynamical problems. The work of these two men shows, however, some interesting resemblances. Both employed the principle of virtual displacements and extended the idea of force to all kinds of forces without distinction. Up to this time gravitational force alone had been considered.

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V

The Treatise on the Pendulum Clock: The Horologium Oscillatorium. Part One Construction and Use of the Pendulum Clock

The great Danish astronomer, Tycho Brahe, used clocks in astronomical observations about 1580, but he was perhaps not the first to introduce time measurement into observations in Astronomy. Certain early Arabian astronomers, according to Robison, used the simple pendulum for measuring short intervals, but the evidence for his supposition is not very clear. No good means really existed until the pendulum clock was invented, and by the middle of the seventeenth century the need of time measurement had become urgent. Galileo used a water clock or clepsydra in his experiments on natural acceleration. He clearly knew of the property of the simple pendulum: that its period is (very nearly) independent of the amplitude of swing. Indeed, it is very probable that this was known to Copernicus (see p. 99). The story that Galileo discovered the property of the simple pendulum from observation of a lamp swinging in Pisa cathedral is not very well established, but the belief is undoubtedly kept alive by a passage in his *Discourses on Two New Sciences,* where he said that he had often observed such oscillations in churches.

Tycho Brahe's clock was a crude instrument employing a balance or verge escapement. Such a regulator has no natural period, but is adjusted empirically by altering the moment of inertia. Although such clocks were used in public buildings, perhaps from the 13th century, they were ill-adapted for the observatory. Tycho found it necessary to correct for the temperamental qualities of his instruments at Uraniborg. It is accordingly very interesting to find that that universal genius Leonardo da Vinci, before the end of the fifteenth century, sketched, even if he did not construct, an escapement employing a pendulum. By the time of Galileo it seems probable that the idea of applying the pendulum was present in many minds. The design attributed to Galileo shows considerable mechanical ingenuity and owes less to the old balance clock than did da Vinci's sketch.

J. Drummond Robertson, in his excellent book, *The Evolution of Clockwork* (1931), compared it with Huygens's design to the latter's disadvantage. Galileo's mechanism, he wrote, " is far more subtle and ingenious than Huygens's method of controlling the regular action of the old faulty escapement by the attachment of a pendulum in place of the balance ". Nevertheless, Huygens's theoretical study of the problem was far more profound and indeed left little to be added, and his escapement also seems in practice to be much superior to that of Galileo. His practical contributions were superseded by the invention of the anchor escapement and later by the dead beat escapement. The latter gives an impulse to the pendulum near its zero position and exerts little frictional drag over the rest of the swing. The ideal of the free-swinging pendulum was not attained until the end of the nineteenth century.

Huygens at first connected the pendulum rod *DF* (Fig. 15) to a circular balance by means of a two pronged fork or crutch. The part *DE* was flexible. This arrangement was, however, soon abandoned and the clock described in the *Horologium* of 1658 showed some improvement, as a comparison of the figures (Fig. 15 and 16) will make clear. The circular balance has gone

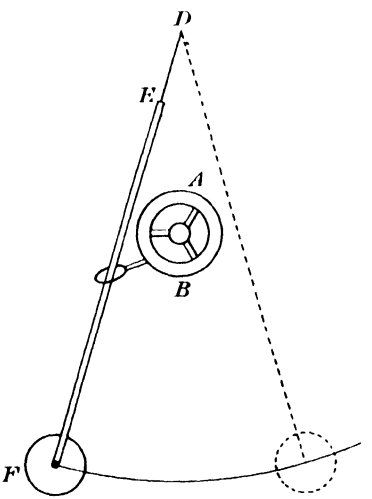


FIG. 15

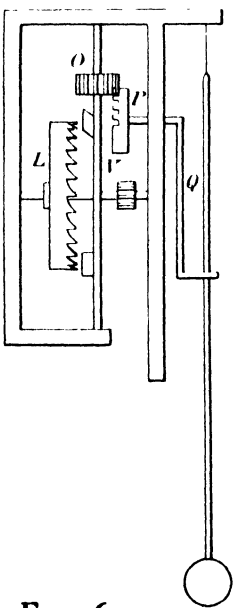


FIG. 16

and the pendulum operates the verge with pallets *V* by means of the fork *Q* and the wheel *P* engaging the pinion *O*. The crown wheel is vertical. A vertical crown wheel was, however, not invariably used. Samuel Coster, clockmaker to Huygens at the Hague, made some clocks with a horizontal crown wheel and one of these, described by J. Drummond Robertson (loc. cit.), was driven by a spring and not by weights. And although the clock bears the date 1657, it bears metal plates or "cheeks designed to correct the period for large swings of the pendulum. These plates may, of course, have been added subsequently. As J. Drummond Robertson points out, it must be supposed that Huygens began with a horizontal crown wheel but changed this to the vertical position in his *Horologium* so as to interpose the wheels *O* and *P* in the figure. As for the use of curved plates on either side of the pendulum it is clear from Huygens's correspondence that he was uncertain whether to design the clock so as to employ a pendulum oscillating over a small arc or to employ the curved "cheeks". In the *Horologium* the cheeks are omitted. Huygens explained to Petit that he could not, therefore, dispense with the wheels *O* and *P* as the latter suggested since these had the effect of restricting the arc of the pendulum oscillations.

There is an interesting letter from Huygens to Boulliau written in December 1657, in which he enquired about clocks which the Grand Duke Fernando de Medici was reported to have under construction "in order that I may know if they also use a pendulum. A year ago yesterday exactly, I made the first model of this kind of clock and in the month of June [1657] I began to show the construction of it to all who asked me for it and among whom perhaps was someone who sent news of it to Italy . . . In a very few days we shall see a much larger form of the clock in a belfry of the village which is near the sea half a league from here [the village of Scheveningue]. The pendulum will be 21 feet long and will weigh about 40 or 50 pounds . . . ." The Medici Palace clock was for some time put forward as being of earlier date than Huygens's clocks. Modern writers do not accept it as antedating Huygens's invention, and it may in fact have been a copy. Against this is the fact that the pendulum of the Medici clock was rigidly attached to the escapement—a feature of both da Vinci's and Galileo's design. This was a defect, since if the pendulum was light the oscillations could

easily become forced and the mechanism would then resemble an inferior balance clock. " . . . the great merits of his invention " wrote J. Drummond Robertson of Huygens's clock," were the free suspension of the pendulum by means of a cord or a steel spring, with the crutch as the means of communication between the pendulum rod and the verge; contrivances which have continued in use ever since in all clocks with the anchor escapement."

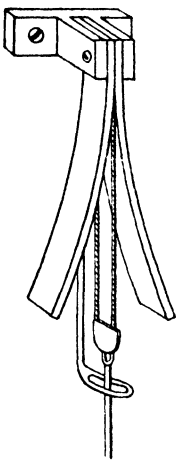


FIG. 17

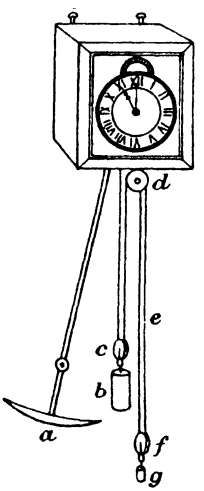


FIG. 18

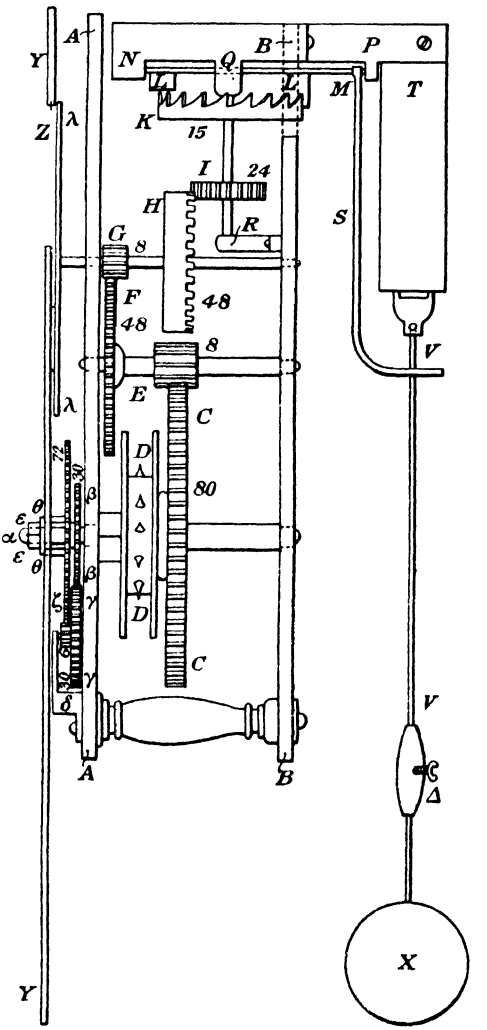


FIG. 19

When one turns to the *Horologium Oscillatorium* of 1673, one finds that Huygens gave up the two toothed wheels interposed between the pendulum and the verge with pallets and reverted to metal cheeks to render the oscillations truly isochronous for all arcs. In 1659 he discovered the theoretical form for these cheeks and showed that it was a cycloid arc. It became necessary to revert to the first design employed by Coster in 1657, and to go back to the horizontal crown wheel as shown in the figure. Figure 17, reproduced from the *Horologium Oscillatorium,* shows the flexible suspension between the cheeks more clearly. The second figure (Fig. 18) shows a pendulum clock with a tapered cylindrical pendulum and the method of using a "maintaining weight" as invented by Huygens. This latter device made it possible to rewind the clock without stopping it. The cord applying the weight passed over a spiked pulley *D* in the large figure. The following quotations are taken from the *Horologium Oscillatorium.*

> The opposite figure [Fig. 19] represents the clock seen from the side, showing first two plates *AA, BB,* half a foot long or a little more and twenty-two inches wide, whose corners are joined by four little columns so that the plates are distant from one another by one-and-a-half inches. In these plates are placed the axes of the principal wheels on both sides. The first and lowest wheel is that which is marked *C,* incised with 80 teeth, and to the axis of which is fixed the pulley *D* spiked with iron points to hold the cord with the weights attached, the reason for which is given later. The wheel *C* thus turns by the force of a weight; this moves the nearest pinion *E* which has eight teeth and at the same time the wheel *F* which has 48 teeth and is attached to the same axis. The latter moves another pinion *G* and a wheel *H* on the same axis, the numbers of whose teeth are the same as those of the preceding pinion and wheel. But this wheel is of the kind that our artisans call a crown wheel. By its teeth are turned the pinion *I* and at the same time the wheel *K* which is on the same perpendicular axis. This drum has 24 teeth and the wheel 15 which are made like those of a saw. Above the centre of the wheel *K* is placed horizontally the rod with pallets *LM* whose ends are supported on each side by the plates *N* and *P* separately attached to the plate *BB.* In the plate *NP* the part *Q* should be noticed, projecting towards the base, through an oblong opening in which the axis *LM* passes and which besides keeps in the vertical position the axis which we have said is common to the wheel *K* and the pinion *I.* In the plate *BB* a large opening is made through which can pass the end of the rod with pallets *LM,* which, inserted by its pointed end into the plate *P* moves thus more freely than if it

were supported by the plate *BB* itself and were at the same time prolonged through it; an extension is necessary so that the crank *S* may be attached to it so as to oscillate with it. For this is an oscillatory or reciprocating movement since the teeth of the wheel *K* make contact in turns with the pallets *LL* in the customary way and which needs no further explanation.

As for the crank *S*, the lower part of which is bent back and pierced with an oblong hole, this engages the iron rod of the pendulum to which the bob *X* is attached. This rod is suspended from above by a double thread between twin plates of which only one, *T*, is visible here. For this reason we have shown at the side a second figure intended to make clear the form of each and the general manner in which the pendulum is suspended. It will be necessary, however, to return to this subject and consider the true curve of the plates.

We will now turn to the movement of the clock for we shall explain the other parts of the figure later. It is easily seen that the pendulum *VX*, when once it has been set in motion by hand, maintains its motion through the force of the wheels driven by a weight; and at the same time the fixed period of the pendulum prescribes for all the wheels and consequently the whole clock, the law and pattern of the movement. Indeed the crank, however slight may be the movement communicated to it by the wheels, not only follows the pendulum which moves it but also contributes to that movement a short impulse at each come and go. It maintains then a movement which without this assistance would decrease little by little, in part at least owing to the resistance of the air, and come to rest. Indeed, the pendulum having the property of always following the same course, and not being diverted from it unless its length change, the wheel *K* is not permitted—once we have obtained the equality of which we spoke above by means of the curved plates between which the pendulum is suspended—to go now more quickly and now more slowly although in common clocks it often endeavours to do so; here its teeth must necessarily pass one after another in equal times. It is clear that the revolutions of the preceding wheels, like those of the hands which come last, are also rendered uniform seeing that all the parts move proportionally. Consequently if there is any fault in the construction or, on account of a change in the timing, the axes of the wheels turn with more difficulty, so long as this difficulty is not enough to cause the complete stopping of the clock there will be no reason to fear any inequality or retardation of the movement; the clock will always measure the time correctly or not measure it at all.

After completing his description of the mechanism, Huygens gave directions for adjusting the clock so that it completes twenty-four hours in the mean solar day. This is the ordinary

adjustment of a clock for everyday use. For use in the observatory the sidereal day and not the mean solar day is the unit required. There is about four minutes difference between these days but the correction is not constant since the solar day is not constant. Huygens gave a table showing the individual corrections for comparing a given (solar) day with the sidereal day.

In astronomy the measurement of the time of passage of a star across the meridian can replace the measurement of meridian altitude which is more difficult and, in the seventeenth century, the latter was always rendered somewhat uncertain through the absence of reliable corrections for atmospheric refraction. The history of the idea goes back some way. To explain it, it is necessary to refer to the diagram (Fig. 20) which shows the planes of the equator and the ecliptic intersecting the celestial sphere. As is well known, the position of a star in the sky is not usually recorded by its altitude and azimuth because these, unlike its right ascension and declination—or its celestial longitude and latitude—vary on the Earth's surface. In the figure O is the position of an observer and the celestial equator and the ecliptic intersect in the equinoctial points γ and $\frown$. The plane of the ecliptic is of course the apparent path of the sun among the stars and the equator is inclined to this at the supplement of the angle of inclination of the Earth's axis to the plane of its orbit, viz., 32° 27′. If S is a star we may record its position by stating its right ascension (arc γM) and declination (arc MS) or its longitude (arc γQ) and latitude (arc QS). All these arcs are measured in angular units except the right ascension, which is more often measured in units of time. For since the celestial sphere appears to revolve once in twenty-four hours, any given star will move 15° in one hour. The interval between the transit of the vernal equinox (the First Point of Aries) and that of a star across the meridian therefore gives its Right Ascension in hours. For this reason the great circles passing through the poles P and P' and drawn at 15° intervals from the equinoxes are called

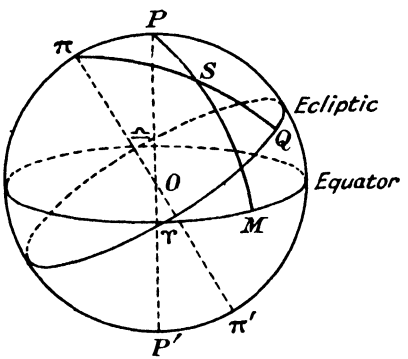


FIG. 20

Hour Circles. Right ascensions may thus be stated either in hours, minutes and seconds, or in degrees.

It is obvious that when recording the observed position of a star by its altitude and azimuth preliminary to reducing these to one or other of the spherical co-ordinates, the exact time at which the observation was made must be noted. This was at first done by recording the altitude of the sun or some other standard star. Purbach and Regiomontanus used this method in the fifteenth century. Bernhard Walther, a pupil of Regiomontanus, is sometimes said to have been the first to use a clock driven by a weight for scientific purposes. His clock was useless for intervals other than about an hour, however, and the first consistent use of time intervals was due to Tycho Brahe. Tycho used quadrants to observe altitudes and usually found the distance along the equator from the meridian with an armillary sphere. His use of a meridian quadrant to observe transits required the measurement of the time interval between the transit of the star and that of the equinoctial line.

In 1667 Huygens described a method which is similar to the well-known method of "equal altitudes" for finding the time of a meridian passage of a star. Roemer in 1690 really established the method of obtaining right ascensions and declinations by the use of transit telescopes. He also set up an instrument having altitude and azimuth circles for the observation of stars at equal altitudes on both sides of the meridian. The clock he used bears some resemblance to that of Huygens. One may take it for certain that the close association of both men with the Paris observatory led Roemer to use a clock of Huygens's design. Until the invention of the anchor escapement (1680) Huygens's clocks were far the most accurate available and in Paris they probably continued to be preferred for many years.

While discussing this subject of time measurement in astronomy one can see how the clock promised (and later supplied) a solution to the problem of determining longitude at sea. On March 21 a sidereal clock on the meridian of Greenwich agrees with a mean time clock. For a place not on this meridian the sidereal time must be corrected by the addition or subtraction of 9.8565 seconds for every hour of longitude difference according as the place is west or east of Greenwich. Huygens's own directions for the use of the marine clock at sea ignored the slight difference between sidereal and mean solar time. These direc-

tions were that the clock should be set going by mean solar time at the starting point of a voyage and that the solar time at sea (from the altitude of the sun) should be compared with the standard time kept by the clock. To the latter it was of course necessary to add the correction for the solar day required to take into account the inequality of the solar days. If, then, the mean solar time given by the clock after correction is earlier than the observed time, the ship has moved east, if it is later the ship has moved west. Each hour difference is equivalent to 15° of longitude. Figs. 21 and 22 show certain features of one of Huygens's later marine clocks.

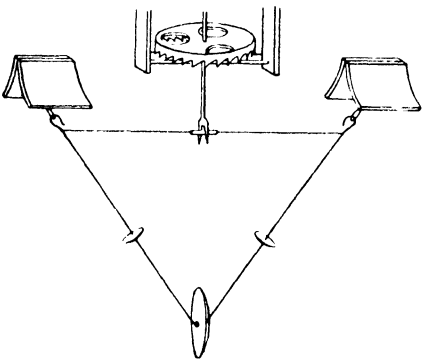


FIG. 21

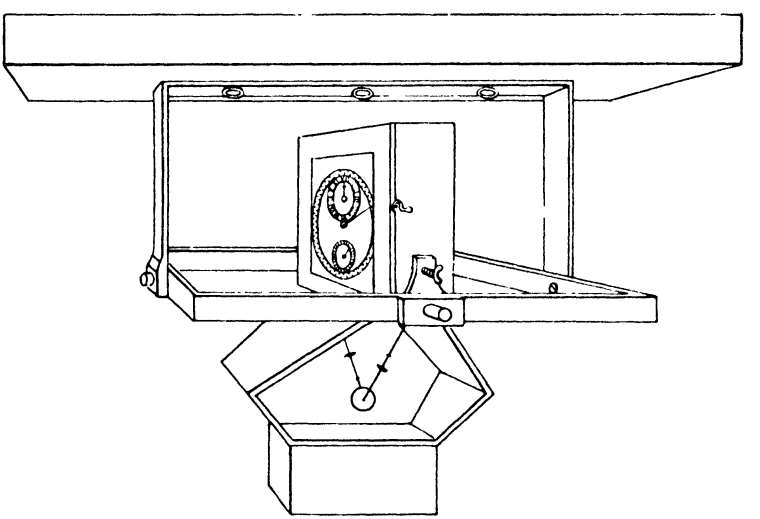


FIG. 22

VI

The Horologium Oscillatorium. Part Two
Oscillation in a Cycloidal Arc

This part of the *Horologium Oscillatorium* contained the first thorough treatment of oscillatory motion ever given. The study of accelerated motion in curved lines was begun by Galileo, and it is worth while looking at his work, so neatly was it completed by Huygens. Both of these writers treated their problems as a branch of " geometry ". It was only after the principles of Newton's *Principia* had been absorbed that mathematicians developed mechanics on the concepts of mass, force and impulse. The difference is perhaps more apparent than real, for Galileo implicitly and Huygens explicitly recognized the quantity termed mass as distinct from weight and concentrated on velocity and acceleration, both capable of geometrical representation. Huygens really developed a treatment of mechanics on the basis of *work done*—what we now term the energy equation—but this was never fully appreciated.

In the *Discourses on Two New Sciences* (Third Day), Galileo gave his classic propositions on naturally accelerated motion. He began with the assumption that the law of acceleration is *simple* and derived the well-known equations on fall from rest under gravity. He showed, among other things, that the times of descent of a smooth body over inclined planes of the same vertical height are proportional to the lengths of the planes: also that chords of the same circle are the paths over which descent takes the same time. The latter theorem was employed by Huygens and Galileo's proof of it may well be given here in symbolic form.

In Figure 23, B and C are any two points on the circumference of a circle, centre O and radius r. AF is the vertical diameter, AD and AE the vertical heights of AB and AC, and AI is the mean proportional of AD and AE. Putting $AD = x_1$, $AE = x_2$, $AI = m$, we have, therefore, $m = \sqrt{x_1 x_2}$.

Now $AC^2 = x_2^2 + CE^2$

and $CE^2 = CE.EC' = r^2 - OE^2$

$\therefore \quad AC^2 = x_2^2 + r^2 - OE^2$

$= x_2^2 + r^2 - (r - x_2)^2$

$= 2rx_2.$

Hence $\dfrac{AC^2}{AB^2} = \dfrac{2rx_2}{2rx_1} = \dfrac{x_2}{x_1},$

But $\dfrac{x_2}{x_1} = \dfrac{m^2}{x_1^2}$

$\therefore \quad \dfrac{AC}{AB} = \dfrac{m}{x_1}$ .....(i).

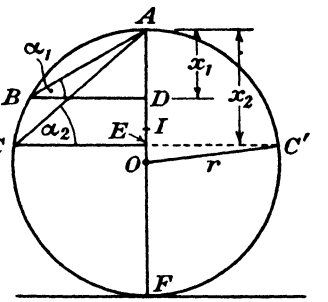


FIG. 23

For the times of descent along the inclined lines we have the equations

$$AB = \tfrac{1}{2} a_1 t_1^2$$
$$AC = \tfrac{1}{2} a_2 t_2^2$$

where a_1 and a_2 are the accelerations produced.

But $\dfrac{a_1}{a_2} = \dfrac{\sin a_1}{\sin a_2} = \dfrac{x_1}{AB} \cdot \dfrac{AC}{x_2},$

Hence $\dfrac{t_2^2}{t_1^2} = \dfrac{AC.\sin a_1}{AB.\sin a_2} = \dfrac{AC^2}{AB^2} \cdot \dfrac{x_1}{x_2},$

i.e. $\dfrac{t_2}{t_1} = \dfrac{AC}{AB}\sqrt{\dfrac{x_1}{x_2}} = \dfrac{AC}{AB} \cdot \dfrac{x_1}{m}$ ..... (ii),

and by (i) above $\dfrac{t_2}{t_1} = 1.$

Galileo actually proved the relation (ii) in a separate proposition.

Galileo pointed out that the speed acquired in descent over an inclined plane is always such as would enable a body to re-ascend another plane of equal vertical height. His experiment with the intercepted pendulum illustrated this point (see p. 121). He went astray, however, in supposing that a circular arc is the path of quickest descent under gravity.

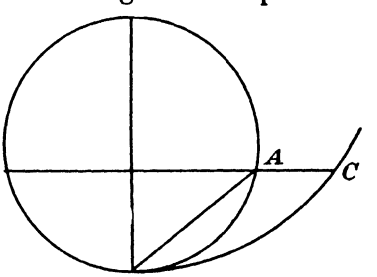


FIG. 24

It is usually supposed that Galileo considered the simple pendulum made isochronous oscillations in all arcs and that

the period was quite independent of the amplitude. This is certainly assumed in the *Discourses,* where Galileo comments on the fact that descent from *A* to *B* over the inclined plane (Fig. 24) takes longer than descent along a circular arc *CB*. He then added: "As to the times of vibration of bodies suspended by threads of different lengths, they bear to each other the same proportion as the square roots of the lengths of the threads . . . ". There is no record in his writings to show that he observed the discrepancy of the periods of large and small swings. The Florentine Academicians, however (*Essayes of Natural Experiments made in the Accademia del Cimento*, translated by R. Waller, 1684), said that Galileo observed the "very near equality" of the swings. Mersenne, in his work *Les Nouvelles Pensées de Galileo touchant Les Mécaniques et la Physique*, said that Galileo did not observe the discrepancy. "If the author had been more exact in his trials," he wrote, he would have noticed it. His regrets appear somewhat out of place, however, when one considers the fertile ideas Galileo drew from his study of the pendulum; an exact relation between the amplitude and the period could have been only a hindrance at this stage. Enough is known of Galileo's methods for us to be sure that an element of idealization came into his treatment of the pendulum, so that he perhaps intentionally ignored small experimental deviations from the formulated law. The laws of nature, he believed, must be characterized by their simplicity.

Huygens did not study the simple pendulum exhaustively. After showing that the oscillations could be regarded as isochronous only when the arc was small he turned his attention to the problem of the tautochrone, that is, the curve over which all oscillations take the same time under gravity. He showed that the cycloid satisfies these requirements, being the curve traced by a point on the rim of a wheel which rolls along a horizontal surface. It was left to Lagrange and Laplace to complete the study of the simple pendulum and derive some form of relation between the period and the amplitude. The former's *Mécanique Analytique* (Chap. 2) (1788) and the latter's *Mécanique Céleste* (Chap. 2) (1799) should be consulted.

Throughout this part of the *Horologium Oscillatorium* Huygens's debt to Galileo is very clear. The first nine propositions, in fact, are really a clear résumé of Galileo's work on natural acceleration with some small additions. A good deal of ground

had to be cleared before Huygens could get to grips with the central problem. Some geometrical properties of the cycloid, a method of drawing a tangent to the curve, and the setting of limits to the length of an arc of a circle occupy the next few

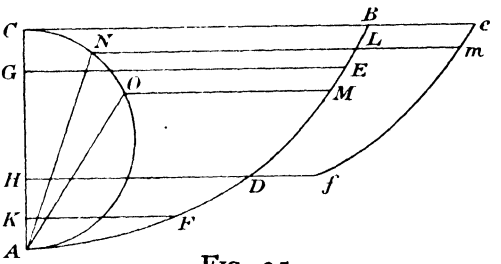


FIG. 25

pages. Then in Proposition 22 Huygens showed quite simply that if two cycloidal arcs of equal vertical height are considered, descent from rest is quicker over the steeper curve. In Fig. 25, *BD* and *EF* are the arcs, equal in vertical height *h* but one less steep than the other.

By a property of the cycloid the tangent at any point *L* is parallel with the chord *NA* of the generating circle. *L* and *M* are taken such that *L* is the same vertical height below *B* as *M* is below *E*. Transferring the arc *EF* to the position *ef* the point *M* is at *m*, level with *L*. Since the inclination of the tangent at *m* is given by the chord *OA* and for all points such as *L* and *m* the inclinations of the tangents to the steeper curve are themselves steeper, the time of descent must be shorter over *BD* than over *ef*.

In the next proposition Huygens compared the time of

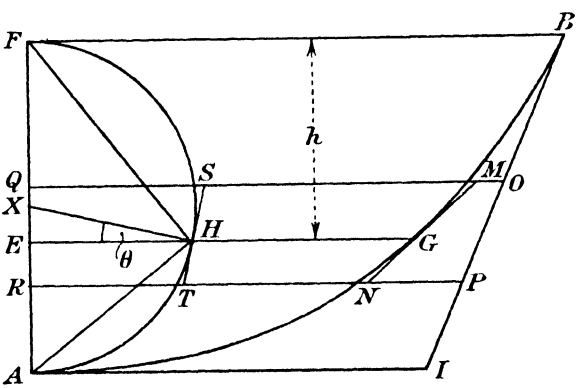


FIG. 26

descent over a short segment of a tangent to the cycloid with that over an inclined plane of equal height. Supposing a body to be released at B in Fig. 26, and to descend along the arc BG, he compared the time of descent over MN with the speed attained at G with the time of descent over OP with the mean velocity acquired during descent over the distance BI, the line BI being the tangent to the cycloid at B. Using Galileo's relation $v^2 = 2gh$ for vertical descent, Huygens took the velocity of a body after descent over BI to be $\sqrt{2g \cdot 2r}$ or $2\sqrt{gr}$ and the velocity at G to be $\sqrt{2gh}$. One half of the former is thus $\sqrt{gr}$.

Thus $$\frac{v_1}{v_2} = \frac{\sqrt{FA}}{\sqrt{FE}}.$$

By similar triangles

$$\frac{FA}{FH} = \frac{FH}{FE}$$

and

$$\frac{FA}{FE} = \frac{FH^2}{FE^2}$$

$$\frac{FA}{FH} = \frac{FH}{FE} = \sqrt{\frac{FA}{FE}}.$$

Hence $$\frac{\frac{1}{2}v_1}{v_2} = \frac{FX}{FH}.$$

Now $$\frac{t_1}{t_2} = \frac{MN}{v_2} \cdot \frac{\frac{1}{2}v_1}{OP} = \frac{FX}{FH} \cdot \frac{MN}{OP}.$$

The remainder of the proof consists of showing by geometry that

$$\frac{MN}{OP} = \frac{FH}{HE}.$$

Hence $$\frac{t_1}{t_2} = \frac{FX}{HE} = \frac{HX}{HE} = \frac{ST}{QR}.$$

The last equality follows since $HE = HX \cos\theta$

$QR = ST \cos\theta$.

It appears a somewhat curious relation to set out for but the time of descent over a cycloid arc is later to be obtained by reference to a relation between the time of descent over a series of tangents ST, and the time of fall along an intercept on the axis FA. Unfortunately, the following theorem is unsuitable for reproduction here in its original form. Anyone who looks up the work in the original will see that here is an interesting method of exhaustion pushed to its limits.

In Fig. 27 a large number of tangents are drawn to the cycloid and to the circle. It is required to show that the time of descent t_1, along an arc BE, is to the time of descent t_2, along the tangent BI (with a uniform speed equal to the mean speed acquired over $B\theta$) as the arc QH is to QG. The intercepts made in BI being supposed equal, the time interval St_2 for motion along each is a fixed quantity. The relation with the previous theorem is then best shown by taking t_1 to be the sum of the intervals δt_1, required for descent over the tangential elements M_1N_1, M_2N_2, etc.

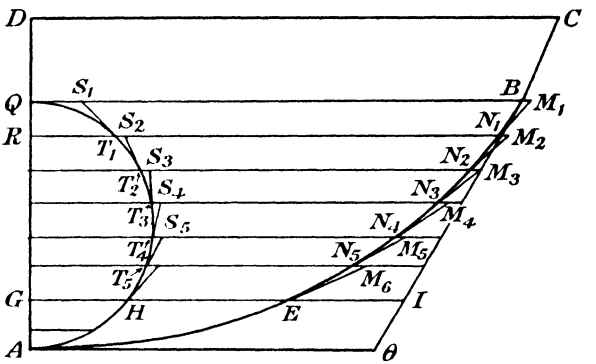


FIG. 27

The equation of the previous theorem is then written in the form

$$\frac{\delta t_1}{\delta t_2} = \frac{S_1T_1}{QR}$$

where QR is the interval between the parallels measured on the diameter QA. Dividing this equation by the number of elementary tangents, n, we get

$$\frac{\delta t_1}{n\delta t_2} = \frac{S_1T_1}{nQR}$$

and there are n of these relations altogether. Adding these equations we get

$$\frac{\Sigma\delta t_1}{n\delta t_2} = \frac{\Sigma ST}{QG}.$$

In the limit, when the elementary tangents are very small and very numerous, $\Sigma ST = QH$, $\Sigma\delta t_1 = t_1$ and we have

$$\frac{t_1}{t_2} = \frac{QH}{QG}.$$

Huygens was then able to complete his demonstration by

showing that the time of descent from any point on a cycloid to the lowest point has, to the time of descent along the axis, the ratio of the semicircumference of a circle to its diameter.

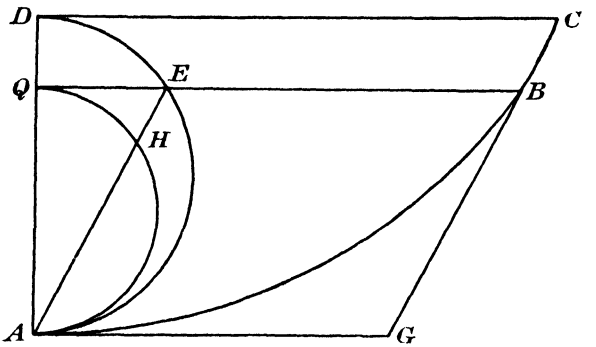


FIG. 28

Applying the previous theorem to the case of descent to the lowest point

$$\frac{t_{BA}}{t_{BG}} = \frac{arc\ QHA}{QA}$$

where t_{BG} represents the time of descent with the mean speed acquired over the tangent BG. This, however, is the same as the time for naturally accelerated fall along BG. Now BG is equal and parallel with EA (by a property of the cycloid) (Fig. 28). Hence:

$$\frac{t_{BA}}{t_{EA}} = \frac{arc\ QHA}{QA} = \frac{\pi r}{2r} = \frac{\pi}{2}$$

By Galileo's theorem (p. 137), t_{EA} may be replaced by t_{DA} since EA and DA are chords of the same circle. This establishes the tautochronism of the cycloidal pendulum.

Huygens did not include in the *Horologium Oscillatorium* his proof that in a cycloidal pendulum the restoring force is proportional to the arc of displacement. This short addendum is, however, important, since Huygens was the first to give the mathematical theory of simple harmonic motion. Leibnitz wrote to Huygens in March 1691: "M. Newton has not treated the laws of the spring; I seem to remember having heard you say on another occasion that you have examined them, and that you had demonstrated the isochonism of the vibrations." We may see

for ourselves that the fact that the cycloidal motion is simple harmonic follows from Huygens's previous discoveries. He seems to have arrived at the conclusion about 1673.

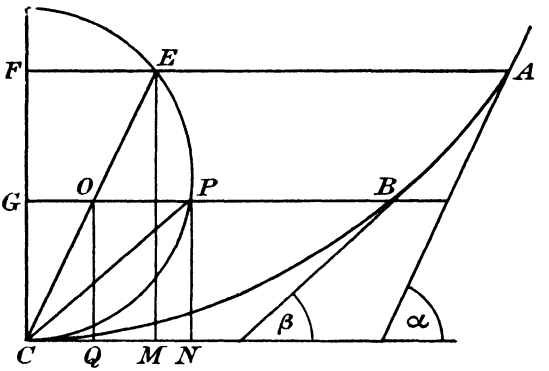


FIG. 29

In Figure 29 the cycloid arc AC is divided at some point B. "Then," Huygens wrote, "the component of gravity along the plane tangential to the curve at A and the component at B is in each case proportional to the slope of the tangent", i.e., in modern terms:

$$\frac{\text{Force down plane at } A}{\text{Force down plane at } B} = \frac{g \sin \alpha}{g \sin \beta}$$
$$= \frac{EM}{EC} \cdot \frac{PC}{PN}$$
$$= \frac{OQ}{OC} \cdot \frac{PC}{OQ}$$
$$= \frac{PC}{OC}.$$

But, by a lemma to the theorem on page 139, Huygens showed by simple geometry that

$$\frac{PC}{OC} = \frac{EC}{PC}.$$

Hence: $$\frac{\text{Force down plane at } A}{\text{Force down plane at } B} = \frac{EC}{PC}.$$

It was not difficult to show that EC is to PC as the lengths of the respective cycloidal arcs AC and BC. This relation may readily be demonstrated by the use of modern methods but Huygens's

proof was not presented in a formal manner. With it the theorem on simple harmonic motion is completed: the acceleration at any point on a cycloidal arc is proportional to the length of the arc measured from the lowest point. It is thus clear from his notebook that Huygens cleared up most of the obvious problems which are raised by a study of oscillatory motion. Such problems can only be dealt with satisfactorily by means of mathematics.

It must be admitted that Hooke, in England, earlier than Huygens, had commented on the conditions required for simple harmonic motion. Birch's *History of the Royal Society* (1756–57) contains a passage by Hooke dated Nov. 21, 1666, on this subject: "The equality of duration of vibrations of differing arches or lengths depends upon the figure of the curve-line, in which the body is moved; which figure being for a very great part near the same with that of a circle, it follows that, the motion in differing arches of the same circle will be very near of equal duration." Hooke's attempt to show mathematically that this followed from (Galileo's) mechanics was rather feeble, however. Even Brouncker, president of the Society and a much more able mathematician, made little headway with this subject. Yet both Brouncker and Hooke started where Huygens started and with the same fundamental conceptions. Hooke's *Potentia Restitutiva,* published in 1678, contained a further reference to the subject and this work is usually regarded as being the starting point in the history of simple harmonic motion. It contains the law "ut tensio sic vis" and many deductions drawn therefrom. Nevertheless his contribution cannot be compared with that of Huygens, whose work was carried out quite independently. The properties of springs were well understood by Huygens in 1675, for this is the date of his invention of the spiral-spring regulator for watches. A further illustration of his grasp of the essential condition may be found in his experiments on the use of a trifilar pendulum. This consisted of a heavy flat ring suspended by three equal vertical cords from three equidistant points. The oscillations of this pendulum were of a torsional character and he hoped that it would prove superior to the spiral spring, especially in regard to the effects of temperature. It did not appear to matter how the restoring force was set up—whether through gravity, elasticity or magnetic attraction—he remarked.

VII

The Horologium Oscillatorium. Part Three Evolutes and the Measurement of Curves

The occasion may be taken for a brief mention of Huygens's mathematical work. This seems to be divisible into two parts. In one part of his mathematical work Huygens may be said to have belonged to the classical schools of Archimedes, Hippocrates of Chios and Eudoxus rather than to the period of Descartes, Newton and Leibnitz. He was, for example, greatly interested in the three great problems of antiquity, those of squaring the circle, trisecting an angle and duplicating a cube, and he was in the classical sense the outstanding geometer of his age. The impress of classical antiquity lies on most of his early work. His earliest published work, *Theoremata de quadratura hyperboles ellipsis et circuli, ex dato portionem gravitatis centro* (1651), was undoubtedly inspired by Archimedes' work in hydrostatics dealing with the flotation of certain geometrical figures. It was in a sense a continuation of Archimedes' *De Æquiponderantibus*, and in finding the area of a given segment of a hyperbola, ellipse or circle, less than the whole figure, Huygens used the classical method of exhaustions. In the form of the "method of indivisibles" this method had been extended by Kepler and Cavalieri, the latter's work being especially influential. Huygens took the opportunity of showing up the fallacy in some work on quadratures by Gregory de St. Vincent, a contemporary Jesuit mathematician who appears not to have understood the newer developments.

Huygens began his treatise by determining limits to the area of a given segment of each of the above figures. He then located the centres of gravity and worked out some theorems, in particular a theorem concerning a relation between the length of an arc of a circle, its chord, the radius and the distance from the centre to the centre of gravity of the segment. This paved the way for an examination or "Excursus" criticizing de St. Vincent's work. This excursus occasioned a dispute which lasted for ten years and in which several mathematicians were involved. It has not left any important results, however.

In January 1652 Huygens began to study problems which lead to equations of the third or fourth degree. These "solid" problems also arose, very probably, from a study of Archimedes' writings. In the course of his work Huygens (1654) carried out a new determination of the circumference of a circle. In the form of a determination of π, van Ceulen, early in the century, had made a new contribution to this ancient problem. Snell, in his *Cyclometricus* of 1621, narrowed the limits set by Archimedes in his original investigation but made use of propositions which were not rigorously proved. In all this, however, there was little if anything which could be called new in Huygens's work. The more interesting parts of his studies on the problems bequeathed by Archimedes are concerned with physics rather than with mathematics. In continuing the latter's studies of flotation, for example, he was able to make use of his fundamental principle that the centre of gravity of a system takes up the lowest position consistent with the restraints. In the first four theorems of his *De Iis quae liquido, supernatant* (1650) he deduced in this way the horizontal surface of a stationary liquid, the equilibrium of floating bodies when the density is equal to that of the supporting fluid, and the celebrated law of Archimedes for the case in which the density of the solid is less than that of the liquid.

In the other part of his work to which we may now turn Huygens was interested less in continuing the study of classical problems than in new and original developments. Reference has been made on more than one occasion to Huygens's fruitful interest in the cycloid. This curve may be taken as a starting point in reviewing briefly his important work on evolutes. Descartes influenced Huygens very much in this connection, for he had always insisted that any curve whose mode of generation could be clearly conceived belonged to Geometry. Descartes accordingly took all curves formed by the intersection of two moving lines, the rates of movement of which had a known ratio.

Huygens did not commence his own researches on the cycloid until twelve years after Mersenne, in 1646, gave him his first information about publications on this interesting curve. Pascal's "Dettonville" problems, as has been mentioned, aroused Huygens for the first time. The problems were to find the area of a half-segment *EBF* of a cycloid (Fig. 30), the position of its centre of gravity and the volumes of the solids produced by revolution of the segment about *BF* and about *EF*.

Huygens at first found the area *EBF* and thence the area of the whole segment *EBO* and he found the distance of the centre of gravity of the segment from the base *EO* and deduced the volume of the solid of revolution about this base. Pascal then suggested the calculation of the centre of gravity of the half-solid of revolution of *ABD* about *AD*. Huygens obtained a par-

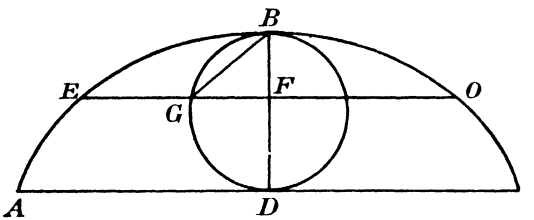


FIG. 30

tial solution to this but found the work so difficult that he doubted if all Pascal's problems could in fact be solved. In passing it may be noticed that Huygens improved on the ordinary proof required for the method of drawing a tangent to a cycloid. In the preceding figure a tangent to the cycloid at *E* must be parallel with the chord *BG* of the generating circle—an interesting property which was given in Schooten's edition of Descartes's *Geometria*.

Pascal's proofs for the Dettonville problems compare favourably with the work carried out by Huygens in being altogether more elegant and at the same time more general. Wallis, however, complained about Pascal's methods in his *De Cycloide* of 1660, in which solutions to the Dettonville problems were obtained by means of Wallis's "arithmetica infinitorium". A dispute between Wallis and Carcavy ensued, Huygens acting as intermediary, but it does not seem to have been of great significance. The really interesting feature of the whole episode is that it started Huygens on his study of evolutes.

The idea of the evolute of a curve may be explained by reference to the parabola. This curve (Fig. 31) may be described as the locus of points equidistant from a given point *F* (the focus) and a given line *XY* (the directrix). With the exception of the circle, all curves show varying curvature; whereas all normals to the circle intersect at the centre, the normals to other curves intersect at a series of points which generate another curved line.

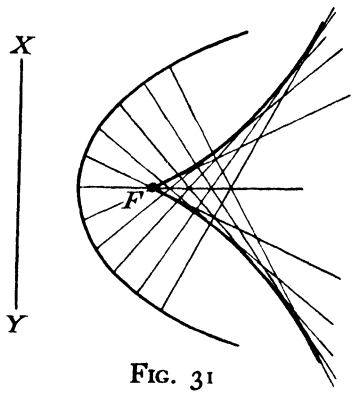


FIG. 31

This is termed the evolute. This will be clear from the figure in the case of the parabola.

The evolute may in fact be defined as the envelope of the normals to the given curve. Huygens no doubt saw, in his experiments with a simple pendulum, that the bob could be made to describe a variety of different arcs according to the shape of the curved "cheeks" between which it was suspended. Propositions 5 and 6 of the third part of the *Horologium Oscillatorium* contain the discovery that the evolutes of a cycloid are themselves cycloid arcs. The proof is of course geometrical. In Fig. 32 the arc AF is equal in length to the arc AC. By establishing this Huygens was able to rectify the curve and show that a cycloid is four times the length of the diameter of the generating circle. It is not considered necessary to recapitulate his method here.

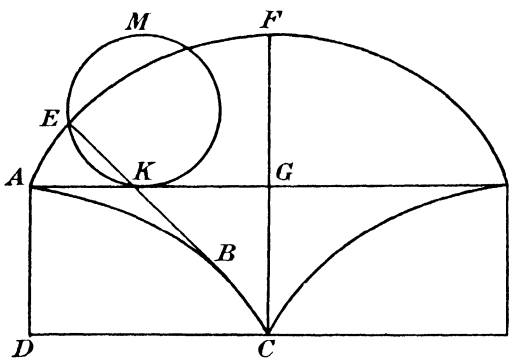


FIG. 32

Huygens is remembered as the discoverer of the evolutes of a cycloid, but it must be pointed out that he dealt also with the evolutes of a parabola (proposition 8), of an ellipse and of a hyperbola (proposition 10). He also showed how to rectify curves for which the evolutes are known. This work has not been given much attention, possibly because of the more important general methods of quadrature worked out by Wallis. Huygens

himself does not appear to have seen that the theory of evolutes would find its most useful application in a field in which he himself was the first to explore: the theory of focal lines in geometrical optics.

To the cubical parabola and cycloid Huygens later added the curve known as the cissoid to be included among curves whose rectification could be accomplished. The French physician, Claude Perrault, set the problem: to determine the path in a fixed plane of a heavy particle attached to one end of a taut string whose other end moves along a straight line in that plane. Huygens and Leibnitz studied this problem in 1693, and worked out the geometry of the tractrix. It was at this time that Huygens solved the problem of the catenary and determined the surfaces of certain solids of revolution.

After his return to Holland in 1681 it seems as if Huygens resorted to pure mathematics and gave less place to physical work. About this time the infinitesimal calculus acted as a great stimulus to mathematicians, and it is not surprising, therefore, to find that Huygens's notebooks contain a great deal of work of a more analytical character done after his return to the Hague. A review of all his mathematical work would be a most valuable contribution to the literature of the history of mathematics for, over a long period of years, from his earliest work on the circle there is a range of subjects, many of which bear a logical relationship, including work on maxima and minima, which links up with the work of Fermat and the English mathematicians of the period and culminates in the work of Leibnitz and Newton.

VIII

The Horologium Oscillatorium. Part Four
The Centre of Oscillation of a Compound Pendulum

Passing reference was made on page 99 to the existence of action and reaction in the case of connected bodies. Galileo was unable to account for the behaviour of two small masses suspended along one thread (Fig. 33). He may have supposed that it should be possible for the masses to swing in unison, whereas in fact two independent vibrations exist in such a system. This is a difficult problem and there was no important practical reason for pursuing it. A more urgent problem was to calculate the period of a given compound pendulum, that is, a *rigid* body suspended so as to oscillate about an axis which passes through it. This is the type of pendulum employed in the pendulum clock, and the problem is to find *l*, the distance from the axis of oscillation to the centre of oscillation. The latter is the point at which the entire mass would have to be concentrated in order to obtain the equivalent simple pendulum, that is, that which possesses the same period of oscillation. The scientific principles of clock construction could not be said to be known until this central problem was solved. What was more important was that this particular problem opens up the whole subject of dynamical systems.

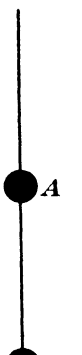


FIG. 33

Huygens was early acquainted with the problem by Mersenne and supposed that it emanated from him. The problem had, however, a longer history. The work *In Mechanica Aristotelis Problemata Exercitationis* (1621), by Baldi, contained some erroneous suggestions on the centre of percussion and the subject even seems to have been discussed by certain Greek writers. A modern illustration of the centre of percussion would be the point on a cricket bat at which the ball must be struck to secure the maximum effect with least effort. The centre of percussion, if it exists, is the same point as the centre of oscillation, a relation which was found empirically by Mersenne and proved theoretically many years later. The interest shown by mathematicians like Descartes and Roberval in this problem must be attributed

in some degree to the practical value of the subject in the design of sword blades. None of these, however, achieved any success. In a letter to Mersenne in 1646, Descartes dealt at some length with certain special cases of the centre of oscillation—those of a long rod, a plane triangular figure and others. He clearly thought that the problem could be reduced to one in statics—that of determining the centre of gravity of certain solids and planes. The work was surprisingly slipshod and amounted to little more than a series of ingenious guesses and no proofs could, of course, be given.

Although Huygens's experiments on the centre of oscillation may have been carried out from as early as 1646, when his correspondence with Mersenne started him on the study of several problems of mechanics, his theoretical studies can hardly be dated earlier than 1659. He appears to have employed experiment much as Galileo did, and his procedure was in fact an excellent illustration of the inductive method. Starting with the simplest case of a linear rigid pendulum, he proceeded to study the oscillation of laminæ oscillating in their own planes. For these cases he was soon successful in finding a way of calculating the length of the equivalent simple pendulum. Expressed in modern form this amounted to an application of the relation:

$$l = \frac{I}{mh},$$

where I is the moment of inertia about the axis of oscillation, m is the total mass and h is the distance of the centre of gravity from the axis of oscillation. The concept of the moment of inertia originated in Huygens's work, but it was given this term later by Euler. Huygens, however, discovered the important theorem which relates the moments of inertia of a lamina about two axes perpendicular to each other. All the laminæ considered had regular shapes possessing an axis of symmetry. From these cases he passed on to the study of solids of revolution produced by rotating the laminæ about the axes of symmetry. For this a method of evaluating Σmr^2 for the relation $I = \Sigma mr^2$ had to be devised and this part of the work makes the hardest reading for the modern reader who is accustomed to solve such problems by means of the integral calculus.

As might be expected, Huygens started with certain problems left unanswered by Galileo and he was able to solve them by the

application of his theorems on impact. As will be seen the subject of moments of inertia came out of the solution obtained in this preliminary work.

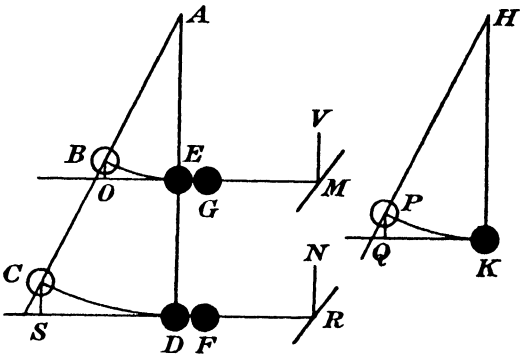


FIG. 34

In Fig. 34, an inflexible weightless bar carries two masses, D at the lower end, and E at some point in AD. Given the masses E and D, and the distances AD and AE, the problem was to find the centre of oscillation. The great difference here is that Huygens made the connection rigid, whereas in Galileo's problem the masses were connected by a light thread.

Huygens's treatment was to suppose the pendulum withdrawn through an arc to the position ABC and then released. After passing through half of a swing the masses are again at the positions E and D and possess speeds which are different from those which they would acquire as the bobs of two separate pendulums of lengths AD and AE. Huygens supposed the two masses at this point to collide with masses which are respectively equal to the given masses but not connected. There is no loss of momentum and if the masses are all perfectly elastic the bodies F and G will acquire the speeds possessed by D and E, the latter being brought exactly to rest by the impact. Huygens then applied the equation connecting potential and kinetic energies to these masses. This manner of treating the problem was quite new. He supposed that the speeds acquired by F and G are such that they can rise against gravity to the positions N and V. The essential point in this process was that the centre of gravity of the masses could not ascend beyond its original height when the pendulum was in the position ABC.

In Fig. 34 is shown the equivalent simple pendulum HK of length x. If we put $AD = a$, $AE = b$, the mass of $D = m_1$, that of $E = m_2$ and their speeds at the lowest points respectively v_1 and v_2 we have

$$\frac{\text{speed of } D}{\text{speed of } K} = \frac{v_1}{v} = \frac{\text{arc } CD}{\text{arc } PK} = \frac{AD}{HK} = \frac{a}{x},$$

where v is the speed of the bob K through its lowest point. If the height CS is put equal to d, the height QP is given by

$$QP = d.\frac{x}{a}.$$

It has to be borne in mind that the equivalent pendulum swings with the same period as the compound pendulum. The height RN to which the masses F can ascend is such that

$$\frac{RN}{QP} = \frac{v_1^2}{v^2} = \frac{a^2}{x^2}.$$

Now $$\frac{x^2}{a^2} = \frac{xd/a}{ad/x}.$$

Hence the height which the mass F can attain is ad/x and for the height MV, for the other mass, we obtain in the same manner b^2d/ax.

The centre of gravity of the two masses will then attain a height which can be calculated quite simply: the work done in raising the masses through their respective elevations NR and MV is equated to the loss of potential energy in the descent of the masses through CS and BO. These heights may then be expressed in terms of x, the length of the equivalent simple pendulum.

$$F.NR + G.MV = D.CS + E.BO$$

or $$m_1\frac{ad}{x} + m_2\frac{b^2d}{ax} = m_1d + m_2\frac{bd}{a}$$

i.e $$m_1a^2d + m_2b^2d = m_1dax + m_2bdx.$$

Thus $$x = \frac{m_1a^2 + m_2b^2}{m_1a + m_2b}.$$

If this treatment is extended to a uniform rod, considered as composed of contiguous masses, the general formula

$$x = \frac{\Sigma mr^2}{\Sigma mr}$$

is obtained. The quantity Σmr^2 was thus introduced into mechanics. In this work Huygens made effective use of the idea of work done against gravity, as can be seen. Unfortunately, his ideas were not made explicit and he did not formulate a general method. His published work, however, showed some improvement on this original form of the "direct" method. The device of supposing an impact with equal masses which are not connected was abandoned and, in the *Horologium Oscillatorium*, Huygens simply supposed that the separate masses constituting the pendulum were freed from their connections at some point in the swing. The mathematics remains unchanged.

It is important to appreciate exactly what Huygens had done in this piece of work, for it has a bearing on the development of the concept of energy in physics. The central idea lay in combining his fundamental principle that the centre of gravity of the masses cannot ascend as a result of a displacement occurring under gravity with Galileo's relation connecting the speed acquired in falling with the square root of the height. In the *Horologium Oscillatorium* the speeds of suspended particles in any point in the path were compared with the corresponding heights of descent, a procedure which really amounted, in combination with the conservation of *vis viva*, to the application of the law of conservation of energy in mechanics. It is possible from this starting point to obtain an equation showing the constancy of the sum of the kinetic and potential energies for an isolated system, namely:

$$T + V = H$$

in the form given by Lagrange. If any doubt existed as to the significance of Huygens's contribution to the subject of energy it may fairly be stated to be removed by a later statement of his ideas. In his MS. of 1693, two years before his death, he wrote: "In all movements of bodies whatsoever, no force is lost or disappears without producing a subsequent effect for the production of which the same amount of force is needed as that which has been lost. By force I mean the power of raising a weight. Thus, a double force is that which is capable of raising the same weight twice as high." The word for force in this passage was *vis*, a word which, like *potentia*, was used in the seventeenth century in the two senses of force and energy. Unfortunately, the defini-

tion is ambiguous but it would fit the notion of work or energy rather better than that of force in the Newtonian sense. The passage clearly reduces to this: there is something about a moving body which enables it to effect changes in the state of either itself or other bodies, and these results are quantitatively related with the cause which is the force or energy involved. The same idea was far less clearly expressed by Leibnitz about this time: " . . . it seems necessary to admit in bodies something other than magnitude and velocity unless we are willing to deny to bodies all power of action ".

To return from this digression to the contents of the *Horologium Oscillatorium*: Huygens found it impossible to apply the " direct " method to determine the centre of oscillation of suspended solids. The location of the centre of oscillation of a suspended sphere, for example, cost him much time and the method given in this work is long and difficult. His ideas appear altogether strange to the modern reader and an explanatory note will not be out of place at this point. To understand how Huygens's ideas took shape it is of interest to know that he read a work entitled *Tractatus Physicus de Motu locali* by one Mousnier, a pupil of Père Honori Fabri, which came out in 1646, and in this there was a novel attempt to solve the problem of the centre of percussion in the general case. Little seems to be known of Mousnier, and his ideas may, to a large extent, have been derived from Fabri. Mousnier used a concept which he termed the " impetus " of motion. By this he meant a quantity varying as the product mv for a particle in motion and he applied this to the elementary parts of an oscillating body as follows: considering the oscillation of a plane surface about an axis in its own plane, to evaluate the total " impetus " of all the elements, Mousnier took lines perpendicular to the surface and these represented by their lengths the speeds of the elements (to which they were normal) at the lowest points in their paths. This construction generated a wedge-shaped solid. For laminæ of simple form the volume of the wedge could be calculated. Mousnier did not know, as Huygens did, that the centres of percussion and oscillation are identical. The identity had not been proved at that time and it was doubted by some, notably by Roberval, who thought the two points were only approximately in the same position. Proceeding by experiment, and at first inductively, Huygens was able to find the equivalent simple pendulum

for a lamina oscillating in the way stated, and this opened the way to deal with suspended solids. The problem in every case is to evaluate the expression Σmr^2.

To take a simple case, the lamina *ABC* (Fig. 35) oscillates about the axis *EAE* which is tangential. Huygens proceeds to construct a wedge-shaped solid on the lamina as base: a second plane is taken at 45° and a generating line *DB*, perpendicular to *ABC*, moves round the boundary of the lamina tracing the projection on the inclined plane. The centre of gravity of the enclosed solid can be found if the base is of simple geometrical form, and a plane of symmetry meets the axis of oscillation perpendicularly at *A*. From the centre of gravity, *X*, Huygens drops a perpendicular *XL* to the base and gives the name *subcentric* to the straight line *AL*. This is only a term for the distance of the centre of percussion from the axis. The wedge is in fact a geometrical representation of the expression $\int v.da$ where *da*, in the ordinary calculus notation, is an element of the area of the lamina and *v* is its linear velocity at the mid-point of the oscillation. If we write *dm* in place of *da* it becomes more clear that the wedge represents the summation of the linear momentum of the lamina as it passes through the mid-point of its swing. By determining the point *L*, Huygens was in fact finding the position at which the total mass *M* would possess the same momentum. Putting $AL = l$, we may, for example, take the simple case where, for the lamina, we have simply a uniform rod. The result for this case is already known by the direct method to be

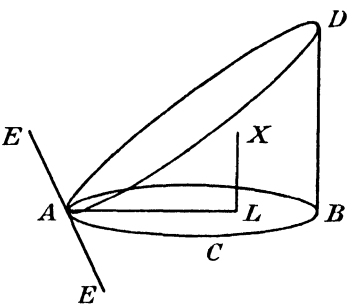


FIG. 35

$$l = \frac{\int y^2 dy}{\int y dy} = \frac{\frac{1}{3}y^3}{\frac{1}{2}y^2} = \frac{2}{3}y$$

(this is a modern form of the expression given before). The same result may also be obtained by the new geometrical method, for the point *X* would obviously lie at the point of intersection of the medians of a right-angled isocceles triangle. It follows from this that $AL = \frac{2}{3}AB$ (Fig. 36).

In the case of a lamina a method of finding the volume of the wedge was required. Huygens showed that this could be done by means of a simple relation which may be expressed:

volume = (area of base)(distance AF)

where F is the centre of gravity of the lamina (Fig. 37). Huygens's

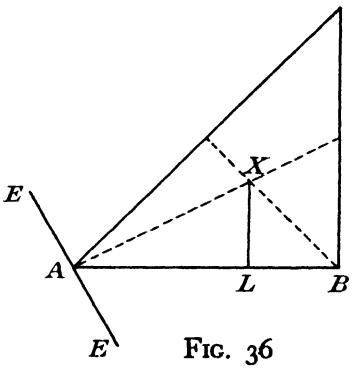


FIG. 36

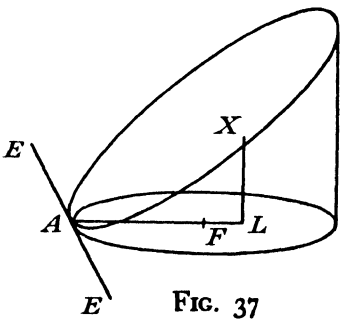


FIG. 37

method is in effect to treat the determination of Σmr^2 about the axis EAE as a geometrical problem. He does not explain how he made his discoveries but shows how Σmr^2 for a lamina of simple shape can be found for several different axes of oscillation—including an axis through the centre of gravity or at a fixed distance from it.

When he took up the problem of finding the moment of inertia of a suspended solid his procedure showed an essential resemblance with that employed when such problems are solved by means of the calculus. In this case the regular solid is divided by planes chosen in a suitable direction so as to form a series of laminæ of simple geometrical form. The expression $y^2.da$ is then integrated between the limits of y, the distance of any element from the axis of oscillation after expressing da in terms of the co-ordinates x and y. The integration is then required for an expression in terms of y and dy. This is scarcely a satisfactory statement about Huygens's method,

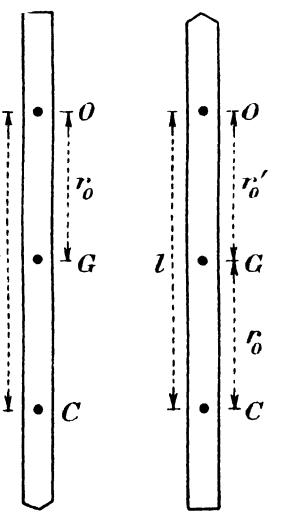


FIG. 38

especially since he lacked a general method of summation, but it does not appear possible to describe his procedure in greater detail and the reader who is interested is referred to the German translation in Ostwald's *Klassiker der Exakten Wissenschaften*, No. 192, to which are appended extensive notes in more modern form. Huygens's method could not very well be applied to more than a few regular solids.

The centre of oscillation C is in all cases lower than the centre of gravity G of the suspended solid. If l is the distance of the former from the axis of oscillation O and r_0 the distance of the latter, Huygens showed how $(l - r_0)$ could be calculated in certain cases. He was impressed by the discovery (1664) that the product $(l - r_0)r_0$ is constant and he later termed this the "rectangulum distantiarum". For parallel axes of oscillation the equation:

$$(l - r_0)r_0 = (l' - r_0')r_0' \quad \text{.....(i).}$$

could be applied, the second set of symbols referring to the second position of the pendulum. This enabled him to calculate exactly the effect of lengthening the suspension. If on the second occasion the axis of oscillation is made to pass through the point which was formerly the centre of oscillation it follows that the new centre of oscillation will be at the position of the former axis, for, from Fig. 38:

$$r_0' = l - r_0 \quad \text{.....(ii).}$$

and since $l - r_0 = r_0'$

$$l' - r_0' = r_0$$

$$l' - r_0' = r_0$$

and from (ii) $l' = r_0 + l - r_0$

$$= l.$$

This means that the compound pendulum has the same period in these two positions. The idea of the reversible pendulum was of course applied very successfully by Kater. The Kater pendulum is in fact the best laboratory method for the determination of the acceleration of gravity, since the distance between the two knife edges on which its period is the same can be accurately measured.

No theorem requisite for the complete theory of the pendulum clock was omitted by Huygens. The initial difficulties being cleared away he completed his treatise with a consider-

ation of the effect of moving a small weight or rider along the rod of the clock pendulum. The latter consists of a rigid rod carrying a heavy sphere at its lower end. To find the centre of oscillation of such a pendulum it was necessary to combine the values of $\frac{\Sigma mr^2}{\Sigma mr}$ for the rod (of length L) and the sphere found separately. Taking the respective masses to be m_1 and m_2 he showed that $\Sigma mr^2 = \frac{1}{3}m_1L^2$ for the rod and $\Sigma mr^2 = m_2L^2$ for the sphere, L being measured from the centre of the sphere. The corresponding values for Σmr were $\frac{1}{2}m_1L$ and m_2L. Hence for the combination

$$\frac{\Sigma mr^2}{\Sigma mr} = \frac{\frac{1}{3}m_1L^2 + m_2L^2}{\frac{1}{2}m_1L + m_2L}.$$

A simplification is introduced into this work in that the radius of gyration of the suspended sphere is not L but $(2/5R^2 + L^2)^{\frac{1}{2}}$ where R is the radius of the sphere. Huygens proceeded to calculate the centre of oscillation for the same pendulum carrying a small spherical rider at a given position on the rod. He showed that for a given alteration in the period there were two positions in general for the rider.

At the end of the work, Huygens proposed a unit of length based on the pendulum. The standard foot, or *pes horarium*, was to be one-third of the length of a simple pendulum which beat seconds at Paris. The size of the bob would, of course, be immaterial since for a sufficiently large bob, which could not be regarded as a simple pendulum, the centre of oscillation could now be found. The objections to this unit were the variation of g, the acceleration of gravity, with latitude and the inaccuracies which must arise in measuring the length of the thread. It was, however, an ingenious suggestion designed to overcome the objections which apply to the dependence on a bar of standard length.

In spite of its forbidding appearance in the original, Huygens's work in this part of the *Horologium Oscillatorium* contains some strikingly original ideas. The idea of work or energy, implicit in some of his work on impact, turns up again here in a more precise form. The idea of *vis viva* indeed comes from the work on impact while that of *work done* is involved in the direct method of finding the centre of oscillation. Someone

indeed was bound to derive the last idea from Galileo's equations of motion which, multiplied by m give (in modern symbols):

$$mv = mgt = Pt \quad \text{.....(i).}$$

$$ms = \tfrac{1}{2}mgt^2 = \tfrac{1}{2}Pt^2 \quad \text{.....(ii).}$$

$$mgs = \tfrac{1}{2}mv^2 \quad \text{.....(iii).}$$

If Huygens's work had been more promptly published, and still more, if he had not cast all he wrote in geometrical form, the third equation might sooner have entered into mechanics. In the work on moments of inertia, as in the contribution to the subject of energy, Huygens has not been duly recognized. Like Newton's *Principia*, the *Horologium* later came to be regarded as pretty hard going for readers who were accustomed to analytical methods.

IX

The Cause of Gravity

The idea of a force which acts towards the centre of the Earth dates from the earliest times[1]. Aristotle's doctrine that the elements, excepting fire, have a tendency to take up their "natural" places was a modified form of the idea and it was reproduced by the scholastic commentators. A clearer statement was given by Copernicus, who wrote "The Earth is spherical, for all its parts strain towards its centre of gravity." Much later Mersenne defined the "centre of the universe" as the point towards which all heavy bodies tend in straight lines. Gilbert (1600) attributed the action of gravity to other bodies besides the Earth, but he did not regard it as universal. The reason for motion under gravity was for him "a substantial form, special and particular, belonging to the primary bodies," which sufficiently indicates the influence of Aristotle. Kepler first considered gravity to be "a mutual attraction between parent bodies which tend to unite and join together". It is, he said, the attraction due to the earth rather than a *tendency* in the stone which causes the latter to fall.

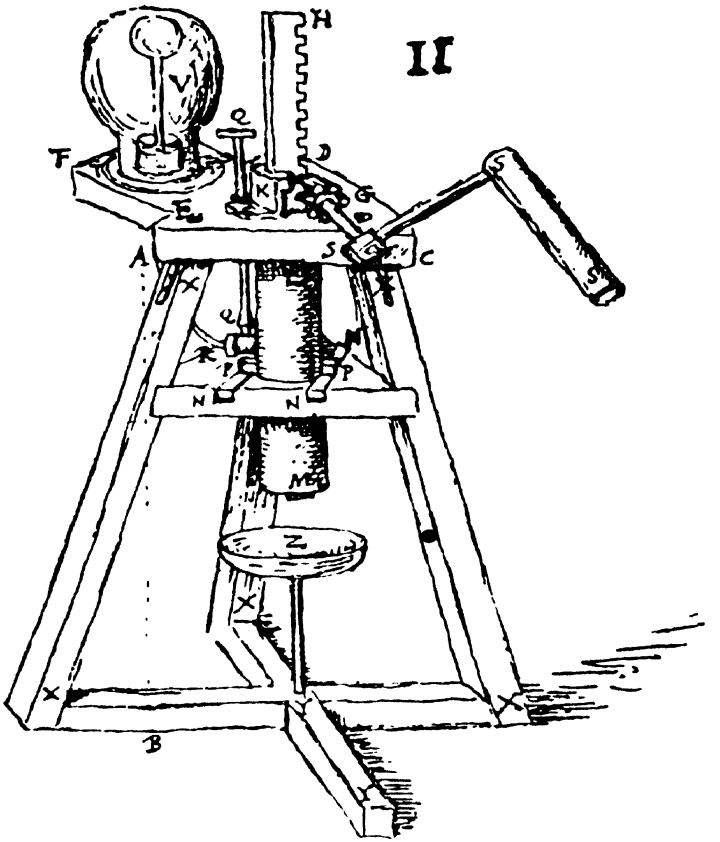
Up to Huygens's formulation of centrifugal force it was a real problem for the early seventeenth century scientists why the earth and moon were not attracted into contact with each other. Borelli, in *Theoriæ Mediceorum planetarum ex causis physicis deductæ* (1665) supposed that there was a tendency for the two bodies to come together, but that this was prevented by some kind of fluid pressure. Following Kepler, Borelli believed that the sun emanated some sort of "virtue" which kept the planets moving in their orbits. It is a most curious fact that although his theorems on centrifugal force were discovered about 1659, very probably Huygens did not appreciate the importance of the work in relation to the idea of a physical basis for the solar system. The possibilities of uniting the idea of gravitational attraction with that of centrifugal force were realized in England by Halley and, of course, by Newton. But

[1] The history of the subject has been very well outlined by Duhem: *La Théorie Physique*, p. 370–414 (1906).

Huygens was debarred from his due place in this most important development by certain unfortunate preconceptions. There is otherwise no obvious reason why he should not have forestalled Newton by many years in regard to this part of the *Principia*. The influence of Descartes here led Huygens to adhere to the hypothesis that all change is brought about through physical contact between bodies, either directly or through the medium of some subtle matter which filled the intervening space. Following Descartes he took an approach which bade to reduce phenomena to kinetics. Besides this, however, he realized the importance of structure or the "conformation" of physical bodies as an explanatory principle. But change effected across empty space seemed to him to be remote from experience and to leave a gap in the cause-effect sequence. Using the idea of force as the scientific mode of cause, Huygens considered it necessary to restrict the term to the operations of bodies on each other—perhaps under the influence of Descartes's dictum that what is true may be clearly conceived. It was this limited view of what can constitute a causal mechanism that led Huygens to postulate his various "media". For the transmission of light he required an ether, for magnetic fields, a magnetic medium, and for gravitational effects a "subtle matter". In the *Traité de la Lumière* the situation is by no means simplified. There is a possibility, he there remarks, "that the particles of the ether, notwithstanding their smallness, are in turn composed of other parts, and that their springiness consists in the very rapid movement of a subtle matter which penetrates them from every side, and constrains their structure to assume such a disposition as to give to this fluid matter the most overt and easy passage possible". Gassendi's atomic doctrines clearly influenced Huygens. As against Boyle, Huygens did not consider that the motion of the atoms of ordinary matter was sufficient to explain elasticity and thermal expansion. Such effects, he wrote, "cannot be explained without supposing the same subtle matter in motion with an extreme speed".

The most interesting remark on the subject of gravitational force to be found in Huygens's writings has been quoted on page 118. In addition there are two notes in his MS. of 1668 and 1669, which are as follows: "Gravitatem sequi quantitatem materiæ cohærentes in quolibet corpore" and "le poids de chaque corps suit précisement la quantité de la matière qui entre dans sa com-

PLATE VI



Drawing by Huygens of his Vacuum Pump of 1668

position". Huygens, then, made the distinction between mass and weight before Newton, but these statements do not imply that Huygens regarded gravity as something inherent in matter.

After 1661 Huygens was occupied with experiments with a simple air pump. His interest in this instrument was aroused during his visit to London. In 1668, an improved pump was constructed. This machine had a brass piston bound with fine flax in place of a wooden piston impregnated with wax. The pump (see Plate VI) was later described by Denis Papin in *Nouvelles Experiences du Vide* (1674). Actually Huygens and Papin in their experiments of 1674–7 added little to the work of Boyle; Huygens was not sufficient of a chemist to pursue the more interesting problems. The observation which most attracted interest was his discovery of the non-descent of columns of water from inverted tubes placed in the evacuated space of the vacuum pump receiver. Much correspondence arose over this anomalous observation which Huygens himself explained by assuming the existence of a subtle mattér which exerted a pressure even after the pressure due to the air had been almost entirely removed. The effect was only noticed with water (and later with mercury) which had been "purged of air". As has been noted in the earlier part of this book, Wallis doubted if Huygens's subtle matter really explained the phenomenon: "For, if this Matter be so subtile as to pass, through the top of the Glass, upon the Quicksilver . . . I do not see, why it should not balance itself (above and below) in the same manner as Common Air would do, if the Tube were pervious to it at both ends, and the Quicksilver, by the preponderance of its own weight, fall presently." Hooke, although a Cartesian, came nearer to the truth. In his *Micrographia* he mentioned the cohesion between liquids and glass. Mercury and glass were too different in their natures to cohere but water, "being somewhat similar to both, is, as it were, a medium to unite both the glass and the mercury together". In the absence of water, however, Hooke felt obliged to accept Huygens's explanation. Even Newton allowed that Huygens's explanation was the probable one. Newton also suggested that the rise of liquids in capillary tubes might be due to the ethereal medium but his own adherence to the hypothesis of an ether was, as is well known, somewhat inconsistent.

His persuasion that a subtle matter exists was a strong argument for the retention of Descartes's vortices by Huygens. In

1667 he attempted to work out a satisfactory explanation of gravity as an effect of circular motion. His hopes that this would prove possible no doubt rose as the existence of a subtle matter became more and more accepted by men of science. By 1669 he felt himself to be in a position to put his view before the Académie Royale, the occasion being a discussion on the subject of gravity in which the other speakers were Roberval and Mariotte. In fact, no other theory was put forward and the discussion became a criticism of Huygens's theory.

Huygens proposed to limit himself to terrestrial gravity and for this purpose considered the Earth to be an isolated system. The Cartesian vortex moved, according to his view, around the Earth in such a way that the subtle matter everywhere moved parallel to great circles on the Earth's surface. So far as one can judge there was no question of gravity extending to the moon. To illustrate his argument Huygens described an experiment in which a bowl of water is rotated about its axis. Heavy particles introduced into the rotating liquid were found to be propelled towards the centre as the rotation slowed down. Huygens proposed that the subtle matter which played such a part in his vacuum pump experiments was in fact the matter of the vortex about the earth. If the speed of rotation were high enough this would account for gravity as a centripetal reaction. But it was necessary to suppose that the circular motion of the vortex was "natural" and not constrained. The subtle matter, Huygens calculated, would have to be in rotation with a speed about seventeen times as great as that of the diurnal rotation of the earth.

To all this Roberval and Mariotte made the more obvious objections, for it is difficult to conceive how an ether which is permeable to matter can exert a pressure on matter. Going further, they questioned the validity of restricting all explanations to the terms of matter and motion. They questioned the evidence for supposing that circular motion was in this instance "natural". Roberval preferred the view that gravity is a mutual attraction between the particles of bodies. He had, he said, maintained this view as early as 1636. Both he and Mariotte considered that Huygens had only replaced one mystery by another.

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X

Huygens's Optical Studies

The study of the propagation of light and its behaviour at reflecting and refracting surfaces has a long history. This is, indeed, the oldest branch of physics and it is necessary to recall that for centuries the subject possessed for many minds a certain mystery which was dispelled only as the phenomena came to be seen as illustrations of general laws. Descartes put the subject of light in a central position in his natural philosophy; one of his works was entitled *Le Monde ou Traité de la Lumière.*

Yet Descartes achieved comparatively little in his study of light and it was Kepler's *Dioptrice* of 1611 which, more than any other single work, laid the foundations of modern optics. Euclid's *Optics* (*c.* 300 B.C.) contained a statement of the equality of the angles of incidence and reflection for a plane surface and Claudius Ptolemy (*c.* A.D. 150) introduced the study of refraction. Kepler spent much time on but failed to discover the relation between the angles of incidence and refraction. His most important relation was in fact (in modern symbols)

$$\frac{i}{D} = \frac{i}{i-r} = \text{constant } (\mu).$$

Here i is the angle of incidence and r the angle of refraction and D the angle of deviation $(i - r)$. That is, the deviation varies as the angle of incidence—a relation which is nearly true for angles of incidence less than 30°. Kepler showed that the constant for ordinary glass was about $\frac{1}{3}$. He then calculated the principal foci in the cases illustrated:

(i) Parallel rays incident on a convex glass surface:

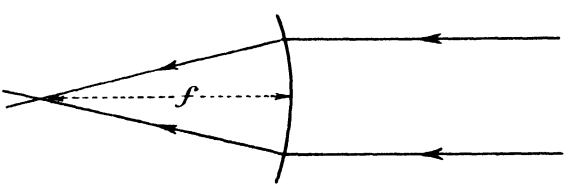


FIG. 39

He obtained the result $f = 3r$ where f is the distance of the principal focus F and r is the radius of curvature:

$$f = \frac{\mu r}{\mu - 1} = \frac{1.5r}{0.5} = 3r.$$

(ii) Parallel rays incident on the inside surface of a glass block having a convex surface:

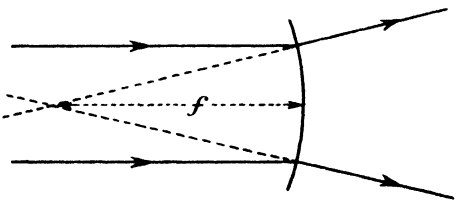


FIG. 40

He obtained $f = 2r$ a result which follows simply from the modern relation:

$$\frac{1}{f} = \frac{1 - \mu}{r} = \frac{0.5}{r}.$$

(iii) Parallel rays incident on a convex lens:

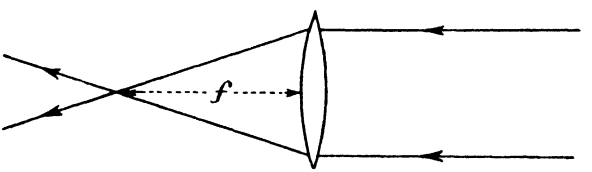


FIG. 41

He obtained the result $f = r$ for the case of a lens having two surfaces of equal radii of curvature (r). This is correct, for:

$$\frac{1}{f} = (\mu - 1)\left(\frac{1}{r_1} - \frac{1}{r_2}\right)$$

$$= (\mu - 1)\frac{(r_2 - r_1)}{r_1 r_2},$$

Putting $r_1 = -r_2$, $f = \frac{1}{0.5} \cdot \frac{r^2}{2r} = r.$

There were, however, no general equations for the treatment of lenses up to the time when Huygens began his work. Cavalieri, following the lines of Kepler's work, in 1647 proved the relation used above for the focal length of a thin lens and Isaac

Barrow in 1674 found by a geometrical method the image formed by a thick lens upon which an axial pencil falls. "Such cumbrous geometrical investigations involving the separate consideration of numerous particular cases," writes Professor Wolf (*A History of Science, Technology and Philosophy*, I, p. 248), "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."

Descartes did more than Kepler to treat the problem of spherical aberration but in this and other respects he arrived at no useful result of practical importance. His recommendations concerning elliptical and hyperbolic surfaces were chimeras and much needless labour was lost in attempting to put his ideas into practice.

Unfortunately Huygens's work in optics belongs to the earlier period referred to by Professor Wolf and his writings are very tedious to read because of the absence of algebraical formulæ. Throughout his life he was constantly amplifying and re-writing his manuscript and only the *Traité de la Lumière*, considered in a later chapter, appeared during his lifetime. Of the rest of his extensive researches some were made public in lectures to the Académie Royale but the remainder was hidden until published in 1703—by which time it had really ceased to possess more than historical importance.

Refractive indices are not widely quoted by Huygens in his *Dioptrica*. He seems to have supposed, from the small number of materials then available, that exact values were unimportant. His method, in the case of glass, was to determine the focal length of a plano-convex lens and to apply the formula

$$\mu = \frac{r+f}{f}.$$

The method of finding μ from the true depth and apparent depth of an object seen through a rectangular block was also

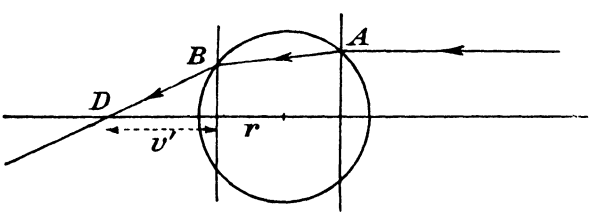


FIG. 42

known to him and was used in his researches on Iceland spar. For liquids his method was to fill a large glass cylinder and find the distance of the focal line when the incident light, perpendicular to the axis, was parallel.

For a cylindrical lens we have:

$$f = -\frac{r\mu}{2(\mu - 1)}.$$

Putting $f = v' - r$ (see figure above):

$$v = \frac{r(\mu - 2)}{2(\mu - 1)}$$

i.e. $2\mu v' - 2v' = \mu r - 2r$

or $2\mu v' - \mu r = 2(v' - r)$,

whence $$\mu = \frac{v' - r}{v - \frac{r}{2}}$$

as given by Huygens. Huygens, however, arrived at this result by purely geometrical methods.

Huygens's method of treating refraction may be illustrated by the problem of finding the principal focus of a convex

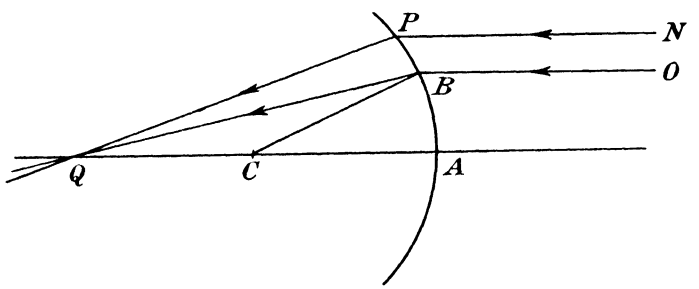


FIG. 43

spherical surface. Huygens showed that if C is the centre of curvature and NP, OB are parallel with the axis AQ, taking $\frac{AQ}{QC} = \mu$ then Q is the point through which the rays will pass. This, of course, is correct, for, using the formula:

$$\frac{\mu}{v} - \frac{1}{u} = \frac{\mu - 1}{r}$$

and putting $u = \infty$, $v = f$,

$$\frac{\mu}{f} = \frac{\mu - 1}{r}, \quad \text{whence } \mu = \frac{-f}{r - f}$$

Needless to say, no formula of the type quoted is used in the work. But the correspondence of Huygens's geometry with modern practice can be perceived if his diagrams are carefully examined. His method of locating the image of a point source produced by a lens is to employ the relation

$$\frac{DO}{DC} = \frac{DC}{DP}$$

or

$$DO.DP = DC^2$$

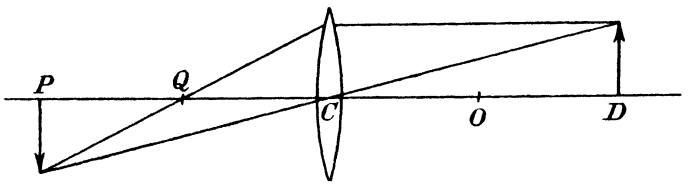


FIG. 44

Now, $DO = u - f$, $DC = u$, $DP = u + v$ and hence $(u - f)(u + v) = u^2$ i.e., $uf + uf = uv$,

or

$$\frac{1}{v} + \frac{1}{u} = \frac{1}{f},$$

which at once appears familiar. The original proof is, however, too long to quote.

Huygens pointed out that there is an optical centre in a lens such that rays passing through emerge after traversing the lens parallel with their original directions. In Fig. 45, E and F are the

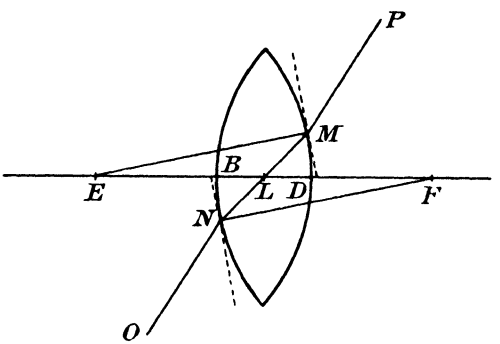


FIG. 45

centres of curvature of the lens faces and ED, FB are radii. The point L can be found since it may be shown that

$$\frac{BL}{LD} = \frac{FB}{ED}$$

Some consideration was given to the effect of a lens immersed in a liquid such as water. Huygens showed how to calculate the refractive index for the two media in contact, knowing the indices with respect to air. He also gave a useful account of the human eye. He distinguished the liquids known as the vitreous and aqueous humours but made the mistake of supposing that the content of the crystalline lens was also a liquid. In considering the location of an image at the least distance of distinct vision, a fundamental idea in the theory of optical instruments, his work was rendered unnecessarily complicated by an unfortunate choice of distances. Huygens measured the distance of the image from the eye and not from the lens through which it was viewed. However, he was led to discover an interesting theorem concerned with the magnification produced by a system of lenses. This, quite briefly, stated that by interchanging the positions of the eye and the object, without altering the positions of the lenses, the object appears to the eye to be of the same size as before. This conclusion is of purely theoretical interest but there is a sequel: Lagrange later obtained equations which accord with this peculiar theorem and this in its turn led to work by Hamilton, Clausius and Kirchhoff. There seems to be a connection between Huygens's work and later developments of the conception of optical distance.

Huygens's more important researches on lenses dealt with the subject of spherical aberration. It was known from the time of Kepler that the middle of a lens having spherical surfaces had not exactly the same focal length as the peripheral part of the lens. To secure better definition it was customary to employ a stop covering all but the middle of the lens. The aperture used was judged by experience. Huygens saw that it should be possible to calculate the aperture permissible for any given lens. It should also be possible to decide the optimum form for a lens of given focal length. So early as 1653 Huygens compared the distortions produced by a plano-convex lens first with the convex and then with the plane side towards the light. He also introduced the idea of optical thickness as measured not by the actual thickness at the middle but by the difference in thickness at the middle and at the edge. For a plano-convex lens the focal length of the peripheral part can be calculated in terms of the radius of curvature and the distance of the incident ray from the axis. For rays close to the axis the focal length is given by

the ordinary formula. Huygens performed the calculation for the two positions of the lens and showed that the separation of the foci was less when the light was incident on the curved face. When the curved face is turned towards the light the rays suffer approximately equal deviations at the two refractions. Huygens saw that spherical aberration increases with the amount of deviation occurring. It thus appeared advantageous to use two lenses at the eye-piece of a telescope instead of only one and it could be secured that the total deviation was divided equally between these two lenses. Huygens showed that this was so when the separation of the lenses was equal to the difference of their focal lengths.

Huygens considered that the Dutch, or as it is sometimes called, the Galilean, telescope could be made more free from spherical aberration than the Keplerian telescope which employed a convex eye-piece. Seeing that the concave eye-piece of the former compensated in some degree for the aberration of the objective, a much greater aperture should be permissible and thus a greater magnification might be obtained without increase of length of the telescope. Huygens recognized, however, that the Dutch telescope suffered from having too narrow a field of view for astronomical purposes. Various means of remedying this were considered but they were abandoned because of the difficulty of grinding lenses to a specified form.

It was still essential, however, to define the practicable limits to the aperture for any given lens, for there continued to be a great deal of confusion on this point. Huygens considered definition to depend simply on the quantity of light per unit area falling on the retina of the eye. His procedure was to start with a telescope of known dimensions which gave good results and to calculate the lengths and apertures for others of the same standard of definition. For this purpose he limited himself to a consideration of the objective and ignored the eye-piece.

If f and f' are the focal lengths of two objectives and ϕ and ϕ' those of the corresponding eye-pieces and g and g' the linear magnifications, d and d' the diameters of the apertures, then, for equal intensity of light at the eye:

$$\frac{d}{d'} = \frac{g}{g'} = \frac{f}{\phi} \cdot \frac{\phi'}{f'}$$

$$\frac{\phi d}{f} = \frac{\phi' d'}{f'} \qquad \text{.....(i).}$$

To compare the aberrations we may suppose, with Huygens, that the lenses are of the "same sort", i.e. that

$$\frac{R_1}{R_2} = \frac{R_1'}{R_2'}$$

where R represents the radius of curvature of a lens surface. In Fig. 46 the distance FF_1 represents the separation of the foci. We may put $FF_1 = \delta$, $F'F_1' = \delta'$; then it may be shown that:

$$\frac{\delta}{\delta'} = \frac{d^2 f'}{d'^2 f} \quad \text{.....(ii).}$$

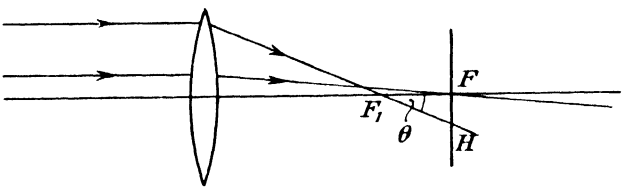


FIG. 46

From the figure it is clear that the rays through the peripheral part of the lens will meet the focal plane through F at points H on the circumference of a circle of radius FH. FH is given by

$$FH = \delta \tan \theta.$$

The focal plane of the objective is also that of the eye-piece of an astronomical telescope. The image of the circle will therefore be the circle of aberration for the telescope and its radius will be given by

$$K \frac{\delta \tan \theta}{\phi} \quad \text{where } K \text{ is a constant.}$$

Putting $\tan \theta = \frac{d}{2f}$ this becomes $K \frac{\delta d}{2f\phi}$. For both telescopes to produce equal circles of aberration

$$\frac{\delta d}{f} = \frac{\delta' d'}{f'} \quad \text{.....(iii).}$$

or by (ii)
$$\frac{d^3}{f^2 \phi} = \frac{d'^3}{f'^2 \phi'} \quad \text{.....(iv).}$$

and by (i)
$$\frac{f}{f'} = \frac{d^{4/3}}{d'^{4/3}}$$

and
$$\frac{\phi}{\phi'} = \frac{f^{\frac{1}{3}}}{f'^{\frac{1}{3}}}.$$

These equations, derived after the manner given in the introductory notes to volume 13 of the *Oeuvres Complètes*, summarize the rules elucidated by Huygens.

As has been mentioned, the existence of spherical aberration was not unknown to Huygens's contemporaries nor to some of his predecessors. Maurolycus (in 1553) even mentioned it. Newton, in treating the aberration of a plano-convex lens with the plane side towards the light found the value of the aberration, FF_1 in the form of a series in which the first term was $\frac{\mu^2 e}{\mu - 1}$ where e was the thickness of the lens and μ the refractive index. Picard also deserves mention for, in his *Fragments de Dioptrique*, he obtained a value of $\frac{7e}{6}$ for the spherical aberration of a glass plano-convex lens receiving light on the convex side. His method resembled that of Huygens. Molyneux gave a single numerical calculation to his *Dioptrica Nova* (1692); he concluded that the "depth of focus" is smallest in the case of a plano-convex lens

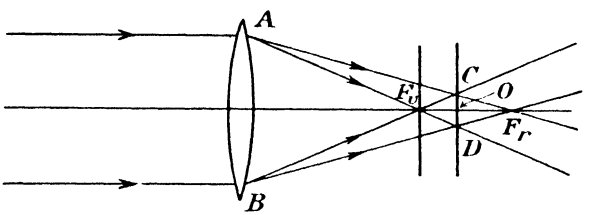


FIG. 47

when the rays are received on the convex surface. Huygens spent much time on the relation between spherical aberration and the inclination of the lens surfaces at the periphery. He saw that for a thin lens the deviation produced at a given point may be regarded as constant. Later he recognized that chromatic aberration was the quantity which was most affected by variation in the angle of inclination.

When he turned to the study of chromatic aberration, Huygens made use of the work of Newton. There appears to be no evidence that Huygens measured the refractive index of glass for different colours. Now Newton estimated the circle of chromatic aberration (radius *CO* in Fig. 47) to be one-fiftieth of the diameter of the lens employed. The fact that Huygens obtained divergent results may have been due to the use of a very different glass or

to the choice of a different circle. Newton's circle was situated midway between F_v and F_r; the choice of the plane through F_v would give a circle of greater diameter.

To compare the relative magnitudes of chromatic and spherical aberrations Huygens took a plano-convex lens of twelve inches focal length and allowed an aperture of half an inch. The optical thickness was thus only 1/192 inch. With the curved surface towards the light the spherical aberration measured along the axis is, by Picard's rule, 1/164 inch. The chromatic aberration may be calculated from the relation $f_r - f_v = \omega f$ where ω is the dispersive power of the glass and f is, of course, the ordinary focal length (for the mean ray). Taking ω to have the value .017 this means that the distance $f_r - f_v$ is 1/5 inch. This is 33 times as great as the spherical aberration. In large telescopes the difference between the effects would be even greater. Following the method previously used, Huygens estimated the aperture of any given lens by comparison with an instrument which gave satisfactory results. He showed that the ratio of the apertures of the lenses used for the objective and eye-piece would be equal to that of the square roots of the focal lengths.

When Huygens used a telescope expressly designed for observation of Saturn to study the moon he found the brightness of the image too great. He accordingly restricted the aperture of the objective very much but was surprised when a point was reached when the clearness of the image suddenly diminished. He considered this was due to some property of the eye. " For also," he wrote, " when one places in front of the naked eye a plate having a hole of 1/5th or 1/6th of a line in diameter, the edges of objects begin to appear less clear, and the confusion becomes greater the more one diminishes the size of the aperture." Huygens does not seem to have identified the effect with that of the diffraction described by Grimaldi in his *Physico-mathesis de Lumine* (1665). It is also mentioned in Newton's *Principia*.

Of course, certain of the conditions affecting the construction of telescopes apply also to the construction of microscopes. Huygens was for a time a keen microscopist and he sided with Leeuwenhoek, Redi and Swammerdam in the view that the evidence was all against spontaneous generation even of protozoa. Huygens treated the optical system of the compound microscope in much the same manner as he treated that of the telescope. The

diagrams he gave to explain the principal features were of more or less modern form; they show the refraction of the rays so that a real image is formed by the objective. The image, as magnified by the eye-lens, is viewed at the least distance of distinct vision. He derived an expression for the magnification of the instrument and showed that if this is increased by decreasing the focal length of the objective there is an inevitable decrease in the depth of focus. As a practical microscopist he found that for certain observations better results were obtained when the object reflected light than when the light was transmitted. He was responsible for the invention of dark ground illumination.

At this point there is a temptation to introduce an account of Huygen's excursions into biology, if only to correct the impression that he was so exclusively a student of the physical sciences. His studies of infusoria were quite notable and in one or two points, original, and of course the fact is that these seventeenth century men of science took no trouble to observe any artificial boundaries to their "subjects". Huygens not only translated Leeuwenhoek's writings on microscopic observations, he was a fellow-observer who repeated and extended his compatriot's experiments. Like Leeuwenhoek, he was opposed to the theory of spontaneous generation and his own experiments on the subject are a prelude to those later carried out with such perfection by Pasteur. There is something immensely stimulating about this all-round activity in science, this feeling of the arresting interest of so many and such diverse problems. But, in reading Huygens's correspondence and in working at his notebooks, one has to clear away the notion of the "specialist" scientist. One sees that his interest in the microscope is as practical as his interest in the telescope was at all times; he wanted a science of optics in order to make his instruments, to perform calculations in astronomy, to understand the phenomena of "false suns" and haloes, to pursue the questioning of Nature out into the remotest spaces of the universe or down to the limits of the smallest living organisms.

*

XI

The Wave Theory of Light

The notion that light is in nature akin to sound is very ancient. Thinking along these lines, Roger Bacon stated that light travelled in successive stages through the air; his language hinted at some kind of vibratory motion. After that no progress beyond a general assumption of some analogy between light and sound was made up to the time of Francis Bacon, and he had little to suggest on this subject. Descartes first gave an interpretation of the facts, so far as they were known, which had some appeal for men of science in the seventeenth century. For him light was to be regarded as a pressure transmitted with infinite speed through the subtle matter which filled his universe. "Light in luminous bodies," he wrote, "is only a certain movement, or a very lively motion which passes towards our eyes . . . in the same way that the movement or resistance of bodies which a blind man meets passes to his hand by the medium of his stick." Again: ". . . it is not so much the movement as the inclination to move of luminous bodies that we must consider as their light . . . the rays of this light are nothing more than the lines along which this inclination tends."

Huygens was not influenced exclusively by Descartes, however, and the views of Gassendi certainly deserve to be mentioned. The important thing about Gassendi's natural philosophy in this connection was his admission of the vacuum as a primary conception. He supposed that atoms of light traversed the empty spaces between the celestial bodies and he left it open to question whether, by analogy with sound, these atoms were emitted periodically. Ideas obtained partly from Descartes, partly from Gassendi, seem to have been combined by Huygens but there was much that was new about his own theory. As has been mentioned, Hooke, in his *Micrographia* of 1665, spoke in general terms of waves of light propagated with finite speed. Grimaldi, in his *Physico-mathesis de Lumine* of the same year, pursued similar ideas; there is in this work a figure which may represent light as propagated by transverse vibrations. He spoke of the motion of a fluid medium as being spiral in form. Whether Huy-

gens read this work is uncertain but it was amongst his books at the date of his death.

Perhaps more importance should be attached to the work of the Jesuit I. G. Pardies (d. 1673), for this amateur student of science showed the greatest faith in the analogy between light and sound. It is known that he showed his completed work to Huygens. The latter never accepted so close an analogy but one point of resemblance has been remarked: Pardies supposed that the pulses by which light was propagated were irregular in period. The work *Optique,* published in 1682 by Ango, contains some of his ideas. The first to develop the idea of periodicity in the propagation of light was Newton (1672), and Malebranche was the first French writer to follow him (1699). Huygens seems to have been led to make an independent study of the problem as a result of the perplexing properties of Iceland spar; it is known that he conceived the idea of spheroidal waves within this crystal while at the Hague in 1677. There is little to be gained, however, by attempting to maintain that Huygens originated the wave theory *de novo.* He may well have obtained the initial idea from Pardies's manuscript. His great achievement lay in presenting the theory in a form in which it could be fruitfully applied through the development of a suitable geometry.

Huygens did not invoke a Cartesian medium for the propagation of light until after 1668, when it seemed to him that he had definite evidence from his vacuum pump experiments for the existence of such a medium. He then sought to combine this hypothesis with an atomic theory similar to that propounded by Boyle. He agreed with the latter that liquids and solids alike are composed of particles in proximity but he supposed that there were intervening spaces and that these were filled with the much smaller particles of a subtle matter. The elasticity of the air demonstrated by Boyle's experiments "seems to prove," he wrote, that it is made up of particles floating and " agitated very rapidly in the ethereal matter composed of much smaller parts ". Huygens's atomism is peculiar therefore in that he extended it to *subtle* as well as to *ponderable* forms of matter. That a considerable proportion even of solid bodies is occupied by the ethereal medium is clear, he considered, from the fact that so dense a substance as gold does not screen off the effect of a magnet or of gravitation from a body.

Huygens considered his kinetic theory of matter was only a

beginning. He was forced to introduce "soft" particles to damp out the motion of light in opaque bodies and his speculations led him in one place to speak of the particles of ether being composed of still smaller parts and penetrated by a second subtle matter. There is some inconsistency in his statements and we are left in some doubt whether he always intended a clear distinction between a luminiferous ether on the one hand and subtle media for the propagation of gravitational and other effects on the other. He did distinguish an *éther* and a *matière subtile* but the grounds of the distinction are not really clear. The nearest Huygens came to simplifying his ideas was to suggest that the media concerned in the propagation of light and in the anomalous vacuum pump experiments are the same. He explained that he could not identify the luminiferous medium with that which causes gravity since the latter was in his view found only near the earth. The weakness of his position here has already been discussed. As Clerk Maxwell remarked, "To fill all space with a new medium whenever any new phenomenon is to be explained is by no means philosophical."

The *Traité*[1] begins with an admission that some of the suggestions are only hypothetical: "whereas the Geometers prove their Propositions by fixed and incontestable Principles, here the Principles are verified by the conclusions to be drawn from them; the nature of these things not allowing of this being done otherwise. It is always possible to attain thereby to a degree of probability which very often is scarcely less than complete proof." It is in keeping with this view of scientific method to start with the hypothesis that light is a form of vibratory motion propagated in spherical waves or surfaces as in the propagation of sound. The chief differences between the two cases were the incomparably greater speed of light estimated by Roemer and the media through which the vibrations travel. It was also obvious that the particles of a luminous body vibrate independently of each other and that the frequency of vibration (if one can use such a term in relation to Huygens's ideas) is very much higher than in the case of sound. Huygens then proceeded to explain how light could be conceived as a succession of compressional or longitudinal vibrations passing through contiguous ether particles. Contrary to the corpuscular theory there was no movement of translation. This explained how two light rays

[1] *Traité de la Lumière.*

could travel in opposite directions in the same space, or cross at an angle, without hindering each other. It also led to Huygens's famous conception of secondary wavelets, for each particle in the path of a disturbance was a centre from which the disturbance spread outwards through all the particles in contact. Huygens saw that there was a difficulty here in that loss of impulses laterally must weaken a ray as it proceeds. The limits he set to the "wave front"—or common tangent—appear quite arbitrary. As is well known, Newton was not satisfied with this discrepancy; it was only much later that wave-spreading in the form of diffraction effects was fully demonstrated.

Huygens's principle is often considered to be open to the objection that a wave-front travelling backwards towards the source can be constructed in theory but is never observed in practice. The answer to this objection is that Huygens did not evolve his principle from geometry so much as from his study of elastic collision. If the particles of a medium are all equal, any impulse received at A will be transmitted through the train of particles until any given particle *C* is moved in the same direction as *D*. After collision with the next stationary particle *E* there will be a rebound only if *C* were smaller in mass than *E*. Huy-

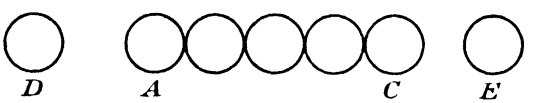


FIG. 48

gens supposed all the particles of the ether to be of the same size but allowed that the effect would exist if smaller particles were present. He doubted if such a back-wave would generate the sensation of light. There is, however, something very unsatisfactory in considering a point of a wave as the centre of another wave.

The construction for regular reflection at a plane surface is sufficiently well known to require only brief notice here. It is given in text books of optics. Instead of progressing towards *GMMMMB* the secondary wavelets can radiate outwards only above the reflecting surface *AB*. While a secondary wavelet from *C* is travelling to *B* a wavelet starting from *A* must have travelled through a distance *AN* equal to *CB*. The radii of intervening wavelets are determined in the same way and thus the common

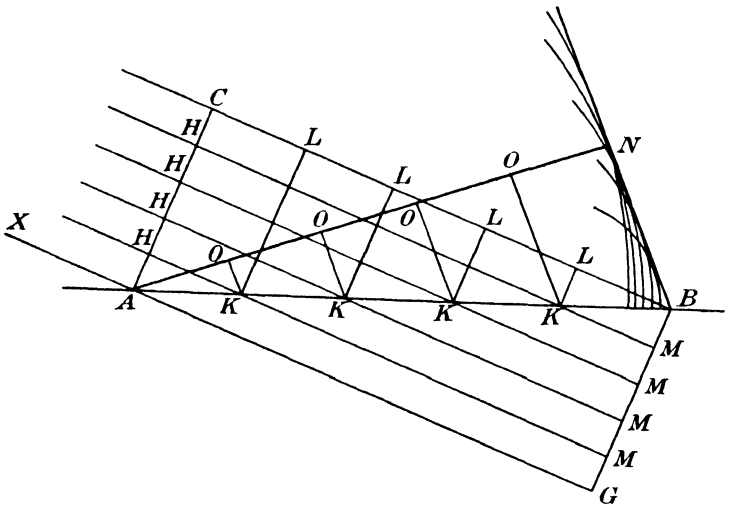


FIG. 49

tangents *BN* can be found. This "terminates the movement" and is the new wave-front. By geometry it may readily be shown that, as a consequence of this construction, the angles of incidence and reflection are equal.

The validity of Huygens's principle here depended, as its author saw, on the constancy of the speed of light during reflection. Without naming the source he referred to Hooke's law in answering this question: "This [constancy of speed] comes about from the property of bodies which act as springs, of which we have spoken above; namely that whether compressed little or much they recoil in equal times."

It was otherwise when the passage of light from one medium to another was considered. An important difference—and one which became decisive—between the wave theory and the corpuscular theory was Huygens's conclusion that light must travel more slowly in the denser medium. This was not necessarily because the medium was different: Huygens considered the ethereal medium penetrated all solids and liquids. The difference in speed resulted from the detours of the waves around the more solid particles of the elements. If we ask how Huygens knew that the ether penetrates substances his answer, given in the *Traité*, adduces the entry of ether into the Torricellean space and that hollow bodies possess an inertia which is in strict proportion to the mass. Like Newton, he could detect no friction of solids

with the ether. In some substances, however, Huygens supposed the material particles were not unaffected by the light vibrations. In this case they transmitted the vibrations also and the existence of this second mode explained double refraction.

In his treatment of ordinary refraction, Huygens supposed a wave-front AC (Fig. 50) to impinge on the surface of a second medium. The speed of light in this second medium (glass) was supposed to be two-thirds of its value in air. The radius of the wavelet from A is then two-thirds of the distance which the wavelet would travel in air in the same time interval. It follows that while a wavelet from C is travelling to B, a wavelet from A

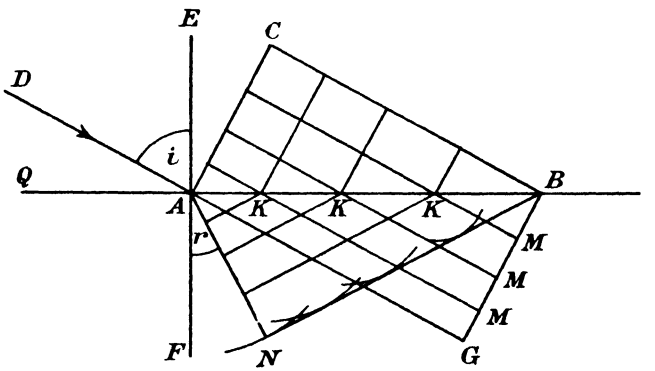


FIG. 50

will spread into the new medium through a distance AN equal to two-thirds of CB. It is clear in each case that the radius of the wavelet from K will be two-thirds of the distance KM which the wavelet would have travelled in air. The new wave-front is the common tangent NB. It follows by geometry that

$$\frac{\sin \angle DAE}{\sin \angle NAF} = \mu = \frac{\text{velocity in air}}{\text{velocity in glass}}$$

When light goes from glass into air the ratio of the speeds is inverted. In the second figure (Fig. 51), therefore, $AN = 3/2\ BC$ or $3/2\ AG$. Here we have

$$\frac{\sin \angle DAE}{\sin \angle NAF} = \frac{2}{3}$$

For larger angles of incidence ($\angle DAE$) it is clear that when $BC = 2/3\ AB$:

$$\sin \angle DAE = \frac{BC}{AB} = \frac{2}{3}$$

$$\sin \angle NAF = \frac{NA}{AB}$$

But $$\frac{\sin \angle DAE}{\sin \angle NAF} = \frac{2}{3}$$

$\therefore$ $\sin \angle NAF = 1$ and the angle of refraction becomes 90°. Beyond this limiting condition, as Huygens puts it, the wave-front BN "cannot be found anywhere, neither consequently can AN . . . thus the incident ray DA does not pierce the surface AB".

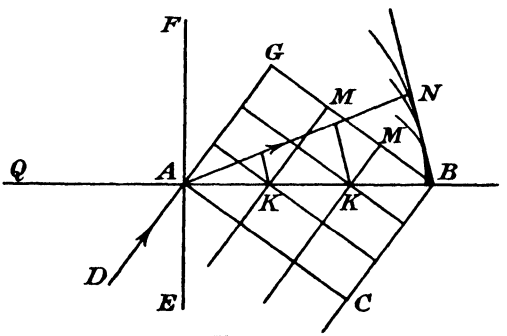


FIG. 51

It was impossible to give a satisfactory physical reason for the failure of the ray to penetrate the surface when the angle of incidence exceeded the critical value. The interior reflection which occurred took place, he supposed, "against the particles of the air or others mingled with the ethereal particles and larger than they". He could not explain how the reflection could take place when the air was replaced by a vacuum.

At the end of his chapter on refraction, Huygens showed that his principle of secondary wavelets was in conformity with Fermat's principle that the path actually taken by a ray of light in passing between two points is the path of least time. Fermat was in strong opposition to Descartes, whose false "demonstration" of Snell's law of refraction required that the velocity of light be greater in a dense medium than in air. In a long letter to de la Chambre, Fermat explained that it was necessary to make the opposite assumption. He showed how the sine law of refraction could be deduced from the least-time principle and a copy of the letter was sent to Huygens in 1662. Huygens

was at first scornful of Fermat's principle, which seemed to him to savour of Aristotelianism. This "pitiable axiom" was one which he had never seen usefully applied, he remarked. Nevertheless, he changed his mind over Fermat's principle and was delighted when he succeeded in deriving it by his own methods.

The relation between Fermat's principle and Huygens's construction in this case may be summed up by the statement that both give the same physical interpretation of the refractive index. The significance of this important theorem was not lost on Huygens, though it is to be doubted if he ever imagined its future development. There is, however, an interesting application of the least-time principle in the *Traité* to atmospheric refraction, a subject of obvious importance to astronomers. Huygens pointed out that spherical wavelets would only be set up in a medium which was homogeneous, or to use the modern term, isotropic. In an anisotropic medium, as will be seen, Huygens showed that the wavelets might be ellipsoidal in form. The problem of the atmosphere was somewhat different. There was here a gradual change of refractive index with density, and Huygens saw that the wavelets must have surfaces of equal time from the source. To include the effect of density in his wave theory it was necessary to assume either that gaseous particles acted as a hindrance to the vibrations or else that they transmitted light themselves but did this inefficiently.

Huygens himself foresaw that later workers would furnish what was needed to complete his "imperfect knowledge," that much yet remained to be done to make a satisfactory theory. The remainder of the *Traité* was taken up with a striking attempt to extend the wave theory to the phenomenon of double refraction. As is well known, Huygens elucidated the nature of the wave surface for the extraordinary ray as an ellipsoid of revolution and this great achievement still stands. What Huygens could not explain (since his "waves" were longitudinal ones) was the effect of superimposing two calcite crystals in different positions. He succeeded, that is, in working out a construction for the extraordinary ray on the basis of an ellipsoidal wave theory, but could not on these grounds account for what we now term the polarization of the transmitted light.

The properties of calcite are described in some of the larger works on optics, notably Mach's *The Principles of Physical Optics*. The essential property is that, in general, an incident

ray, on entering the crystal, is split up into two rays, one of which (the *ordinary* ray) is refracted according to the usual laws of refraction, while the other (*extraordinary* ray) is not. The substance was, Huygens admitted, anomalous and its behaviour "seemed to overturn our preceding explanation of regular refraction". From the first, however, Huygens, on account of his atomic theory, inclined to the view that the optical properties of crystalline solids were to be correlated with their fine structure. He therefore began his account in the *Traité* with a description of the geometry of the Iceland spar crystal.

The crystal (Fig. 52) has the form of an oblique parallelopiped and there are cleavage planes in three directions—parallel with the three pairs of parallel faces. The angles of the parallelogram sides were given by Huygens as 101° 52′ and 78° 8′. The crystal has two opposite corners which are formed by three obtuse angles while the others are formed each by two acute and one obtuse angles. If the obtuse angle *ACB* of the parallelogram face at the blunt corner *C* is bisected by the line *CE* and a plane is imagined to pass through *CE* perpendicular to the

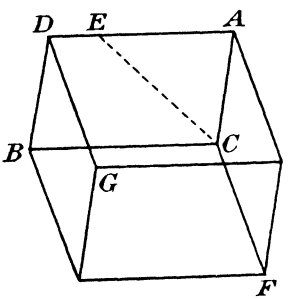


FIG. 52

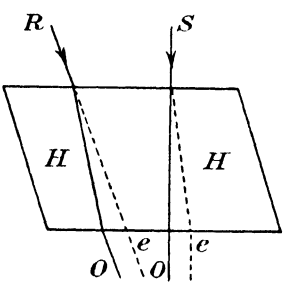


FIG. 53

parallelogram face, the plane also contains the edge *CF*. The plane thus determined, and any other plane parallel to it, was termed by Huygens a principal section.

As Bartholinus had shown, every incident ray, with certain exceptions, gave rise to two refracted rays, one of which was normal in its behaviour and exactly comparable with the refracted ray in any ordinary medium. The second ray showed, in general, marked abnormalities. So long as the plane of incidence coincides with a principal section *HH* (Fig. 53), *both* of the

refracted rays remain in this plane. For other planes of incidence the extraordinary ray is formed in a different plane. Moreover, while a ray *S* incident normally and an oblique ray *R* making a certain angle of incidence, both in the plane of a principal section, are refracted in the normal manner (rays marked *O*), the extraordinary ray formed exhibits a peculiarity. This is that a ray incident normally in the principal section gives an extraordinary ray *e* which is deviated by 6° 40′ towards the blunt corner *C;* on the other hand, a ray *R*, incident in the principal section at 73° 20′ (almost parallel with the edge *CF* in the first figure) gives an undeviated extraordinary ray *e*.

By a method which was in effect the measurement of the true and apparent depth of a small object seen through the crystal, Huygens found that the refractive index for ordinary rays in the principal section (or any other plane) was constant and approximately 5/3. Using a similar procedure to find the refractive index for extraordinary rays it was obvious that this was not a constant. The apparent depth of a point source of light varied with the orientation of the crystal. Nevertheless, Huygens discovered one important rule for the extraordinary refraction which may be explained as follows. In Fig. 54, the parallelogram *GCFH* is the principal section.

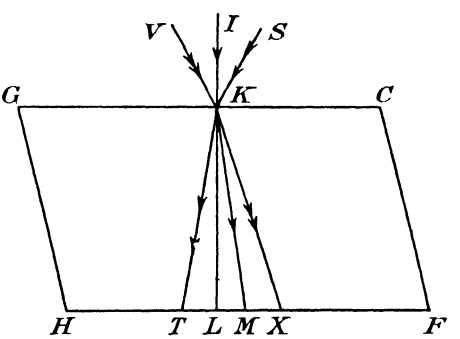


FIG. 54

IK is a ray normal to the surface and *KM* is the extraordinary ray. Huygens found that for rays *VK, SK,* making equal angles on either side of the normal *IK,* the extraordinary refracted rays *KX* and *KT* make *MX* and *MT* in *HF* equal. This may be called Huygens's rule for the extraordinary refraction in this plane.

These facts had, if possible, to be collated in a single theory of transmission. The ordinary refraction offered no difficulties. For this the theory of spherical wavelets spreading with a speed less than that in air was adequate. "As to the other emanation which should produce the irregular refraction," Huygens wrote, "I wished to try what elliptical waves, or rather spheroidal waves would do . . . ". These perhaps spread indifferently in both ethereal and material particles and in the regular arrangement of the particles might lie the source of the spheroidal wave form: "I scarcely doubted that there were in this crystal such an arrangement of equal and similar particles because of its figure and of its angles with their determinate and invariable measure".

Working on this assumption, Huygens's construction for the extraordinary ray from normal incident light was as follows. In Fig. 55, *RC* is a wave front and *AB* is the surface of the crystal. The plane is that of the principal section. Hemispheroidal waves originate at *AKkkB*. The axes or major diameters of these are oblique to the plane of *AB* as shown by *AV*: "I say axis or major diameter because the same ellipse *SVT* may be considered as the section of a spheroid of which the axis is *AZ* perpendicular to *AV*," wrote Huygens. For the present he considers only sections of the spheroid which are elliptical in the given plane of the figure. The common tangent to the semi-ellipses is *NQ*, and this is the propagation of *RC* as in Huygens's original theory. *NQ* is parallel with *AB*, but is displaced laterally as required by the refraction of the extraordinary ray.

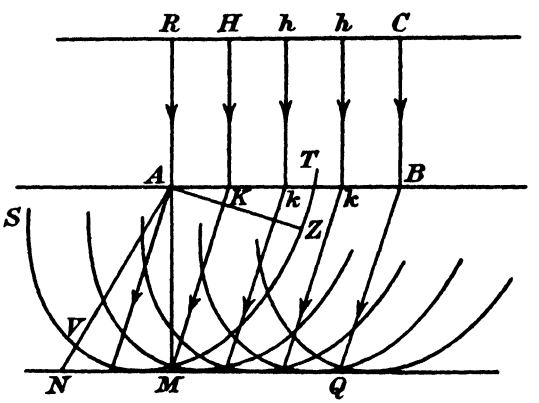


FIG. 55

It was next necessary to find the exact form of the ellipsoid of revolution and the orientation of axes in the crystal[1]. Fortunately all six faces of the parallelopiped produce the same refractions: the substance was uniaxial. Picturing the appearance of the blunt corner of a calcite rhomb (Fig. 56), and imagining the three principal sections respectively normal to each of the three faces, these intersect in a line, called by Huygens the axis of the corner, subtending equal angles with each of the three edges to the corner. If, now, the direction of the axis of the wave spheroid of rotation of which Huygens first thought did not coincide with that of the axis of the corner, each of the three principal sections would not be characterized by the same optical

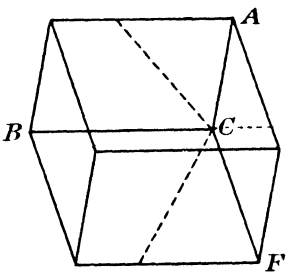


FIG. 56

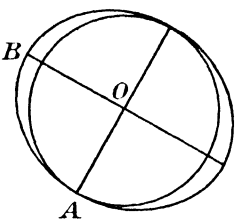


FIG. 57

properties. The inclination of the axis of the corner to each of the faces of the corner amounts to 45° 20′. The orientation of the spheroid being known, the fact that for normal incidence the extraordinary ray is deviated in the principal section from the ordinary ray by 6° 40′ towards the blunt corner is sufficient to establish the shape of the spheroid. By calculations based on his data, Huygens found the following to be in agreement with the facts. If OA (in Fig. 57) is the axial direction of the calcite and the ordinary wave spreading out from the point O of the crystal is represented by a sphere of radius OA, the surrounding oblate spheroid of rotation AB with axis of rotation OA represents the corresponding extraordinary wave emerging simultaneously from O. The ratio of OA to OB is as 8 to 9 (very nearly) while the ratio of OA to the corresponding path in air is as 3 to 5.

The construction for the refracted rays from oblique incident light may be explained as follows. The plane of incidence is con-

[1] I am indebted for this passage to the excellent summary of Huygens's *Traité* in Mach's *Principles of Physical Optics*.

sidered to lie in the plane of the paper; *MN* is the calcite-air surface (Fig. 58) and *SO* an incident ray.

Let *SO* be produced to *S'* and let a sphere of any convenient radius be described about *O* as centre. At its intersection with *SOS'* let a tangent plane be constructed intersecting *MN* in *Q*. If now a sphere of three-fifths the radius of the former is described

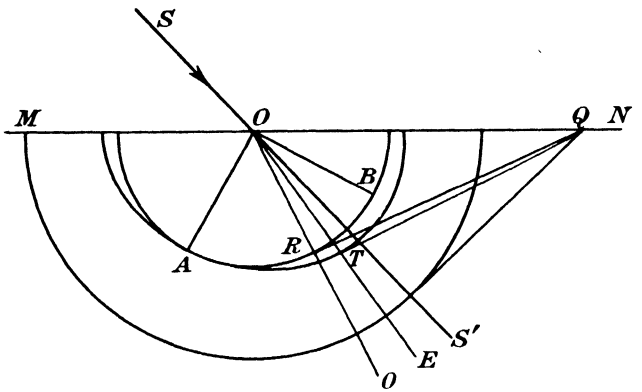


FIG. 58

about *O*, its point of contact *R* with the tangent plane through *Q* gives the ordinary ray *OR*. Let *OA* be the direction of the axis of the calcite rhomb. A spheroid is now described about the smaller sphere such that its axis of rotation *OA* (the minor axis), which equals *OR*, is 8/9 the length of the major axis. The point of contact *T* of the tangent plane through *Q* to the spheroid then gives the extraordinary ray *OT*. This construction is confined to one plane only when the axis is symmetrical with respect to the plane of incidence, that is, either coincident with it or perpendicular to it. For any other orientation the extraordinary ray is inclined to the plane of incidence. The results were studied experimentally by Huygens, who cut the crystal so that the optic axis was normal to the surface, parallel with the surface and in the plane of incidence, among other forms. He also found experimentally that for rays in the principal section the extraordinary ray was not refracted when the angle of incidence was 16° 40′. What he had to do here was to show that for this angle the rays continue without refraction since they are directed along the major axis of the ellipsoid. The fact that there is no bending in spite of the change in velocity in the new medium is of course explained by the fact that the new wave

front is not normal to the direction of the rays as is the case for spherical wavelets.

On the basis of his theory, Huygens clearly expected that it would in general be possible to split up rays which emerged from one crystal of calcite by passing them through a second crystal. Excluding certain special positions of the crystals which he himself understood, it would be expected that the ordinary and extraordinary rays would be split up again on entering a second crystal. This was found to be by no means the case. Huygens was considerably perturbed by the discovery and laboured hard to explain it, but in vain. He was, as Mach remarks, on the threshold of a great discovery—the transverse nature of light waves—but his conceptions hindered his taking this step forward. "Before finishing the treatise on this crystal," he wrote, "I will add one more marvellous phenomenon which I discovered after having written all the foregoing. For though I have not been able till now to find its cause, I do not for that reason wish to desist from describing it, in order to give opportunity to others to investigate it. It seems that it will be necessary to make still further suppositions besides those which I have made; but these will for all that not cease to keep their probability after

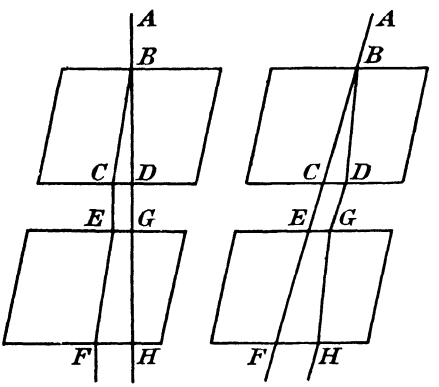


FIG. 59

having been confirmed by so many tests." In this Huygens was correct, his geometrical analysis of the ellipsoidal wave still stands. He went on (Fig. 59): "The phenomenon is, that taking two pieces of this crystal and applying them one over the other, or rather holding them with a space between the two, if all the sides of one are parallel to those of the other, then a ray of light,

such as *AB*, is divided into two in the first piece, namely, *BD* and *BC*, following the two refractions, regular and irregular. On penetrating thence into the other piece each ray will pass there without further dividing itself in two; but that one which underwent the regular refraction, as here *DG*, will undergo again only a regular refraction at *GH;* and the other, *CE*, an irregular refraction at *EF*. And the same thing occurs not only in this disposition, but also in all those cases in which the principal section of each of the pieces is situated in one and the same plane, without it being needful for the two neighbouring surfaces to be parallel."

In these words Huygens described his discovery of the polarization of light. He went on: "Now it is marvellous why the rays *CE* and *DG*, incident from the air on the lower crystal, do not divide themselves the same as the first ray *AB*. One would say that it must be that the ray *DG* in passing through the upper piece has lost something which is necessary to move the matter which serves for irregular refraction." " . . . It seems that one is obliged to conclude that the waves of light, after having passed through the first crystal, acquired a certain form or disposition in virtue of which, when meeting the texture of the second crystal, in certain positions, they can move the two different kinds of matter which serve for the two species of refraction; and when meeting the second crystal in another position are able to move only one of these kinds of matter. But to tell how this occurs, I have hitherto found nothing which satisfies me." The twenty-sixth query at the end of Newton's *Optics* referred to this problem. Has not a ray of light two sides, Newton asked, and his question became one of extreme significance after Young made the suggestion that the wave motion of light is not longitudinal but transverse.

Huygens did not in the *Traité de la Lumière* attempt a detailed physical explanation of the production of a spheroidal wave but he communicated on this subject with Papin. In a letter written in December 1690, he wrote: "As to the kinds of matter contained in Iceland crystal, I suppose one composed of small spheroids, and another which occupies the interstices around these spheroids, and which serves to bind them together. Besides these, there is the matter of ether permeating all the crystal, both between and within the parcels of the two kinds of matter just mentioned: for I suppose both the little spheroids,

and the matter which occupies the intervals around them, to be composed of small fixed particles, amongst which are diffused in perpetual motion the still finer particles of ether. There is now no reason why the ordinary ray in the crystal should not be due to waves propagated in this ethereal matter. To account for the extraordinary refraction, I conceive another kind of waves which have for vehicle both the ethereal matter and the two other kinds of matter constituting the crystal. Of these latter, I suppose that the matter of the small spheroids transmits the waves a little more quickly than the ethereal matter, while that around the spheroids transmits these waves a little more slowly than the same ethereal matter . . . These same waves, when they travel in the direction of the breadth of the spheroids, meet with more of the matter of the spheroids, or at least pass with less obstruction, and so are propagated a little more quickly in this sense than in the other; thus the light-disturbance is propagated as a spheroidal sheet." Papin and Huygens did not, however, see eye to eye over atomic doctrines and Huygens was critical of Papin's more or less orthodox Cartesian ideas.

Professor Sylvanus Thompson, in his "Note by the Translator" which prefaces the English edition of Huygens's *Traité*, remarked: "If Huygens had no conception of transverse vibrations, of the principle of interference, or of the existence of the ordered sequence of waves in trains, he nevertheless attained to a remarkably clear understanding of the principles of wave-propagation; and his exposition of the subject marks an epoch in the treatment of optical problems." There seems to be fairly general agreement that Huygens's theory ought more properly to be called a pulse theory. The concept of periodicity was nowhere introduced. Some evidence, though not of very substantial character, can be found in Huygens's note-books to show that he did speculate that waves (*ondes*) exist. He did not show much interest in the problem of accounting for colour, and although he made some favourable comments on Hooke's explanation of colour, he himself felt the matter lay outside mathematical treatment. Newton first introduced the idea that colour is related with frequency, but he received scant encouragement from Huygens. It is curious that Newton, the founder of the corpuscular theory, at least in his later speculations, came nearer to a wave theory of the modern type than Huygens did. He never threw over the corpusular theory, however, on the grounds

that first, a wave motion should show some lateral spreading (not yet identified with Grimaldi's discovery of diffraction) and second, that no waves as then conceived could produce the effects discovered by Huygens when light passed successively through two crystals of Iceland spar. As Whittaker remarked, "his objections are perfectly valid against the wave theory as it was understood by his contemporaries, though not against the theory which was put forward a century later by Young and Fresnel". The idea of transverse vibrations was, in Whittaker's opinion, narrowly missed by John Bernoulli the younger, whose prize essay on the ether was written in 1736.

The very last part of Huygens's *Traité* is an echo of Descartes. Here Huygens wrote on "the Figures of the transparent Bodies which serve for Refraction and Reflection". Here Huygens treated problems of aplanatic surfaces by means of his wave construction and by employing the least-time principle. In this way he gave proofs for some of the propositions Descartes had merely stated and for which his method had not been recorded. Huygens acknowledged the importance of conic sections. He proved that concavo-convex lenses of the form described by Descartes would be aplanatic, and determined the conjugate foci. Following Descartes he proposed that the concave surface of such a lens should be spherical. The impression given by the work, however, is that its interest is from the start purely theoretical. Huygens certainly recognized that it was of little use to demonstrate that refracting surfaces of unlimited aperture could be aplanatic so long as chromatic aberration was without remedy. His constructions for reflection from curved surfaces were, however, of permanent value and his general method of finding the focus from a consideration of wave fronts has proved most useful.

There is no question that Huygens's principle of the propagation of light by secondary wavelets has been of great importance in optics. Many of the questions left unanswered by Huygens were settled by Fresnel. But the early history of Huygens's wave theory was disappointing. It is true that the *Traité de la Lumière* was well received. The wave theory was even expounded at the University of Wittenberg in 1693. Soon after Huygens's death, however, the theory seems to have fallen into general neglect. In Holland neither s'Gravesande nor Musschenbroek adopted it.

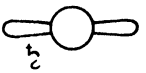
XII

Saturn

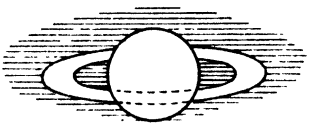
As has been described in the notes on Huygens's life, the planet Saturn presented a puzzling appearance when viewed through the first telescopes. Jupiter's satellites were a spectacle which could not but strengthen belief in the principles enunciated by Copernicus, but this was far from being the case with Saturn. Galileo ended his studies of Saturn with the words *altissimum planetam tergeminum observavi,* but concealed in the form of an anagram: "I have observed the most distant planet to be triform." A system of three bodies, two of them rotating about the middle one would, however, be a sufficiently curious spectacle to arouse further attention.

Some account of the contents of Huygens's *Systema* has been given in the early part of this book. The work of observing the planet, when the weather was favourable, occupied Huygens during the years 1655–9, and this involved a considerable amount of work in the construction of telescopes. The rings were fortunately becoming more inclined during this period, and in 1664, five years after his book had appeared, Huygens drew the rings at a great inclination. He was, of course, mistaken in supposing a single solid ring. The Cassini division is visible in a small telescope only when the rings appear fully open, a condition which was not fulfilled during Huygens's earlier studies. In 1675, however, he made a drawing which suggests that he abandoned the view that the ring is solid (page 194).

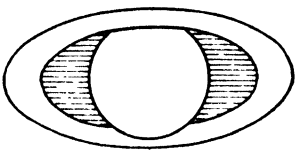
In regard to the sidereal period of the planet and the constant inclination of the rings to the ecliptic Huygens was more or less correct. He remarked in his preface on the equal inclination of the axes of Saturn and the Earth to the ecliptic, and he made it abundantly clear that the book was essentially a comment on Copernicanism. He accordingly did not consider it out of place to mention his search for moons of Mars, Venus and Mercury. In observing the four known moons of Jupiter, he remarked a variation in the appearance of the disc of the planet which he ascribed to the presence of vapours comparable with our clouds. Less related with his main subject, but worthy of record, was his



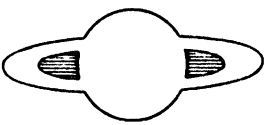
I. p. 322. One of the first drawings.



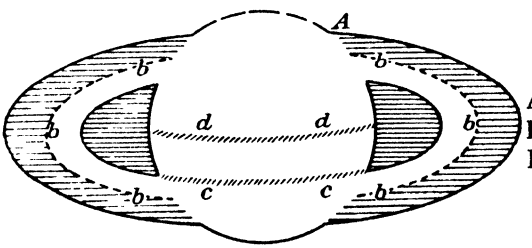
II. p. 224. Letter of Sept. 24, 1658.



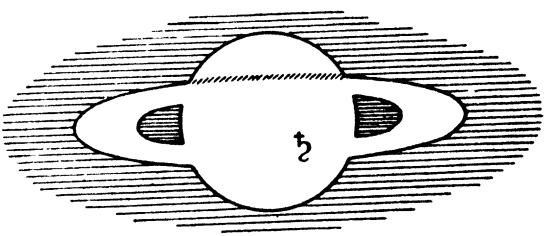
V. p. 109. In letter to Moray, Aug. 29, 1664.



VI. p. 443. Saturn observed May, 1669 (Huygens, Picard and Cassini).



A drawing from Huygens's Manuscript K. (1675).



Dec. 27, 1657.

Drawings of Saturn after those made by Huygens

The references are to the volumes of the *Oeuvres Complètes*.

description of a nebula in Orion. This nebula, discovered by Cysat in 1619, he considered to be essentially different from the Milky Way, in that it did not lose its nebulosity when viewed through a telescope. He suggested that the light came from a more distant region visible through a hole in the black sky. Much later (1733), Derham questioned if the nebulæ "in all Probability, be Chasms, or Openings into an immense Region of Light, beyond the Fix'd Stars."

In regard to Saturn, Huygens's method was to collect together all drawings of the planet then published and show that nearly all could be explained on the hypothesis of an imperfectly visible ring. From the outset, as he himself admitted, it had always seemed obvious that the planets were analogous with the Earth; consequently it is probable that all turn on their axes. A rather more dangerous proposition was that all the matter between Saturn and its moon (Titan) turns about the planet with periods increasing with the radius. But it seemed obvious that the disposition of rotating matter was at any rate symmetrical, for the appearance of the planet changed only slowly. Huygens found that the plane of the ring was inclined at about 20° to the ecliptic and that this angle remained constant as in the case of the inclination of the Earth's equator. He could not accept any suggestion that the ring was merely an exhalation and an evanescent phenomenon. The solid and permanent nature of the ring could, he said, be clearly perceived. The figure he gave in the *Systema* (Fig. 60) left no room for doubt as to his own view on this point. But such a ring was certain to appear

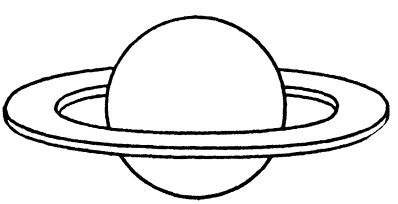


FIG. 60

eminently unstable unless one accepted its equilibrium under central gravitational forces. This Huygens proposed was the true explanation. He did not suggest that the gravitational force on the elements of the ring was balanced by the centrifugal force due to rotation and thus did not anticipate Newton's

theory. His conception was a static one and required that the ring be mechanically able to withstand the gravitational effect without fracture. Rotation then did not come into the question. But the idea that Saturn's gravity extended to the ring may well have been an important advance, and may even have started Newton on some interesting speculations.

Having disposed of effects due to aberration and obscurity of early telescopes, Huygens was faced with the necessity of showing how the hypothesis of a ring inclined to the ecliptic provided an explanation of all the various appearances of the planet. With the telescopes used at the time the ring appeared invisible when there was anything up to 2° between the line of sight and the plane of the ring. The problem of forecasting the future appearances was a purely geometrical one. In Fig. 61, *ANC* represents the orbit of Saturn, *DEF* that of the Earth, and *L* the position of the Sun. The orbits are regarded as circular and lie in the same plane. Since the inclinations of the axes of

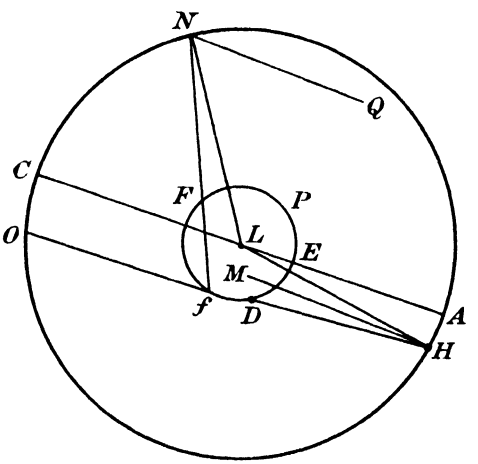


FIG. 61

the Earth and of Saturn are parallel, the equinoctial line *AC* passes through the equinoctial points for both planets. Now since the axis of Saturn remains parallel with itself the line of intersection of the planes of the ring and the orbit is always parallel with the line *AC*. If Saturn is at *H*, and the Earth at *D*, and *HM* is the line of intersection of the planes of the ring and orbit, if the angular displacement of the Earth from the line *AC*

is greater than that of Saturn, the line *HM* will fall between the Sun and the Earth. The plane of the ring passes between *L* and *D* and no reflected light from the ring can reach *D*. On the contrary, when the Earth is at less angular displacement than Saturn, as in the relative positions *N* and *F*, or on opposite sides of *CA*, as at *N* and *f*, the same surface of the ring would be visible from either *F* or *f*.

Knowing the synodic period of Saturn, that is the interval between two successive oppositions of the planet, it was possible to calculate the dates of reappearance of the round form. Huygens showed that the ring would appear thin from April to June 1671, and vanish from sight in July or August. Not until July or August 1672 (after an heliacal rising and setting) would the ring appear again. It would then remain visible until 1685. In this year, and again in 1700, the planet would appear in the round form. Actually, Huygens had to recognize errors in these predictions since events were slightly ahead of schedule even in 1671. Huygens did not make very exhaustive observations, and it appears likely that his fundamental determinations were not sufficiently refined. He did not attach great importance to long-continued and uninterrupted observation. Indeed, many of his observations passed unrecorded.

The telescope first used in the observation of Saturn had a magnification of about fifty. The objective was plano-convex, and the eyepiece was a simple lens of about eight centimetres focal length. An objective answering to Huygens's description of it, and dated February 3rd, 1655, was discovered at Utrecht University in 1867 by Harting (who wrote a short biography of Huygens in Dutch). Huygens's second telescope was one of 23 feet—about twice as long—and twice as powerful as the first. This was used after February 19, 1656. The method of computing the magnification was either by determination of the ratio of the focal lengths of the objective and the eyepiece or by comparison of the angular dimensions of a distant object seen through the telescope and by direct vision.

In his work on Saturn, Huygens used a micrometer consisting of a lamina of brass. This form, which has already been mentioned, was used up to the end of 1659. In 1666, he adopted the use of cross wires arranged in squares. The movable thread micrometer invented by Gascoigne superseded this. The values for the diameters measured by Huygens were all too large, but

they were an improvement on those given by Riccioli. Also they bear a very fair relation among themselves. As is shown in the table, Huygens's ratios for the diameters of the planets compared with the Sun are all in the direction of over-estimating the planetary diameters.

Planet.	Ratio of diameter to that of Sun.	True values.
Venus	1 : 84	1 : 112
Mars	1 : 166	1 : 202
Jupiter	1 : 5·5	1 : 9.8
Saturn's ring	1 : 7·4	1 : 11.6

While studying the apparent diameters of Jupiter and Mars, Huygens noted the existence of bands or zones across these planets. Drawings were given in the *Systema Saturnium*.

Huygens adopted Copernicus's proportions for the planetary distances from the sun. There was, until 1672, no agreement among astronomers over the distance of the earth from the sun. It was therefore necessary to proceed on a probable estimate if such could be found. Huygens's method was, having observed the apparent diameters given above, to adopt for the Earth a size which agreed best " with the order and good disposition of the whole system ". In a way which reminds one of Kepler, this disposition seemed to Huygens to rest on a proportionality between size and distance from the sun (Jupiter and Saturn being exceptional). Thus the earth, being intermediate between Mars and Venus, probably possessed a volume intermediate between these planets. From the figures given above, the mean (of 1/166 and 1/84) is 1/111. Huygens took the estimate that the sun's diameter was 1/113 of its mean distance from the earth. This gave the diameter of the earth as 1/12543 of the sun's mean distance. The maximum and minimum distances of Saturn came out at 122000 and 100344 terrestrial diameters respectively. A modern estimate would put the result at about 123600 and 100200 respectively, so that, considering the precarious basis of Huygens's calculation, the result was much better than might have been expected.

In his account of his method of observing apparent diameters Huygens explained that he used a diaphragm at the focus of the objective. This diaphragm had a hole a little smaller than the diameter of the eye-piece and in this way a sharp edge to the field

of view was obtained. It was easy to find the angular size of the field of view by timing the passage of a star across it, using the pendulum clock " recently invented ". His own telescope (presumably the one of twenty-three feet) embraced a field of 0° 17′ 15″. The description of a micrometer consisting of copper rods of diminishing diameters which could be inserted in the focal plane of the eye-piece ends the *Systema.*

Huygens made further observations of Saturn in later years and redetermined, among other things, the ratio of the diameters of the ring and the planet. On July 16, 1667, Huygens determined, probably with Buot, the hour at which the ring appeared parallel with the horizon at Paris. This enabled him to calculate the inclination of the ring to the ecliptic, but the details of the work are lacking. The most important work on Saturn in later years was done by Cassini, working at the Paris observatory, Cassini discovered a second satellite (October 1671) and observed the division in the ring which now bears his name.

XIII

Cosmotheoros

During the later part of the seventeenth century, Fontenelle, historian of the Académie Royale des Sciences, attracted much attention by writing descriptions of the earth as seen by hypothetical inhabitants of Mars and Saturn. The little work, *Cosmotheoros,* written by Huygens, and published posthumously in 1698, appears therefore to have been written in imitation of Fontenelle and its chief interest is that it shows Huygens, at the time of its composition, as notably Cartesian in outlook. It also shows him, as Leibnitz had hoped, in a more human light than most of his published work. The work was translated into French by Dufour in 1702 and into English in 1722—when it came out under the title *The Celestial Worlds Discover'd*; it is from this work that quotations have been taken.

Huygens began by remarking that scientific conjectures about the planets should not be judged contrary to the scriptures nor useless or impious. On the contrary, " besides the Nobleness and Pleasure of the Studies, may not we be so bold as to say, they are no small help to the Advancement of Wisdom and Morality, so far are they from being of no use at all? For here we may count from this dull Earth, and viewing it from on high, consider whether Nature has laid out all her Cost and Finery upon this small Speck of Dirt." The English style of the translation takes one far from the spirit of Huygens's Latin.

Nevertheless the work is throughout in an intimate vein, being written in the form of a letter to Huygens's brother Constantin. When Huygens outlined the Copernican theory he gave a diagram " like what you have seen in my Clock at home ". Perhaps he was referring to his machine for showing the planetary motions. At all events, the reader recognizes much of Huygens's early work in new dress—the planetary magnitudes, the planets' distances from the earth and his views on the earth itself as a planet. " . . . we are so skilful nowadays," runs the translation, " as to be able to tell how much more or less the Gravitation in *Jupiter* or *Saturn* is than here . . . ". His general view was that the planets are so like the earth in most essentials as to have

inhabitants of some kind in all probability. Among these essentials he reckoned the existence of water but perhaps with different properties from our own. It must have a lower freezing point on the cold planets. If some kind of human life exists, he suggested, there must be other forms of life upon which the human beings would be dependent. Man, he thought, on these other planets, probably had the same vices and the same power of reason. If his senses were not too different his studies were probably the same as ours. Huygens pictured the night sky as it would appear to the inhabitant of Jupiter or Saturn. Letting his imagination go, he remarked: "What a wonderful and amazing Scheme have we here of the magnificent Vastness of the Universe! So many Suns, so many Earths, and every one of them stock'd with so many Herbs, Trees and Animals, and adorn'd with so many Seas and Mountains! And how must our Wonder and Admiration be encreased when we consider the prodigious Distance and Multitude of the Stars!" "I must be of the same Opinion with all the greatest Philosophers of our Age," he added, "that the Sun is of the same Nature with the fix'd Stars." He criticized Kepler, therefore, for making the Sun superior to all other bodies in the sky.

At the end he stated his modified vortex theory. "I am of the Opinion," he wrote, "that every Sun is surrounded with a Whirl-pool or Vortex of Matter in a very swift Motion; tho' not in the least like *Cartes's* either in their Bulk or manner of Motion. For *Cartes* makes his so large, as every one of them to touch all the others round them, in a flat Surface, just as you have seen the Bladders that Boys blow up in Soap-suds do; and would have the whole Vortex to move round the same way." Descartes's views, he asserted, needed to be corrected in the light of Newton's work, in particular to take account of the gravity of the planets towards the Sun and how "from that Cause proceeds the Ellipticity of the Orbs of the Planets, found out by Kepler". His own vortices, he explained, were composed of matter which does not move all in the same way, "but after such a manner as to have its Parts carry'd different ways on all Sides. And yet there is no fear of its being destroyed by such an irregular Motion, because the Æther round it, which is at rest, keeps the Parts of it from flying out."

Such were Huygens's last words on the solar system. To the modern reader they seem curiously conflicting views. Huygens

accepted all the mathematical part of Newton's work but not his interpretations. He was unable to accept a purely empirical view of gravity and, as we have seen, considered his own experiments on the existence of an ether were decisive. In his own way, Huygens was only seeking to banish what he conceived to be occult properties. "Le grand mérite de Descartes est avoir vu que le problème du monde est un problème de mécanique," wrote D'Alembert. This outlook was certainly the one which was consistently adopted by Huygens. He did not see that his own work was in far better accordance with the Newtonian system, that it helped to expose and did not heal the wounds inflicted on Descartes's natural philosophy.

The *Cosmotheoros* reveals Huygens's religious outlook to a far greater extent than his other published works. It does not appear that he was ever a proclaimed rationalist, and indeed the evidence rather shows, on the contrary, that he continued to support Protestantism up to the end of his life. There were many points, however, on which his beliefs appear to have been unorthodox. While, for example, he considered that the constitution of the world argued the existence of an intelligent power behind phenomena, he felt that whatever divinity belonged to man belonged to his rational mind; through this mind man can apprehend the ways of the Creator, but equally this mind precluded an acceptance of the cruder superstitions. Huygens does not seem to have accepted belief in the Devil, and perhaps he rejected personal immortality. Yet his outlook clearly belongs to that noble period of Protestant thought which found nothing alien in the new and enlarged horizons revealed by Science. As an admirer of the writings of Cicero, Huygens refers to the glory to be found in Nature as in some way a warranty that man is not without his significance in the scheme of things. Stoic philosophy found a response within the breast of this seventeenth century scientist, and one is reminded that from the beginning of the Christian era there had been an infiltration from this philosophy of the grand belief in the ultimate rationality of the world. This belief Huygens certainly supported; it was, one might say, the essence of his religious outlook.

XIV

The Place of Huygens in the History of Science

Descartes and Galileo were the brightest stars in the scientific firmament at Huygens's birth and they influenced him one way or another all his life. In Huygens's early days the world of science may be said to have been divided into those who followed the empiricism of Galileo and those who, with Descartes, ultimately distrusted it. This division leaves on one side, however, the Aristotelians who, mainly for religious reasons, could not accept Descartes's ingenious reconciliation of Copernicanism with dogma. The universities which, on the whole, sheltered the Aristotelians were in consequence left behind by the faster-moving currents of contemporary thought. In time, it is true, they became affected by Descartes's thought and this acted as an introduction for more scientific ideas. Newton's views were first introduced at Cambridge as a sort of commentary on Descartes; at Oxford, the Savilian professorship of Astronomy almost alone was renowned for progressive ideas. The scientific societies of the early part of the seventeenth century were unacademic, amateur, spontaneous offshoots of the artistic renaissance, owing their existence partly to the rediscovery of Greek writings of analytical character, partly to the conflict of ideas in Astronomy and the development of Mathematics, and partly to the writings of Francis Bacon and of Descartes. The sources of the scientific renaissance include other tributary streams but when all is said the explanations seem incomplete and hardly concern us here. The important fact is that Huygens was born in the period when these scientific societies were in their infancy. As a young man, he must have heard of the work of the Florentine Accademia del Cimento and of the interest kindled in Paris through the reports of the well-known traveller Pieresc, who visited Florence. Huygens early studied the works of Galileo and through Mersenne obtained his introduction to a small world which was preoccupied with new problems and becoming more and more confident that it possessed a new technique of discovery.

For the attack on Aristotle's science had been long prepared. Roger Bacon, Da Vinci, Benedetti and Stevinus preceded Galileo

and began the initial liberation of Mechanics. The first criticisms, however, dealt with certain of Aristotle's postulates rather than with his entire method and Galileo was the first to replace the substance and attributes of scholastic description by what we must term scientific data. The whole movement of the seventeenth century empiricists, then, took this direction. Instead of substance, essence, matter, form and other categories adapted to Aristotle's logic, an analysis was developed using those of space, time, mass, force and the like. The categories of thought were transformed. All this Huygens himself realized remarkably clearly. He perceived that even the sixteenth century writers had retained many of the occult properties of the Aristotelians. Gilbert, Telesius and Campanella, he noted, had not enough inventiveness or mathematics. Even Gassendi was not much better. Bacon had seen the insufficiences of Aristotle and had in addition pointed out good methods for building a better system, but, he wrote, "he did nothing to advance mathematics and lacked penetration for physical matters, not having been able to conceive the motion of the Earth, at which he mocked". "Galileo, on the other hand, had the mind and all the knowledge of mathematics he needed to make progress in Physics and it must be admitted that he had been the first to make fine discoveries touching the nature of movement, although he left very considerable parts of it to be done. He had not sufficient boldness nor presumption to wish to undertake the explanation of all natural causes, nor the vanity to wish to be *chef de secte.* He was modest and loved truth too much; he believed besides that he had acquired enough fame and that, through his new discoveries, it would last for ever."

Another feature of contemporary scientific thought was the assumption of a mathematical simplicity in the relations traceable between data. This assumption is found in the work of Galileo and also in that of Kepler, Copernicus and, to some extent, perhaps, even in that of Da Vinci. Kepler expressed the idea in two aphorisms: "Natura simplicitatem amat" and "Natura semper quod potest per faciliora non agit per ambages difficiles." Burtt has pointed out[1] that the decline of Aristotelianism in the sixteenth and seventeenth centuries coincided with a rise of neo-Platonism in which there was a strong Pythagorean element. For Kepler, in the extreme instance, the mathematical

[1] *The Metaphysical Foundations of Modern Science* (1932).

harmony discoverable in the facts of Nature and even in the celestial regions was the *reason* why things are as they are. Nevertheless, this neo-Platonic mysticism was combined, in Kepler, with a reverence for exactitude in the mathematical formulation. Later this view was greatly modified; the existence of mathematical regularity was felt to indicate a mechanical explanation. The difficulty lay in combining the mathematical laws with a mechanism which would sacrifice none of their exactitude. Descartes's mechanism failed here. His cosmology was in fact founded on an antithesis which was truly scholastic, since he tried to reconcile Copernicanism with an unmoved earth—an earth at rest in its heaven. Huygens became convinced that Descartes in this and other matters had in fact repeated the errors of Scholasticism, for he hoped to found a demonstrative and deductive system. Descartes, he noted, " who appears to me to have been very jealous of the renown of Galileo, had this great desire to pass for the author of a new philosophy. Which was clear from his efforts and his hopes to have it taught in the academies in place of that of Aristotle . . . ". Descartes's ideas, he admitted in another passage, were presented with all the force of verified conclusions; the novelty of the shapes of his ultimate particles of matter and the beauty of his vortices all exerted a compelling influence. " It seemed that when I read this book, the *Principia*, the first time that everything in the world became clearer, and I was sure that when I found some difficulty, that it was my fault that I did not understand his thought. I was then only fifteen or sixteen years old. But having since discovered from time to time things clearly false and others very improbable, I came back strongly to the preoccupation I was in, and at the present time I find scarcely anything I can accept as true in all the physics, metaphysics and meteors."

Huygens, in fact, returned to the outlook of Galileo. He had learnt that quantitative study of data and not scholastic logic furnished the technique of discovery. Descartes's powers as a mathematician compelled Huygens's admiration and he found it all the more unforgivable that exact agreement with the facts should not be uppermost in Descartes's work. The difference between Descartes and Huygens did not lie in their conception of the physical processes so much as in the regard paid to accurate definition of physical conditions. More than geometry was needed, Huygens perceived, to deal with this. There were more properties

than mere extension, which was quite inadequate to account for the results of collision between elastic bodies and for accelerated motion.

These considerations are, however, as yet inadequate as an account of the revolution in Mechanics which we have been led to discuss. For the change in mental categories and the expression of laws in corresponding mathematical form was combined with a profound change in the method of explanation involved in all scientific work and especially in Mechanics. In broad terms, the revolution against Aristotelianism was the rejection of Aristotle's *final* cause in favour of a scientific mode of his *efficient cause*; men asked not what purpose but what process lay behind phenomena. How radical this change was we can realize only if we remember that for Aristotle, with his biological outlook, all events were part of a natural process of fulfilment, of realization of what was innate. Things converged towards an appointed and necessary end; it was consequently more interesting to enquire what that end is, rather than what mechanism underlies events. In Dynamics all motion was motion to an appointed place; all nature bore the character of an innate impulse to movement.

Movement and change set greater problems for the philosophy of Plato. Whereas movement was regarded as continuous by Aristotle, to Plato all change appeared as a succession of Forms and consequently movement must be essentially discontinuous. It may well be, therefore, that for an Aristotelian of the sixteenth century movement held less interest and less to be studied than it did for those who were influenced by the revival of neo-Platonic ideas.

There was, however, a second reason for the revolt against Aristotle, whose authority so dominated the scholastics. Quite apart from the difference in the nature of cause itself, there was the objection that no knowledge of the world as it is could be derived by deductions from *a priori* metaphysical principles. Aristotle's system was out of touch with brute facts, his method unsuited to their investigation. Aristotle, in fact, did not see that induction demanded a correlating idea; that it could not be reduced to some kind of syllogism, and consequently that knowledge of the world in the last resort cannot be made part of logic. Bacon, though not a scientist, was clear on this "unfruitfulness" of Aristotle and expressed the new attitude to facts in his

famous aphorism: "Nature to be commanded must be obeyed". It became clear from this time onwards that much remained to be discovered in respect of even the simplest events.

Nevertheless, it is clear that what we now hold to be the typical method of the physical sciences appeared then to be a much more individual question. Galileo had given an excellent example but Huygens carried the inter-relation of mathematics and experiment a long way further. All his work illustrates this quality, and certain continental writers have even argued that Huygens's conception of scientific method was in some respects superior to that of Newton. Certainly in regard to the position of hypotheses in scientific work a case may be made out for Huygens's superiority. "... the main Business of natural Philosophy is to argue from Phænomena without feigning Hypotheses, and to deduce Causes from Effects, till we come to the very first Cause, which certainly is not mechanical," wrote Newton in the twenty-eighth query of his *Optics*. Against this we might place Huygens's remarks on the essential place of hypothesis in scientific work which come at the beginning of his *Traité de la Lumière*.

It was in the use of abstractions that Newton made the greatest contribution. He had the strongest objection to hypothetical *entities* because he wished to concentrate on mathematical *relations*. The space and time of Newton's system were not identical with the space and time of ordinary experience; they were abstractions. Huygens was unable to understand Newton's more positivist attitude here. The realist nature of his own conceptions makes him, in fact, a convenient starting point from which to trace the second main stream of scientific thought. Huygens might have accepted the term "correlate" as equivalent to hypothesis but, in the manner of all atomists, his concepts borrowed a garb from perceptual phenomena. For him the atom was a potential phenomenon, as it still is for some modern men of science. He did not see as clearly as Newton that for Science what is needed is a number of principles of quantitative correlation. This, surely, is the true end of scientific induction. In practice, in the scientific interpretation of phenomena not capable of treatment by classical mechanics, much use is always made of entities which must possess the qualities of hypotheses. This method Huygens, if not Newton, would have supported. We may summarize this discussion by saying that the Huygensian

method confers greater freedom than the Newtonian and, in regard to the broad conception of the place of hypotheses, Huygens was a more profound methodologist than Newton.

Huygens's work was very influential in its day and it is certain that Newton was at least stimulated by him. An interesting example is Huygens's account of the ether—setting aside, for the moment, the distinction of the subtle matter. Newton did not accept this sort of explanation of gravity but equally he recognized the difficulties of the so-called "action at a distance". He believed with justification that his *Principia* disposed completely of the Cartesian vortices of subtle matter and he returned to this subject in his *Optics*. ". . . against filling the heavens with fluid mediums, unless they be exceeding rare, a great objection arises from the regular and very lasting motions of the planets and comets in all manner of courses through the heavens. For thence it is manifest, that the heavens are void of all sensible resistance and by consequence of all sensible matter." But Newton never denied that an ether might exist and that it might be conceived as a medium susceptible of transmitting vibrations. Burtt[1] even writes: "Having taken over the notion from the current of the times, and feeling it to be thus well grounded, it was easy for Newton to extend its use to other phenomena which involved action at a distance and which others were accounting for in the same fashion, such as gravity, magnetism, electric attraction, and the like." Newton was, however, not at all consistent on the subject of the ether and it is clear that his conception of it was different from that of Hooke. There is some probability that it was Huygens's work on the subject and particularly the experiments using the vacuum pump which influenced him most. Accounts of Huygens's experiments on the non-descent of columns of water and mercury were common in England, notably in the *Philosophical Transactions,* and the observations were also made by Boyle, Brouncker and others. Most of Newton's comments on the ether are to be found in his *Optics,* in which they first appeared in the Latin edition of 1706.

The only acknowledged debt of Newton to Huygens was the statement of the theorems of centrifugal force in the *Horologium Oscillatorium*. Newton, as L. T. More has remarked, "must have seen that Huygens's law of centrifugal force was easily deducible from his own calculation on the attraction of the moon, and that

[1] E. Burtt. *op. cit.* p. 265.

by neglecting to follow up his work, Huygens had preceded him." This is suggested by Newton's statement: "What Mr. Huygens has published since about centrifugal forces I suppose he had before me." The view which is now most widely held in regard to the difficult problem of the order of Newton's ideas is that he solved the problem of centrifugal force independently of Huygens but neglected to publish anything until he had proved the important theorem concerning the gravitational field due to a large solid sphere. Newton was uninfluenced by Huygens's very important development of the conceptions of energy and work done in mechanical systems. An account of the growth of Huygens's ideas has been given in the sections on impact and on the centre of oscillation.[1] Unfortunately, the only direct influence of Huygens's ideas is to be found in the work of Leibnitzians, who made the conservation of *vis viva* a cosmic principle instead of treating it, as Huygens did, as only half the true law of the conservation of mechanical energy. Huygens did not go so far as Newton towards complete scientific positivism but he agreed that mathematical law is in itself the most important end of scientific work. For Newton the essential aim of science, in his own words, was to replace "occult properties supposed to result from specific Forms of Things" by "general laws of Nature". The wisdom of this limited aim has been fully demonstrated.

If Huygens saw the direction in which Leibnitz's ideas were to lead philosophy it is certain that he could not have sympathized with them. With his increasing recognition of Descartes's errors in purely physical matters, Huygens, as we know, paid less attention to his philosophical system and so felt unperturbed by the dilemma of his dualism. If this had not been so, Huygens might have inclined to a materialism of the kind elaborated by his contemporary, Hobbes, but it is inconceivable that he would have tolerated many of the latter's crudities. Huygens was something of a materialist and he rejected the orthodox religious doctrines: nevertheless, if he had written philosophy it would have been without the harshness of Hobbes's determinist schemes.

Yet the bifurcation of the world into physical and mental spheres was as much the work of men like Huygens as it was of Descartes, who first presented the bifurcation in its most uncompromising form. Kepler took the first step of distinguishing primary from secondary qualities. For him only those qualities

[1] *Nature*, 1943, p. 519.

which could be measured were primary and this attitude was found in Galileo, who defined the two classes more clearly and made the corresponding distinction between true knowledge (of primary qualities) and mere opinion. Colour, smell, taste and sound were for him matters of opinion—subjective impressions resulting from the operations of atoms or vibrations on the sense organs. This set the stage for the Cartesian dualism. It is not perhaps properly recognized how far Huygens contributed to the adoption of Galileo's attitude. But we have only to recall that Huygens's whole work was the reduction of more phenomena to quantitative treatment—their transformation from matters of opinion to those of knowledge. Scientific time measure for short intervals began with Huygens; he related the standard of length with that of time, he provided a geometrical treatment of reflection and refraction of light, he greatly extended Mechanics and elucidated the true nature of more celestial phenomena in accordance with Copernicanism—all this is a direct continuation of lines of thought to be found in Galileo. Through him, in fact, the main stream of scientific thought may be said to have been diverted from following Descartes and instead directed into the channel which Newton's work deepened to a river. But Huygens would not have had us forget the imaginative stimulus of Descartes's writings. As late as 1691 he remarked: "We owe much to Descartes because he revealed new paths in the study of Physics and started the idea that everything must be reduced to mechanical laws."

Huygens came nearest to explaining his own method in scientific study in a letter to Tschirnhaus in 1687. In the problems of Physics, he wrote, very great difficulties are felt at first and these cannot be overcome "except by starting from experiments . . . and then by conceiving certain hypotheses . . . But even so very much hard work remains to be done and one needs not only great perspicacity but often a degree of good fortune." One is reminded of Whitehead's remark ". . . it is the establishment of the procedure of taking the consequences seriously which marks the real discovery of a theory". At times Huygens followed his own procedure of taking the consequences seriously up to the point at which his elegant geometrical constructions were threatened. For him, as for Kepler, there seems to have been a disposition to believe that mathematical elegance is in some way an index to reality. This feeling for form may be

found in many men of science but in few has it been so marked as in Huygens. This sense for elegant theoretical construction, roused by the symmetry and order which can be found in Nature, is well illustrated by the research on Iceland spar in which one has, as it were, the essence of Huygens's thought. Yet it was the weakness which predisposed him to believe in the rotation of the *matière subtile* around the earth. He agreed with Leibnitz's comparison of Galileo and Descartes: " Galileo excels in the art of reducing mechanics to science; Descartes is admirable at explaining by beautiful guesses the causes for the effects of nature." He agreed, too, with Leibnitz's remark that Descartes's work was " un beau roman de Physique " ; one feels that it was with regret. And yet, without Huygens's careful studies how could Leibnitz have coined that phrase?

PERSONS MENTIONED

Many of the persons mentioned in the first part of this book will be unknown to the general reader, and some of them are but little known to the historian. The following notes provide some of the chief facts known about them.

Auzout, Adrien (d. 1691). French mathematician and astronomer. Auzout is chiefly remembered for his invention of a micrometer consisting of movable hairs, mounted parallel in the field of view of the astronomical telescope. With Picard (below) he was a pioneer in the application of the telescope to graduated scales, thus replacing the use of open sights upon which Tycho Brahe had had to rely. He worked in Paris with Huygens and helped in the development of the " aerial " telescope.

Barrow, Isaac (1630–77). This noted English mathematician was the first holder of the Lucasian professorship at Cambridge; on his arrival he became Newton's tutor and he undoubtedly influenced Newton through his own interests in geometrical optics and mathematics. Previously he had had a somewhat eventful life, his political and religious views as a young man in his twenties making it desirable that he should travel on the Continent. He became F.R.S. in 1662, the year before he went to Cambridge. In 1669 he resigned the Lucasian chair in Newton's favour, desiring among other things to devote himself to theology.

Boulliau, Ismael (1605–94). French astronomer. Before working in Paris, where he met Huygens, he spent some time travelling in Italy, Holland and Poland, becoming acquainted with the leading men of science. He was a friend of Leopold de Medici and corresponded with him on scientific matters of the time.

Campanella, Tomasso (1568–1639). He does not come into the period covered by this book, but his writings and opinions were well-known to Huygens and he is to be remembered as one of the earliest champions of the experimental method. His most famous work is his *Defence of Galileo* (1622), in which he quoted the great religious thinkers from Augustin to Aquinas to show how unjustified the persecution of Galileo was.

Carcavy, Pierre de (d. 1684). Did important administrative work in Paris during the period under review. Colbert made Carcavy librarian to the King (1663) and it was during Carcavy's period of office that the royal library was moved to new rooms which became also the meeting place of the Académie Royale des Sciences. Carcavy's mathematical knowledge was good and he took part in 1645 in the dispute over the quadrature of the circle, claiming to demonstrate that the quadrature was impossible. He was a friend of Pascal, Descartes, Roberval and Fermat.

Cassini, G. Domenico (1652–1712). Born at Perinaldo near Nice, and educated by the Jesuits at Genoa, Cassini at twenty-five became professor of astronomy at the famous university of Bologna. Here he became distinguished and his fame spread to Paris when he published ephemerides for the satellites of Jupiter. Through the efforts of Picard he was invited to Paris and after 1671 he was virtually the director of the Paris observatory. He collaborated with Richer in 1672 on the observations of the parallax of Mars. With Picard and Auzout he did a great deal towards founding the great tradition of the Paris observatory. His most famous observation was the discovery of the satellites of Saturn and the division in its ring now known by his name.

Fermat, Pierre de (1601–65). One of the greatest of French mathematicians. Fermat anticipated Descartes's invention of analytical geometry in nearly all respects, and he contributed important work on the treatment of maxima and minima. With Pascal he laid the foundations of the mathematical study of probability (Huygens also wrote on this subject), and he discovered the important "least-time principle" in optics. He adversely criticized some of Descartes's optical theory.

Frenicle de Bessy (1605?–1675). Frenicle held an official position in Paris, where he acquired a great reputation as a demon calculator. He could solve the most complex numerical problems in arithmetic and so quickly as to astonish even Fermat, Roberval and Wallis. His method of working was kept a secret and was examined after his death. His "method of exclusions", which he used so much, is now only an object of curiosity and he contributed nothing of permanent value.

Hevelius (or, Johann Hevel) (1611–87). Noted astronomer of Danzig. Although he made first-class observations of the planets and the moon employing telescopes, he preferred to make all his measurements with open sights. This led to a dispute with the English astronomers and he was visited in Danzig by Halley, who observed with him and employed telescopic sights. Neither astronomer converted the other.

Mariotte, Edmé (1620–84). A Roman Catholic abbé with a weakness for experimentation. He joined the Académie Royale des Sciences in the year of its foundation. His most valuable work was on impact; he independently enunciated the law known as Boyle's law [cf. his *Discours de la Nature de l'Air* (1676)].

Papin, Denis (1647–1712). Had a rather precarious existence and died in obscurity in London. For a time he assisted Huygens, notably with his experiments on the air pump and on the expansion of steam as a source of power. Later he came to London to work with Boyle and in 1684 he became temporary curator to the Royal Society. In 1687 he was appointed professor of mathematics at Marburg. From there he went to Cassel (1696) and then on to London (1707).

Petit, Pierre (1594–1677). An able civil servant who was given his chance by Richelieu. He was quite a distinguished mathematician and he had quite a lot of contact with Huygens during the latter's early visits to Paris. His daughter was attractive. A friend of Pascal also, he helped him in his experiments on the vacuum.

Picard, Jean (1620–82). One of the most famous astronomers of the seventeenth century. He first of all observed with Gassendi, whom he replaced as professor of astronomy at the Collége de France. In 1666 he became an abbé but without any interruption of his scientific output. In 1669 he read to the Académie an important memoir on the new methods in astronomy resulting from work by Huygens and himself. He went to Uraniborg, Tycho Brahe's famous observatory (of which nothing then remained), in order to fix more accurately its latitude and longitude. He met Roemer on this journey and got him to come back to Paris with him. Picard's *Mésure de la Terre* was an important work and provided Newton with the information he needed in his calculations on the earth's gravity.

Roberval, Giles Persone de (1602–75). A French mathematician who rose to eminence from obscure origins. He came to Paris in 1627 and stayed with Mersenne. In 1631 he was appointed to a chair of philosophy and then to the professorship of mathematics at the Collége de France. He was one of the original members of the Académie Royale des Sciences. After 1638 he had strong differences with Descartes and became hostile to Descartes's philosophy. He was a man of decided originality of mind and his mathematical work is of importance.

Roemer, Olaus (1644–1710). Noted Danish astronomer. Roemer worked at first at the Round Tower of Copenhagen, which had been built by Christian IV for one of Tycho Brahe's assistants. He is chiefly remembered for bringing the transit telescope into general use. His calculation of the velocity of light is also famous. After his death his instruments and records were destroyed in the great fire of Copenhagen (1728), but it was found possible to reconstruct much of what was lost.

Sorbière, Samuel de (1615–70). French writer. Sorbière was intended at first for the Protestant ministry and was brought up by an uncle after the death of his parents. He studied medicine in Paris and practised in Holland up to about 1650. In 1653 he became a Catholic, and in 1655 he went to Rome and was received by the Pope. Lacking any more solid rewards, Sorbière sought patronage in Paris and became loosely attached to the circle of scientific amateurs. He was not thought to have much originality or learning by his contemporaries.

Wallis, John (1616–1703). Famous English mathematician. Wallis was ordained in the English Church after completing his time at Emmanuel College, Cambridge, and he then served as chaplain to a noble family. His fortune was made by his discovery of his ability to decipher codes—for the parliamentary party—during the civil war. In 1649 Cromwell appointed him Savilian professor of Geometry at Oxford. He became a friend of Newton and his *Arithmetica Infinitorum* led the latter direct to his discovery of the binomial theorem. He became associated with Boyle and helped to found the Royal Society.

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