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Science in the Seventeenth Century*

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THE AUTHORITY OF ARISTOTLE

THE sixteenth century was a period of reference to written authority, and in science the supreme rulers were Aristotle in all that pertained to the exact sciences and to general biology, and Galen in all that pertained to medicine. Occasionally a doubter arose, such as Paracelsus, only to be, at best, overwhelmed with ridicule and contempt, or, at worst, more drastically suppressed, as exemplified by the case of Giordano Bruno, who was burnt in 1600. The opinion widely expressed was that the world, Nature and particularly the human intellect, were in process of decay which had set in since the glorious times of classical Greece: it was seriously contended that the great minds of antiquity represented a race of giants which had died out just as the race of physical giants had died out. In particular, the works of Aristotle were supposed to fix the limits to which the human intelligence could reach, and the obvious thing to do, when seeking information as to the way in which Nature worked, was to refer to these works.

The method of Aristotle was, of course, speculative. Starting with certain philosophical ideas as to the nature of matter, derived partly by direct apprehension, partly by reasoning from a few observed facts of Nature, such as the circular motion of the stars, he proceeded by logical processes to argue how Nature should behave in order to conform with his innate ideas as to harmony and balance. This is no place to discuss

the Aristotelian doctrine of form, which exercised so profound an influence on those who came after him; but it is necessary for our purpose to refer briefly to his ideas on motion and mechanics in general. He made a clear distinction between natural motion and violent, or artificial, motion. From the motion of the heavens he concluded that motion in circles was a natural motion. From the spontaneous motion of dropped bodies downwards, and of smoke and fire upwards, he concluded that a vertical motion was also a natural motion, adding to the idea of gravity downwards that of levity upwards. The four elements, earth, fire, air, and water played an essential part in the Aristotelian scheme; earth and water strove downwards, air and fire upwards.

The motion of a projectile or any propelled body, was a violent, or non-natural, motion. His fundamental error in considering these non-natural motions lay in his belief that bodies only moved so long as a force was acting upon them; he supposed that a thrown body continued in motion after it left the hand only because the moved air beat on it behind and carried it forward. It may be pointed out that this was an idealization, not an observed fact. A body set in motion by a force is observed ultimately to come to rest; he supposed that it would come to rest at once if the force ceased at once, but that the action of the force was continued by the air, ultimately dying out. Another idealization of the observed fact is to suppose that a body set in motion would continue to move, after the force had ceased to act, with a uniform velocity for

* Friday evening discourse delivered at the Royal Institution on February 18.

Fig. 1.

SANTBECH'S CONCEPTION OF THE TRAJECTORY OF A PROJECTILE;
MODUS VIOLENTUS AND MODUS NATURALIS

ever, were it not that adventitious forces, such as friction, act upon it, and ultimately bring it to rest. The latter is Galileo and Newton's idealization, which now lies at the basis of our mechanics. Neither of the two is inherently more probable than the other; only experiment and observation have convinced us that the modern view is the scientifically correct one.

As regards fall in particular, Aristotle considered that a moving body was acted upon by a power moving it and by a resistance: if power and resistance were equal the body did not move. This is a fundamental mistake. It was argued by others (but apparently not explicitly by him) from his fundamental conceptions that the time of fall of a body was inversely as its weight. Experiment had no part in his scheme, probably because he was dealing with first causes, and isolated material phenomena he held to be without fundamental significance. Form was more important than matter. We see something of his point of view in the works of certain modern economists, who regard actual economic events as of little significance.

How profound was the influence of Aristotelian mechanics in the sixteenth century, even in discussion of practical matters, is well exemplified in Santbech's discussion of the motion of projectiles, published in 1561. He states that the motion of a projectile consists of an oblique violent motion which suddenly ceases, and is succeeded by a natural motion of vertical fall. He gives diagrams (of which Fig. 1 is an example) showing the path of a cannon ball as two sides of a right-angled triangle, the hypotenuse representing the violent motion, and the vertical side representing the vertical fall on to the object aimed at. On this basis he proceeds to solve problems in ballistics. Other writers of about this time, apparently feeling uneasy about the sharp angle at the highest point of the trajectory, replaced it by a circular arc joining the straight-line oblique motion to the straight-line natural fall, the motion in this arc being a mixed mode, so that the whole trajectory consists of a straight line *modus violentus*, a curved

modus mixtus, and a straight vertical *modus naturalis* (Fig. 2). It is a strange comment on the way in which these writers refused to let observation influence them that in a book by Rivius, published in 1547, the discussion of ballistics is carried out in terms of trajectories of this kind,

Fig. 2.

MODUS VIOLENTUS, MODUS MIXTUS AND MODUS NATURALIS

while in pictures in the same book, in which the artist shows the bombardment of a town, the trajectories of the bombs are shown as tolerably correct representations of the path of a real projectile.

Let us look at the position at the opening of the seventeenth century. Copernicus had published his "*De Revolutionibus*" in 1543, when he was on his death-bed, but the astronomers who followed him were divided in their attitude towards his theory. Giordano Bruno, who upheld the Copernican hypothesis, was burnt in 1600; Rheticus, Reinhold and Mästlin contended for the theory; not only Luther and Melancthon, but also the great observer Tycho Brahe, who died in 1601, pronounced against it. Kepler (1571-1630) was, on the other hand, a Copernican. He had published his "*Prodromus*" in 1596, in which book he traced a mystical connexion between the distances of the planets, and the radii of six concentric spheres, between which the five regular solids were fitted. This was, of course, in the spirit of Pythagoras, who saw in the heavens a mysterious harmony which could be expressed in numbers.

Kepler's famous laws were not published until the century had well started, the first and second in the "*Astronomia Nova*", 1609, and the third in the "*Harmonices Mundi*", 1619. These laws gave the geometrical form of the planetary orbit, and a time-table for their progress in these orbits, but envisaged no quantitative mechanism to account for that form and that timetable. At the beginning of the century he considered that planets must have some kind of souls to urge them on in their paths, but later he resorted to a magnetic influence emanating from the sun, of a kind, however, that did not in any way correspond to the laws of magnetism as we know them, for he supposed that it was the rotation of the sun on its axis that was responsible for the urge. His great difficulty in the way of mechanics was that he had no conception of inertia. Of mutual gravitation he had, however, some idea, and even explained the tides as due to an attraction of the moon for the waters of the earth. Probably his magnetic hypothesis was due to Gilbert's book, "*De Magnete*", the appearance of which in 1600 aptly inaugurated the century which saw the triumph of the experimental method.

THE MECHANICS OF GALILEO

The first great name with which we have to deal, if we wish to trace the rise of modern mechanics, is Galileo, who was born in 1564, the

year in which Michelangelo died. Everybody knows the legend that, as a young man in Pisa, he confuted the Aristotelians by letting spheres of different masses fall from the tower of Pisa, and observing that they all took the same time to reach the ground—a legend, because the indefatigable researches of Wohlwill have shown that it is highly improbable that he ever carried out this experiment. The story that he did so is due to Viviani, who published his book in 1654, twelve years after Galileo's death, and who first met Galileo towards the end of his life. Galileo was in Pisa from the seventeenth to the twenty-eighth year of his age, and there seems no doubt, that, as we should expect, when he first began to lecture he believed what he had been taught, and was a devout Aristotelian. The experiment which he actually carried out was to let fall a lead and a wooden sphere, and what he noted was that the wood fell more quickly at first, but that later on the lead overtook it, and reached the ground first! He was at the time too much of an Aristotelian to attach much importance to this finding that, of two bodies of equal size, the heavier fell slower at first but afterwards quicker.*

Later he turned against the pseudo-Aristotelian belief that the rate of fall was proportional to the weight. This was not due to experiment, but to an appeal as to what was reasonable. He says "If we let two stones fall from a high tower, one twice as big as the other, it is ridiculous to suppose that when the big one has reached the ground, the other will be only half way down", but he adds "we will work more with reasoning than with examples, for what we seek is the cause of phenomena, which experience does not provide", a very Aristotelian sentiment. It was not until later that he showed that genius for experiment which is one of his great claims to fame. In short, he did not suddenly, as a young man, reject the whole body of knowledge in which he had been brought up, but he gradually freed himself from the scholastic point of view, and laid the foundations of modern science.

In popular estimation, Galileo is, perhaps, best known as the man who first turned the telescope to the serious study of the heavens—in fact he is often attributed with the discovery of

* The question of the supposed experiment at Pisa is discussed in a very scholarly manner by Prof. Lane Cooper in a little book "*Aristotle, Galileo and the Tower of Pisa*", which has come to my notice since this discourse was delivered. The learned author, who is professor of the English language and literature in Cornell University, sets an example to some of his scientific brethren who write on such matters, by his constant reference to the original sources. I am glad to find that his conclusions support what is written above.

the telescope, although actually what happened was that, having heard that such an instrument had been made in Holland, he himself made one without being supplied with details as to the method of construction. His observations were of the first importance. The discovery of the mountains of the moon struck the popular fancy. It is the one point to which Milton, who visited Galileo in his old age, refers in "Paradise Lost".

"his pond'rous shield
Ethereal temper, massy, large and round,
Behind him cast; the broad circumference
Hung on his shoulders like the Moon, whose Orb
Through Optic Glass the *Tuscan Artist* views
At Ev'ning from the top of *Fesolè*,
Or in *Valdarno*, to descry new Lands,
Rivers or Mountains on her spotty Globe."

The lunar landscape at once suggested that the moon might be inhabited, as discussed, for example, in Wilkins' "The Discovery of a World in the Moone" (1638). The discovery of the phases of Venus established the rotation of that planet round the sun, and, as a matter of fact, the fact that they were not observed with the naked eye had been recognized as a difficulty by Copernicus, who explained it away by suggesting that Venus was transparent. In the discovery of sunspots, which was a blow for the Aristotelians, since it showed that the eye of the world was not perfect, he was anticipated by Fabricius. He observed that the Milky Way consisted of innumerable stars. As regards Saturn, owing to the position of the rings and the imperfect resolution of his telescope, he concluded that the planet was accompanied by two satellites practically touching it, or, as he said, "I have observed the planet Saturn to be triple"—*altissimum planetam tergeminum observavi*.

Probably the most important discovery which Galileo announced in his "Siderius Nuntius", or "Messenger from the Stars", was the existence of the satellites of Jupiter, which, revolving round the planet, gave a picture of the Copernican system. His astronomical discoveries, then, form an imposing collection, but Lagrange, whose opinion on anything that pertains to the science of mechanics must carry immense weight, makes light of them in comparison with Galileo's services as the founder of the science of mechanics, "Les découvertes des satellites de Jupiter, des phases de Vénus, des taches du Soleil, etc. ne demandaient que des télescopes et de l'assiduité; mais il fallait un génie extraordinaire pour démêler les lois de la nature dans des phénomènes que l'on avait toujours eus sous les yeux, mais dont l'explication

avait néanmoins toujours échappé aux recherches des philosophes." Although we may not agree that anyone with a telescope—especially such a telescope as Galileo possessed—and industry, could have made his astronomical discoveries, yet there is no doubt that the way in which Galileo made mechanics into a science is a most striking testimony to his genius.

The great book in which he announced his fundamental discoveries in mechanics, "Discorsi e dimostrazioni matematiche intorno a due nuove scienze", appeared in 1638. In it he established the law of fall, and the parabolic motion of a projectile in a non-resisting medium. He deduced the law of fall from the supposition that there were equal increments of velocity in equal small intervals of time; in the course of his arguments he considered any time interval as capable of being divided into an infinite number of instants, and so foreshadowed the differential method. He did not, however, connect a uniform acceleration with a uniform force, but apparently attributed the increase in velocity to an increase in force during the fall, and, further, he did not think of the fall as due to the attraction of the earth but rather to some urge inherent in the body. As to the true cause of the acceleration, however, he expresses the same kind of reticence as was shown by Newton concerning the nature of the gravitational force. We may take, for example, Salviati's speech on the third day of the dialogues: "The present does not seem to be the proper time to investigate the causes of the acceleration of natural motions, concerning which various opinions have been expressed by various philosophers, some explaining it by attraction to the centre, others by repulsion between the very small parts of the body, whilst others attribute it to a certain stress in the surrounding medium, which closes in behind the falling body and drives it from one of its positions to another, which fantasies, and others like them, create problems requiring examination, but there would be little gain were they solved. At present it is the purpose of our author merely to investigate and to demonstrate some of the properties of accelerated motion, whatever the cause of the acceleration may be."

Galileo perfectly understood inertia in the case of horizontal travel, and used the fact that, in the absence of horizontal force, a body travels horizontally with a uniform speed, in his investigation of the path of a projectile. It is strange, then, that he seems to have no conception of inertia in

the case of the vertical motion of free fall. It has been suggested that this is possibly a relic of his Aristotelian training, a horizontal motion in the direction of the earth's surface, that is, in a very large circle, being a perfect motion, like that of the heavenly bodies. There seems no need, however, to resort to this supposition; an accelerated motion is clearly harder to bring into any scheme than a uniform translation, as is sufficiently shown by the history of Einstein's theory of relativity. In any event, assuming equal increments of velocity in equal elements of time, he found, by what is essentially graphical integration, that the space traversed was proportional to the square of the time.

He verified this law experimentally by measuring the time taken by a body rolling on an inclined plane to traverse various distances, measuring the time by a water clock, and "in such experiments, repeated a full hundred times, we always found that the spaces traversed were to each other as the squares of the times, and this was true for all inclinations of the plane along which we rolled the ball. We also observed that the times of descent for various inclinations of the plane bore to one another precisely that relation which, as we shall see later, the author had predicted and demonstrated for them."*

The law referred to in the last sentence is essentially that the velocity is given uniquely by the vertical drop. This Galileo proved in a most ingenious way by drawing aside the bob of a heavy pendulum until it reached a fixed height, and letting it fall so that the string struck a nail vertically under the point of support. No matter at what height this nail was placed, that is, no matter what the curvature of the arc along which the bob rose, the height which it attained was always the same, and equal to that through which it fell. The velocity at the lowest point being always the same, it follows, considering the motion reversed, that the velocity is independent of the curvature of the arc along which the bob is constrained to move. From this he concluded that a similar result would hold true for planes of different slopes.

The parabolic law for projectiles Galileo proved by combining a uniform velocity in the horizontal direction with the law of fall, having previously proved that, with a body projected vertically upwards, the time of rise to the highest point is equal to the time of fall from that point. With

* It need scarcely be said, perhaps, that the author referred to in this quotation is himself.

great ingenuity he verified the parabolic form by rolling a metal ball on a slightly inclined plane metallic mirror: "In the execution of this method, it is advisable to heat and moisten the ball slightly by rolling it in the hand in order that its trace upon the mirror may be more distinct." The experiment may be carried out to-day by covering with carbon copying paper a sheet of white paper attached to a board and rolling a heavy metal ball over it.

It is hoped that these few words will be enough to suggest the acumen and experimental skill with which Galileo founded the science of particle dynamics. This work was the first great step towards the Newtonian synthesis, but of universal gravitation he had not the slightest inkling, as a passage where he speaks of dropping stones from the moon on to the earth is sufficient to show.

BACON AND DESCARTES

In any account of the scientific thought of the seventeenth century there are two philosophers whose names always appear prominently, Francis Bacon and Descartes. Bacon was a Lord Chancellor and Descartes was a great mathematician, but what we have here to consider are their writings on method, especially scientific method. As regards Bacon's services to science, we have a great diversity of opinion among those who have made a careful study of his works. Macaulay, Spedding, and very recently, Prof. R. F. Jones of the University of Washington, speak of him in terms of the highest commendation. On the other hand, William Harvey, the discoverer of the circulation of the blood; Liebig, the great chemist; the philosophers Lasson and Dühring; the Anglo-American man of science J. W. Draper, who carried out important researches in photo-chemistry and was the author of the "History of the Intellectual Development of Europe"; and Sir Oliver Lodge, all speak strongly against his claims to be considered an important influence in the history of science.

Macaulay says of Bacon, for example, "As we have lying before us that incomparable volume, the noblest and most useful of all the works of the human reason, the *Novum Organum* . . .", which passage we may, perhaps, characterize in Macaulay's own words as the "noxious sweetness of his undiscerning praise". On the other hand, Aubrey tells us of Harvey that "he had been physician to the Lord Chancellor Bacon, whom he esteemed much for his wit and style, but would not allow him to be a great philosopher. . . .

'He wrote philosophy like a Lord Chancellor', he said to me speaking in derision". Draper is violent in his condemnation, saying of Bacon that he was "a pretender in science, a time-serving politician, an insidious lawyer, a corrupt judge, a treacherous friend, a bad man"; but he is no more violent than Liebig. Elsewhere Draper says, with justice, that Bacon's "chief admirers have been persons of a literary turn, who have an idea that scientific discoveries are accomplished by a mechanico-mental operation". Finally we may quote Sir Oliver Lodge, the kindest and most courteous of men, whose verdict is that "on the solid progress of science he may be said to have had little or no effect"

Bacon had a purely utilitarian view of science, saying that "the real and legitimate good of the sciences is the endowment of human life with new inventions and order". To him is often attributed the adoption of the experimental method, as distinguished from the speculative method of Aristotle, and he himself is continually boasting that he brings a new method to the overthrow of Aristotle; but his whole terminology is scholastic, with its sensible qualities, matter and form, and he totally lacks any feeling for the methods by which science has been advanced. He was a stranger to quantitative work, and he had an aversion from the method of the working hypothesis, to which science actually owes its advances. One instance of this is his condemnation of the Copernican system, which he holds to be "the speculations of one who cares not what fictions he introduces into nature provided his calculations answer".

It is characteristic of Bacon that, while continually laying stress on his own work as a fore-runner, and of his importance as *instaurator artium*, he totally ignores the pioneering scientific work of his great contemporaries. He says nothing of Galileo, although it is quite clear, from a letter of Sir Tobie Matthew's which we possess, that the work had been brought to his notice. He derides Gilbert, and when he himself discusses the magnet he ignores the repulsion between like poles. Kepler and Harvey made no impression upon him, although, as we have seen, he was in close personal touch with the latter. In short, he could not recognize real scientific advances when he saw them.

In spite of Bacon's attack on Aristotle, his outlook was really Aristotelian. He says, for example, that we must investigate what bodies are light, what heavy, and what neither light nor

heavy. His whole treatment of motion is spun from his own mind, in the Aristotelian tradition. He lays down, for example, in the "*Novum Organum*", that there are nineteen different kinds of motion, which he describes at length in the spirit of the Middle Ages, rather than that of science as we understand it. Of the last kind of motion, for example, he says, "let the nineteenth and last motion be one which can scarcely be termed a motion, and yet is one; and which we may call the motion of *repose*, or of abhorrence of motion. It is by this motion that the earth stands by its own weight. . . . It is owing to the same tendency, that all bodies of considerable density abhor motion, and their only tendency is not to move, which nature they preserve although excited and urged in a variety of ways to motion."

For a general description of Bacon's method we will, so as to avoid any appearance of prejudice, adopt that of his worshipper, Macaulay: "Make as large a list as possible of those bodies in which, however widely they differ from each other in appearance, we perceive heat: and as large a list as possible of those which, while they bear a general resemblance to hot bodies, are nevertheless not hot. Observe the different degrees of heat in different hot bodies: and then, if there be something which is found in all hot bodies, and of which the increase or diminution is always accompanied by an increase or diminution of heat, we may hope that we have really discovered the object of our search." We will only add that among the hot bodies which he classifies are horse-dung and pepper.

The collection of the material Bacon was prepared to delegate to any journeyman. The man of science was then to go through this material in a mechanical way, according to the prescribed rules, and so make great discoveries. I hold that we can, without unfairness, say that he worked out elaborate and impossible schemes for investigating phenomena of no importance. On the other hand, it is indisputable that he was often cited with admiration by some of the great experimenters who founded the Royal Society, and held up as the man who fought for the experimental method. He appears, for example, in the frontispiece designed by John Evelyn and etched by Wenceslas Hollar for Spratt's "*History of the Royal Society*", which, published in 1667, is really a history of the foundation and objects of the Royal Society. Robert Boyle, in particular, always speaks of him with the highest praise,

even though he points out that Bacon's determination of the specific gravity of quicksilver is somewhat incorrect: "Whereby it appears the weight of water to quicksilver is as $1:13\frac{1}{2}$, though our illustrious Verulam (questionless not for want of judgement or care, but of exact instruments) makes the proportion between these two liquors to be greater than $1:17$."

Newton, however, never refers to Bacon, and Hooke certainly very seldom. I believe that the respect that some working men of science in the seventeenth century felt for him was due to his lofty position as Lord Chancellor, and to the fact that he, influential and exalted as he was, spoke so strongly of the need for the experimental method, and attacked Aristotle. That Bacon had no conception of the right way of applying the experimental method, and himself used the terminology and almost the method of Aristotle, is unimportant compared with the fact that he continually spoke of the need for a new method which should handle observed properties. Again, in the "New Atlantis", Bacon describes Solomon's House, "dedicated to the study of the Works and Creatures of God", which was his scheme of a great institution for experimental research. A foundation of this kind was the dream of the men who did most to found the Royal Society, and Bacon's fiction of a munificently endowed college for scientific research was one that set him high in the esteem of the enthusiasts of the new learning.

Renee Descartes offered a mechanical explanation of the universe, evolved by him from a philosophical theory of first causes. His system had a far-reaching influence. Its ready acceptance was, no doubt, due to the way in which he explained all phenomena in general and pictorial terms, which could be followed without any mathematical discipline, for, great as were Descartes's mathematical powers, his mechanism of the universe was purely quantitative. In spite of the overthrow of the system by Newton in 1687, his influence continued far beyond the end of the seventeenth century, especially in France, where great controversies between Cartesians and Newtonians took place round about 1735.

Descartes set out, in the Aristotelian spirit, to build up his scheme from first causes, and he criticizes Galileo for ignoring such causes, saying that by doing so he had built without foundation. Descartes assumed extension as the essential property of matter: as there could be no extension

Fig. 3.

DESCARTES'S SCHEME OF VORTICES, SHOWING THE VORTEX OF THE SOLAR SYSTEM IN THE MIDDLE.

without matter, there could be no vacuum. Motion of matter was only possible if something material occupied the space vacated by the moving body. It was from arguments of this kind that Descartes evolved his vortices, in which matter moved in closed paths. He distinguished three kinds of matter. The original kind, called into being by the Creator, consisted of medium-sized particles, which, from his fundamental ideas, cannot have been spherical but must have filled

all space without interstices. These particles, when rubbed down by their continual motion, constituted his second kind of particles, or heavenly matter. The dust rubbed off from them constituted the first kind of matter. Some of these particles forced through the interstices of the particles of the second kind, became spiral and matted together to form the third kind of particles. The luminous sun and stars were formed of the first kind of matter, the heavens of the second, and the crust of the earth and planets were formed of the third kind of particle.

The heavenly vortices (Fig. 3) themselves consisted of circulations of particles of the second kind, carried round by the rotation of the central bodies. The planets were carried round by

that bodies which move in a circle always tend to move away from the centre of the circle. The third law dealt with impact.

THE ROYAL SOCIETY AND ROBERT HOOKE

Although he dealt with the laws of impact, and with the laws of optics, Descartes laid no emphasis on experiment. He did, however, attempt a general synthesis, explaining heavenly and terrestrial mechanics in terms of the same conceptions, and so made a great advance. Descartes's life ran from 1596 to 1650. Somewhere towards the end of it (the exact time is not known) Guericke carried out his great experiment on the formation of a vacuum, but Guericke's book "De Vacuo Spatio" was not published until 1672.

In the meantime, Copernican views were gaining strength. In 1638, for example, John Wilkins published a stout defence of the Copernican system. John Wilkins is a figure of great importance for English science, although he carried out no original work, for he was instrumental in the formation of the Royal Society, the foundation of which, with the other great European academies, the Académie Royale des Sciences and the Accademia del Cimento, gave an immense impulse to the experimental method.

It was in 1645 that Wilkins began to have in his rooms those gatherings of natural philosophers which formed the beginnings of the Royal Society. We have not leisure to trace the history of these meetings, nor the relative parts played by Oxford and London. Suffice it to say that the Royal Society was founded in 1662, with Wilkins as the first secretary, and numbered among its original members Seth Ward, Robert Boyle, Sir William Petty, Lord Brouncker, Robert Hooke, John Wallis, Francis Willughby and Sir Christopher Wren, as well as Christian Huygens.

The foundation of the Royal Society was followed by a burst of scientific discovery, in which Robert Hooke played a great part. Robert Hooke, physically insignificant, crooked and weakly, was one of the greatest mechanical geniuses that the world has seen: Pepys wrote of him "who is the most, and promises the least, of any man in the world that ever I saw" He lacked Newton's gigantic mathematical powers and searching analysis, but he had a wonderful instinct for scientific truth, and an extraordinary fertility of invention. One or two quotations from

Fig. 4.

DESCARTES'S SCHEME OF THE EARTH, SHOWING THE FORMATION OF MOUNTAINS.

the sun's vortex. Descartes develops his system in great detail, making it account for the formation of the earth, with its mountains due to the movements of the crust (Fig. 4); for the phenomena of light and magnetism, in which latter connexion he was the first to show magnetic lines of force by means of iron filings; and for the motion of the heavenly bodies, including the comets, although the latter gave him particular difficulty. The whole of his heavenly mechanics is purely descriptive: he gives the particles of his vortices any velocities and properties that he likes, and he makes no attempt, for example, to express quantitative laws, such as Kepler's laws, in terms of his fundamental conceptions.

Nevertheless, Descartes does lay down laws of motion, which foreshadow Newton's laws. His first law is practically the law of inertia—every body, so far as it can, will persist in its particular state, so that what is once in movement will always be in movement. The second law is that every body tends to move in a straight line and

his "Attempt to prove the motion of the earth", published in 1674, may give a taste of his quality. In this treatise he describes, among other things, a very careful attempt which he made to measure the parallax due to the earth's motion with reference to the fixed stars, and he correctly attributes his negative result to the distances of these stars, saying:

"To me indeed the Universe seems to be vastly bigger then 'tis hitherto asserted by any Writer, when I consider the many differing magnitudes of the fixt Stars, and the continual increase of their number according as they are looked after with better and longer Telescopes. And could we certainly determine and measure their Diameters, and distinguish what part of their appearing magnitude were to be attributed to their bulk, and what to their brightness, I am apt to believe we should make another distribution of their magnitudes, then what is already made by *Ptolomy, Ticho, Kepler, Bayer, Clavius, Grienbergerus, Piff, Hevelius* and others."

At the end of the treatise Hooke quotes three "suppositions", upon which he says the system of the world should be founded:

"First, That all Cœlestial Bodies whatsoever, have an attraction or gravitating power towards their own Centers, whereby they attract not only their own parts, and keep them from flying from them, as we may observe the Earth to do, but that they do also attract all the other Cœlestial Bodies that are within the sphere of their activity; . . . The second supposition is this, That all bodies whatsoever that are put into a direct and simple motion, will so continue to move forward in a straight line, till they are by some other effectual powers deflected and bent into a Motion, describing a Circle, Ellipsis, or some other compounded Curve Line. The third supposition is, That their attractive powers are so much the more powerful in operating, by how much the nearer the body wrought upon is to their own Centers. Now what these several degrees are I have not yet experimentally verified; but it is a notion, which if fully prosecuted as it ought to be, will mightily assist the Astronomer to reduce all the Cœlestial Motions to a certain rule, which I doubt will never be done true without it."

Before saying anything further about the part that Hooke played as a forerunner of Newton, we will briefly refer to some of his performances in other fields. He was the first man to make a really successful compound microscope, and with it made numerous discoveries, including that of cells in plants, which he was the first to name. In connexion with his work on springs he enunciated Hooke's law, and he discovered the balance wheel

of watches, which probably Huygens discovered independently—the matter is disputed. He invented a group of meteorological instruments—a hygrometer, an anemometer, a self-registering thermometer and a self-registering rain gauge. In astronomy he devised the first clock-driven telescope (Fig. 5), and the first circle-dividing machine, which Ramsden afterwards perfected, and he insisted upon the importance of telescopic sights in this connexion, explaining and measuring the resolving power of the eye. He was the first to carry out systematic experiments on the colours of thin plates, and he invented an early form of Savart's wheel, to be used as a siren. His geological work, especially that on fossils, is fundamental. Many other instances could be given of his fertile genius.

Returning to the laws of celestial motion, in 1679 Hooke, who was then secretary of the Royal Society, wrote to Newton to ask him for a paper, to be read before the Society. Newton replied with a letter concerning the place where a falling body would hit the surface of the earth, pointing out that owing to the motion of the earth, it would fall east of the perpendicular. He also stated that the path should be a portion of a spiral terminating at the centre of the earth. Commenting on this letter, Hooke said that Newton was in error: the path would be an "eccentric elliptoid, supposing no resistance of the medium: but supposing a resistance, it would be an eccentric ellipti-spiral . . . the fall of the body would not be perfectly east as Mr. Newton supposed: but to the south-east, and more to the south than the east". What Hooke meant by an "eccentric elliptoid" we can only conjecture. Probably he meant a curve something like an ellipse, with the attracting point away from the centre: his extraordinary physical instinct may well have been sufficient to assure him of this without his being able to prove that it was an ellipse. Hooke's criticisms were substantially correct, as Newton realized, and to be caught in error irritated Newton in the highest degree. In his reply Newton admitted that the body would fall to the south, and that "if gravity be supposed uniform it will not descend in the spiral to the very centre, but circulate with an alternating ascent and descent made by its *vis centrifuga* and gravity alternately overbalancing one another".

It is clear from this and the rest of the reply, that Newton had not yet attained his later clarity on the problem of gravitation. There is little

doubt that this correspondence revived his interest in the problem, which, as we shall see, had been very keen some thirteen years earlier. The bitterness between Newton and Hooke, which continued until Hooke's death, was a sad feature of the lives of two great men, both wholeheartedly devoted to scientific truth. It seems certain that by the intercession of tactful friends it might have been much diminished, if not entirely removed. Unfortunately, it was exacerbated by Oldenburg, long the secretary of the Royal Society, who missed no opportunity of embittering the relationship.

stated with the greatest clarity the attitude which has dominated physics from his time until the present day, with possibly one or two very recent exceptions. This attitude we cannot express better than in his own words,

"Hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses, *hypotheses non fingo*; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have

Fig. 5.

AN ENGRAVING FROM HOOKE'S "ANIMADVERSIONS", SHOWING HOOKE'S JOINT, THE CLOCK-DRIVEN TELESCOPE, THE METHOD OF DIAGONAL DIVISIONS, STEPPED AND HELICAL GEARS AND THE BUBBLE LEVEL. THE LAST-NAMED IS CLAIMED FOR THEVENOT, BUT THERE SEEMS NO DOUBT THAT HOOKE INVENTED IT INDEPENDENTLY.

ISAAC NEWTON: LAWS AND HYPOTHESES

There is general agreement that Newton is the greatest scientific mind that the world has known. He produced that body of mechanics, founded on a few precise laws—the three laws of motion and the inverse square law of gravitation—which, in its essence, is still sufficient to cover the whole body of celestial and terrestrial mechanics of macroscopic bodies moving with velocities small compared with that of light. He founded the science of mathematical physics, and was, in addition, an experimenter of the very highest order, as his "Opticks" and, for example, his experiments with pendulums suffice to show. He also

no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction. Thus it was that the impenetrability, the mobility, and the impulsive force of bodies, and the laws of motion and of gravitation, were discovered. To us it is enough that gravity does really exist, and act according to the laws which we have explained, and abundantly serves to account for all the motions of the celestial bodies, and of our sea."

His attitude here expressed is in direct contradiction to that of Descartes, who made hypotheses as a result of pure reflection and from these hypotheses constructed the world.

The history of Newton's theory of gravity begins at Woolsthorpe, where he was in 1665–66 to avoid the plague. His own account of this period was written in 1714, and his recollections may have been coloured by what had happened in the intervening period, but his words are of such interest that I cannot refrain from giving them :

"In the same year (1666) I began to think of gravity extending to the orb of the moon, and having found out how to estimate the force with which a globe revolving within a sphere presses the surface of the sphere, from Kepler's Rule of the periodical times of the planets being in a sesquialterate proportion of their distances from the centres of their orbs I deduced that the forces which keep the planets in their orbs must [be] reciprocally as the squares of their distances from the centres about which they revolve: and thereby compared the force requisite to keep the moon in her orb with the force of gravity at the surface of the earth, and found them answer pretty nearly. All this was in the two plague years of 1665 and 1666, for in those days I was in the prime of my age for invention, and minded mathematics and philosophy more than at any time since."

The extraordinary thing is that, at the time, Newton published nothing on the subject—extraordinary, perhaps, but less extraordinary with Newton than with any other man, because he always had to be urged to publication, largely, I think, because of his horror of controversy. The generally accepted story is that, owing to a discrepancy of some 16 per cent, due to his taking an incorrect radius of the earth then current, Newton put the work aside, being dissatisfied with this degree of accuracy. This does not, however, seem likely: nobody better than Newton could estimate what degree of precision was likely to have been attained in a given measurement. We must remember the hypotheses on which the calculation is based, namely, that the gravitational effect of the earth extends to the moon, falling off inversely as the square of the distance, and that the gravitational acceleration at the surface of the earth is only another manifestation of the earth's attraction. It is clear that when we are considering the moon it does not much matter at what particular point of the earth we consider the earth's mass to be concentrated, but when we are considering the surface acceleration this point is of the greatest importance. It is far from being obvious that the gravitational force at the surface of a sphere, each small part of which is exerting a force proportional to its mass and inversely proportional to the square of the distance,

is the same as it would be if the whole of the mass of the sphere were concentrated at its centre. Until this has been proved the whole calculation is unsatisfactory, especially to a mind so acute and so scientifically honest as that of Newton. He would see at once that criticism could fasten on this point, and it seems far more likely that he put aside the work on account of his inability to answer such criticism than on account of the discrepancy to which his silence is usually attributed.

By 1685 Newton had succeeded in proving that the gravitational attraction of a spherical shell of uniform density on an external point is the same as it would be if the mass were concentrated at the centre. There was another point of difficulty: it was by no means clear *a priori* that the gravitational attraction was proportional to the motional inertia of a body, or that the ratio of the two was the same for all bodies. This point Newton settled by his famous experiments of the hollow pendulum carried out with enclosures of equal masses of very different substances—metals, glass, sand, salt, wood, wheat.

The incident of 1679, to which we have referred, brought Newton back to the problem of the orbit of a particle in a central gravitational field. In January 1683/4, Edmond Halley had a talk with Christopher Wren and Hooke on this problem. All three, apparently, tended towards the belief that celestial mechanics could be explained on the inverse square law of attraction, and Hooke declared that he was able to demonstrate this mathematically. Wren said that he would give a prize of "a book of 40s." to either of the others who could, in fact, bring a convincing demonstration. It is quite clear that Hooke never did so, and equally clear that he was incapable of such a mathematical feat. In August 1684, Halley, being at Cambridge, asked Newton what would be the curve described by a planet in an inverse square field of force and Newton immediately answered, "An ellipse", and, in reply to an amazed inquiry, said that he had calculated it. Later he produced the proof.

There is no space here to discuss how Halley instigated Newton to write the "Principia", saw the work through the press, and, although not a rich man, bore the charges. It is pleasant to reflect that he probably got his money back, as the work sold very quickly. As a minor historical mystery there seems no certainty at what price it was sold: More says probably for ten or twelve shillings a copy.

The spirit of the "Principia" may be said to be summarized in the first sentence of the "Opticks", "My design in this Book is not to explain the Properties of Light by Hypotheses, but to propose and prove them by Reason and Experiments". The book is astonishingly rich in matter fundamental to mathematical physics, containing, for example, the foundations of the theory of sound and of hydrodynamics. The treatment of Descartes's theory of vortices is typical. Newton points out that it is impossible, on Descartes's theory, to give a quantitative explanation of celestial mechanics, in particular to deduce Kepler's laws. In order to drag the planets round, the fluid matter of the vortices must behave in a viscous manner. Newton does not actually use the phrase "viscous liquid", but he lays down the fundamental behaviour of such liquids in the following "hypothesis": "the resistance arising from the want of lubricity in the paths of a fluid is, other things being equal, proportional to the velocity with which the parts are separated from one another", that is, the shear in a viscous fluid is proportional to the velocity gradient. From this he deduces that the periodic times of the paths of a fluid surrounding a revolving sphere are proportional to the squares of the distances from the centre of the sphere. He further points out fundamental difficulties in the vortex hypothesis in connexion with the motions at perihelion. He does not discuss the adequacy of the vortex hypothesis in general terms, but demonstrates clearly that the hypothesis could not account for the observed laws of planetary motion. The situation reminds us of the fate of the nineteenth century ether of space, which gave the same kind of easily grasped picture as did the vortices, but which had to be abandoned because it would not give the quantitative results required by the Michelson-Morley experiment.

Newton's attitude towards the question of the nature of gravitational force is typical of the man. Richard Bentley pressed him strongly to declare himself on this point, and we possess the letters which passed on both sides. Two passages may be cited as of particular interest: incidentally, in the Royal Institution copy of Horsley's "Newton" these passages are marked in pencil, and in each case we have the note at the side, in Dr. Bence-Jones's handwriting, "Mr. Faraday's marks". Newton says,

"You sometimes speak of gravity as essential and inherent to matter. Pray do not ascribe that notion to me; for the cause of gravity is what I do not pretend to know, and therefore would take some time to consider it", and again, "It is inconceivable, that inanimate brute matter should, without the mediation of something else, which is not material, operate upon, and affect other matter without mutual contact; as it must do, if gravitation, in the sense of *Epicurus*, be essential and inherent in it. And this is one reason why I desired you would not ascribe innate gravity to me. That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the meditation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers".

This completes the particular story which I have tried to tell. What Newton ignores is what Aristotle and Descartes tried to start with.

I have endeavoured to show how, within a century, the method of constructing Nature by reasoning, more or less logically, from accepted philosophical ideas, was replaced by the method of appealing to Nature by experiment, and adopting only such laws as were in quantitative agreement with the results of experiment. I have tried to point out how the search in the mind for first causes gave way to a search in Nature for observable order and regularity, coupled with a disinclination to speculate beyond the facts. In a recent Friday Evening Discourse at the Royal Institution, Prof. Dingle spoke of a return to the Aristotelian method, which he detected, and deprecated, in the writings of one or two of our great contemporaries in science. What's to come is still unsure—it may be that a new spirit in science is being born, just as it may be that future ages will envy us for having been contemporary with the creation of Mr. T. S. Eliot's austere poems and Mr. Epstein's mighty monoliths. Be the future, however, what it may, it is the spirit of Newton that has guided us hitherto and that is the legitimate father of the work of a Faraday, a Rutherford and a Bragg.