A History of Science, V2

Henry Smith Williams

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A
HISTORY OF SCIENCE
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BOOK II. THE BEGINNINGS OF MODERN SCIENCE

The studies of the present book cover the progress of science from the close of the Roman period in the fifth century A.D. to about the middle of the eighteenth century. In tracing the course of events through so long a period, a difficulty becomes prominent which everywhere besets the historian in less degree—a difficulty due to the conflict between the strictly chronological and the topical method of treatment. We must hold as closely as possible to the actual sequence of events, since, as already pointed out, one discovery leads on to another. But, on the other hand, progressive steps are taken contemporaneously in the various fields of science, and if we were to attempt to introduce these in strict chronological order we should lose all sense of topical continuity.

Our method has been to adopt a compromise, following the course of a single science in each great epoch to a convenient stopping−point, and then turning back to bring forward the story of another science. Thus, for example, we tell the story of Copernicus and Galileo, bringing the record of cosmical and mechanical progress down to about the middle of the seventeenth century, before turning back to take up the physiological progress of the fifteenth and sixteenth centuries. Once the latter stream is entered, however, we follow it without interruption to the time of Harvey and his contemporaries in the middle of the seventeenth century, where we leave it to return to the field of mechanics as exploited by the successors of Galileo, who were also the predecessors and contemporaries of Newton.

In general, it will aid the reader to recall that, so far as possible, we hold always to the same sequences of topical treatment of contemporary events; as a rule we treat first the cosmical, then the physical, then the biological sciences. The same order of treatment will be held to in succeeding volumes.

Several of the very greatest of scientific generalizations are developed in the period covered by the present book: for example, the Copernican theory of the solar system, the true doctrine of planetary motions, the laws of motion, the theory of the circulation of the blood, and the Newtonian theory of gravitation. The labors of the investigators of the early decades of the eighteenth century, terminating with Franklin's discovery of the nature of lightning and with the Linnaean classification of plants and animals, bring us to the close of our second great epoch; or, to put it otherwise, to the threshold of the modern period,

I. SCIENCE IN THE DARK AGE

An obvious distinction between the classical and mediaeval epochs may be found in the fact that the former produced, whereas the latter failed to produce, a few great thinkers in each generation who were imbued with that scepticism which is the foundation of the investigating spirit; who thought for themselves and supplied more or less rational explanations of observed phenomena. Could we eliminate the work of some score or so of classical observers and thinkers, the classical epoch would seem as much a dark age as does the epoch that succeeded it.

But immediately we are met with the question: Why do no great original investigators appear during all these later centuries? We have already offered a part explanation in the fact that the borders of civilization, where racial mingling naturally took place, were peopled with semi−barbarians. But we must not forget that in the centres of civilization all along there were many men of powerful intellect. Indeed, it would violate the principle of historical continuity to suppose that there was any sudden change in the level of mentality of the Roman world at the close of the classical period. We must assume, then, that the direction in which the great minds turned was for some reason changed. Newton is said to have alleged that he made his discoveries by "intending" his mind in a certain direction continuously. It is probable that the same explanation may be given of almost every great scientific discovery. Anaxagoras could not have thought out the theory of the moon's phases; Aristarchus could not have found out the true mechanism of the solar system; Eratosthenes could not have developed his plan for measuring the earth, had not each of these investigators "intended" his mind persistently towards the problems in question.

Nor can we doubt that men lived in every generation of the dark age who were capable of creative thought in the field of science, bad they chosen similarly to "intend" their minds in the right direction. The difficulty was that they did not so choose. Their minds had a quite different bent. They were under the spell of different ideals; all their mental efforts were directed into different channels. What these different channels were cannot be in doubt—they were the channels of oriental ecclesiasticism. One all−significant fact speaks volumes here. It is the fact that, as Professor Robinson[1] points out, from the time of Boethius (died 524 or 525 A.D.) to that of Dante (1265−1321 A.D.) there was not a single writer of renown in western Europe who was not a professional churchman. All the learning of the time, then, centred in the priesthood. We know that the same condition of things pertained in Egypt, when science became static there. But, contrariwise, we have seen that in Greece and early Rome the scientific workers were largely physicians or professional teachers; there was scarcely a professional theologian among them.

Similarly, as we shall see in the Arabic world, where alone there was progress in the mediaeval epoch, the learned men were, for the most part, physicians. Now the meaning of this must be self−evident. The physician naturally "intends" his mind towards the practicalities. His professional studies tend to make him an investigator of the operations of nature. He is usually a sceptic, with a spontaneous interest in practical science. But the theologian "intends" his mind away from practicalities and towards mysticism. He is a professional believer in the supernatural; he discounts the value of merely "natural" phenomena. His whole attitude of mind is unscientific; the fundamental tenets of his faith are based on alleged occurrences which inductive science cannot admit—namely, miracles. And so the minds "intended" towards the supernatural achieved only the hazy mysticism of mediaeval thought. Instead of investigating natural laws, they paid heed (as, for example, Thomas Aquinas does in his Summa Theologia) to the "acts of angels," the "speaking of angels," the "subordination of angels," the "deeds of guardian angels," and the like. They disputed such important questions as, How many angels can stand upon the point of a needle? They argued pro and con as to whether Christ were coeval with God, or whether he had been merely created "in the beginning," perhaps ages before the creation of the world. How could it be expected that science should flourish when the greatest minds of the age could concern themselves with problems such as these?

Despite our preconceptions or prejudices, there can be but one answer to that question. Oriental superstition cast its blight upon the fair field of science, whatever compensation it may or may not have brought in other fields. But we must be on our guard lest we overestimate or incorrectly estimate this influence. Posterity, in glancing backward, is always prone to stamp any given age of the past with one idea, and to desire to characterize it with a single phrase; whereas in reality all ages are diversified, and any generalization regarding an epoch is sure to do that epoch something less or something more than justice. We may be sure, then, that the ideal of ecclesiasticism is not solely responsible for the scientific stasis of the dark age. Indeed, there was another influence of a totally different character that is too patent to be overlooked—the influence, namely, of the economic condition of western Europe during this period. As I have elsewhere pointed out,[2] Italy, the centre of western civilization, was at this time impoverished, and hence could not provide the monetary stimulus so essential to artistic and scientific no less than to material progress. There were no patrons of science and literature such as the Ptolemies of that elder Alexandrian day. There were no great libraries; no colleges to supply opportunities and afford stimuli to the rising generation. Worst of all, it became increasingly difficult to secure books.

This phase of the subject is often overlooked. Yet a moment's consideration will show its importance. How should we fare to−day if no new scientific books were being produced, and if the records of former generations were destroyed? That is what actually happened in Europe during the Middle Ages. At an earlier day books were made and distributed much more abundantly than is sometimes supposed. Bookmaking had, indeed, been an important profession in Rome, the actual makers of books being slaves who worked under the direction of a publisher. It was through the efforts of these workers that the classical works in Greek and Latin were multiplied and disseminated. Unfortunately the climate of Europe does not conduce to the indefinite preservation of a book; hence very few remnants of classical works have come down to us in the original from a remote period. The rare exceptions are certain papyrus fragments, found in Egypt, some of which are Greek manuscripts dating from the third century B.C. Even from these sources the output is meagre; and the only other repository of classical books is a single room in the buried city of Herculaneum, which contained several hundred manuscripts, mostly in a charred condition, a considerable number of which, however, have been unrolled and found more or less legible. This library in the buried city was chiefly made up of philosophical works, some of which were quite unknown to the modern world until discovered there.

But this find, interesting as it was from an archaeological stand−point, had no very important bearing on our knowledge of the literature of antiquity. Our chief dependence for our knowledge of that literature must still be placed in such copies of books as were made in the successive generations. Comparatively few of the extant manuscripts are older than the tenth century of our era. It requires but a momentary consideration of the conditions under which ancient books were produced to realize how slow and difficult the process was before the invention of printing. The taste of the book−buying public demanded a clearly written text, and in the Middle Ages it became customary to produce a richly ornamented text as well. The script employed being the prototype of the modern printed text, it will be obvious that a scribe could produce but a few pages at best in a day. A large work would therefore require the labor of a scribe for many months or even for several years. We may assume, then, that it would be a very flourishing publisher who could produce a hundred volumes all told per annum; and probably there were not many publishers at any given time, even in the period of Rome's greatest glory, who had anything like this output.

As there was a large number of authors in every generation of the classical period, it follows that most of these authors must have been obliged to content themselves with editions numbering very few copies; and it goes without saying that the greater number of books were never reproduced in what might be called a second edition. Even books that retained their popularity for several generations would presently fail to arouse sufficient interest to be copied; and in due course such works would pass out of existence altogether. Doubtless many hundreds of books were thus lost before the close of the classical period, the names of their authors being quite forgotten, or preserved only through a chance reference; and of course the work of elimination went on much more rapidly during the Middle Ages, when the interest in classical literature sank to so low an ebb in the West. Such collections of references and quotations as the Greek Anthology and the famous anthologies of Stobaeus and Athanasius and Eusebius give us glimpses of a host of writers—more than seven hundred are quoted by Stobaeus—a very large proportion of whom are quite unknown except through these brief excerpts from their lost works.

Quite naturally the scientific works suffered at least as largely as any others in an age given over to ecclesiastical dreamings. Yet in some regards there is matter for surprise as to the works preserved. Thus, as we have seen, the very extensive works of Aristotle on natural history, and the equally extensive natural history of

Pliny, which were preserved throughout this period, and are still extant, make up relatively bulky volumes. These works seem to have interested the monks of the Middle Ages, while many much more important scientific books were allowed to perish. A considerable bulk of scientific literature was also preserved through the curious channels of Arabic and Armenian translations. Reference has already been made to the Almagest of Ptolemy, which, as we have seen, was translated into Arabic, and which was at a later day brought by the Arabs into western Europe and (at the instance of Frederick II of Sicily) translated out of their language into mediaeval Latin.

It remains to inquire, however, through what channels the Greek works reached the Arabs themselves. To gain an answer to this question we must follow the stream of history from its Roman course eastward to the new seat of the Roman empire in Byzantium. Here civilization centred from about the fifth century A.D., and here the European came in contact with the civilization of the Syrians, the Persians, the Armenians, and finally of the Arabs. The Byzantines themselves, unlike the inhabitants of western Europe, did not ignore the literature of old Greece; the Greek language became the regular speech of the Byzantine people, and their writers made a strenuous effort to perpetuate the idiom and style of the classical period. Naturally they also made transcriptions of the classical authors, and thus a great mass of literature was preserved, while the corresponding works were quite forgotten in western Europe.

Meantime many of these works were translated into Syriac, Armenian, and Persian, and when later on the Byzantine civilization degenerated, many works that were no longer to be had in the Greek originals continued to be widely circulated in Syriac, Persian, Armenian, and, ultimately, in Arabic translations. When the Arabs started out in their conquests, which carried them through Egypt and along the southern coast of the Mediterranean, until they finally invaded Europe from the west by way of Gibraltar, they carried with them their translations of many a Greek classical author, who was introduced anew to the western world through this strange channel.

We are told, for example, that Averrhoes, the famous commentator of Aristotle, who lived in Spain in the twelfth century, did not know a word of Greek and was obliged to gain his knowledge of the master through a Syriac translation; or, as others alleged (denying that he knew even Syriac), through an Arabic version translated from the Syriac. We know, too, that the famous chronology of Eusebius was preserved through an Armenian translation; and reference has more than once been made to the Arabic translation of Ptolemy's great work, to which we still apply its Arabic title of Almagest.

The familiar story that when the Arabs invaded Egypt they burned the Alexandrian library is now regarded as an invention of later times. It seems much more probable that the library bad been largely scattered before the coming of the Moslems. Indeed, it has even been suggested that the Christians of an earlier day removed the records of pagan thought. Be that as it may, the famous Alexandrian library had disappeared long before the revival of interest in classical learning. Meanwhile, as we have said, the Arabs, far from destroying the western literature, were its chief preservers. Partly at least because of their regard for the records of the creative work of earlier generations of alien peoples, the Arabs were enabled to outstrip their contemporaries. For it cannot be in doubt that, during that long stretch of time when the western world was ignoring science altogether or at most contenting itself with the casual reading of Aristotle and Pliny, the Arabs had the unique distinction of attempting original investigations in science. To them were due all important progressive steps which were made in any scientific field whatever for about a thousand years after the time of Ptolemy and Galen. The progress made even by the Arabs during this long period seems meagre enough, yet it has some significant features. These will now demand our attention.

II. MEDIAEVAL SCIENCE AMONG THE ARABIANS

The successors of Mohammed showed themselves curiously receptive of the ideas of the western people whom they conquered. They came in contact with the Greeks in western Asia and in Egypt, and, as has been said, became their virtual successors in carrying forward the torch of learning. It must not be inferred, however, that the Arabian scholars, as a class, were comparable to their predecessors in creative genius. On the contrary, they retained much of the conservative oriental spirit. They were under the spell of tradition, and, in the main, what they accepted from the Greeks they regarded as almost final in its teaching. There were, however, a few notable exceptions among their men of science, and to these must be ascribed several discoveries of some importance.

The chief subjects that excited the interest and exercised the ingenuity of the Arabian scholars were astronomy, mathematics, and medicine. The practical phases of all these subjects were given particular attention. Thus it is well known that our so−called Arabian numerals date from this period. The revolutionary effect of these characters, as applied to practical mathematics, can hardly be overestimated; but it is generally considered, and in fact was admitted by the Arabs themselves, that these numerals were really borrowed from the Hindoos, with whom the Arabs came in contact on the east. Certain of the Hindoo alphabets, notably that of the Battaks of Sumatra, give us clews to the originals of the numerals. It does not seem certain, however, that the Hindoos employed these characters according to the decimal system, which is the prime element of their importance. Knowledge is not forthcoming as to just when or by whom such application was made. If this was an Arabic innovation, it was perhaps the most important one with which that nation is to be credited. Another mathematical improvement was the introduction into trigonometry of the sine—the half−chord of the double arc—instead of the chord of the arc itself which the Greek astronomers had employed. This improvement was due to the famous Albategnius, whose work in other fields we shall examine in a moment.

Another evidence of practicality was shown in the Arabian method of attempting to advance upon Eratosthenes' measurement of the earth. Instead of trusting to the measurement of angles, the Arabs decided to measure directly a degree of the earth's surface—or rather two degrees. Selecting a level plain in Mesopotamia for the experiment, one party of the surveyors progressed northward, another party southward, from a given point to the distance of one degree of arc, as determined by astronomical observations. The result found was fifty−six miles for the northern degree, and fifty−six and two−third miles for the southern. Unfortunately, we do not know the precise length of the mile in question, and therefore cannot be assured as to the accuracy of the measurement. It is interesting to note, however, that the two degrees were found of unequal lengths, suggesting that the earth is not a perfect sphere—a suggestion the validity of which was not to be put to the test of conclusive measurements until about the close of the eighteenth century. The Arab measurement was made in the time of Caliph Abdallah al−Mamun, the son of the famous Harun−al−Rashid. Both father and son were famous for their interest in science. Harun−al−Rashid was, it will be recalled, the friend of Charlemagne. It is said that he sent that ruler, as a token of friendship, a marvellous clock which let fall a metal ball to mark the hours. This mechanism, which is alleged to have excited great wonder in the West, furnishes yet another instance of Arabian practicality.

Perhaps the greatest of the Arabian astronomers was Mohammed ben Jabir Albategnius, or El−batani, who was born at Batan, in Mesopotamia, about the year 850 A.D., and died in 929. Albategnius was a student of the Ptolemaic astronomy, but he was also a practical observer. He made the important discovery of the motion of the solar apogee. That is to say, he found that the position of the sun among the stars, at the time of its greatest distance from the earth, was not what it had been in the time of Ptolemy. The Greek astronomer placed the sun in longitude 65 degrees, but Albategnius found it in longitude 82 degrees, a distance too great to be accounted for by inaccuracy of measurement. The modern inference from this observation is that the solar system is moving through space; but of course this inference could not well be drawn while the earth was regarded as the fixed centre of the universe.

In the eleventh century another Arabian discoverer, Arzachel, observing the sun to be less advanced than Albategnius had found it, inferred incorrectly that the sun had receded in the mean time. The modern explanation of this observation is that the measurement of Albategnius was somewhat in error, since we know that the sun's motion is steadily progressive. Arzachel, however, accepting the measurement of his predecessor, drew the false inference of an oscillatory motion of the stars, the idea of the motion of the solar system not being permissible. This assumed phenomenon, which really has no existence in point of fact, was named the "trepidation of the fixed stars," and was for centuries accepted as an actual phenomenon. Arzachel explained this supposed phenomenon by assuming that the equinoctial points, or the points of intersection of the equator and the ecliptic, revolve in circles of eight degrees' radius. The first points of Aries and Libra were supposed to describe the circumference of these circles in about eight hundred years. All of which illustrates how a difficult and false explanation may take the place of a simple and correct one. The observations of later generations have shown conclusively that the sun's shift of position is regularly progressive, hence that there is no "trepidation" of the stars and no revolution of the equinoctial points.

If the Arabs were wrong as regards this supposed motion of the fixed stars, they made at least one correct observation as to the inequality of motion of the moon. Two inequalities of the motion of this body were already known. A third, called the moon's variation, was discovered by an Arabian astronomer who lived at Cairo and observed at Bagdad in 975, and who bore the formidable name of Mohammed Aboul Wefaal−Bouzdjani. The inequality of motion in question, in virtue of which the moon moves quickest when she is at new or full, and slowest at the first and third quarter, was rediscovered by Tycho Brahe six centuries later; a fact which in itself evidences the neglect of the Arabian astronomer's discovery by his immediate successors.

In the ninth and tenth centuries the Arabian city of Cordova, in Spain, was another important centre of scientific influence. There was a library of several hundred thousand volumes here, and a college where mathematics and astronomy were taught. Granada, Toledo, and Salamanca were also important centres, to which students flocked from western Europe. It was the proximity of these Arabian centres that stimulated the scientific interests of Alfonso X. of Castile, at whose instance the celebrated Alfonsine tables were constructed. A familiar story records that Alfonso, pondering the complications of the Ptolemaic cycles and epicycles, was led to remark that, had he been consulted at the time of creation, he could have suggested a much better and simpler plan for the universe. Some centuries were to elapse before Copernicus was to show that it was not the plan of the universe, but man's interpretation of it, that was at fault.

Another royal personage who came under Arabian influence was Frederick II. of Sicily—the "Wonder of the World," as he was called by his contemporaries. The Almagest of Ptolemy was translated into Latin at his instance, being introduced to the Western world through this curious channel. At this time it became quite usual for the Italian and Spanish scholars to understand Arabic although they were totally ignorant of Greek.

In the field of physical science one of the most important of the Arabian scientists was Alhazen. His work, published about the year 1100 A.D., had great celebrity throughout the mediaeval period. The original investigations of Alhazen had to do largely with optics. He made particular studies of the eye itself, and the names given by him to various parts of the eye, as the vitreous humor, the cornea, and the retina, are still retained by anatomists. It is known that Ptolemy had studied the refraction of light, and that he, in common with his immediate predecessors, was aware that atmospheric refraction affects the apparent position of stars near the horizon. Alhazen carried forward these studies, and was led through them to make the first recorded scientific estimate of the phenomena of twilight and of the height of the atmosphere. The persistence of a glow in the atmosphere after the sun has disappeared beneath the horizon is so familiar a phenomenon that the ancient philosophers seem not to have thought of it as requiring an explanation. Yet a moment's consideration makes it clear that, if light travels in straight lines and the rays of the sun were in no wise deflected, the complete darkness of night should instantly succeed to day when the sun passes below the horizon. That this sudden change does not occur, Alhazen explained as due to the reflection of light by the earth's atmosphere.

Alhazen appears to have conceived the atmosphere as a sharply defined layer, and, assuming that twilight continues only so long as rays of the sun reflected from the outer surface of this layer can reach the spectator at any given point, he hit upon a means of measurement that seemed to solve the hitherto inscrutable problem as to the atmospheric depth. Like the measurements of Aristarchus and Eratosthenes, this calculation of Alhazen is simple enough in theory. Its defect consists largely in the difficulty of fixing its terms with precision, combined with the further fact that the rays of the sun, in taking the slanting course through the earth's atmosphere, are really deflected from a straight line in virtue of the constantly increasing density of the air near the earth's surface. Alhazen must have been aware of this latter fact, since it was known to the later Alexandrian astronomers, but he takes no account of it in the present measurement. The diagram will make the method of Alhazen clear.

His important premises are two: first, the well−recognized fact that, when light is reflected from any surface, the angle of incidence is equal to the angle of reflection; and, second, the much more doubtful observation that twilight continues until such time as the sun, according to a simple calculation, is nineteen degrees below the horizon. Referring to the diagram, let the inner circle represent the earth's surface, the outer circle the limits of the atmosphere, C being the earth's centre, and RR radii of the earth. Then the observer at the point A will continue to receive the reflected rays of the sun until that body reaches the point S, which is, according to the hypothesis, nineteen degrees below the horizon line of the observer at A. This horizon line, being represented by AH, and the sun's ray by SM, the angle HMS is an angle of nineteen degrees. The complementary angle SMA is, obviously, an angle of (180−19) one hundred and sixty−one degrees. But since M is the reflecting surface and the angle of incidence equals the angle of reflection, the angle AMC is an angle of one−half of one hundred and sixty−one degrees, or eighty degrees and thirty minutes. Now this angle AMC, being known, the right−angled triangle MAC is easily resolved, since the side AC of that triangle, being the radius of the earth, is a known dimension. Resolution of this triangle gives us the length of the hypotenuse MC, and the difference between this and the radius (AC), or CD, is obviously the height of the atmosphere (h), which was the measurement desired. According to the calculation of Alhazen, this h, or the height of the atmosphere, represents from twenty to thirty miles. The modern computation extends this to about fifty miles. But, considering the various ambiguities that necessarily attended the experiment, the result was a remarkably close approximation to the truth.

Turning from physics to chemistry, we find as perhaps the greatest Arabian name that of Geber, who taught in the College of Seville in the first half of the eighth century. The most important researches of this really remarkable experimenter had to do with the acids. The ancient world had had no knowledge of any acid more powerful than acetic. Geber, however, vastly increased the possibilities of chemical experiment by the discovery of sulphuric, nitric, and nitromuriatic acids. He made use also of the processes of sublimation and filtration, and his works describe the water bath and the chemical oven. Among the important chemicals which he first differentiated is oxide of mercury, and his studies of sulphur in its various compounds have peculiar interest. In particular is this true of his observation that, tinder certain conditions of oxidation, the weight of a metal was lessened.

From the record of these studies in the fields of astronomy, physics, and chemistry, we turn to a somewhat extended survey of the Arabian advances in the field of medicine.

ARABIAN MEDICINE

The influence of Arabian physicians rested chiefly upon their use of drugs rather than upon anatomical knowledge. Like the mediaeval Christians, they looked with horror on dissection of the human body; yet there were always among them investigators who turned constantly to nature herself for hidden truths, and were ready to uphold the superiority of actual observation to mere reading. Thus the physician Abd el−Letif, while in Egypt, made careful studies of a mound of bones containing more than twenty thousand skeletons. While examining these bones he discovered that the lower jaw consists of a single bone, not of two, as had been taught by Galen. He also discovered several other important mistakes in Galenic anatomy, and was so impressed with his discoveries that he contemplated writing a work on anatomy which should correct the great classical authority's mistakes.

It was the Arabs who invented the apothecary, and their pharmacopoeia, issued from the hospital at Gondisapor, and elaborated from time to time, formed the basis for Western pharmacopoeias. Just how many drugs originated with them, and how many were borrowed from the Hindoos, Jews, Syrians, and Persians, cannot be determined. It is certain, however, that through them various new and useful drugs, such as senna, aconite, rhubarb, camphor, and mercury, were handed down through the Middle Ages, and that they are responsible for the introduction of alcohol in the field of therapeutics.

In mediaeval Europe, Arabian science came to be regarded with superstitious awe, and the works of certain Arabian physicians were exalted to a position above all the ancient writers. In modern times, however, there has been a reaction and a tendency to depreciation of their work. By some they are held to be mere copyists or translators of Greek books, and in no sense original investigators in medicine. Yet there can be little doubt that while the Arabians did copy and translate freely, they also originated and added considerably to medical knowledge. It is certain that in the time when Christian monarchs in western Europe were paying little attention to science or education, the caliphs and vizirs were encouraging physicians and philosophers, building schools, and erecting libraries and hospitals. They made at least a creditable effort to uphold and advance upon the scientific standards of an earlier age.

The first distinguished Arabian physician was Harets ben Kaladah, who received his education in the Nestonian school at Gondisapor, about the beginning of the seventh century. Notwithstanding the fact that Harets was a Christian, he was chosen by Mohammed as his chief medical adviser, and recommended as such to his successor, the Caliph Abu Bekr. Thus, at the very outset, the science of medicine was divorced from religion among the Arabians; for if the prophet himself could employ the services of an unbeliever, surely others might follow his example. And that this example was followed is shown in the fact that many Christian physicians were raised to honorable positions by succeeding generations of Arabian monarchs. This broad−minded view of medicine taken by the Arabs undoubtedly assisted as much as any one single factor in upbuilding the science, just as the narrow and superstitious view taken by Western nations helped to destroy it.

The education of the Arabians made it natural for them to associate medicine with the natural sciences, rather than with religion. An Arabian savant was supposed to be equally well educated in philosophy, jurisprudence, theology, mathematics, and medicine, and to practise law, theology, and medicine with equal skill upon occasion. It is easy to understand, therefore, why these religious fanatics were willing to employ unbelieving physicians, and their physicians themselves to turn to the scientific works of Hippocrates and Galen for medical instruction, rather than to religious works. Even Mohammed himself professed some knowledge of medicine, and often relied upon this knowledge in treating ailments rather than upon prayers or incantations. He is said, for example, to have recommended and applied the cautery in the case of a friend who, when suffering from angina, had sought his aid.

The list of eminent Arabian physicians is too long to be given here, but some of them are of such importance in their influence upon later medicine that they cannot be entirely ignored. One of the first of these was Honain ben Isaac (809−873 A.D.), a Christian Arab of Bagdad. He made translations of the works of Hippocrates, and practised the art along the lines indicated by his teachings and those of Galen. He is considered the greatest translator of the ninth century and one of the greatest philosophers of that period.

Another great Arabian physician, whose work was just beginning as Honain's was drawing to a close, was Rhazes (850−923 A.D.), who during his life was no less noted as a philosopher and musician than as a physician. He continued the work of Honain, and advanced therapeutics by introducing more extensive use of chemical remedies, such as mercurial ointments, sulphuric acid, and aqua vitae. He is also credited with being the first physician to describe small−pox and measles accurately.

While Rhazes was still alive another Arabian, Haly Abbas (died about 994), was writing his famous encyclopaedia of medicine, called The Royal Book. But the names of all these great physicians have been considerably obscured by the reputation of Avicenna (980−1037), the Arabian "Prince of Physicians," the greatest name in Arabic medicine, and one of the most remarkable men in history. Leclerc says that "he was perhaps never surpassed by any man in brilliancy of intellect and indefatigable activity." His career was a most varied one. He was at all times a boisterous reveller, but whether flaunting gayly among the guests of an emir or biding in some obscure apothecary cellar, his work of philosophical writing was carried on steadily. When a friendly emir was in power, he taught and wrote and caroused at court; but between times, when some unfriendly ruler was supreme, he was hiding away obscurely, still pouring out his great mass of manuscripts. In this way his entire life was spent.

By his extensive writings he revived and kept alive the best of the teachings of the Greek physicians, adding to them such observations as he had made in anatomy, physiology, and materia medica. Among his discoveries is that of the contagiousness of pulmonary tuberculosis. His works for several centuries continued to be looked upon as the highest standard by physicians, and he should undoubtedly be credited with having at least retarded the decline of mediaeval medicine.

But it was not the Eastern Arabs alone who were active in the field of medicine. Cordova, the capital of the western caliphate, became also a great centre of learning and produced several great physicians. One of these, Albucasis (died in 1013 A.D.), is credited with having published the first illustrated work on surgery, this book being remarkable in still another way, in that it was also the first book, since classical times, written from the practical experience of the physician, and not a mere compilation of ancient authors. A century after Albucasis came the great physician Avenzoar (1113−1196), with whom he divides about equally the medical honors of the western caliphate. Among Avenzoar's discoveries was that of the cause of "itch"—a little parasite, "so small that he is hardly visible." The discovery of the cause of this common disease seems of minor importance now, but it is of interest in medical history because, had Avenzoar's discovery been remembered a hundred years ago, "itch struck in" could hardly have been considered the cause of three−fourths of all diseases, as it was by the famous Hahnemann.

The illustrious pupil of Avenzoar, Averrhoes, who died in 1198 A.D., was the last of the great Arabian physicians who, by rational conception of medicine, attempted to stem the flood of superstition that was overwhelming medicine. For a time he succeeded; but at last the Moslem theologians prevailed, and he was degraded and banished to a town inhabited only by the despised Jews.

ARABIAN HOSPITALS

To early Christians belong the credit of having established the first charitable institutions for caring for the sick; but their efforts were soon eclipsed by both Eastern and Western Mohammedans. As early as the eighth century the Arabs had begun building hospitals, but the flourishing time of hospital building seems to have begun early in the tenth century. Lady Seidel, in 918 A.D., opened a hospital at Bagdad, endowed with an amount corresponding to about three hundred pounds sterling a month. Other similar hospitals were erected in the years immediately following, and in 977 the Emir Adad−adaula established an enormous institution with a staff of twenty−four medical officers. The great physician Rhazes is said to have selected the site for one of these hospitals by hanging pieces of meat in various places about the city, selecting the site near the place at which putrefaction was slowest in making its appearance. By the middle of the twelfth century there were something like sixty medical institutions in Bagdad alone, and these institutions were free to all patients and supported by official charity.

The Emir Nureddin, about the year 1160, founded a great hospital at Damascus, as a thank−offering for his victories over the Crusaders. This great institution completely overshadowed all the earlier Moslem hospitals in size and in the completeness of its equipment. It was furnished with facilities for teaching, and was conducted for several centuries in a lavish manner, regardless of expense. But little over a century after its foundation the fame of its methods of treatment led to the establishment of a larger and still more luxurious institution—the Mansuri hospital at Cairo. It seems that a certain sultan, having been cured by medicines from the Damascene hospital, determined to build one of his own at Cairo which should eclipse even the great Damascene institution.

In a single year (1283−1284) this hospital was begun and completed. No efforts were spared in hurrying on the good work, and no one was exempt from performing labor on the building if he chanced to pass one of the adjoining streets. It was the order of the sultan that any person passing near could be impressed into the work, and this order was carried out to the letter, noblemen and beggars alike being forced to lend a hand. Very naturally, the adjacent thoroughfares became unpopular and practically deserted, but still the holy work progressed rapidly and was shortly completed.

This immense structure is said to have contained four courts, each having a fountain in the centre; lecture−halls, wards for isolating certain diseases, and a department that corresponded to the modern hospital's "out−patient" department. The yearly endowment amounted to something like the equivalent of one hundred and twenty−five thousand dollars. A novel feature was a hall where musicians played day and night, and another where story−tellers were employed, so that persons troubled with insomnia were amused and melancholiacs cheered. Those of a religious turn of mind could listen to readings of the Koran, conducted continuously by a staff of some fifty chaplains. Each patient on leaving the hospital received some gold pieces, that he need not be obliged to attempt hard labor at once.

In considering the astonishing tales of these sumptuous Arabian institutions, it should be borne in mind that our accounts of them are, for the most part, from Mohammedan sources. Nevertheless, there can be little question that they were enormous institutions, far surpassing any similar institutions in western Europe. The so−called hospitals in the West were, at this time, branches of monasteries under supervision of the monks, and did not compare favorably with the Arabian hospitals.

But while the medical science of the Mohammedans greatly overshadowed that of the Christians during this period, it did not completely obliterate it. About the year 1000 A.D. came into prominence the Christian medical school at Salerno, situated on the Italian coast, some thirty miles southeast of Naples. Just how long this school had been in existence, or by whom it was founded, cannot be determined, but its period of greatest influence was the eleventh, twelfth, and thirteenth centuries. The members of this school gradually adopted Arabic medicine, making use of many drugs from the Arabic pharmacopoeia, and this formed one of the stepping−stones to the introduction of Arabian medicine all through western Europe.

It was not the adoption of Arabian medicines, however, that has made the school at Salerno famous both in rhyme and prose, but rather the fact that women there practised the healing art. Greatest among them was Trotula, who lived in the eleventh century, and whose learning is reputed to have equalled that of the greatest physicians of the day. She is accredited with a work on Diseases of Women, still extant, and many of her writings on general medical subjects were quoted through two succeeding centuries. If we may judge from these writings, she seemed to have had many excellent ideas as to the proper methods of treating diseases, but it is difficult to determine just which of the writings credited to her are in reality hers. Indeed, the uncertainty is even greater than this implies, for, according to some writers, "Trotula" is merely the title of a book. Such an authority as Malgaigne, however, believed that such a woman existed, and that the works accredited to her are authentic. The truth of the matter may perhaps never be fully established, but this at least is certain—the tradition in regard to Trotula could never have arisen had not women held a far different position among the Arabians of this period from that accorded them in contemporary Christendom.

III. MEDIAEVAL SCIENCE IN THE WEST

We have previously referred to the influence of the Byzantine civilization in transmitting the learning of antiquity across the abysm of the dark age. It must be admitted, however, that the importance of that civilization did not extend much beyond the task of the common carrier. There were no great creative scientists in the later Roman empire of the East any more than in the corresponding empire of the West. There was, however, one field in which the Byzantine made respectable progress and regarding which their efforts require a few words of special comment. This was the field of medicine.

The Byzantines of this time could boast of two great medical men, Aetius of Amida (about 502−575 A.D.) and Paul of Aegina (about 620−690). The works of Aetius were of value largely because they recorded the teachings of many of his eminent predecessors, but he was not entirely lacking in originality, and was perhaps the first physician to mention diphtheria, with an allusion to some observations of the paralysis of the palate which sometimes follows this disease.

Paul of Aegina, who came from the Alexandrian school about a century later, was one of those remarkable men whose ideas are centuries ahead of their time. This was particularly true of Paul in regard to surgery, and his attitude towards the supernatural in the causation and treatment of diseases. He was essentially a surgeon, being particularly familiar with military surgery, and some of his descriptions of complicated and difficult operations have been little improved upon even in modern times. In his books he describes such operations as the removal of foreign bodies from the nose, ear, and esophagus; and he recognizes foreign growths such as polypi in the air−passages, and gives the method of their removal. Such operations as tracheotomy, tonsellotomy, bronchotomy, staphylotomy, etc., were performed by him, and he even advocated and described puncture of the abdominal cavity, giving careful directions as to the location in which such punctures should be made. He advocated amputation of the breast for the cure of cancer, and described extirpation of the uterus. Just how successful this last operation may have been as performed by him does not appear; but he would hardly have recommended it if it had not been sometimes, at least, successful. That he mentions it at all, however, is significant, as this difficult operation is considered one of the great triumphs of modern surgery.

But Paul of Aegina is a striking exception to the rule among Byzantine surgeons, and as he was their greatest, so he was also their last important surgeon. The energies of all Byzantium were so expended in religious controversies that medicine, like the other sciences, was soon relegated to a place among the other superstitions, and the influence of the Byzantine school was presently replaced by that of the conquering Arabians.

THIRTEENTH−CENTURY MEDICINE

The thirteenth century marks the beginning of a gradual change in medicine, and a tendency to leave the time−worn rut of superstitious dogmas that so long retarded the progress of science. It is thought that the great epidemics which raged during the Middle Ages acted powerfully in diverting the medical thought of the times into new and entirely different channels. It will be remembered that the teachings of Galen were handed through mediaeval times as the highest and best authority on the subject of all diseases. When, however, the great epidemics made their appearance, the medical men appealed to the works of Galen in vain for enlightenment, as these works, having been written several centuries before the time of the plagues, naturally contained no information concerning them. It was evident, therefore, that on this subject, at least, Galen was not infallible; and it would naturally follow that, one fallible point having been revealed, others would be sought for. In other words, scepticism in regard to accepted methods would be aroused, and would lead naturally, as such scepticism usually does, to progress. The devastating effects of these plagues, despite prayers and incantations, would arouse doubt in the minds of many as to the efficacy of superstitious rites and ceremonies in curing diseases. They had seen thousands and tens of thousands of their fellow−beings swept away by these awful scourges. They had seen the ravages of these epidemics continue for months or even years, notwithstanding the fact that multitudes of God−fearing people prayed hourly that such ravages might be checked. And they must have observed also that when even very simple rules of cleanliness and hygiene were followed there was a diminution in the ravages of the plague, even without the aid of incantations. Such observations as these would have a tendency to awaken a suspicion in the minds of many of the physicians that disease was not a manifestation of the supernatural, but a natural phenomenon, to be treated by natural methods.

But, be the causes what they may, it is a fact that the thirteenth century marks a turning−point, or the beginning of an attitude of mind which resulted in bringing medicine to a much more rational position. Among the thirteenth−century physicians, two men are deserving of special mention. These are Arnald of Villanova (1235−1312) and Peter of Abano (1250−1315). Both these men suffered persecution for expressing their belief in natural, as against the supernatural, causes of disease, and at one time Arnald was obliged to flee from Barcelona for declaring that the "bulls" of popes were human works, and that "acts of charity were dearer to God than hecatombs." He was also accused of alchemy. Fleeing from persecution, he finally perished by shipwreck.

Arnald was the first great representative of the school of Montpellier. He devoted much time to the study of chemicals, and was active in attempting to re−establish the teachings of Hippocrates and Galen. He was one of the first of a long line of alchemists who, for several succeeding centuries, expended so much time and energy in attempting to find the "elixir of life." The Arab discovery of alcohol first deluded him into the belief that the "elixir" had at last been found; but later he discarded it and made extensive experiments with brandy, employing it in the treatment of certain diseases—the first record of the administration of this liquor as a medicine. Arnald also revived the search for some anaesthetic that would produce insensibility to pain in surgical operations. This idea was not original with him, for since very early times physicians had attempted to discover such an anaesthetic, and even so early a writer as Herodotus tells how the Scythians, by inhalation of the vapors of some kind of hemp, produced complete insensibility. It may have been these writings that stimulated Arnald to search for such an anaesthetic. In a book usually credited to him, medicines are named and methods of administration described which will make the patient insensible to pain, so that "he may be cut and feel nothing, as though he were dead." For this purpose a mixture of opium, mandragora, and henbane is to be used. This mixture was held at the patient's nostrils much as ether and chloroform are administered by the modern surgeon. The method was modified by Hugo of Lucca (died in 1252 or 1268), who added certain other narcotics, such as hemlock, to the mixture, and boiled a new sponge in this decoction. After boiling for a certain time, this sponge was dried, and when wanted for use was dipped in hot water and applied to the nostrils.

Just how frequently patients recovered from the administration of such a combination of powerful poisons does not appear, but the percentage of deaths must have been very high, as the practice was generally condemned. Insensibility could have been produced only by swallowing large quantities of the liquid, which dripped into the nose and mouth when the sponge was applied, and a lethal quantity might thus be swallowed. The method was revived, with various modifications, from time to time, but as often fell into disuse. As late as 1782 it was sometimes attempted, and in that year the King of Poland is said to have been completely anaesthetized and to have recovered, after a painless amputation had been performed by the surgeons.

Peter of Abano was one of the first great men produced by the University of Padua. His fate would have been even more tragic than that of the shipwrecked Arnald had he not cheated the purifying fagots of the church by dying opportunely on the eve of his execution for heresy. But if his spirit had cheated the fanatics, his body could not, and his bones were burned for his heresy. He had dared to deny the existence of a devil, and had suggested that the case of a patient who lay in a trance for three days might help to explain some miracles, like the raising of Lazarus.

His great work was Conciliator Differentiarum, an attempt to reconcile physicians and philosophers. But his researches were not confined to medicine, for he seems to have had an inkling of the hitherto unknown fact that air possesses weight, and his calculation of the length of the year at three hundred and sixty−five days, six hours, and four minutes, is exceptionally accurate for the age in which he lived. He was probably the first of the Western writers to teach that the brain is the source of the nerves, and the heart the source of the vessels. From this it is seen that he was groping in the direction of an explanation of the circulation of the blood, as demonstrated by Harvey three centuries later.

The work of Arnald and Peter of Abano in "reviving" medicine was continued actively by Mondino (1276−1326) of Bologna, the "restorer of anatomy," and by Guy of Chauliac: (born about 1300), the "restorer of surgery." All through the early Middle Ages dissections of human bodies had been forbidden, and even dissection of the lower animals gradually fell into disrepute because physicians detected in such practices were sometimes accused of sorcery. Before the close of the thirteenth century, however, a reaction had begun, physicians were protected, and dissections were occasionally sanctioned by the ruling monarch. Thus Emperor Frederick H.

(1194−1250 A.D.)—whose services to science we have already had occasion to mention—ordered that at least one human body should be dissected by physicians in his kingdom every five years. By the time of Mondino dissections were becoming more frequent, and he himself is known to have dissected and demonstrated several bodies. His writings on anatomy have been called merely plagiarisms of Galen, but in all probability be made many discoveries independently, and on the whole, his work may be taken as more advanced than Galen's. His description of the heart is particularly accurate, and he seems to have come nearer to determining the course of the blood in its circulation than any of his predecessors. In this quest he was greatly handicapped by the prevailing belief in the idea that blood−vessels must contain air as well as blood, and this led him to assume that one of the cavities of the heart contained "spirits," or air. It is probable, however, that his accurate observations, so far as they went, were helpful stepping−stones to Harvey in his discovery of the circulation.

Guy of Chauliac, whose innovations in surgery reestablished that science on a firm basis, was not only one of the most cultured, but also the most practical surgeon of his time. He had great reverence for the works of Galen, Albucasis, and others of his noted predecessors; but this reverence did not blind him to their mistakes nor prevent him from using rational methods of treatment far in advance of theirs. His practicality is shown in some of his simple but useful inventions for the sick−room, such as the device of a rope, suspended from the ceiling over the bed, by which a patient may move himself about more easily; and in some of his improvements in surgical dressings, such as stiffening bandages by dipping them in the white of an egg so that they are held firmly. He treated broken limbs in the suspended cradle still in use, and introduced the method of making "traction" on a broken limb by means of a weight and pulley, to prevent deformity through shortening of the member. He was one of the first physicians to recognize the utility of spectacles, and recommended them in cases not amenable to treatment with lotions and eye−waters. In some of his surgical operations, such as trephining for fracture of the skull, his technique has been little improved upon even in modern times. In one of these operations he successfully removed a portion of a man's brain.

Surgery was undoubtedly stimulated greatly at this period by the constant wars. Lay physicians, as a class, had been looked down upon during the Dark Ages; but with the beginning of the return to rationalism, the services of surgeons on the battle−field, to remove missiles from wounds, and to care for wounds and apply dressings, came to be more fully appreciated. In return for his labors the surgeon was thus afforded better opportunities for observing wounds and diseases, which led naturally to a gradual improvement in surgical methods.

FIFTEENTH−CENTURY MEDICINE

The thirteenth and fourteenth centuries had seen some slight advancement in the science of medicine; at least, certain surgeons and physicians, if not the generality, had made advances; but it was not until the fifteenth century that the general revival of medical learning became assured. In this movement, naturally, the printing−press played an all−important part. Medical books, hitherto practically inaccessible to the great mass of physicians, now became common, and this output of reprints of Greek and Arabic treatises revealed the fact that many of the supposed true copies were spurious. These discoveries very naturally aroused all manner of doubt and criticism, which in turn helped in the development of independent thought.

A certain manuscript of the great Cornelius Celsus, the De Medicine, which had been lost for many centuries, was found in the church of St. Ambrose, at Milan, in 1443, and was at once put into print. The effect of the publication of this book, which had lain in hiding for so many centuries, was a revelation, showing the medical profession how far most of their supposed true copies of Celsus had drifted away from the original. The indisputable authenticity of this manuscript, discovered and vouched for by the man who shortly after became Pope Nicholas V., made its publication the more impressive. The output in book form of other authorities followed rapidly, and the manifest discrepancies between such teachers as Celsus, Hippocrates, Galen, and Pliny heightened still more the growing spirit of criticism.

These doubts resulted in great controversies as to the proper treatment of certain diseases, some physicians following Hippocrates, others Galen or Celsus, still others the Arabian masters. One of the most bitter of these contests was over the question of "revulsion," and "derivation"—that is, whether in cases of pleurisy treated by bleeding, the venesection should be made at a point distant from the seat of the disease, as held by the "revulsionists," or at a point nearer and on the same side of the body, as practised by the "derivationists." That any great point for discussion could be raised in the fifteenth or sixteenth centuries on so simple a matter as it seems to−day shows how necessary to the progress of medicine was the discovery of the circulation of the blood made by Harvey two centuries later. After Harvey's discovery no such discussion could have been possible, because this discovery made it evident that as far as the general effect upon the circulation is concerned, it made little difference whether the bleeding was done near a diseased part or remote from it. But in the sixteenth century this question was the all−absorbing one among the doctors. At one time the faculty of Paris condemned "derivation"; but the supporters of this method carried the war still higher, and Emperor Charles V. himself was appealed to. He reversed the decision of the Paris faculty, and decided in favor of "derivation." His decision was further supported by Pope Clement VII., although the discussion dragged on until cut short by Harvey's discovery.

But a new form of injury now claimed the attention of the surgeons, something that could be decided by neither Greek nor Arabian authors, as the treatment of gun−shot wounds was, for obvious reasons, not given in their writings. About this time, also, came the great epidemics, "the sweating sickness" and scurvy; and upon these subjects, also, the Greeks and Arabians were silent. John of Vigo, in his book, the Practica Copiosa, published in 1514, and repeated in many editions, became the standard authority on all these subjects, and thus supplanted the works of the ancient writers.

According to Vigo, gun−shot wounds differed from the wounds made by ordinary weapons—that is, spear, arrow, sword, or axe—in that the bullet, being round, bruised rather than cut its way through the tissues; it burned the flesh; and, worst of all, it poisoned it. Vigo laid especial stress upon treating this last condition, recommending the use of the cautery or the oil of elder, boiling hot. It is little wonder that gun−shot wounds were so likely to prove fatal. Yet, after all, here was the germ of the idea of antisepsis.

NEW BEGINNINGS IN GENERAL SCIENCE

We have dwelt thus at length on the subject of medical science, because it was chiefly in this field that progress was made in the Western world during the mediaeval period, and because these studies furnished the point of departure for the revival all along the line. It will be understood, however, from what was stated in the preceding chapter, that the Arabian influences in particular were to some extent making themselves felt along other lines. The opportunity afforded a portion of the Western world—notably Spain and Sicily —to gain access to the scientific ideas of antiquity through Arabic translations could not fail of influence. Of like character, and perhaps even more pronounced in degree, was the influence wrought by the Byzantine refugees, who, when Constantinople began to be threatened by the Turks, migrated to the West in considerable numbers, bringing with them a knowledge of Greek literature and a large number of precious works which for centuries had been quite forgotten or absolutely ignored in Italy. Now Western scholars began to take an interest in the Greek language, which had been utterly neglected since the beginning of the Middle Ages. Interesting stories are told of the efforts made by such men as Cosmo de' Medici to gain possession of classical manuscripts. The revival of learning thus brought about had its first permanent influence in the fields of literature and art, but its effect on science could not be long delayed. Quite independently of the Byzantine influence, however, the striving for better intellectual things had manifested itself in many ways before the close of the thirteenth century. An illustration of this is found in the almost simultaneous development of centres of teaching, which developed into the universities of Italy, France, England, and, a little later, of Germany.

The regular list of studies that came to be adopted everywhere comprised seven nominal branches, divided into two groups—the so−called quadrivium, comprising music, arithmetic, geometry, and astronomy; and the trivium comprising grammar, rhetoric, and logic. The vagueness of implication of some of these branches gave opportunity to the teacher for the promulgation of almost any knowledge of which he might be possessed, but there can be no doubt that, in general, science had but meagre share in the curriculum. In so far as it was given representation, its chief field must have been Ptolemaic astronomy. The utter lack of scientific thought and scientific method is illustrated most vividly in the works of the greatest men of that period—such men as Albertus Magnus, Thomas Aquinas, Bonaventura, and the hosts of other scholastics of lesser rank. Yet the mental awakening implied in their efforts was sure to extend to other fields, and in point of fact there was at least one contemporary of these great scholastics whose mind was intended towards scientific subjects, and who produced writings strangely at variance in tone and in content with the others. This anachronistic thinker was the English monk, Roger Bacon.

ROGER BACON

Bacon was born in 1214 and died in 1292. By some it is held that he was not appreciated in his own time because he was really a modern scientist living in an age two centuries before modern science or methods of modern scientific thinking were known. Such an estimate, however, is a manifest exaggeration of the facts, although there is probably a grain of truth in it withal. His learning certainly brought him into contact with the great thinkers of the time, and his writings caused him to be imprisoned by his fellow−churchmen at different times, from which circumstances we may gather that he was advanced thinker, even if not a modern scientist.

Although Bacon was at various times in durance, or under surveillance, and forbidden to write, he was nevertheless a marvellously prolific writer, as is shown by the numerous books and unpublished manuscripts of his still extant. His master−production was the Opus Majus. In Part IV. of this work he attempts to show that all sciences rest ultimately on mathematics; but Part V., which treats of perspective, is of particular interest to modern scientists, because in this he discusses reflection and refraction, and the properties of mirrors and lenses. In this part, also, it is evident that he is making use of such Arabian writers as Alkindi and Alhazen, and this is of especial interest, since it has been used by his detractors, who accuse him of lack of originality, to prove that his seeming inventions and discoveries were in reality adaptations of the Arab scientists. It is difficult to determine just how fully such criticisms are justified. It is certain, however, that in this part he describes the anatomy of the eye with great accuracy, and discusses mirrors and lenses.

The magnifying power of the segment of a glass sphere had been noted by Alhazen, who had observed also that the magnification was increased by increasing the size of the segment used. Bacon took up the discussion of the comparative advantages of segments, and in this discussion seems to show that he understood how to trace the progress of the rays of light through a spherical transparent body, and how to determine the place of the image. He also described a method of constructing a telescope, but it is by no means clear that he had ever actually constructed such an instrument. It is also a mooted question as to whether his instructions as to the construction of such an instrument would have enabled any one to construct one. The vagaries of the names of terms as he uses them allow such latitude in interpretation that modern scientists are not agreed as to the practicability of Bacon's suggestions. For example, he constantly refers to force under such names as virtus, species, imago, agentis, and a score of other names, and this naturally gives rise to the great differences in the interpretations of his writings, with corresponding differences in estimates of them.

The claim that Bacon originated the use of lenses, in the form of spectacles, cannot be proven. Smith has determined that as early as the opening years of the fourteenth century such lenses were in use, but this proves nothing as regards Bacon's connection with their invention. The knowledge of lenses seems to be very ancient, if we may judge from the convex lens of rock crystal found by Layard in his excavations at Nimrud. There is nothing to show, however, that the ancients ever thought of using them to correct defects of vision. Neither, apparently, is it feasible to determine whether the idea of such an application originated with Bacon.

Another mechanical discovery about which there has been a great deal of discussion is Bacon's supposed invention of gunpowder. It appears that in a certain passage of his work he describes the process of making a substance that is, in effect, ordinary gunpowder; but it is more than doubtful whether he understood the properties of the substance he describes. It is fairly well established, however, that in Bacon's time gunpowder was known to the Arabs, so that it should not be surprising to find references made to it in Bacon's work, since there is reason to believe that he constantly consulted Arabian writings.

The great merit of Bacon's work, however, depends on the principles taught as regards experiment and the observation of nature, rather than on any single invention. He had the all−important idea of breaking with tradition. He championed unfettered inquiry in every field of thought. He had the instinct of a scientific worker—a rare instinct indeed in that age. Nor need we doubt that to the best of his opportunities he was himself an original investigator.

LEONARDO DA VINCI

The relative infertility of Bacon's thought is shown by the fact that he founded no school and left no trace of discipleship. The entire century after his death shows no single European name that need claim the attention of the historian of science. In the latter part of the fifteenth century, however, there is evidence of a renaissance of science no less than of art. The German Muller became famous under the latinized named of Regio Montanus (1437−1472), although his actual scientific attainments would appear to have been important only in comparison with the utter ignorance of his contemporaries. The most distinguished worker of the new era was the famous Italian Leonardo da Vinci—a man who has been called by Hamerton the most universal genius that ever lived. Leonardo's position in the history of art is known to every one. With that, of course, we have no present concern;

but it is worth our while to inquire at some length as to the famous painter's accomplishments as a scientist.

From a passage in the works of Leonardo, first brought to light by Venturi,[1] it would seem that the great painter anticipated Copernicus in determining the movement of the earth. He made mathematical calculations to prove this, and appears to have reached the definite conclusion that the earth does move—or what amounts to the same thing, that the sun does not move. Muntz is authority for the statement that in one of his writings he declares, "Il sole non si mouve"—the sun does not move.[2]

Among his inventions is a dynamometer for determining the traction power of machines and animals, and his experiments with steam have led some of his enthusiastic partisans to claim for him priority to Watt in the invention of the steam−engine. In these experiments, however, Leonardo seems to have advanced little beyond Hero of Alexandria and his steam toy. Hero's steam−engine did nothing but rotate itself by virtue of escaping jets of steam forced from the bent tubes, while Leonardo's "steam−engine" "drove a ball weighing one talent over a distance of six stadia." In a manuscript now in the library of the Institut de France, Da Vinci describes this engine minutely. The action of this machine was due to the sudden conversion of small quantities of water into steam ("smoke," as he called it) by coming suddenly in contact with a heated surface in a proper receptacle, the rapidly formed steam acting as a propulsive force after the manner of an explosive. It is really a steam−gun, rather than a steam−engine, and it is not unlikely that the study of the action of gunpowder may have suggested it to Leonardo.

It is believed that Leonardo is the true discoverer of the camera−obscura, although the Neapolitan philosopher, Giambattista Porta, who was not born until some twenty years after the death of Leonardo, is usually credited with first describing this device. There is little doubt, however, that Da Vinci understood the principle of this mechanism, for he describes how such a camera can be made by cutting a small, round hole through the shutter of a darkened room, the reversed image of objects outside being shown on the opposite wall.

Like other philosophers in all ages, he had observed a great number of facts which he was unable to explain correctly. But such accumulations of scientific observations are always interesting, as showing how many centuries of observation frequently precede correct explanation. He observed many facts about sounds, among others that blows struck upon a bell produced sympathetic sounds in a bell of the same kind; and that striking the string of a lute produced vibration in corresponding strings of lutes strung to the same pitch. He knew, also, that sounds could be heard at a distance at sea by listening at one end of a tube, the other end of which was placed in the water; and that the same expedient worked successfully on land, the end of the tube being placed against the ground.

The knowledge of this great number of unexplained facts is often interpreted by the admirers of Da Vinci, as showing an almost occult insight into science many centuries in advance of his time. Such interpretations, however, are illusive. The observation, for example, that a tube placed against the ground enables one to hear movements on the earth at a distance, is not in itself evidence of anything more than acute scientific observation, as a similar method is in use among almost every race of savages, notably the American Indians. On the other hand, one is inclined to give credence to almost any story of the breadth of knowledge of the man who came so near anticipating Hutton, Lyell, and Darwin in his interpretation of the geological records as he found them written on the rocks.

It is in this field of geology that Leonardo is entitled to the greatest admiration by modern scientists. He had observed the deposit of fossil shells in various strata of rocks, even on the tops of mountains, and he rejected once for all the theory that they had been deposited there by the Deluge. He rightly interpreted their presence as evidence that they had once been deposited at the bottom of the sea. This process he assumed bad taken hundreds and thousands of centuries, thus tacitly rejecting the biblical tradition as to the date of the creation.

Notwithstanding the obvious interest that attaches to the investigations of Leonardo, it must be admitted that his work in science remained almost as infertile as that of his great precursor, Bacon. The really stimulative work of this generation was done by a man of affairs, who knew little of theoretical science except in one line, but who pursued that one practical line until he achieved a wonderful result. This man was Christopher Columbus. It is not necessary here to tell the trite story of his accomplishment. Suffice it that his practical demonstration of the rotundity of the earth is regarded by most modern writers as marking an epoch in history. With the year of his voyage the epoch of the Middle Ages is usually regarded as coming to an end. It must not be supposed that any very sudden change came over the aspect of scholarship of the time, but the preliminaries of great things had been achieved, and when Columbus made his famous voyage in 1492, the man was already alive who was to bring forward the first great vitalizing thought in the field of pure science that the Western world had originated for more than a thousand years. This man bore the name of Kopernik, or in its familiar Anglicized form, Copernicus. His life work and that of his disciples will claim our attention in the succeeding chapter.

IV. THE NEW COSMOLOGY—COPERNICUS TO KEPLER AND GALILEO

We have seen that the Ptolemaic astronomy, which was the accepted doctrine throughout the Middle Ages, taught that the earth is round. Doubtless there was a popular opinion current which regarded the earth as flat, but it must be understood that this opinion had no champions among men of science during the Middle Ages. When, in the year 1492, Columbus sailed out to the west on his memorable voyage, his expectation of reaching India had full scientific warrant, however much it may have been scouted by certain ecclesiastics and by the average man of the period. Nevertheless, we may well suppose that the successful voyage of Columbus, and the still more demonstrative one made about thirty years later by Magellan, gave the theory of the earth's rotundity a certainty it could never previously have had. Alexandrian geographers had measured the size of the earth, and had not hesitated to assert that by sailing westward one might reach India. But there is a wide gap between theory and practice, and it required the voyages of Columbus and his successors to bridge that gap.

After the companions of Magellan completed the circumnavigation of the globe, the general shape of our earth would, obviously, never again be called in question. But demonstration of the sphericity of the earth had, of course, no direct bearing upon the question of the earth's position in the universe. Therefore the voyage of Magellan served to fortify, rather than to dispute, the Ptolemaic theory. According to that theory, as we have seen, the earth was supposed to lie immovable at the centre of the universe; the various heavenly bodies, including the sun, revolving about it in eccentric circles. We have seen that several of the ancient Greeks, notably Aristarchus, disputed this conception, declaring for the central position of the sun in the universe, and the motion of the earth and other planets about that body. But this revolutionary theory seemed so opposed to the ordinary observation that, having been discountenanced by Hipparchus and Ptolemy, it did not find a single important champion for more than a thousand years after the time of the last great Alexandrian astronomer.

The first man, seemingly, to hark back to the Aristarchian conception in the new scientific era that was now dawning was the noted cardinal, Nikolaus of Cusa, who lived in the first half of the fifteenth century, and was distinguished as a philosophical writer and mathematician. His De Docta Ignorantia expressly propounds the doctrine of the earth's motion. No one, however, paid the slightest attention to his suggestion, which, therefore, merely serves to furnish us with another interesting illustration of the futility of propounding even a correct hypothesis before the time is ripe to receive it—particularly if the hypothesis is not fully fortified by reasoning based on experiment or observation.

The man who was destined to put forward the theory of the earth's motion in a way to command attention was born in 1473, at the village of Thorn, in eastern Prussia. His name was Nicholas Copernicus. There is no more famous name in the entire annals of science than this, yet posterity has never been able fully to establish the lineage of the famous expositor of the true doctrine of the solar system. The city of Thorn lies in a province of that border territory which was then under control of Poland, but which subsequently became a part of Prussia. It is claimed that the aspects of the city were essentially German, and it is admitted that the mother of Copernicus belonged to that race. The nationality of the father is more in doubt, but it is urged that Copernicus used German as his mother−tongue. His great work was, of course, written in Latin, according to the custom of the time; but it is said that, when not employing that language, he always wrote in German. The disputed nationality of Copernicus strongly suggests that he came of a mixed racial lineage, and we are reminded again of the influences of those ethnical minglings to which we have previously more than once referred. The acknowledged centres of civilization towards the close of the fifteenth century were Italy and Spain. Therefore, the birthplace of Copernicus lay almost at the confines of civilization, reminding us of that earlier period when Greece was the centre of culture, but when the great Greek thinkers were born in Asia Minor and in Italy.

As a young man, Copernicus made his way to Vienna to study medicine, and subsequently he journeyed into Italy and remained there many years, About the year 1500 he held the chair of mathematics in a college at Rome. Subsequently he returned to his native land and passed his remaining years there, dying at Domkerr, in Frauenburg, East Prussia, in the year 1543.

It would appear that Copernicus conceived the idea of the heliocentric system of the universe while he was a comparatively young man, since in the introduction to his great work, which he addressed to Pope Paul III., he states that he has pondered his system not merely nine years, in accordance with the maxim of Horace, but well into the fourth period of nine years. Throughout a considerable portion of this period the great work of Copernicus was in manuscript, but it was not published until the year of his death. The reasons for the delay are not very fully established. Copernicus undoubtedly taught his system throughout the later decades of his life. He himself tells us that he had even questioned whether it were not better for him to confine himself to such verbal teaching, following thus the example of Pythagoras. Just as his life was drawing to a close, he decided to pursue the opposite course, and the first copy of his work is said to have been placed in his hands as he lay on his deathbed.

The violent opposition which the new system met from ecclesiastical sources led subsequent commentators to suppose that Copernicus had delayed publication of his work through fear of the church authorities. There seems, however, to be no direct evidence for this opinion. It has been thought significant that Copernicus addressed his work to the pope. It is, of course, quite conceivable that the aged astronomer might wish by this means to demonstrate that he wrote in no spirit of hostility to the church. His address to the pope might have been considered as a desirable shield precisely because the author recognized that his work must needs meet with ecclesiastical criticism. Be that as it may, Copernicus was removed by death from the danger of attack, and it remained for his disciples of a later generation to run the gauntlet of criticism and suffer the charges of heresy.

The work of Copernicus, published thus in the year 1543 at Nuremberg, bears the title De Orbium Coelestium Revolutionibus.

It is not necessary to go into details as to the cosmological system which Copernicus advocated, since it is familiar to every one. In a word, he supposed the sun to be the centre of all the planetary motions, the earth taking its place among the other planets, the list of which, as known at that time, comprised Mercury, Venus, the Earth, Mars, Jupiter, and Saturn. The fixed stars were alleged to be stationary, and it was necessary to suppose that they are almost infinitely distant, inasmuch as they showed to the observers of that time no parallax; that is to say, they preserved the same apparent position when viewed from the opposite points of the earth's orbit.

But let us allow Copernicus to speak for himself regarding his system, His exposition is full of interest. We quote first the introduction just referred to, in which appeal is made directly to the pope.

"I can well believe, most holy father, that certain people, when they hear of my attributing motion to the earth in these books of mine, will at once declare that such an opinion ought to be rejected. Now, my own theories do not please me so much as not to consider what others may judge of them. Accordingly, when I began to reflect upon what those persons who accept the stability of the earth, as confirmed by the opinion of many centuries, would say when I claimed that the earth moves, I hesitated for a long time as to whether I should publish that which I have written to demonstrate its motion, or whether it would not be better to follow the example of the Pythagoreans, who used to hand down the secrets of philosophy to their relatives and friends only in oral form. As I well considered all this, I was almost impelled to put the finished work wholly aside, through the scorn I had reason to anticipate on account of the newness and apparent contrariness to reason of my theory.

"My friends, however, dissuaded me from such a course and admonished me that I ought to publish my book, which had lain concealed in my possession not only nine years, but already into four times the ninth year. Not a few other distinguished and very learned men asked me to do the same thing, and told me that I ought not, on account of my anxiety, to delay any longer in consecrating my work to the general service of mathematicians.

"But your holiness will perhaps not so much wonder that I have dared to bring the results of my night labors to the light of day, after having taken so much care in elaborating them, but is waiting instead to hear how it entered my mind to imagine that the earth moved, contrary to the accepted opinion of mathematicians—nay, almost contrary to ordinary human understanding. Therefore I will not conceal from your holiness that what moved me to consider another way of reckoning the motions of the heavenly bodies was nothing else than the fact that the mathematicians do not agree with one another in their investigations. In the first place, they are so uncertain about the motions of the sun and moon that they cannot find out the length of a full year. In the second place, they apply neither the same laws of cause and effect, in determining the motions of the sun and moon and of the five planets, nor the same proofs. Some employ only concentric circles, others use eccentric and epicyclic ones, with which, however, they do not fully attain the desired end. They could not even discover nor compute the main thing—namely, the form of the universe and the symmetry of its parts. It was with them as if some should, from different places, take hands, feet, head, and other parts of the body, which, although very beautiful, were not drawn in their proper relations, and, without making them in any way correspond, should construct a monster instead of a human being.

"Accordingly, when I had long reflected on this uncertainty of mathematical tradition, I took the trouble to read again the books of all the philosophers I could get hold of, to see if some one of them had not once believed that there were other motions of the heavenly bodies. First I found in Cicero that Niceties had believed in the motion of the earth. Afterwards I found in Plutarch, likewise, that some others had held the same opinion. This induced me also to begin to consider the movability of the earth, and, although the theory appeared contrary to reason, I did so because I knew that others before me had been allowed to assume rotary movements at will, in order to explain the phenomena of these celestial bodies. I was of the opinion that I, too, might be permitted to see whether, by presupposing motion in the earth, more reliable conclusions than hitherto reached could not be discovered for the rotary motions of the spheres. And thus, acting on the hypothesis of the motion which, in the following book, I ascribe to the earth, and by long and continued observations, I have finally discovered that if the motion of the other planets be carried over to the relation of the earth and this is made the basis for the rotation of every star, not only will the phenomena of the planets be explained thereby, but also the laws and the size of the stars; all their spheres and the heavens themselves will appear so harmoniously connected that nothing could be changed in any part of them without confusion in the remaining parts and in the whole universe. I do not doubt that clever and learned men will agree with me if they are willing fully to comprehend and to consider the proofs which I advance in the book before us. In order, however, that both the learned and the unlearned may see that I fear no man's judgment, I wanted to dedicate these, my night labors, to your holiness, rather than to any one else, because you, even in this remote corner of the earth where I live, are held to be the greatest in dignity of station and in love for all sciences and for mathematics, so that you, through your position and judgment, can easily suppress the bites of slanderers, although the proverb says that there is no remedy against the bite of calumny."

In chapter X. of book I., "On the Order of the Spheres," occurs a more detailed presentation of the system, as follows:

"That which Martianus Capella, and a few other Latins, very well knew, appears to me extremely noteworthy. He believed that Venus and Mercury revolve about the sun as their centre and that they cannot go farther away from it than the circles of their orbits permit, since they do not revolve about the earth like the other planets. According to this theory, then, Mercury's orbit would be included within that of Venus, which is more than twice as great, and would find room enough within it for its revolution.

"If, acting upon this supposition, we connect Saturn, Jupiter, and Mars with the same centre, keeping in mind the greater extent of their orbits, which include the earth's sphere besides those of Mercury and Venus, we cannot fail to see the explanation of the regular order of their motions. He is certain that Saturn, Jupiter, and Mars are always nearest the earth when they rise in the evening—that is, when they appear over against the sun, or the earth stands between them and the sun—but that they are farthest from the earth when they set in the evening—that is, when we have the sun between them and the earth. This proves sufficiently that their centre belongs to the sun and is the same about which the orbits of Venus and Mercury circle. Since, however, all have one centre, it is necessary for the space intervening between the orbits of Venus and Mars to include the earth with her accompanying moon and all that is beneath the moon; for the moon, which stands unquestionably nearest the earth, can in no way be separated from her, especially as there is sufficient room for the moon in the aforesaid space. Hence we do not hesitate to claim that the whole system, which includes the moon with the earth for its centre, makes the round of that great circle between the planets, in yearly motion about the sun, and revolves about the centre of the universe, in which the sun rests motionless, and that all which looks like motion in the sun is explained by the motion of the earth. The extent of the universe, however, is so great that, whereas the distance of the earth from the sun is considerable in comparison with the size of the other planetary orbits, it disappears when compared with the sphere of the fixed stars. I hold this to be more easily comprehensible than when the mind is confused by an almost endless number of circles, which is necessarily the case with those who keep the earth in the middle of the universe. Although this may appear incomprehensible and contrary to the opinion of many, I shall, if God wills, make it clearer than the sun, at least to those who are not ignorant of mathematics.

"The order of the spheres is as follows: The first and lightest of all the spheres is that of the fixed stars, which includes itself and all others, and hence is motionless as the place in the universe to which the motion and position of all other stars is referred.

"Then follows the outermost planet, Saturn, which completes its revolution around the sun in thirty years; next comes Jupiter with a twelve years' revolution; then Mars, which completes its course in two years. The fourth one in order is the yearly revolution which includes the earth with the moon's orbit as an epicycle. In the fifth place is Venus with a revolution of nine months. The sixth place is taken by Mercury, which completes its course in eighty days. In the middle of all stands the sun, and who could wish to place the lamp of this most beautiful temple in another or better place. Thus, in fact, the sun, seated upon the royal throne, controls the family of the stars which circle around him. We find in their order a harmonious connection which cannot be found elsewhere. Here the attentive observer can see why the waxing and waning of Jupiter seems greater than with Saturn and smaller than with Mars, and again greater with Venus than with Mercury. Also, why Saturn, Jupiter, and Mars are nearer to the earth when they rise in the evening than when they disappear in the rays of the sun. More prominently, however, is it seen in the case of Mars, which when it appears in the heavens at night, seems to equal Jupiter in size, but soon afterwards is found among the stars of second magnitude. All of this results from the same cause—namely, from the earth's motion. The fact that nothing of this is to be seen in the case of the fixed stars is a proof of their immeasurable distance, which makes even the orbit of yearly motion or its counterpart invisible to us."[1]

The fact that the stars show no parallax had been regarded as an important argument against the motion of the earth, and it was still so considered by the opponents of the system of Copernicus. It had, indeed, been necessary for Aristarchus to explain the fact as due to the extreme distance of the stars; a perfectly correct explanation, but one that implies distances that are altogether inconceivable. It remained for nineteenth−century astronomers to show, with the aid of instruments of greater precision, that certain of the stars have a parallax. But long before this demonstration had been brought forward, the system of Copernicus had been accepted as a part of common knowledge.

While Copernicus postulated a cosmical scheme that was correct as to its main features, he did not altogether break away from certain defects of the Ptolemaic hypothesis. Indeed, he seems to have retained as much of this as practicable, in deference to the prejudice of his time. Thus he records the planetary orbits as circular, and explains their eccentricities by resorting to the theory of epicycles, quite after the Ptolemaic method. But now, of course, a much more simple mechanism sufficed to explain the planetary motions, since the orbits were correctly referred to the central sun and not to the earth.

Needless to say, the revolutionary conception of Copernicus did not meet with immediate acceptance. A number of prominent astronomers, however, took it up almost at once, among these being Rhaeticus, who wrote a commentary on the evolutions; Erasmus Reinhold, the author of the Prutenic tables; Rothmann, astronomer to the Landgrave of Hesse, and Maestlin, the instructor of Kepler. The Prutenic tables, just referred to, so called because of their Prussian origin, were considered an improvement on the tables of Copernicus, and were highly esteemed by the astronomers of the time. The commentary of Rhaeticus gives us the interesting information that it was the observation of the orbit of Mars and of the very great difference between his apparent diameters at different times which first led Copernicus to conceive the heliocentric idea. Of Reinhold it is recorded that he considered the orbit of Mercury elliptical, and that he advocated a theory of the moon, according to which her epicycle revolved on an elliptical orbit, thus in a measure anticipating one of the great discoveries of Kepler to which we shall refer presently. The Landgrave of Hesse was a practical astronomer, who produced a catalogue of fixed stars which has been compared with that of Tycho Brahe. He was assisted by Rothmann and by Justus Byrgius. Maestlin, the preceptor of Kepler, is reputed to have been the first modern observer to give a correct explanation of the light seen on portions of the moon not directly illumined by the sun. He explained this as not due to any proper light of the moon itself, but as light reflected from the earth. Certain of the Greek philosophers, however, are said to have given the same explanation, and it is alleged also that Leonardo da Vinci anticipated Maestlin in this regard.[2]

While, various astronomers of some eminence thus gave support to the Copernican system, almost from the beginning, it unfortunately chanced that by far the most famous of the immediate successors of Copernicus declined to accept the theory of the earth's motion. This was Tycho Brahe, one of the greatest observing astronomers of any age. Tycho Brahe was a Dane, born at Knudstrup in the year 1546. He died in 1601 at Prague, in Bohemia. During a considerable portion of his life he found a patron in Frederick, King of Denmark, who assisted him to build a splendid observatory on the Island of Huene. On the death of his patron Tycho moved to Germany, where, as good luck would have it, he came in contact with the youthful Kepler, and thus, no doubt, was instrumental in stimulating the ambitions of one who in later years was to be known as a far greater theorist than himself. As has been said, Tycho rejected the Copernican theory of the earth's motion. It should be added, however, that he accepted that part of the Copernican theory which makes the sun the centre of all the planetary motions, the earth being excepted. He thus developed a system of his own, which was in some sort a compromise between the Ptolemaic and the Copernican systems. As Tycho conceived it, the sun revolves about the earth, carrying with it the planets−Mercury, Venus, Mars, Jupiter, and Saturn, which planets have the sun and not the earth as the centre of their orbits. This cosmical scheme, it should be added, may be made to explain the observed motions of the heavenly bodies, but it involves a much more complex mechanism than is postulated by the Copernican theory.

Various explanations have been offered of the conservatism which held the great Danish astronomer back from full acceptance of the relatively simple and, as we now know, correct Copernican doctrine. From our latter−day point of view, it seems so much more natural to accept than to reject the Copernican system, that we find it difficult to put ourselves in the place of a sixteenth−century observer. Yet if we recall that the traditional view, having warrant of acceptance by nearly all thinkers of every age, recorded the earth as a fixed, immovable body, we shall see that our surprise should be excited rather by the thinker who can break away from this view than by the one who still tends to cling to it.

Moreover, it is useless to attempt to disguise the fact that something more than a mere vague tradition was supposed to support the idea of the earth's overshadowing importance in the cosmical scheme. The sixteenth−century mind was overmastered by the tenets of ecclesiasticism, and it was a dangerous heresy to doubt that the Hebrew writings, upon which ecclesiasticism based its claim, contained the last word regarding matters of science. But the writers of the Hebrew text had been under the influence of that Babylonian conception of the universe which accepted the earth as unqualifiedly central—which, indeed, had never so much as conceived a contradictory hypothesis; and so the Western world, which had come to accept these writings as actually supernatural in origin, lay under the spell of Oriental ideas of a pre−scientific era. In our own day, no one speaking with authority thinks of these Hebrew writings as having any scientific weight whatever. Their interest in this regard is purely antiquarian; hence from our changed point of view it seems scarcely credible that Tycho Brahe can have been in earnest when he quotes the Hebrew traditions as proof that the sun revolves about the earth. Yet we shall see that for almost three centuries after the time of Tycho, these same dreamings continued to be cited in opposition to those scientific advances which new observations made necessary; and this notwithstanding the fact that the Oriental phrasing is, for the most part, poetically ambiguous and susceptible of shifting interpretations, as the criticism of successive generations has amply testified.

As we have said, Tycho Brahe, great observer as he was, could not shake himself free from the Oriental incubus. He began his objections, then, to the Copernican system by quoting the adverse testimony of a Hebrew prophet who lived more than a thousand years B.C. All of this shows sufficiently that Tycho Brahe was not a great theorist. He was essentially an observer, but in this regard he won a secure place in the very first rank. Indeed, he was easily the greatest observing astronomer since Hipparchus, between whom and himself there were many points of resemblance. Hipparchus, it will be recalled, rejected the Aristarchian conception of the universe just as Tycho rejected the conception of Copernicus.

But if Tycho propounded no great generalizations, the list of specific advances due to him is a long one, and some of these were to prove important aids in the hands of later workers to the secure demonstration of the Copernican idea. One of his most important series of studies had to do with comets. Regarding these bodies there had been the greatest uncertainty in the minds of astronomers. The greatest variety of opinions regarding them prevailed; they were thought on the one hand to be divine messengers, and on the other to be merely igneous phenomena of the earth's atmosphere. Tycho Brahe declared that a comet which he observed in the year 1577 had no parallax, proving its extreme distance. The observed course of the comet intersected the planetary orbits, which fact gave a quietus to the long−mooted question as to whether the Ptolemaic spheres were transparent solids or merely imaginary; since the comet was seen to intersect these alleged spheres, it was obvious that they could not be the solid substance that they were commonly imagined to be, and this fact in itself went far towards discrediting the Ptolemaic system. It should be recalled, however, that this supposition of tangible spheres for the various planetary and stellar orbits was a mediaeval interpretation of Ptolemy's theory rather than an interpretation of Ptolemy himself, there being nothing to show that the Alexandrian astronomer regarded his cycles and epicycles as other than theoretical.

An interesting practical discovery made by Tycho was his method of determining the latitude of a place by means of two observations made at an interval of twelve hours. Hitherto it had been necessary to observe the sun's angle on the equinoctial days, a period of six months being therefore required. Tycho measured the angle of elevation of some star situated near the pole, when on the meridian, and then, twelve hours later, measured the angle of elevation of the same star when it again came to the meridian at the opposite point of its apparent circle about the polestar. Half the sum of these angles gives the latitude of the place of observation.

As illustrating the accuracy of Tycho's observations, it may be noted that he rediscovered a third inequality of the moon's motion at its variation, he, in common with other European astronomers, being then quite unaware that this inequality had been observed by an Arabian astronomer. Tycho proved also that the angle of inclination of the moon's orbit to the ecliptic is subject to slight variation.

The very brilliant new star which shone forth suddenly in the constellation of Cassiopeia in the year 1572, was made the object of special studies by Tycho, who proved that the star had no sensible parallax and consequently was far beyond the planetary regions. The appearance of a new star was a phenomenon not unknown to the ancients, since Pliny records that Hipparchus was led by such an appearance to make his catalogue of the fixed stars. But the phenomenon is sufficiently uncommon to attract unusual attention. A similar phenomenon occurred in the year 1604, when the new star—in this case appearing in the constellation of Serpentarius—was explained by Kepler as probably proceeding from a vast combustion. This explanation—in which Kepler is said to have followed. Tycho—is fully in accord with the most recent theories on the subject, as we shall see in due course. It is surprising to hear Tycho credited with so startling a theory, but, on the other hand, such an explanation is precisely what should be expected from the other astronomer named. For Johann Kepler, or, as he was originally named, Johann von Kappel, was one of the most speculative astronomers of any age. He was forever theorizing, but such was the peculiar quality of his mind that his theories never satisfied him for long unless he could put them to the test of observation. Thanks to this happy combination of qualities, Kepler became the discoverer of three famous laws of planetary motion which lie at the very foundation of modern astronomy, and which were to be largely instrumental in guiding Newton to his still greater generalization. These laws of planetary motion were vastly important as corroborating the Copernican theory of the universe, though their position in this regard was not immediately recognized by contemporary thinkers. Let us examine with some detail into their discovery, meantime catching a glimpse of the life history of the remarkable man whose name they bear.

JOHANN KEPLER AND THE LAWS OF PLANETARY MOTION

Johann Kepler was born the 27th of December, 1571, in the little town of Weil, in Wurtemburg. He was a weak, sickly child, further enfeebled by a severe attack of small−pox. It would seem paradoxical to assert that the parents of such a genius were mismated, but their home was not a happy one, the mother being of a nervous temperament, which perhaps in some measure accounted for the genius of the child. The father led the life of a soldier, and finally perished in the campaign against the Turks. Young Kepler's studies were directed with an eye to the ministry. After a preliminary training he attended the university at Tubingen, where he came under the influence of the celebrated Maestlin and became his life−long friend.

Curiously enough, it is recorded that at first Kepler had no taste for astronomy or for mathematics. But the doors of the ministry being presently barred to him, he turned with enthusiasm to the study of astronomy, being from the first an ardent advocate of the Copernican system. His teacher, Maestlin, accepted the same doctrine, though he was obliged, for theological reasons, to teach the Ptolemaic system, as also to oppose the Gregorian reform of the calendar.

The Gregorian calendar, it should be explained, is so called because it was instituted by Pope Gregory XIII., who put it into effect in the year 1582, up to which time the so−called Julian calendar, as introduced by Julius Caesar, had been everywhere accepted in Christendom. This Julian calendar, as we have seen, was a great improvement on preceding ones, but still lacked something of perfection inasmuch as its theoretical day differed appreciably from the actual day. In the course of fifteen hundred years, since the time of Caesar, this defect amounted to a discrepancy of about eleven days. Pope Gregory proposed to correct this by omitting ten days from the calendar, which was done in September, 1582. To prevent similar inaccuracies in the future, the Gregorian calendar provided that once in four centuries the additional day to make a leap−year should be omitted, the date selected for such omission being the last year of every fourth century. Thus the years 1500, 1900, and 2300, A.D., would not be leap−years. By this arrangement an approximate rectification of the calendar was effected, though even this does not make it absolutely exact.

Such a rectification as this was obviously desirable, but there was really no necessity for the omission of the ten days from the calendar. The equinoctial day had shifted so that in the year 1582 it fell on the 10th of March and September. There was no reason why it should not have remained there. It would greatly have simplified the task of future historians had Gregory contented himself with providing for the future stability of the calendar without making the needless shift in question. We are so accustomed to think of the 21st of March and 21st of September as the natural periods of the equinox, that we are likely to forget that these are purely arbitrary dates for which the 10th might have been substituted without any inconvenience or inconsistency.

But the opposition to the new calendar, to which reference has been made, was not based on any such considerations as these. It was due, largely at any rate, to the fact that Germany at this time was under sway of the Lutheran revolt against the papacy. So effective was the opposition that the Gregorian calendar did not come into vogue in Germany until the year 1699. It may be added that England, under stress of the same manner of prejudice, held out against the new reckoning until the year 1751, while Russia does not accept it even now.

As the Protestant leaders thus opposed the papal attitude in a matter of so practical a character as the calendar, it might perhaps have been expected that the Lutherans would have had a leaning towards the Copernican theory of the universe, since this theory was opposed by the papacy. Such, however, was not the case. Luther himself pointed out with great strenuousness, as a final and demonstrative argument, the fact that Joshua commanded the sun and not the earth to stand still; and his followers were quite as intolerant towards the new teaching as were their ultramontane opponents. Kepler himself was, at various times, to feel the restraint of ecclesiastical opposition, though he was never subjected to direct persecution, as was his friend and contemporary, Galileo. At the very outset of Kepler's career there was, indeed, question as to the publication of a work he had written, because that work took for granted the truth of the Copernican doctrine. This work appeared, however, in the year 1596. It bore the title Mysterium Cosmographium, and it attempted to explain the positions of the various planetary bodies. Copernicus had devoted much time to observation of the planets with reference to measuring their distance, and his efforts had been attended with considerable success. He did not, indeed, know the actual distance of the sun, and, therefore, was quite unable to fix the distance of any planet; but, on the other hand, he determined the relative distance of all the planets then known, as measured in terms of the sun's distance, with remarkable accuracy.

With these measurements as a guide, Kepler was led to a very fanciful theory, according to which the orbits of the five principal planets sustain a peculiar relation to the five regular solids of geometry. His theory was this: "Around the orbit of the earth describe a dodecahedron—the circle comprising it will be that of Mars; around Mars describe a tetrahedron—the circle comprising it will be that of Jupiter; around Jupiter describe a cube—the circle comprising it will be that of Saturn; now within the earth's orbit inscribe an icosahedron—the inscribed circle will be that of Venus; in the orbit of Venus inscribe an octahedron —the circle inscribed will be that of Mercury."[3]

Though this arrangement was a fanciful one, which no one would now recall had not the theorizer obtained subsequent fame on more substantial grounds, yet it evidenced a philosophical spirit on the part of the astronomer which, misdirected as it was in this instance, promised well for the future. Tycho Brahe, to whom a copy of the work was sent, had the acumen to recognize it as a work of genius. He summoned the young astronomer to be his assistant at Prague, and no doubt the association thus begun was instrumental in determining the character of Kepler's future work. It was precisely the training in minute observation that could avail most for a mind which, like Kepler's, tended instinctively to the formulation of theories. When Tycho Brahe died, in 1601, Kepler became his successor. In due time he secured access to all the unpublished observations of his great predecessor, and these were of inestimable value to him in the progress of his own studies.

Kepler was not only an ardent worker and an enthusiastic theorizer, but he was an indefatigable writer, and it pleased him to take the public fully into his confidence, not merely as to his successes, but as to his failures. Thus his works elaborate false theories as well as correct ones, and detail the observations through which the incorrect guesses were refuted by their originator. Some of these accounts are highly interesting, but they must not detain us here. For our present purpose it must suffice to point out the three important theories, which, as culled from among a score or so of incorrect ones, Kepler was able to demonstrate to his own satisfaction and to that of subsequent observers. Stated in a few words, these theories, which have come to bear the name of Kepler's Laws, are the following:

1. That the planetary orbits are not circular, but elliptical, the sun occupying one focus of the ellipses.

2. That the speed of planetary motion varies in different parts of the orbit in such a way that an imaginary line drawn from the sun to the planet—that is to say, the radius vector of the planet's orbit—always sweeps the same area in a given time.

These two laws Kepler published as early as 1609. Many years more of patient investigation were required before he found out the secret of the relation between planetary distances and times of revolution which his third law expresses. In 1618, however, he was able to formulate this relation also, as follows:

3. The squares of the distance of the various planets from the sun are proportional to the cubes of their periods of revolution about the sun.

All these laws, it will be observed, take for granted the fact that the sun is the centre of the planetary orbits. It must be understood, too, that the earth is constantly regarded, in accordance with the Copernican system, as being itself a member of the planetary system, subject to precisely the same laws as the other planets. Long familiarity has made these wonderful laws of Kepler seem such a matter of course that it is difficult now to appreciate them at their full value. Yet, as has been already pointed out, it was the knowledge of these marvellously simple relations between the planetary orbits that laid the foundation for the Newtonian law of universal gravitation. Contemporary judgment could not, of course, anticipate this culmination of a later generation. What it could understand was that the first law of Kepler attacked one of the most time−honored of metaphysical conceptions—namely, the Aristotelian idea that the circle is the perfect figure, and hence that the planetary orbits must be circular. Not even Copernicus had doubted the validity of this assumption. That Kepler dared dispute so firmly fixed a belief, and one that seemingly had so sound a philosophical basis, evidenced the iconoclastic nature of his genius. That he did not rest content until he had demonstrated the validity of his revolutionary assumption shows how truly this great theorizer made his hypotheses subservient to the most rigid inductions.

GALILEO GALILEI

While Kepler was solving these riddles of planetary motion, there was an even more famous man in Italy whose championship of the Copernican doctrine was destined to give the greatest possible publicity to the new ideas. This was Galileo Galilei, one of the most extraordinary scientific observers of any age. Galileo was born at Pisa, on the 18th of February (old style), 1564. The day of his birth is doubly memorable, since on the same day the greatest Italian of the preceding epoch, Michael Angelo, breathed his last. Persons fond of symbolism have found in the coincidence a forecast of the transit from the artistic to the scientific epoch of the later Renaissance. Galileo came of an impoverished noble family. He was educated for the profession of medicine, but did not progress far before his natural proclivities directed him towards the physical sciences. Meeting with opposition in Pisa, he early accepted a call to the chair of natural philosophy in the University of Padua, and later in life he made his home at Florence. The mechanical and physical discoveries of Galileo will claim our attention in another chapter. Our present concern is with his contribution to the Copernican theory.

Galileo himself records in a letter to Kepler that he became a convert to this theory at an early day. He was not enabled, however, to make any marked contribution to the subject, beyond the influence of his general teachings, until about the year 1610. The brilliant contributions which he made were due largely to a single discovery—namely, that of the telescope. Hitherto the astronomical observations had been made with the unaided eye. Glass lenses had been known since the thirteenth century, but, until now, no one had thought of their possible use as aids to distant vision. The question of priority of discovery has never been settled. It is admitted, however, that the chief honors belong to the opticians of the Netherlands.

As early as the year 1590 the Dutch optician Zacharias Jensen placed a concave and a convex lens respectively at the ends of a tube about eighteen inches long, and used this instrument for the purpose of magnifying small objects—producing, in short, a crude microscope. Some years later, Johannes Lippershey, of whom not much is known except that he died in 1619, experimented with a somewhat similar combination of lenses, and made the startling observation that the weather−vane on a distant church−steeple seemed to be brought much nearer when viewed through the lens. The combination of lenses he employed is that still used in the construction of opera−glasses; the Germans still call such a combination a Dutch telescope.

Doubtless a large number of experimenters took the matter up and the fame of the new instrument spread rapidly abroad. Galileo, down in Italy, heard rumors of this remarkable contrivance, through the use of which it was said "distant objects might be seen as clearly as those near at hand." He at once set to work to construct for himself a similar instrument, and his efforts were so far successful that at first he "saw objects three times as near and nine times enlarged." Continuing his efforts, he presently so improved his glass that objects were enlarged almost a thousand times and made to appear thirty times nearer than when seen with the naked eye. Naturally enough, Galileo turned this fascinating instrument towards the skies, and he was almost immediately rewarded by several startling discoveries. At the very outset, his magnifying−glass brought to view a vast number of stars that are invisible to the naked eye, and enabled the observer to reach the conclusion that the hazy light of the Milky Way is merely due to the aggregation of a vast number of tiny stars.

Turning his telescope towards the moon, Galileo found that body rough and earth−like in contour, its surface covered with mountains, whose height could be approximately measured through study of their shadows. This was disquieting, because the current Aristotelian doctrine supposed the moon, in common with the planets, to be a perfectly spherical, smooth body. The metaphysical idea of a perfect universe was sure to be disturbed by this seemingly rough workmanship of the moon. Thus far, however, there was nothing in the observations of Galileo to bear directly upon the Copernican theory; but when an inspection was made of the planets the case was quite different. With the aid of his telescope, Galileo saw that Venus, for example, passes through phases precisely similar to those of the moon, due, of course, to the same cause. Here, then, was demonstrative evidence that the planets are dark bodies reflecting the light of the sun, and an explanation was given of the fact, hitherto urged in opposition to the Copernican theory, that the inferior planets do not seem many times brighter when nearer the earth than when in the most distant parts of their orbits; the explanation being, of course, that when the planets are between the earth and the sun only a small portion of their illumined surfaces is visible from the earth.

On inspecting the planet Jupiter, a still more striking revelation was made, as four tiny stars were observed to occupy an equatorial position near that planet, and were seen, when watched night after night, to be circling about the planet, precisely as the moon circles about the earth. Here, obviously, was a miniature solar system—a tangible object−lesson in the Copernican theory. In honor of the ruling Florentine house of the period, Galileo named these moons of Jupiter, Medicean stars.

Turning attention to the sun itself, Galileo observed on the surface of that luminary a spot or blemish which gradually changed its shape, suggesting that changes were taking place in the substance of the sun—changes obviously incompatible with the perfect condition demanded by the metaphysical theorists. But however disquieting for the conservative, the sun's spots served a most useful purpose in enabling Galileo to demonstrate that the sun itself revolves on its axis, since a given spot was seen to pass across the disk and after disappearing to reappear in due course. The period of rotation was found to be about twenty−four days.

It must be added that various observers disputed priority of discovery of the sun's spots with Galileo. Unquestionably a sun−spot had been seen by earlier observers, and by them mistaken for the transit of an inferior planet. Kepler himself had made this mistake. Before the day of the telescope, he had viewed the image of the sun as thrown on a screen in a camera−obscura, and had observed a spot on the disk which be interpreted as representing the planet Mercury, but which, as is now known, must have been a sun−spot, since the planetary disk is too small to have been revealed by this method. Such observations as these, however interesting, cannot be claimed as discoveries of the sun−spots. It is probable, however, that several discoverers (notably Johann Fabricius) made the telescopic observation of the spots, and recognized them as having to do with the sun's surface, almost simultaneously with Galileo. One of these claimants was a Jesuit named Scheiner, and the jealousy of this man is said to have had a share in bringing about that persecution to which we must now refer.

There is no more famous incident in the history of science than the heresy trial through which Galileo was led to the nominal renunciation of his cherished doctrines. There is scarcely another incident that has been commented upon so variously. Each succeeding generation has put its own interpretation on it. The facts, however, have been but little questioned. It appears that in the year 1616 the church became at last aroused to the implications of the heliocentric doctrine of the universe. Apparently it seemed clear to the church authorities that the authors of the Bible believed the world to be immovably fixed at the centre of the universe. Such, indeed, would seem to be the natural inference from various familiar phrases of the Hebrew text, and what we now know of the status of Oriental science in antiquity gives full warrant to this interpretation. There is no reason to suppose that the conception of the subordinate place of the world in the solar system had ever so much as occurred, even as a vague speculation, to the authors of Genesis. In common with their contemporaries, they believed the earth to be the all−important body in the universe, and the sun a luminary placed in the sky for the sole purpose of giving light to the earth. There is nothing strange, nothing anomalous, in this view; it merely reflects the current notions of Oriental peoples in antiquity. What is strange and anomalous is the fact that the Oriental dreamings thus expressed could have been supposed to represent the acme of scientific knowledge. Yet such a hold had these writings taken upon the Western world that not even a Galileo dared contradict them openly; and when the church fathers gravely declared the heliocentric theory necessarily false, because contradictory to Scripture, there were probably few people in Christendom whose mental attitude would permit them justly to appreciate the humor of such a pronouncement. And, indeed, if here and there a man might have risen to such an appreciation, there were abundant reasons for the repression of the impulse, for there was nothing humorous about the response with which the authorities of the time were wont to meet the expression of iconoclastic opinions. The burning at the stake of Giordano Bruno, in the year 1600, was, for example, an object−lesson well calculated to restrain the enthusiasm of other similarly minded teachers.

Doubtless it was such considerations that explained the relative silence of the champions of the Copernican theory, accounting for the otherwise inexplicable fact that about eighty years elapsed after the death of Copernicus himself before a single text−book expounded his theory. The text−book which then appeared, under date of 1622, was written by the famous Kepler, who perhaps was shielded in a measure from the papal consequences of such hardihood by the fact of residence in a Protestant country. Not that the Protestants of the time favored the heliocentric doctrine—we have already quoted Luther in an adverse sense—but of course it was characteristic of the Reformation temper to oppose any papal pronouncement, hence the ultramontane declaration of 1616 may indirectly have aided the doctrine which it attacked, by making that doctrine less obnoxious to Lutheran eyes. Be that as it may, the work of Kepler brought its author into no direct conflict with the authorities. But the result was quite different when, in 1632, Galileo at last broke silence and gave the world, under cover of the form of dialogue, an elaborate exposition of the Copernican theory. Galileo, it must be explained, had previously been warned to keep silent on the subject, hence his publication doubly offended the authorities. To be sure, he could reply that his dialogue introduced a champion of the Ptolemaic system to dispute with the upholder of the opposite view, and that, both views being presented with full array of argument, the reader was left to reach a verdict for himself, the author having nowhere pointedly expressed an opinion. But such an argument, of course, was specious, for no one who read the dialogue could be in doubt as to the opinion of the author. Moreover, it was hinted that Simplicio, the character who upheld the Ptolemaic doctrine and who was everywhere worsted in the argument, was intended to represent the pope himself—a suggestion which probably did no good to Galileo's cause.

The character of Galileo's artistic presentation may best be judged from an example, illustrating the vigorous assault of Salviati, the champion of the new theory, and the feeble retorts of his conservative antagonist:

"Salviati. Let us then begin our discussion with the consideration that, whatever motion may be attributed to the earth, yet we, as dwellers upon it, and hence as participators in its motion, cannot possibly perceive anything of it, presupposing that we are to consider only earthly things. On the other hand, it is just as necessary that this same motion belong apparently to all other bodies and visible objects, which, being separated from the earth, do not take part in its motion. The correct method to discover whether one can ascribe motion to the earth, and what kind of motion, is, therefore, to investigate and observe whether in bodies outside the earth a perceptible motion may be discovered which belongs to all alike. Because a movement which is perceptible only in the moon, for instance, and has nothing to do with Venus or Jupiter or other stars, cannot possibly be peculiar to the earth, nor can its seat be anywhere else than in the moon. Now there is one such universal movement which controls all others—namely, that which the sun, moon, the other planets, the fixed stars—in short, the whole universe, with the single exception of the earth—appears to execute from east to west in the space of twenty−four hours. This now, as it appears at the first glance anyway, might just as well be a motion of the earth alone as of all the rest of the universe with the exception of the earth, for the same phenomena would result from either hypothesis. Beginning with the most general, I will enumerate the reasons which seem to speak in favor of the earth's motion. When we merely consider the immensity of the starry sphere in comparison with the smallness of the terrestrial ball, which is contained many million times in the former, and then think of the rapidity of the motion which completes a whole rotation in one day and night, I cannot persuade myself how any one can hold it to be more reasonable and credible that it is the heavenly sphere which rotates, while the earth stands still.

"Simplicio. I do not well understand how that powerful motion may be said to as good as not exist for the sun, the moon, the other planets, and the innumerable host of fixed stars. Do you call that nothing when the sun goes from one meridian to another, rises up over this horizon and sinks behind that one, brings now day, and now night; when the moon goes through similar changes, and the other planets and fixed stars in the same way?

"Salviati. All the changes you mention are such only in respect to the earth. To convince yourself of it, only imagine the earth out of existence. There would then be no rising and setting of the sun or of the moon, no horizon, no meridian, no day, no night—in short, the said motion causes no change of any sort in the relation of the sun to the moon or to any of the other heavenly bodies, be they planets or fixed stars. All changes are rather in respect to the earth; they may all be reduced to the simple fact that the sun is first visible in China, then in Persia, afterwards in Egypt, Greece, France, Spain, America, etc., and that the same thing happens with the moon and the other heavenly bodies. Exactly the same thing happens and in exactly the same way if, instead of disturbing so large a part of the universe, you let the earth revolve about itself. The difficulty is, however, doubled, inasmuch as a second very important problem presents itself. If, namely, that powerful motion is ascribed to the heavens, it is absolutely necessary to regard it as opposed to the individual motion of all the planets, every one of which indubitably has its own very leisurely and moderate movement from west to east. If, on the other hand, you let the earth move about itself, this opposition of motion disappears.

"The improbability is tripled by the complete overthrow of that order which rules all the heavenly bodies in which the revolving motion is definitely established. The greater the sphere is in such a case, so much longer is the time required for its revolution; the smaller the sphere the shorter the time. Saturn, whose orbit surpasses those of all the planets in size, traverses it in thirty years. Jupiter[4] completes its smaller course in twelve years, Mars in two; the moon performs its much smaller revolution within a month. Just as clearly in the Medicean stars, we see that the one nearest Jupiter completes its revolution in a very short time—about forty−two hours; the next in about three and one−half days, the third in seven, and the most distant one in sixteen days. This rule, which is followed throughout, will still remain if we ascribe the twenty−four−hourly motion to a rotation of the earth. If, however, the earth is left motionless, we must go first from the very short rule of the moon to ever greater ones—to the two−yearly rule of Mars, from that to the twelve−yearly one of Jupiter, from here to the thirty−yearly one of Saturn, and then suddenly to an incomparably greater sphere, to which also we must ascribe a complete rotation in twenty−four hours. If, however, we assume a motion of the earth, the rapidity of the periods is very well preserved; from the slowest sphere of Saturn we come to the wholly motionless fixed stars. We also escape thereby a fourth difficulty, which arises as soon as we assume that there is motion in the sphere of the stars. I mean the great unevenness in the movement of these very stars, some of which would have to revolve with extraordinary rapidity in immense circles, while others moved very slowly in small circles, since some of them are at a greater, others at a less, distance from the pole. That is likewise an inconvenience, for, on the one hand, we see all those stars, the motion of which is indubitable, revolve in great circles, while, on the other hand, there seems to be little object in placing bodies, which are to move in circles, at an enormous distance from the centre and then let them move in very small circles. And not only are the size of the different circles and therewith the rapidity of the movement very different in the different fixed stars, but the same stars also change their orbits and their rapidity of motion. Therein consists the fifth inconvenience. Those stars, namely, which were at the equator two thousand years ago, and hence described great circles in their revolutions, must to−day move more slowly and in smaller circles, because they are many degrees removed from it. It will even happen, after not so very long a time, that one of those which have hitherto been continually in motion will finally coincide with the pole and stand still, but after a period of repose will again begin to move. The other stars in the mean while, which unquestionably move, all have, as was said, a great circle for an orbit and keep this unchangeably.

"The improbability is further increased—this may be considered the sixth inconvenience—by the fact that it is impossible to conceive what degree of solidity those immense spheres must have, in the depths of which so many stars are fixed so enduringly that they are kept revolving evenly in spite of such difference of motion without changing their respective positions. Or if, according to the much more probable theory, the heavens are fluid, and every star describes an orbit of its own, according to what law then, or for what reason, are their orbits so arranged that, when looked at from the earth, they appear to be contained in one single sphere? To attain this it seems to me much easier and more convenient to make them motionless instead of moving, just as the paving−stones on the market−place, for instance, remain in order more easily than the swarms of children running about on them.

"Finally, the seventh difficulty: If we attribute the daily rotation to the higher region of the heavens, we should have to endow it with force and power sufficient to carry with it the innumerable host of the fixed stars —every one a body of very great compass and much larger than the earth—and all the planets, although the latter, like the earth, move naturally in an opposite direction. In the midst of all this the little earth, single and alone, would obstinately and wilfully withstand such force—a supposition which, it appears to me, has much against it. I could also not explain why the earth, a freely poised body, balancing itself about its centre, and surrounded on all sides by a fluid medium, should not be affected by the universal rotation. Such difficulties, however, do not confront us if we attribute motion to the earth—such a small, insignificant body in comparison with the whole universe, and which for that very reason cannot exercise any power over the latter.

"Simplicio. You support your arguments throughout, it seems to me, on the greater ease and simplicity with which the said effects are produced. You mean that as a cause the motion of the earth alone is just as satisfactory as the motion of all the rest of the universe with the exception of the earth; you hold the actual event to be much easier in the former case than in the latter. For the ruler of the universe, however, whose might is infinite, it is no less easy to move the universe than the earth or a straw balm. But if his power is infinite, why should not a greater, rather than a very small, part of it be revealed to me?

"Salviati. If I had said that the universe does not move on account of the impotence of its ruler, I should have been wrong and your rebuke would have been in order. I admit that it is just as easy for an infinite power to move a hundred thousand as to move one. What I said, however, does not refer to him who causes the motion, but to that which is moved. In answer to your remark that it is more fitting for an infinite power to reveal a large part of itself rather than a little, I answer that, in relation to the infinite, one part is not greater than another, if both are finite. Hence it is unallowable to say that a hundred thousand is a larger part of an infinite number than two, although the former is fifty thousand times greater than the latter. If, therefore, we consider the moving bodies, we must unquestionably regard the motion of the earth as a much simpler process than that of the universe; if, furthermore, we direct our attention to so many other simplifications which may be reached only by this theory, the daily movement of the earth must appear much more probable than the motion of the universe without the earth, for, according to Aristotle's just axiom, 'Frustra fit per plura, quod potest fieri per p auciora' (It is vain to expend many means where a few are sufficient)."[2]

The work was widely circulated, and it was received with an interest which bespeaks a wide−spread undercurrent of belief in the Copernican doctrine. Naturally enough, it attracted immediate attention from the church authorities. Galileo was summoned to appear at Rome to defend his conduct. The philosopher, who was now in his seventieth year, pleaded age and infirmity. He had no desire for personal experience of the tribunal of the Inquisition; but the mandate was repeated, and Galileo went to Rome. There, as every one knows, he disavowed any intention to oppose the teachings of Scripture, and formally renounced the heretical doctrine of the earth's motion. According to a tale which so long passed current that every historian must still repeat it though no one now believes it authentic, Galileo qualified his renunciation by muttering to himself, "E pur si muove" (It does move, none the less), as he rose to his feet and retired from the presence of his persecutors. The tale is one of those fictions which the dramatic sense of humanity is wont to impose upon history, but, like most such fictions, it expresses the spirit if not the letter of truth; for just as no one believes that Galileo's lips uttered the phrase, so no one doubts that the rebellious words were in his mind.

After his formal renunciation, Galileo was allowed to depart, but with the injunction that he abstain in future from heretical teaching. The remaining ten years of his life were devoted chiefly to mechanics, where his experiments fortunately opposed the Aristotelian rather than the Hebrew teachings. Galileo's death occurred in 1642, a hundred years after the death of Copernicus. Kepler had died thirteen years before, and there remained no astronomer in the field who is conspicuous in the history of science as a champion of the Copernican doctrine. But in truth it might be said that the theory no longer needed a champion. The researches of Kepler and Galileo had produced a mass of evidence for the Copernican theory which amounted to demonstration. A generation or two might be required for this evidence to make itself everywhere known among men of science, and of course the ecclesiastical authorities must be expected to stand by their guns for a somewhat longer period. In point of fact, the ecclesiastical ban was not technically removed by the striking of the Copernican books from the list of the Index Expurgatorius until the year 1822, almost two hundred years after the date of Galileo's dialogue. But this, of course, is in no sense a guide to the state of general opinion regarding the theory. We shall gain a true gauge as to this if we assume that the greater number of important thinkers had accepted the heliocentric doctrine before the middle of the seventeenth century, and that before the close of that century the old Ptolemaic idea had been quite abandoned. A wonderful revolution in man's estimate of the universe had thus been effected within about two centuries after the birth of Copernicus.

V. GALILEO AND THE NEW PHYSICS

After Galileo had felt the strong hand of the Inquisition, in 1632, he was careful to confine his researches, or at least his publications, to topics that seemed free from theological implications. In doing so he reverted to the field of his earliest studies —namely, the field of mechanics; and the Dialoghi delle Nuove Scienze, which he finished in 1636, and which was printed two years later, attained a celebrity no less than that of the heretical dialogue that had preceded it. The later work was free from all apparent heresies, yet perhaps it did more towards the establishment of the Copernican doctrine, through the teaching of correct mechanical principles, than the other work had accomplished by a more direct method.

Galileo's astronomical discoveries were, as we have seen, in a sense accidental; at least, they received their inception through the inventive genius of another. His mechanical discoveries, on the other hand, were the natural output of his own creative genius. At the very beginning of his career, while yet a very young man, though a professor of mathematics at Pisa, he had begun that onslaught upon the old Aristotelian ideas which he was to continue throughout his life. At the famous leaning tower in Pisa, the young iconoclast performed, in the year 1590, one of the most theatrical demonstrations in the history of science. Assembling a multitude of champions of the old ideas, he proposed to demonstrate the falsity of the Aristotelian doctrine that the velocity of falling bodies is proportionate to their weight. There is perhaps no fact more strongly illustrative of the temper of the Middle Ages than the fact that this doctrine, as taught by the Aristotelian philosopher, should so long have gone unchallenged. Now, however, it was put to the test; Galileo released a half−pound weight and a hundred−pound cannon−ball from near the top of the tower, and, needless to say, they reached the ground together. Of course, the spectators were but little pleased with what they saw. They could not doubt the evidence of their own senses as to the particular experiment in question; they could suggest, however, that the experiment involved a violation of the laws of nature through the practice of magic. To controvert so firmly established an idea savored of heresy. The young man guilty of such iconoclasm was naturally looked at askance by the scholarship of his time. Instead of being applauded, he was hissed, and he found it expedient presently to retire from Pisa.

Fortunately, however, the new spirit of progress had made itself felt more effectively in some other portions of Italy, and so Galileo found a refuge and a following in Padua, and afterwards in Florence; and while, as we have seen, he was obliged to curb his enthusiasm regarding the subject that was perhaps nearest his heart—the promulgation of the Copernican theory—yet he was permitted in the main to carry on his experimental observations unrestrained. These experiments gave him a place of unquestioned authority among his contemporaries, and they have transmitted his name to posterity as that of one of the greatest of experimenters and the virtual founder of modern mechanical science. The experiments in question range over a wide field; but for the most part they have to do with moving bodies and with questions of force, or, as we should now say, of energy. The experiment at the leaning tower showed that the velocity of falling bodies is independent of the weight of the bodies, provided the weight is sufficient to overcome the resistance of the atmosphere. Later experiments with falling bodies led to the discovery of laws regarding the accelerated velocity of fall. Such velocities were found to bear a simple relation to the period of time from the beginning of the fall. Other experiments, in which balls were allowed to roll down inclined planes, corroborated the observation that the pull of gravitation gave a velocity proportionate to the length of fall, whether such fall were direct or in a slanting direction.

These studies were associated with observations on projectiles, regarding which Galileo was the first to entertain correct notions. According to the current idea, a projectile fired, for example, from a cannon, moved in a straight horizontal line until the propulsive force was exhausted, and then fell to the ground in a perpendicular line. Galileo taught that the projectile begins to fall at once on leaving the mouth of the cannon and traverses a parabolic course. According to his idea, which is now familiar to every one, a cannon−ball dropped from the level of the cannon's muzzle will strike the ground simultaneously with a ball fired horizontally from the cannon. As to the paraboloid course pursued by the projectile, the resistance of the air is a factor which Galileo could not accurately compute, and which interferes with the practical realization of his theory. But this is a minor consideration. The great importance of his idea consists in the recognition that such a force as that of gravitation acts in precisely the same way upon all unsupported bodies, whether or not such bodies be at the same time acted upon by a force of translation.

Out of these studies of moving bodies was gradually developed a correct notion of several important general laws of mechanics—laws a knowledge of which was absolutely essential to the progress of physical science. The belief in the rotation of the earth made necessary a clear conception that all bodies at the surface of the earth partake of that motion quite independently of their various observed motions in relation to one another. This idea was hard to grasp, as an oft−repeated argument shows. It was asserted again and again that, if the earth rotates, a stone dropped from the top of a tower could not fall at the foot of the tower, since the earth's motion would sweep the tower far away from its original position while the stone is in transit.

This was one of the stock arguments against the earth's motion, yet it was one that could be refuted with the greatest ease by reasoning from strictly analogous experiments. It might readily be observed, for example, that a stone dropped from a moving cart does not strike the ground directly below the point from which it is dropped, but partakes of the forward motion of the cart. If any one doubt this he has but to jump from a moving cart to be given a practical demonstration of the fact that his entire body was in some way influenced by the motion of translation. Similarly, the simple experiment of tossing a ball from the deck of a moving ship will convince any one that the ball partakes of the motion of the ship, so that it can be manipulated precisely as if the manipulator were standing on the earth. In short, every−day experience gives us illustrations of what might be called compound motion, which makes it seem altogether plausible that, if the earth is in motion, objects at its surface will partake of that motion in a way that does not interfere with any other movements to which they may be subjected. As the Copernican doctrine made its way, this idea of compound motion naturally received more and more attention, and such experiments as those of Galileo prepared the way for a new interpretation of the mechanical principles involved.

The great difficulty was that the subject of moving bodies had all along been contemplated from a wrong point of view. Since force must be applied to an object to put it in motion, it was perhaps not unnaturally assumed that similar force must continue to be applied to keep the object in motion. When, for example, a stone is thrown from the hand, the direct force applied necessarily ceases as soon as the projectile leaves the hand. The stone, nevertheless, flies on for a certain distance and then falls to the ground. How is this flight of the stone to be explained? The ancient philosophers puzzled more than a little over this problem, and the Aristotelians reached the conclusion that the motion of the hand had imparted a propulsive motion to the air, and that this propulsive motion was transmitted to the stone, pushing it on. Just how the air took on this propulsive property was not explained, and the vagueness of thought that characterized the time did not demand an explanation. Possibly the dying away of ripples in water may have furnished, by analogy, an explanation of the gradual dying out of the impulse which propels the stone.

All of this was, of course, an unfortunate maladjustment of the point of view. As every one nowadays knows, the air retards the progress of the stone, enabling the pull of gravitation to drag it to the earth earlier than it otherwise could. Were the resistance of the air and the pull of gravitation removed, the stone as projected from the hand would fly on in a straight line, at an unchanged velocity, forever. But this fact, which is expressed in what we now term the first law of motion, was extremely difficult to grasp. The first important step towards it was perhaps implied in Galileo's study of falling bodies. These studies, as we have seen, demonstrated that a half−pound weight and a hundred−pound weight fall with the same velocity. It is, however, matter of common experience that certain bodies, as, for example, feathers, do not fall at the same rate of speed with these heavier bodies. This anomaly demands an explanation, and the explanation is found in the resistance offered the relatively light object by the air. Once the idea that the air may thus act as an impeding force was grasped, the investigator of mechanical principles had entered on a new and promising course.

Galileo could not demonstrate the retarding influence of air in the way which became familiar a generation or two later; he could not put a feather and a coin in a vacuum tube and prove that the two would there fall with equal velocity, because, in his day, the air−pump had not yet been invented. The experiment was made only a generation after the time of Galileo, as we shall see; but, meantime, the great Italian had fully grasped the idea that atmospheric resistance plays a most important part in regard to the motion of falling and projected bodies. Thanks largely to his own experiments, but partly also to the efforts of others, he had come, before the end of his life, pretty definitely to realize that the motion of a projectile, for example, must be thought of as inherent in the projectile itself, and that the retardation or ultimate cessation of that motion is due to the action of antagonistic forces. In other words, he had come to grasp the meaning of the first law of motion. It remained, however, for the great Frenchman Descartes to give precise expression to this law two years after Galileo's death. As Descartes expressed it in his Principia Philosophiae, published in 1644, any body once in motion tends to go on in a straight line, at a uniform rate of speed, forever. Contrariwise, a stationary body will remain forever at rest unless acted on by some disturbing force.

This all−important law, which lies at the very foundation of all true conceptions of mechanics, was thus worked out during the first half of the seventeenth century, as the outcome of numberless experiments for which Galileo's experiments with failing bodies furnished the foundation. So numerous and so gradual were the steps by which the reversal of view regarding moving bodies was effected that it is impossible to trace them in detail. We must be content to reflect that at the beginning of the Galilean epoch utterly false notions regarding the subject were entertained by the very greatest philosophers—by Galileo himself, for example, and by Kepler—whereas at the close of that epoch the correct and highly illuminative view had been attained.

We must now consider some other experiments of Galileo which led to scarcely less−important results. The experiments in question had to do with the movements of bodies passing down an inclined plane, and with the allied subject of the motion of a pendulum. The elaborate experiments of Galileo regarding the former subject were made by measuring the velocity of a ball rolling down a plane inclined at various angles. He found that the velocity acquired by a ball was proportional to the height from which the ball descended regardless of the steepness of the incline. Experiments were made also with a ball rolling down a curved gutter, the curve representing the are of a circle. These experiments led to the study of the curvilinear motions of a weight suspended by a cord; in other words, of the pendulum.

Regarding the motion of the pendulum, some very curious facts were soon ascertained. Galileo found, for example, that a pendulum of a given length performs its oscillations with the same frequency though the arc described by the pendulum be varied greatly.[1] He found, also, that the rate of oscillation for pendulums of different lengths varies according to a simple law. In order that one pendulum shall oscillate one−half as fast as another, the length of the pendulums must be as four to one. Similarly, by lengthening the pendulums nine times, the oscillation is reduced to one−third, In other words, the rate of oscillation of pendulums varies inversely as the square of their length. Here, then, is a simple relation between the motions of swinging bodies which suggests the relation which Kepler bad discovered between the relative motions of the planets. Every such discovery coming in this age of the rejuvenation of experimental science had a peculiar force in teaching men the all−important lesson that simple laws lie back of most of the diverse phenomena of nature, if only these laws can be discovered.

Galileo further observed that his pendulum might be constructed of any weight sufficiently heavy readily to overcome the atmospheric resistance, and that, with this qualification, neither the weight nor the material had any influence upon the time of oscillation, this being solely determined by the length of the cord. Naturally, the practical utility of these discoveries was not overlooked by Galileo. Since a pendulum of a given length oscillates with unvarying rapidity, here is an obvious means of measuring time. Galileo, however, appears not to have met with any great measure of success in putting this idea into practice. It remained for the mechanical ingenuity of Huyghens to construct a satisfactory pendulum clock.

As a theoretical result of the studies of rolling and oscillating bodies, there was developed what is usually spoken of as the third law of motion—namely, the law that a given force operates upon a moving body with an effect proportionate to its effect upon the same body when at rest. Or, as Whewell states the law: "The dynamical effect of force is as the statical effect; that is, the velocity which any force generates in a given time, when it puts the body in motion, is proportional to the pressure which this same force produces in a body at rest."[2] According to the second law of motion, each one of the different forces, operating at the same time upon a moving body, produces the same effect as if it operated upon the body while at rest.

STEVINUS AND THE LAW OF EQUILIBRIUM

It appears, then, that the mechanical studies of Galileo, taken as a whole, were nothing less than revolutionary. They constituted the first great advance upon the dynamic studies of Archimedes, and then led to the secure foundation for one of the most important of modern sciences. We shall see that an important company of students entered the field immediately after the time of Galileo, and carried forward the work he had so well begun. But before passing on to the consideration of their labors, we must consider work in allied fields of two men who were contemporaries of Galileo and whose original labors were in some respects scarcely less important than his own. These men are the Dutchman Stevinus, who must always be remembered as a co−laborer with Galileo in the foundation of the science of dynamics, and the Englishman Gilbert, to whom is due the unqualified praise of first subjecting the phenomenon of magnetism to a strictly scientific investigation.

Stevinus was born in the year 1548, and died in 1620. He was a man of a practical genius, and he attracted the attention of his non−scientific contemporaries, among other ways, by the construction of a curious land−craft, which, mounted on wheels, was to be propelled by sails like a boat. Not only did he write a book on this curious horseless carriage, but he put his idea into practical application, producing a vehicle which actually traversed the distance between Scheveningen and Petton, with no fewer than twenty−seven passengers, one of them being Prince Maurice of Orange. This demonstration was made about the year 1600. It does not appear, however, that any important use was made of the strange vehicle; but the man who invented it put his mechanical ingenuity to other use with better effect. It was he who solved the problem of oblique forces, and who discovered the important hydrostatic principle that the pressure of fluids is proportionate to their depth, without regard to the shape of the including vessel.

The study of oblique forces was made by Stevinus with the aid of inclined planes. His most demonstrative experiment was a very simple one, in which a chain of balls of equal weight was hung from a triangle; the triangle being so constructed as to rest on a horizontal base, the oblique sides bearing the relation to each other of two to one. Stevinus found that his chain of balls just balanced when four balls were on the longer side and two on the shorter and steeper side. The balancing of force thus brought about constituted a stable equilibrium, Stevinus being the first to discriminate between such a condition and the unbalanced condition called unstable equilibrium. By this simple experiment was laid the foundation of the science of statics. Stevinus had a full grasp of the principle which his experiment involved, and he applied it to the solution of oblique forces in all directions. Earlier investigations of Stevinus were published in 1608. His collected works were published at Leyden in 1634.

This study of the equilibrium of pressure of bodies at rest led Stevinus, not unnaturally, to consider the allied subject of the pressure of liquids. He is to be credited with the explanation of the so−called hydrostatic paradox. The familiar modern experiment which illustrates this paradox is made by inserting a long perpendicular tube of small caliber into the top of a tight barrel. On filling the barrel and tube with water, it is possible to produce a pressure which will burst the barrel, though it be a strong one, and though the actual weight of water in the tube is comparatively insignificant. This illustrates the fact that the pressure at the bottom of a column of liquid is proportionate to the height of the column, and not to its bulk, this being the hydrostatic paradox in question. The explanation is that an enclosed fluid under pressure exerts an equal force upon all parts of the circumscribing wall; the aggregate pressure may, therefore, be increased indefinitely by increasing the surface. It is this principle, of course, which is utilized in the familiar hydrostatic press. Theoretical explanations of the pressure of liquids were supplied a generation or two later by numerous investigators, including Newton, but the practical refoundation of the science of hydrostatics in modern times dates from the experiments of Stevinus.

GALILEO AND THE EQUILIBRIUM OF FLUIDS

Experiments of an allied character, having to do with the equilibrium of fluids, exercised the ingenuity of Galileo. Some of his most interesting experiments have to do with the subject of floating bodies. It will be recalled that Archimedes, away back in the Alexandrian epoch, had solved the most important problems of hydrostatic equilibrium. Now, however, his experiments were overlooked or forgotten, and Galileo was obliged to make experiments anew, and to combat fallacious views that ought long since to have been abandoned. Perhaps the most illuminative view of the spirit of the times can be gained by quoting at length a paper of Galileo's, in which he details his own experiments with floating bodies and controverts the views of his opponents. The paper has further value as illustrating Galileo's methods both as experimenter and as speculative reasoner.

The current view, which Galileo here undertakes to refute, asserts that water offers resistance to penetration, and that this resistance is instrumental in determining whether a body placed in water will float or sink. Galileo contends that water is non−resistant, and that bodies float or sink in virtue of their respective weights. This, of course, is merely a restatement of the law of Archimedes. But it remains to explain the fact that bodies of a certain shape will float, while bodies of the same material and weight, but of a different shape, will sink. We shall see what explanation Galileo finds of this anomaly as we proceed.

In the first place, Galileo makes a cone of wood or of wax, and shows that when it floats with either its point or its base in the water, it displaces exactly the same amount of fluid, although the apex is by its shape better adapted to overcome the resistance of the water, if that were the cause of buoyancy. Again, the experiment may be varied by tempering the wax with filings of lead till it sinks in the water, when it will be found that in any figure the same quantity of cork must be added to it to raise the surface.

"But," says Galileo, "this silences not my antagonists; they say that all the discourse hitherto made by me imports little to them, and that it serves their turn; that they have demonstrated in one instance, and in such manner and figure as pleases them best —namely, in a board and in a ball of ebony—that one when put into the water sinks to the bottom, and that the other stays to swim on the top; and the matter being the same, and the two bodies differing in nothing but in figure, they affirm that with all perspicuity they have demonstrated and sensibly manifested what they undertook. Nevertheless, I believe, and think I can prove, that this very experiment proves nothing against my theory. And first, it is false that the ball sinks and the board not; for the board will sink, too, if you do to both the figures as the words of our question require; that is, if you put them both in the water; for to be in the water implies to be placed in the water, and by Aristotle's own definition of place, to be placed imports to be environed by the surface of the ambient body; but when my antagonists show the floating board of ebony, they put it not into the water, but upon the water; where, being detained by a certain impediment (of which more anon), it is surrounded, partly with water, partly with air, which is contrary to our agreement, for that was that bodies should be in the water, and not part in the water, part in the air.

"I will not omit another reason, founded also upon experience, and, if I deceive not myself, conclusive against the notion that figure, and the resistance of the water to penetration, have anything to do with the buoyancy of bodies. Choose a piece of wood or other matter, as, for instance, walnut−wood, of which a ball rises from the bottom of the water to the surface more slowly than a ball of ebony of the same size sinks, so that, clearly, the ball of ebony divides the water more readily in sinking than the ball of wood does in rising. Then take a board of walnut−tree equal to and like the floating one of my antagonists; and if it be true that this latter floats by reason of the figure being unable to penetrate the water, the other of walnut−tree, without a question, if thrust to the bottom, ought to stay there, as having the same impeding figure, and being less apt to overcome the said resistance of the water. But if we find by experience that not only the thin board, but every other figure of the same walnut−tree, will return to float, as unquestionably we shall, then I must desire my opponents to forbear to attribute the floating of the ebony to the figure of the board, since the resistance of the water is the same in rising as in sinking, and the force of ascension of the walnut−tree is less than the ebony's force for going to the bottom.

"Now let us return to the thin plate of gold or silver, or the thin board of ebony, and let us lay it lightly upon the water, so that it may stay there without sinking, and carefully observe the effect. It will appear clearly that the plates are a considerable matter lower than the surface of the water, which rises up and makes a kind of rampart round them on every side. But if it has already penetrated and overcome the continuity of the water, and is of its own nature heavier than the water, why does it not continue to sink, but stop and suspend itself in that little dimple that its weight has made in the water? My answer is, because in sinking till its surface is below the water, which rises up in a bank round it, it draws after and carries along with it the air above it, so that that which, in this case, descends in the water is not only the board of ebony or the plate of iron, but a compound of ebony and air, from which composition results a solid no longer specifically heavier than the water, as was the ebony or gold alone. But, gentlemen, we want the same matter; you are to alter nothing but the shape, and, therefore, have the goodness to remove this air, which may be done simply by washing the surface of the board, for the water having once got between the board and the air will run together, and the ebony will go to the bottom; and if it does not, you have won the day.

"But methinks I hear some of my antagonists cunningly opposing this, and telling me that they will not on any account allow their boards to be wetted, because the weight of the water so added, by making it heavier than it was before, draws it to the bottom, and that the addition of new weight is contrary to our agreement, which was that the matter should be the same.

"To this I answer, first, that nobody can suppose bodies to be put into the water without their being wet, nor do I wish to do more to the board than you may do to the ball. Moreover, it is not true that the board sinks on account of the weight of the water added in the washing; for I will put ten or twenty drops on the floating board, and so long as they stand separate it shall not sink; but if the board be taken out and all that water wiped off, and the whole surface bathed with one single drop, and put it again upon the water, there is no question but it will sink, the other water running to cover it, being no longer hindered by the air. In the next place, it is altogether false that water can in any way increase the weight of bodies immersed in it, for water has no weight in water, since it does not sink. Now just as he who should say that brass by its own nature sinks, but that when formed into the shape of a kettle it acquires from that figure the virtue of lying in water without sinking, would say what is false, because that is not purely brass which then is put into the water, but a compound of brass and air; so is it neither more nor less false that a thin plate of brass or ebony swims by virtue of its dilated and broad figure. Also, I cannot omit to tell my opponents that this conceit of refusing to bathe the surface of the board might beget an opinion in a third person of a poverty of argument on their side, especially as the conversation began about flakes of ice, in which it would be simple to require that the surfaces should be kept dry; not to mention that such pieces of ice, whether wet or dry, always float, and so my antagonists say, because of their shape.

"Some may wonder that I affirm this power to be in the air of keeping plate of brass or silver above water, as if in a certain sense I would attribute to the air a kind of magnetic virtue for sustaining heavy bodies with which it is in contact. To satisfy all these doubts I have contrived the following experiment to demonstrate how truly the air does support these bodies; for I have found, when one of these bodies which floats when placed lightly on the water is thoroughly bathed and sunk to the bottom, that by carrying down to it a little air without otherwise touching it in the least, I am able to raise and carry it back to the top, where it floats as before. To this effect, I take a ball of wax, and with a little lead make it just heavy enough to sink very slowly to the bottom, taking care that its surface be quite smooth and even. This, if put gently into the water, submerges almost entirely, there remaining visible only a little of the very top, which, so long as it is joined to the air, keeps the ball afloat; but if we take away the contact of the air by wetting this top, the ball sinks to the bottom and remains there. Now to make it return to the surface by virtue of the air which before sustained it, thrust into the water a glass with the mouth downward, which will carry with it the air it contains, and move this down towards the ball until you see, by the transparency of the glass, that the air has reached the top of it; then gently draw the glass upward, and you will see the ball rise, and afterwards stay on the top of the water, if you carefully part the glass and water without too much disturbing it."[3]

It will be seen that Galileo, while holding in the main to a correct thesis, yet mingles with it some false ideas. At the very outset, of course, it is not true that water has no resistance to penetration; it is true, however, in the sense in which Galileo uses the term—that is to say, the resistance of the water to penetration is not the determining factor ordinarily in deciding whether a body sinks or floats. Yet in the case of the flat body it is not altogether inappropriate to say that the water resists penetration and thus supports the body. The modern physicist explains the phenomenon as due to surface−tension of the fluid. Of course, Galileo's disquisition on the mixing of air with the floating body is utterly fanciful. His experiments were beautifully exact; his theorizing from them was, in this instance, altogether fallacious. Thus, as already intimated, his paper is admirably adapted to convey a double lesson to the student of science.

WILLIAM GILBERT AND THE STUDY OF MAGNETISM

It will be observed that the studies of Galileo and Stevinus were chiefly concerned with the force of gravitation. Meanwhile, there was an English philosopher of corresponding genius, whose attention was directed towards investigation of the equally mysterious force of terrestrial magnetism. With the doubtful exception of Bacon, Gilbert was the most distinguished man of science in England during the reign of Queen Elizabeth. He was for many years court physician, and Queen Elizabeth ultimately settled upon him a pension that enabled him to continue his researches in pure science.

His investigations in chemistry, although supposed to be of great importance, are mostly lost; but his great work, De Magnete, on which he labored for upwards of eighteen years, is a work of sufficient importance, as Hallam says, "to raise a lasting reputation for its author." From its first appearance it created a profound impression upon the learned men of the continent, although in England Gilbert's theories seem to have been somewhat less favorably received. Galileo freely expressed his admiration for the work and its author; Bacon, who admired the author, did not express the same admiration for his theories; but Dr. Priestley, later, declared him to be "the father of modern electricity."

Strangely enough, Gilbert's book had never been translated into English, or apparently into any other language, until recent years, although at the time of its publication certain learned men, unable to read the book in the original, had asked that it should be. By this neglect, or oversight, a great number of general readers as well as many scientists, through succeeding centuries, have been deprived of the benefit of writings that contained a good share of the fundamental facts about magnetism as known to−day.

Gilbert was the first to discover that the earth is a great magnet, and he not only gave the name of "pole" to the extremities of the magnetic needle, but also spoke of these "poles" as north and south pole, although he used these names in the opposite sense from that in which we now use them, his south pole being the extremity which pointed towards the north, and vice versa. He was also first to make use of the terms "electric force," "electric emanations," and "electric attractions."

It is hardly necessary to say that some of the views taken by Gilbert, many of his theories, and the accuracy of some of his experiments have in recent times been found to be erroneous. As a pioneer in an unexplored field of science, however, his work is remarkably accurate. "On the whole," says Dr. John Robinson, "this performance contains more real information than any writing of the age in which he lived, and is scarcely exceeded by any that has appeared since."[4]

In the preface to his work Gilbert says: "Since in the discovery of secret things, and in the investigation of hidden causes, stronger reasons are obtained from sure experiments and demonstrated arguments than from probable conjectures and the opinions of philosophical speculators of the common sort, therefore, to the end of that noble substance of that great loadstone, our common mother (the earth), still quite unknown, and also that the forces extraordinary and exalted of this globe may the better be understood, we have decided, first, to begin with the common stony and ferruginous matter, and magnetic bodies, and the part of the earth that we may handle and may perceive with senses, and then to proceed with plain magnetic experiments, and to penetrate to the inner parts of the earth."[5]

Before taking up the demonstration that the earth is simply a giant loadstone, Gilbert demonstrated in an ingenious way that every loadstone, of whatever size, has definite and fixed poles. He did this by placing the stone in a metal lathe and converting it into a sphere, and upon this sphere demonstrated how the poles can be found. To this round loadstone he gave the name of terrella—that is, little earth.

"To find, then, poles answering to the earth," he says, "take in your hand the round stone, and lay on it a needle or a piece of iron wire: the ends of the wire move round their middle point, and suddenly come to a standstill. Now, with ochre or with chalk, mark where the wire lies still and sticks. Then move the middle or centre of the wire to another spot, and so to a third and fourth, always marking the stone along the length of the wire where it stands still; the lines so marked will exhibit meridian circles, or circles like meridians, on the stone or terrella; and manifestly they will all come together at the poles of the stone. The circle being continued in this way, the poles appear, both the north and the south, and betwixt these, midway, we may draw a large circle for an equator, as is done by the astronomer in the heavens and on his spheres, and by the geographer on the terrestrial globe."[6]

Gilbert had tried the familiar experiment of placing the loadstone on a float in water, and observed that the poles always revolved until they pointed north and south, which he explained as due to the earth's magnetic attraction. In this same connection he noticed that a piece of wrought iron mounted on a cork float was attracted by other metals to a slight degree, and he observed also that an ordinary iron bar, if suspended horizontally by a thread, assumes invariably a north and south direction. These, with many other experiments of a similar nature, convinced him that the earth "is a magnet and a loadstone," which he says is a "new and till now unheard−of view of the earth."

Fully to appreciate Gilbert's revolutionary views concerning the earth as a magnet, it should be remembered that numberless theories to explain the action of the electric needle had been advanced. Columbus and Paracelsus, for example, believed that the magnet was attracted by some point in the heavens, such as a magnetic star. Gilbert himself tells of some of the beliefs that had been held by his predecessors, many of whom he declares "wilfully falsify." One of his first steps was to refute by experiment such assertions as that of Cardan, that "a wound by a magnetized needle was painless"; and also the assertion of Fracastoni that loadstone attracts silver; or that of Scalinger, that the diamond will attract iron; and the statement of Matthiolus that "iron rubbed with garlic is no longer attracted to the loadstone."

Gilbert made extensive experiments to explain the dipping of the needle, which had been first noticed by William Norman. His deduction as to this phenomenon led him to believe that this was also explained by the magnetic attraction of the earth, and to predict where the vertical dip would be found. These deductions seem the more wonderful because at the time he made them the dip had just been discovered, and had not been studied except at London. His theory of the dip was, therefore, a scientific prediction, based on a preconceived hypothesis. Gilbert found the dip to be 72 degrees at London; eight years later Hudson found the dip at 75 degrees 22' north latitude to be 89 degrees 30'; but it was not until over two hundred years later, in 1831, that the vertical dip was first observed by Sir James Ross at about 70 degrees 5' north latitude, and 96 degrees 43' west longitude. This was not the exact point assumed by Gilbert, and his scientific predictions, therefore, were not quite correct; but such comparatively slight and excusable errors mar but little the excellence of his work as a whole.

A brief epitome of some of his other important discoveries suffices to show that the exalted position in science accorded him by contemporaries, as well as succeeding generations of scientists, was well merited. He was first to distinguish between magnetism and electricity, giving the latter its name. He discovered also the "electrical charge," and pointed the way to the discovery of insulation by showing that the charge could be retained some time in the excited body by covering it with some non−conducting substance, such as silk; although, of course, electrical conduction can hardly be said to have been more than vaguely surmised, if understood at all by him. The first electrical instrument ever made, and known as such, was invented by him, as was also the first magnetometer, and the first electrical indicating device. Although three centuries have elapsed since his death, the method of magnetizing iron first introduced by him is in common use to−day.

He made exhaustive experiments with a needle balanced on a pivot to see how many substances he could find which, like amber, on being rubbed affected the needle. In this way he discovered that light substances were attracted by alum, mica, arsenic, sealing−wax, lac sulphur, slags, beryl, amethyst, rock−crystal, sapphire, jet, carbuncle, diamond, opal, Bristol stone, glass, glass of antimony, gum−mastic, hard resin, rock−salt, and, of course, amber. He discovered also that atmospheric conditions affected the production of electricity, dryness being unfavorable and moisture favorable.

Galileo's estimate of this first electrician is the verdict of succeeding generations. "I extremely admire and envy this author," he said. "I think him worthy of the greatest praise for the many new and true observations which he has made, to the disgrace of so many vain and fabling authors."

STUDIES OF LIGHT, HEAT, AND ATMOSPHERIC PRESSURE

We have seen that Gilbert was by no means lacking in versatility, yet the investigations upon which his fame is founded were all pursued along one line, so that the father of magnetism may be considered one of the earliest of specialists in physical science. Most workers of the time, on the other band, extended their investigations in many directions. The sum total of scientific knowledge of that day had not bulked so large as to exclude the possibility that one man might master it all. So we find a Galileo, for example, making revolutionary discoveries in astronomy, and performing fundamental experiments in various fields of physics. Galileo's great contemporary, Kepler, was almost equally versatile, though his astronomical studies were of such pre−eminent importance that his other investigations sink into relative insignificance. Yet he performed some notable experiments in at least one department of physics. These experiments had to do with the refraction of light, a subject which Kepler was led to investigate, in part at least, through his interest in the telescope.

We have seen that Ptolemy in the Alexandrian time, and Alhazen, the Arab, made studies of refraction. Kepler repeated their experiments, and, striving as always to generalize his observations, he attempted to find the law that governed the observed change of direction which a ray of light assumes in passing from one medium to another. Kepler measured the angle of refraction by means of a simple yet ingenious trough−like apparatus which enabled him to compare readily the direct and refracted rays. He discovered that when a ray of light passes through a glass plate, if it strikes the farther surface of the glass at an angle greater than 45 degrees it will be totally refracted instead of passing through into the air. He could not well fail to know that different mediums refract light differently, and that for the same medium the amount of light valies with the change in the angle of incidence. He was not able, however, to generalize his observations as he desired, and to the last the law that governs refraction escaped him. It remained for Willebrord Snell, a Dutchman, about the year 1621, to discover the law in question, and for Descartes, a little later, to formulate it. Descartes, indeed, has sometimes been supposed to be the discoverer of the law. There is reason to believe that he based his generalizations on the experiment of Snell, though he did not openly acknowledge his indebtedness. The law, as Descartes expressed it, states that the sine of the angle of incidence bears a fixed ratio to the sine of the angle of refraction for any given medium. Here, then, was another illustration of the fact that almost infinitely varied phenomena may be brought within the scope of a simple law. Once the law had been expressed, it could be tested and verified with the greatest ease; and, as usual, the discovery being made, it seems surprising that earlier investigators—in particular so sagacious a guesser as Kepler—should have missed it.

Galileo himself must have been to some extent a student of light, since, as we have seen, he made such notable contributions to practical optics through perfecting the telescope; but he seems not to have added anything to the theory of light. The subject of heat, however, attracted his attention in a somewhat different way, and he was led to the invention of the first contrivance for measuring temperatures. His thermometer was based on the afterwards familiar principle of the expansion of a liquid under the influence of heat; but as a practical means of measuring temperature it was a very crude affair, because the tube that contained the measuring liquid was exposed to the air, hence barometric changes of pressure vitiated the experiment. It remained for Galileo's Italian successors of the Accademia del Cimento of Florence to improve upon the apparatus, after the experiments of Torricelli—to which we shall refer in a moment—had thrown new light on the question of atmospheric pressure. Still later the celebrated Huygens hit upon the idea of using the melting and the boiling point of water as fixed points in a scale of measurements, which first gave definiteness to thermometric tests.

TORRICELLI

In the closing years of his life Galileo took into his family, as his adopted disciple in science, a young man, Evangelista Torricelli (1608−1647), who proved himself, during his short lifetime, to be a worthy follower of his great master. Not only worthy on account of his great scientific discoveries, but grateful as well, for when he had made the great discovery that the "suction" made by a vacuum was really nothing but air pressure, and not suction at all, he regretted that so important a step in science might not have been made by his great teacher, Galileo, instead of by himself. "This generosity of Torricelli," says Playfair, "was, perhaps, rarer than his genius: there are more who might have discovered the suspension of mercury in the barometer than who would have been willing to part with the honor of the discovery to a master or a friend."

Torricelli's discovery was made in 1643, less than two years after the death of his master. Galileo had observed that water will not rise in an exhausted tube, such as a pump, to a height greater than thirty−three feet, but he was never able to offer a satisfactory explanation of the principle. Torricelli was able to demonstrate that the height at which the water stood depended upon nothing but its weight as compared with the weight of air. If this be true, it is evident that any fluid will be supported at a definite height, according to its relative weight as compared with air. Thus mercury, which is about thirteen times more dense than water, should only rise to one−thirteenth the height of a column of water—that is, about thirty inches. Reasoning in this way, Torricelli proceeded to prove that his theory was correct. Filling a long tube, closed at one end, with mercury, he inverted the tube with its open orifice in a vessel of mercury. The column of mercury fell at once, but at a height of about thirty inches it stopped and remained stationary, the pressure of the air on the mercury in the vessel maintaining it at that height. This discovery was a shattering blow to the old theory that had dominated that field of physics for so many centuries. It was completely revolutionary to prove that, instead of a mysterious something within the tube being responsible for the suspension of liquids at certain heights, it was simply the ordinary atmospheric pressure mysterious enough, it is true—pushing upon them from without. The pressure exerted by the atmosphere was but little understood at that time, but Torricelli's discovery aided materially in solving the mystery. The whole class of similar phenomena of air pressure, which had been held in the trammel of long−established but false doctrines, was now reduced to one simple law, and the door to a solution of a host of unsolved problems thrown open.

It had long been suspected and believed that the density of the atmosphere varies at certain times. That the air is sometimes "heavy" and at other times "light" is apparent to the senses without scientific apparatus for demonstration. It is evident, then, that Torricelli's column of mercury should rise and fall just in proportion to the lightness or heaviness of the air. A short series of observations proved that it did so, and with those observations went naturally the observations as to changes in the weather. It was only necessary, therefore, to scratch a scale on the glass tube, indicating relative atmospheric pressures, and the Torricellian barometer was complete.

Such a revolutionary theory and such an important discovery were, of course, not to be accepted without controversy, but the feeble arguments of the opponents showed how untenable the old theory had become. In 1648 Pascal suggested that if the theory of the pressure of air upon the mercury was correct, it could be demonstrated by ascending a mountain with the mercury tube. As the air was known to get progressively lighter from base to summit, the height of the column should be progressively lessened as the ascent was made, and increase again on the descent into the denser air. The experiment was made on the mountain called the Puy−de−Dome, in Auvergne, and the column of mercury fell and rose progressively through a space of about three inches as the ascent and descent were made.

This experiment practically sealed the verdict on the new theory, but it also suggested something more. If the mercury descended to a certain mark on the scale on a mountain−top whose height was known, why was not this a means of measuring the heights of all other elevations? And so the beginning was made which, with certain modifications and corrections in details, is now the basis of barometrical measurements of heights.

In hydraulics, also, Torricelli seems to have taken one of the first steps. He did this by showing that the water which issues from a hole in the side or bottom of a vessel does so at the same velocity as that which a body would acquire by falling from the level of the surface of the water to that of the orifice. This discovery was of the greatest importance to a correct understanding of the science of the motions of fluids. He also discovered the valuable mechanical principle that if any number of bodies be connected so that by their motion there is neither ascent nor descent of their centre of gravity, these bodies are in equilibrium.

Besides making these discoveries, he greatly improved the microscope and the telescope, and invented a simple microscope made of a globule of glass. In 1644 he published a tract on the properties of the cycloid in which he suggested a solution of the problem of its quadrature. As soon as this pamphlet appeared its author was accused by Gilles Roberval (1602−1675) of having appropriated a solution already offered by him. This led to a long debate, during which Torricelli was seized with a fever, from the effects of which he died, in Florence, October 25, 1647. There is reason to believe, however, that while Roberval's discovery was made before Torricelli's, the latter reached his conclusions independently.

VI. TWO PSEUDO−SCIENCES—ALCHEMY AND ASTROLOGY

In recent chapters we have seen science come forward with tremendous strides. A new era is obviously at hand. But we shall misconceive the spirit of the times if we fail to understand that in the midst of all this progress there was still room for mediaeval superstition and for the pursuit of fallacious ideals. Two forms of pseudo−science were peculiarly prevalent —alchemy and astrology. Neither of these can with full propriety be called a science, yet both were pursued by many of the greatest scientific workers of the period. Moreover, the studies of the alchemist may with some propriety be said to have laid the foundation for the latter−day science of chemistry; while astrology was closely allied to astronomy, though its relations to that science are not as intimate as has sometimes been supposed.

Just when the study of alchemy began is undetermined. It was certainly of very ancient origin, perhaps Egyptian, but its most flourishing time was from about the eighth century A.D. to the eighteenth century. The stories of the Old Testament formed a basis for some of the strange beliefs regarding the properties of the magic "elixir," or "philosopher's stone." Alchemists believed that most of the antediluvians, perhaps all of them, possessed a knowledge of this stone. How, otherwise, could they have prolonged their lives to nine and a half centuries? And Moses was surely a first−rate alchemist, as is proved by the story of the Golden Calf.[1] After Aaron had made the calf of gold, Moses performed the much more difficult task of grinding it to powder and "strewing it upon the waters," thus showing that he had transmuted it into some lighter substance.

But antediluvians and Biblical characters were not the only persons who were thought to have discovered the coveted. "elixir." Hundreds of aged mediaeval chemists were credited with having made the discovery, and were thought to be living on through the centuries by its means. Alaies de Lisle, for example, who died in 1298, at the age of 110, was alleged to have been at the point of death at the age of fifty, but just at this time he made the fortunate discovery of the magic stone, and so continued to live in health and affluence for sixty years more. And De Lisle was but one case among hundreds.

An aged and wealthy alchemist could claim with seeming plausibility that he was prolonging his life by his magic; whereas a younger man might assert that, knowing the great secret, he was keeping himself young through the centuries. In either case such a statement, or rumor, about a learned and wealthy alchemist was likely to be believed, particularly among strangers; and as such a man would, of course, be the object of much attention, the claim was frequently made by persons seeking notoriety. One of the most celebrated of these impostors was a certain Count de Saint−Germain, who was connected with the court of Louis XV. His statements carried the more weight because, having apparently no means of maintenance, he continued to live in affluence year after year—for two thousand years, as he himself admitted—by means of the magic stone. If at any time his statements were doubted, he was in the habit of referring to his valet for confirmation, this valet being also under the influence of the elixir of life.

"Upon one occasion his master was telling a party of ladies and gentlemen, at dinner, some conversation he had had in Palestine, with King Richard I., of England, whom he described as a very particular friend of his. Signs of astonishment and incredulity were visible on the faces of the company, upon which Saint−Germain very coolly turned to his servant, who stood behind his chair, and asked him if he had not spoken the truth. 'I really cannot say,' replied the man, without moving a muscle; 'you forget, sir, I have been only five hundred years in your service.' 'Ah, true,' said his master, 'I remember now; it was a little before your time!' "[2]

In the time of Saint−Germain, only a little over a century ago, belief in alchemy had almost disappeared, and his extraordinary tales were probably regarded in the light of amusing stories. Still there was undoubtedly a lingering suspicion in the minds of many that this man possessed some peculiar secret. A few centuries earlier his tales would hardly have been questioned, for at that time the belief in the existence of this magic something was so strong that the search for it became almost a form of mania; and once a man was seized with it, lie gambled away health, position, and life itself in pursuing the coveted stake. An example of this is seen in Albertus Magnus, one of the most learned men of his time, who it is said resigned his position as bishop of Ratisbon in order that he might pursue his researches in alchemy.

If self−sacrifice was not sufficient to secure the prize, crime would naturally follow, for there could be no limit to the price of the stakes in this game. The notorious Marechal de Reys, failing to find the coveted stone by ordinary methods of laboratory research, was persuaded by an impostor that if he would propitiate the friendship of the devil the secret would be revealed. To this end De Reys began secretly capturing young children as they passed his castle and murdering them. When he was at last brought to justice it was proved that he had murdered something like a hundred children within a period of three years. So, at least, runs one version of the story of this perverted being.

Naturally monarchs, constantly in need of funds, were interested in these alchemists. Even sober England did not escape, and Raymond Lully, one of the most famous of the thirteenth and fourteenth century alchemists, is said to have been secretly invited by King Edward I. (or II.) to leave Milan and settle in England. According to some accounts, apartments were assigned to his use in the Tower of London, where he is alleged to have made some six million pounds sterling for the monarch, out of iron, mercury, lead, and pewter.

Pope John XXII., a friend and pupil of the alchemist Arnold de Villeneuve, is reported to have learned the secrets of alchemy from his master. Later he issued two bulls against "pretenders" in the art, which, far from showing his disbelief, were cited by alchemists as proving that he recognized pretenders as distinct from true masters of magic.

To moderns the attitude of mind of the alchemist is difficult to comprehend. It is, perhaps, possible to conceive of animals or plants possessing souls, but the early alchemist attributed the same thing—or something kin to it—to metals also. Furthermore, just as plants germinated from seeds, so metals were supposed to germinate also, and hence a constant growth of metals in the ground. To prove this the alchemist cited cases where previously exhausted gold−mines were found, after a lapse of time, to contain fresh quantities of gold. The "seed" of the remaining particles of gold had multiplied and increased. But this germinating process could only take place under favorable conditions, just as the seed of a plant must have its proper surroundings before germinating; and it was believed that the action of the philosopher's stone was to hasten this process, as man may hasten the growth of plants by artificial means. Gold was looked upon as the most perfect metal, and all other metals imperfect, because not yet "purified." By some alchemists they were regarded as lepers, who, when cured of their leprosy, would become gold. And since nature intended that all things should be perfect, it was the aim of the alchemist to assist her in this purifying process, and incidentally to gain wealth and prolong his life.

By other alchemists the process of transition from baser metals into gold was conceived to be like a process of ripening fruit. The ripened product was gold, while the green fruit, in various stages of maturity, was represented by the base metals. Silver, for example, was more nearly ripe than lead; but the difference was only one of "digestion," and it was thought that by further "digestion" lead might first become silver and eventually gold. In other words, Nature had not completed her work, and was wofully slow at it at best; but man, with his superior faculties, was to hasten the process in his laboratories—if he could but hit upon the right method of doing so.

It should not be inferred that the alchemist set about his task of assisting nature in a haphazard way, and without training in the various alchemic laboratory methods. On the contrary, he usually served a long apprenticeship in the rudiments of his calling. He was obliged to learn, in a general way, many of the same things that must be understood in either chemical or alchemical laboratories. The general knowledge that certain liquids vaporize at lower temperatures than others, and that the melting−points of metals differ greatly, for example, was just as necessary to alchemy as to chemistry. The knowledge of the gross structure, or nature, of materials was much the same to the alchemist as to the chemist, and, for that matter, many of the experiments in calcining, distilling, etc., were practically identical.

To the alchemist there were three principles—salt, sulphur, and mercury—and the sources of these principles were the four elements—earth, water, fire, and air. These four elements were accountable for every substance in nature. Some of the experiments to prove this were so illusive, and yet apparently so simple, that one is not surprised that it took centuries to disprove them. That water was composed of earth and air seemed easily proven by the simple process of boiling it in a tea−kettle, for the residue left was obviously an earthy substance, whereas the steam driven off was supposed to be air. The fact that pure water leaves no residue was not demonstrated until after alchemy had practically ceased to exist. It was possible also to demonstrate that water could be turned into fire by thrusting a red−hot poker under a bellglass containing a dish of water. Not only did the quantity of water diminish, but, if a lighted candle was thrust under the glass, the contents ignited and burned, proving, apparently, that water had been converted into fire. These, and scores of other similar experiments, seemed so easily explained, and to accord so well with the "four elements" theory, that they were seldom questioned until a later age of inductive science.

But there was one experiment to which the alchemist pinned his faith in showing that metals could be "killed" and "revived," when proper means were employed. It had been known for many centuries that if any metal, other than gold or silver, were calcined in an open crucible, it turned, after a time, into a peculiar kind of ash. This ash was thought by the alchemist to represent the death of the metal. But if to this same ash a few grains of wheat were added and heat again applied to the crucible, the metal was seen to "rise from its ashes" and resume its original form—a well−known phenomenon of reducing metals from oxides by the use of carbon, in the form of wheat, or, for that matter, any other carbonaceous substance. Wheat was, therefore, made the symbol of the resurrection of the life eternal. Oats, corn, or a piece of charcoal would have "revived" the metals from the ashes equally well, but the mediaeval alchemist seems not to have known this. However, in this experiment the metal seemed actually to be destroyed and revivified, and, as science had not as yet explained this striking phenomenon, it is little wonder that it deceived the alchemist.

Since the alchemists pursued their search of the magic stone in such a methodical way, it would seem that they must have some idea of the appearance of the substance they sought. Probably they did, each according to his own mental bias; but, if so, they seldom committed themselves to writing, confining their discourses largely to speculations as to the properties of this illusive substance. Furthermore, the desire for secrecy would prevent them from expressing so important a piece of information. But on the subject of the properties, if not on the appearance of the "essence," they were voluminous writers. It was supposed to be the only perfect substance in existence, and to be confined in various substances, in quantities proportionate to the state of perfection of the substance. Thus, gold being most nearly perfect would contain more, silver less, lead still less, and so on. The "essence" contained in the more nearly perfect metals was thought to be more potent, a very small quantity of it being capable of creating large quantities of gold and of prolonging life indefinitely.

It would appear from many of the writings of the alchemists that their conception of nature and the supernatural was so confused and entangled in an inexplicable philosophy that they themselves did not really understand the meaning of what they were attempting to convey. But it should not be forgotten that alchemy was kept as much as possible from the ignorant general public, and the alchemists themselves had knowledge of secret words and expressions which conveyed a definite meaning to one of their number, but which would appear a meaningless jumble to an outsider. Some of these writers declared openly that their writings were intended to convey an entirely erroneous impression, and were sent out only for that purpose.

However, while it may have been true that the vagaries of their writings were made purposely, the case is probably more correctly explained by saying that the very nature of the art made definite statements impossible. They were dealing with something that did not exist—could not exist. Their attempted descriptions became, therefore, the language of romance rather than the language of science.

But if the alchemists themselves were usually silent as to the appearance of the actual substance of the philosopher's stone, there were numberless other writers who were less reticent. By some it was supposed to be a stone, by others a liquid or elixir, but more commonly it was described as a black powder. It also possessed different degrees of efficiency according to its degrees of purity, certain forms only possessing the power of turning base metals into gold, while others gave eternal youth and life or different degrees of health. Thus an alchemist, who had made a partial discovery of this substance, could prolong life a certain number of years only, or, possessing only a small and inadequate amount of the magic powder, he was obliged to give up the ghost when the effect of this small quantity had passed away.

This belief in the supernatural power of the philosopher's stone to prolong life and heal diseases was probably a later phase of alchemy, possibly developed by attempts to connect the power of the mysterious essence with Biblical teachings. The early Roman alchemists, who claimed to be able to transmute metals, seem not to have made other claims for their magic stone.

By the fifteenth century the belief in the philosopher's stone had become so fixed that governments began to be alarmed lest some lucky possessor of the secret should flood the country with gold, thus rendering the existing coin of little value. Some little consolation was found in the thought that in case all the baser metals were converted into gold iron would then become the "precious metal," and would remain so until some new philosopher's stone was found to convert gold back into iron—a much more difficult feat, it was thought.

However, to be on the safe side, the English Parliament, in 1404, saw fit to pass an act declaring the making of gold and silver to be a felony. Nevertheless, in 1455, King Henry VI. granted permission to several "knights, citizens of London, chemists, and monks" to find the philosopher's stone, or elixir, that the crown might thus be enabled to pay off its debts. The monks and ecclesiastics were supposed to be most likely to discover the secret process, since "they were such good artists in transubstantiating bread and wine."

In Germany the emperors Maximilian I., Rudolf II., and Frederick II. gave considerable attention to the search, and the example they set was followed by thousands of their subjects. It is said that some noblemen developed the unpleasant custom of inviting to their courts men who were reputed to have found the stone, and then imprisoning the poor alchemists until they had made a certain quantity of gold, stimulating their activity with tortures of the most atrocious kinds. Thus this danger of being imprisoned and held for ransom until some fabulous amount of gold should be made became the constant menace of the alchemist. It was useless for an alchemist to plead poverty once it was noised about that he had learned the secret. For how could such a man be poor when, with a piece of metal and a few grains of magic powder, he was able to provide himself with gold? It was, therefore, a reckless alchemist indeed who dared boast that he had made the coveted discovery.

The fate of a certain indiscreet alchemist, supposed by many to have been Seton, a Scotchman, was not an uncommon one. Word having been brought to the elector of Saxony that this alchemist was in Dresden and boasting of his powers, the elector caused him to be arrested and imprisoned. Forty guards were stationed to see that he did not escape and that no one visited him save the elector himself. For some time the elector tried by argument and persuasion to penetrate his secret or to induce him to make a certain quantity of gold; but as Seton steadily refused, the rack was tried, and for several months he suffered torture, until finally, reduced to a mere skeleton, be was rescued by a rival candidate of the elector, a Pole named Michael Sendivogins, who drugged the guards. However, before Seton could be "persuaded" by his new captor, he died of his injuries.

But Sendivogins was also ambitious in alchemy, and, since Seton was beyond his reach, he took the next best step and married his widow. From her, as the story goes, he received an ounce of black powder—the veritable philosopher's stone. With this he manufactured great quantities of gold, even inviting Emperor Rudolf II. to see him work the miracle. That monarch was so impressed that he caused a tablet to be inserted in the wall of the room in which he had seen the gold made.

Sendivogins had learned discretion from the misfortune of Seton, so that he took the precaution of concealing most of the precious powder in a secret chamber of his carriage when he travelled, having only a small quantity carried by his steward in a gold box. In particularly dangerous places, he is said to have exchanged clothes with his coachman, making the servant take his place in the carriage while he mounted the box.

About the middle of the seventeenth century alchemy took such firm root in the religious field that it became the basis of the sect known as the Rosicrucians. The name was derived from the teaching of a German philosopher, Rosenkreutz, who, having been healed of a dangerous illness by an Arabian supposed to possess the philosopher's stone, returned home and gathered about him a chosen band of friends, to whom he imparted the secret. This sect came rapidly into prominence, and for a short time at least created a sensation in Europe, and at the time were credited with having "refined and spiritualized" alchemy. But by the end of the seventeenth century their number had dwindled to a mere handful, and henceforth they exerted little influence.

Another and earlier religious sect was the Aureacrucians, founded by Jacob Bohme, a shoemaker, born in Prussia in 1575. According to his teachings the philosopher's stone could be discovered by a diligent search of the Old and the New Testaments, and more particularly the Apocalypse, which contained all the secrets of alchemy. This sect found quite a number of followers during the life of Bohme, but gradually died out after his death; not, however, until many of its members had been tortured for heresy, and one at least, Kuhlmann, of Moscow, burned as a sorcerer.

The names of the different substances that at various times were thought to contain the large quantities of the "essence" during the many centuries of searching for it, form a list of practically all substances that were known, discovered, or invented during the period. Some believed that acids contained the substance; others sought it in minerals or in animal or vegetable products; while still others looked to find it among the distilled "spirits"—the alcoholic liquors and distilled products. On the introduction of alcohol by the Arabs that substance became of all−absorbing interest, and for a long time allured the alchemist into believing that through it they were soon to be rewarded. They rectified and refined it until "sometimes it was so strong that it broke the vessels containing it," but still it failed in its magic power. Later, brandy was substituted for it, and this in turn discarded for more recent discoveries.

There were always, of course, two classes of alchemists: serious investigators whose honesty could not be questioned, and clever impostors whose legerdemain was probably largely responsible for the extended belief in the existence of the philosopher's stone. Sometimes an alchemist practised both, using the profits of his sleight−of−hand to procure the means of carrying on his serious alchemical researches. The impostures of some of these jugglers deceived even the most intelligent and learned men of the time, and so kept the flame of hope constantly burning. The age of cold investigation had not arrived, and it is easy to understand how an unscrupulous mediaeval Hermann or Kellar might completely deceive even the most intelligent and thoughtful scholars. In scoffing at the credulity of such an age, it should not be forgotten that the "Keely motor" was a late nineteenth−century illusion.

But long before the belief in the philosopher's stone had died out, the methods of the legerdemain alchemist had been investigated and reported upon officially by bodies of men appointed to make such investigations, although it took several generations completely to overthrow a superstition that had been handed down through several thousand years. In April of 1772 Monsieur Geoffroy made a report to the Royal Academy of Sciences, at Paris, on the alchemic cheats principally of the sixteenth and seventeenth centuries. In this report he explains many of the seemingly marvellous feats of the unscrupulous alchemists. A very common form of deception was the use of a double−bottomed crucible. A copper or brass crucible was covered on the inside with a layer of wax, cleverly painted so as to resemble the ordinary metal. Between this layer of wax and the bottom of the crucible, however, was a layer of gold dust or silver. When the alchemist wished to demonstrate his power, he had but to place some mercury or whatever substance he chose in the crucible, heat it, throw in a grain or two of some mysterious powder, pronounce a few equally mysterious phrases to impress his audience, and, behold, a lump of precious metal would be found in the bottom of his pot. This was the favorite method of mediocre performers, but was, of course, easily detected.

An equally successful but more difficult way was to insert surreptitiously a lump of metal into the mixture, using an ordinary crucible. This required great dexterity, but was facilitated by the use of many mysterious ceremonies on the part of the operator while performing, just as the modern vaudeville performer diverts the attention of the audience to his right hand while his left is engaged in the trick. Such ceremonies were not questioned, for it was the common belief that the whole process "lay in the spirit as much as in the substance," many, as we have seen, regarding the whole process as a divine manifestation.

Sometimes a hollow rod was used for stirring the mixture in the crucible, this rod containing gold dust, and having the end plugged either with wax or soft metal that was easily melted. Again, pieces of lead were used which had been plugged with lumps of gold carefully covered over; and a very simple and impressive demonstration was making use of a nugget of gold that had been coated over with quicksilver and tarnished so as to resemble lead or some base metal. When this was thrown into acid the coating was removed by chemical action, leaving the shining metal in the bottom of the vessel. In order to perform some of these tricks, it is obvious that the alchemist must have been well supplied with gold, as some of them, when performing before a royal audience, gave the products to their visitors. But it was always a paying investment, for once his reputation was established the gold−maker found an endless variety of ways of turning his alleged knowledge to account, frequently amassing great wealth.

Some of the cleverest of the charlatans often invited royal or other distinguished guests to bring with them iron nails to be turned into gold ones. They were transmuted in the alchemist's crucible before the eyes of the visitors, the juggler adroitly extracting the iron nail and inserting a gold one without detection. It mattered little if the converted gold nail differed in size and shape from the original, for this change in shape could be laid to the process of transmutation; and even the very critical were hardly likely to find fault with the exchange thus made. Furthermore, it was believed that gold possessed the property of changing its bulk under certain conditions, some of the more conservative alchemists maintaining that gold was only increased in bulk, not necessarily created, by certain forms of the magic stone. Thus a very proficient operator was thought to be able to increase a grain of gold into a pound of pure metal, while one less expert could only double, or possibly treble, its original weight.

The actual number of useful discoveries resulting from the efforts of the alchemists is considerable, some of them of incalculable value. Roger Bacon, who lived in the thirteenth century, while devoting much of his time to alchemy, made such valuable discoveries as the theory, at least, of the telescope, and probably gunpowder. Of this latter we cannot be sure that the discovery was his own and that he had not learned of it through the source of old manuscripts. But it is not impossible nor improbable that he may have hit upon the mixture that makes the explosives while searching for the philosopher's stone in his laboratory. "Von Helmont, in the same pursuit, discoverd the properties of gas," says Mackay; "Geber made discoveries in chemistry, which were equally important; and Paracelsus, amid his perpetual visions of the transmutation of metals, found that mercury was a remedy for one of the most odious and excruciating of all the diseases that afflict humanity."' As we shall see a little farther on, alchemy finally evolved into modern chemistry, but not until it had passed through several important transitional stages.

ASTROLOGY

In a general way modern astronomy may be considered as the outgrowth of astrology, just as modern chemistry is the result of alchemy. It is quite possible, however, that astronomy is the older of the two; but astrology must have developed very shortly after. The primitive astronomer, having acquired enough knowledge from his observations of the heavenly bodies to make correct predictions, such as the time of the coming of the new moon, would be led, naturally, to believe that certain predictions other than purely astronomical ones could be made by studying the heavens. Even if the astronomer himself did not believe this, some of his superstitious admirers would; for to the unscientific mind predictions of earthly events would surely seem no more miraculous than correct predictions as to the future movements of the sun, moon, and stars. When astronomy had reached a stage of development so that such things as eclipses could be predicted with anything like accuracy, the occult knowledge of the astronomer would be unquestioned. Turning this apparently occult knowledge to account in a mercenary way would then be the inevitable result, although it cannot be doubted that many of the astrologers, in all ages, were sincere in their beliefs.

Later, as the business of astrology became a profitable one, sincere astronomers would find it expedient to practise astrology as a means of gaining a livelihood. Such a philosopher as Kepler freely admitted that he practised astrology "to keep from starving," although he confessed no faith in such predictions. "Ye otherwise philosophers," he said, "ye censure this daughter of astronomy beyond her deserts; know ye not that she must support her mother by her charms."

Once astrology had become an established practice, any considerable knowledge of astronomy was unnecessary, for as it was at best but a system of good guessing as to future events, clever impostors could thrive equally well without troubling to study astronomy. The celebrated astrologers, however, were usually astronomers as well, and undoubtedly based many of their predictions on the position and movements of the heavenly bodies. Thus, the casting of a horoscope that is, the methods by which the astrologers ascertained the relative position of the heavenly bodies at the time of a birth—was a simple but fairly exact procedure. Its basis was the zodiac, or the path traced by the sun in his yearly course through certain constellations. At the moment of the birth of a child, the first care of the astrologer was to note the particular part of the zodiac that appeared on the horizon. The zodiac was then divided into "houses"—that is, into twelve spaces—on a chart. In these houses were inserted the places of the planets, sun, and moon, with reference to the zodiac. When this chart was completed it made a fairly correct diagram of the heavens and the position of the heavenly bodies as they would appear to a person standing at the place of birth at a certain time.

Up to this point the process was a simple one of astronomy. But the next step—the really important one—that of interpreting this chart, was the one which called forth the skill and imagination of the astrologer. In this interpretation, not in his mere observations, lay the secret of his success. Nor did his task cease with simply foretelling future events that were to happen in the life of the newly born infant. He must not only point out the dangers, but show the means whereby they could be averted, and his prophylactic measures, like his predictions, were alleged to be based on his reading of the stars.

But casting a horoscope at the time of births was, of course, only a small part of the astrologer's duty. His offices were sought by persons of all ages for predictions as to their futures, the movements of an enemy, where to find stolen goods, and a host of everyday occurrences. In such cases it is more than probable that the astrologers did very little consulting of the stars in making their predictions. They became expert physiognomists and excellent judges of human nature, and were thus able to foretell futures with the same shrewdness and by the same methods as the modern "mediums," palmists, and fortune−tellers. To strengthen belief in their powers, it became a common thing for some supposedly lost document of the astrologer to be mysteriously discovered after an important event, this document purporting to foretell this very event. It was also a common practice with astrologers to retain, or have access to, their original charts, cleverly altering them from time to time to fit conditions.

The dangers attendant upon astrology were of such a nature that the lot of the astrologer was likely to prove anything but an enviable one. As in the case of the alchemist, the greater the reputation of an astrologer the greater dangers he was likely to fall into. If he became so famous that he was employed by kings or noblemen, his too true or too false prophecies were likely to bring him into disrepute—even to endanger his life.

Throughout the dark age the astrologers flourished, but the sixteenth and seventeenth centuries were the golden age of these impostors. A skilful astrologer was as much an essential to the government as the highest official, and it would have been a bold monarch, indeed, who would undertake any expedition of importance unless sanctioned by the governing stars as interpreted by these officials.

It should not be understood, however, that belief in astrology died with the advent of the Copernican doctrine. It did become separated from astronomy very shortly after, to be sure, and undoubtedly among the scientists it lost much of its prestige. But it cannot be considered as entirely passed away, even to−day, and even if we leave out of consideration street−corner "astrologers" and fortune−tellers, whose signs may be seen in every large city, there still remains quite a large class of relatively intelligent people who believe in what they call "the science of astrology." Needless to say, such people are not found among the scientific thinkers; but it is significant that scarcely a year passes that some book or pamphlet is not published by some ardent believer in astrology, attempting to prove by the illogical dogmas characteristic of unscientific thinkers that astrology is a science. The arguments contained in these pamphlets are very much the same as those of the astrologers three hundred years ago, except that they lack the quaint form of wording which is one of the features that lends interest to the older documents. These pamphlets need not be taken seriously, but they are interesting as exemplifying how difficult it is, even in an age of science, to entirely stamp out firmly established superstitions. Here are some of the arguments advanced in defence of astrology, taken from a little brochure entitled "Astrology Vindicated," published in 1898: It will be found that a person born when the Sun is in twenty degrees Scorpio has the left ear as his exceptional feature and the nose (Sagittarius) bent towards the left ear. A person born when the Sun is in any of the latter degrees of Taurus, say the twenty−fifth degree, will have a small, sharp, weak chin, curved up towards Gemini, the two vertical lines on the upper lip."[4] The time was when science went out of its way to prove that such statements were untrue; but that time is past, and such writers are usually classed among those energetic but misguided persons who are unable to distinguish between logic and sophistry.

In England, from the time of Elizabeth to the reign of William and Mary, judicial astrology was at its height. After the great London fire, in 1666, a committee of the House of Commons publicly summoned the famous astrologer, Lilly, to come before Parliament and report to them on his alleged prediction of the calamity that had befallen the city. Lilly, for some reason best known to himself, denied having made such a prediction, being, as he explained, "more interested in determining affairs of much more importance to the future welfare of the country." Some of the explanations of his interpretations will suffice to show their absurdities, which, however, were by no means regarded as absurdities at that time, for Lilly was one of the greatest astrologers of his day. He said that in 1588 a prophecy had been printed in Greek characters which foretold exactly the troubles of England between the years 1641. and 1660. "And after him shall come a dreadful dead man," ran the prophecy, "and with him a royal G of the best blood in the world, and he shall have the crown and shall set England on the right way and put out all heresies. His interpretation of this was that, "Monkery being extinguished above eighty or ninety years, and the Lord General's name being Monk, is the dead man. The royal G or C (it is gamma in the Greek, intending C in the Latin, being the third letter in the alphabet) is Charles II., who, for his extraction, may be said to be of the best blood of the world."[5]

This may be taken as a fair sample of Lilly's interpretations of astrological prophesies, but many of his own writings, while somewhat more definite and direct, are still left sufficiently vague to allow his skilful interpretations to set right an apparent mistake. One of his famous documents was "The Starry Messenger," a little pamphlet purporting to explain the phenomenon of a "strange apparition of three suns" that were seen in London on November 19, 1644—−the anniversary of the birth of Charles I., then the reigning monarch. This phenomenon caused a great stir among the English astrologers, coming, as it did, at a time of great political disturbance.

Prophecies were numerous, and Lilly's brochure is only one of many that appeared at that time, most of which, however, have been lost. Lilly, in his preface, says: "If there be any of so prevaricate a judgment as to think that the apparition of these three Suns doth intimate no Novelle thing to happen in our own Climate, where they were manifestly visible, I shall lament their indisposition, and conceive their brains to be shallow, and voyde of understanding humanity, or notice of common History."

Having thus forgiven his few doubting readers, who were by no means in the majority in his day, he takes up in review the records of the various appearances of three suns as they have occurred during the Christian era, showing how such phenomena have governed certain human events in a very definite manner. Some of these are worth recording.

"Anno 66. A comet was seen, and also three Suns: In which yeer, Florus President of the Jews was by them slain. Paul writes to Timothy. The Christians are warned by a divine Oracle, and depart out of Jerusalem. Boadice a British Queen, killeth seventy thousand Romans. The Nazareni, a scurvie Sect, begun, that boasted much of Revelations and Visions. About a year after Nero was proclaimed enemy to the State of Rome."

Again, "Anno 1157, in September, there were seen three Suns together, in as clear weather as could be: And a few days after, in the same month, three Moons, and, in the Moon that stood in the middle, a white Crosse. Sueno, King of Denmark, at a great Feast, killeth Canutus: Sueno is himself slain, in pursuit of Waldemar. The Order of Eremites, according to the rule of Saint Augustine, begun this year; and in the next, the Pope submits to the Emperour: (was not this miraculous?) Lombardy was also adjudged to the Emperour."

Continuing this list of peculiar phenomena he comes down to within a few years of his own time.

"Anno 1622, three Suns appeared at Heidelberg. The woful Calamities that have ever since fallen upon the Palatinate, we are all sensible of, and of the loss of it, for any thing I see, for ever, from the right Heir. Osman the great Turk is strangled that year; and Spinola besiegeth Bergen up Zoom, etc."

Fortified by the enumeration of these past events, he then proceeds to make his deductions. "Only this I must tell thee," he writes, "that the interpretation I write is, I conceive, grounded upon probable foundations; and who lives to see a few years over his head, will easily perceive I have unfolded as much as was fit to discover, and that my judgment was not a mile and a half from truth."

There is a great significance in this "as much as was fit to discover"—a mysterious something that Lilly thinks it expedient not to divulge. But, nevertheless, one would imagine that he was about to make some definite prediction about Charles I., since these three suns appeared upon his birthday and surely must portend something concerning him. But after rambling on through many pages of dissertations upon planets and prophecies, he finally makes his own indefinite prediction.

"O all you Emperors, Kings, Princes, Rulers and Magistrates of Europe, this unaccustomed Apparition is like the Handwriting in Daniel to some of you; it premonisheth you, above all other people, to make your peace with God in time. You shall every one of you smart, and every one of you taste (none excepted) the heavie hand of God, who will strengthen your subjects with invincible courage to suppress your misgovernments and Oppressions in Church or Common−wealth; . . . Those words are general: a word for my own country of England. . . . Look to yourselves; here's some monstrous death towards you. But to whom? wilt thou say. Herein we consider the Signe, Lord thereof, and the House; The Sun signifies in that Royal Signe, great ones; the House signifies captivity, poison, Treachery: From which is derived thus much, That some very great man, what King, Prince, Duke, or the like, I really affirm I perfectly know not, shall, I say, come to some such untimely end."[6]

Here is shown a typical example of astrological prophecy, which seems to tell something or nothing, according to the point of view of the reader. According to a believer in astrology, after the execution of Charles I., five years later, this could be made to seem a direct and exact prophecy. For example, he says: "You Kings, Princes, etc., ... it premonisheth you ... to make your peace with God.... Look to yourselves; here's some monstrous death towards you. ... That some very great man, what King, Prince, . shall, I say, come to such untimely end."

But by the doubter the complete prophecy could be shown to be absolutely indefinite, and applicable as much to the king of France or Spain as to Charles I., or to any king in the future, since no definite time is stated. Furthermore, Lilly distinctly states, "What King, Prince, Duke, or the like, I really affirm I perfectly know not"—which last, at least, was a most truthful statement. The same ingenuity that made "Gen. Monk" the "dreadful dead man," could easily make such a prediction apply to the execution of Charles I. Such a definite statement that, on such and such a day a certain number of years in the future, the monarch of England would be beheaded—such an exact statement can scarcely be found in any of the works on astrology. It should be borne in mind, also, that Lilly was of the Cromwell party and opposed to the king.

After the death of Charles I., Lilly admitted that the monarch had given him a thousand pounds to cast his horoscope. "I advised him," says Lilly, "to proceed eastwards; he went west, and all the world knows the result." It is an unfortunate thing for the cause of astrology that Lilly failed to mention this until after the downfall of the monarch. In fact, the sudden death, or decline in power, of any monarch, even to−day, brings out the perennial post−mortem predictions of astrologers.

We see how Lilly, an opponent of the king, made his so−called prophecy of the disaster of the king and his army. At the same time another celebrated astrologer and rival of Lilly, George Wharton, also made some predictions about the outcome of the eventful march from Oxford. Wharton, unlike Lilly, was a follower of the king's party, but that, of course, should have had no influence in his "scientific" reading of the stars. Wharton's predictions are much less verbose than Lilly's, much more explicit, and, incidentally, much more incorrect in this particular instance. "The Moon Lady of the 12," he wrote, "and moving betwixt the 8 degree, 34 min., and 21 degree, 26 min. of Aquarius, gives us to understand that His Majesty shall receive much contentment by certain Messages brought him from foreign parts; and that he shall receive some sudden and unexpected supply of . . . by the means of some that assimilate the condition of his Enemies: And withal this comfort; that His Majesty shall be exceeding successful in Besieging Towns, Castles, or Forts, and in persuing the enemy.

"Mars his Sextile to the Sun, Lord of the Ascendant (which happeneth the 18 day of May) will encourage our Soldiers to advance with much alacrity and cheerfulness of spirit; to show themselves gallant in the most dangerous attempt.... And now to sum up all: It is most apparent to every impartial and ingenuous judgment; That although His Majesty cannot expect to be secured from every trivial disaster that may befall his army, either by the too much Presumption, Ignorance, or Negligence of some particular Persons (which is frequently incident and unavoidable in the best of Armies), yet the several positions of the Heavens duly considered and compared among themselves, as well in the prefixed Scheme as at the Quarterly Ingresses, do generally render His Majesty and his whole Army unexpectedly victorious and successful in all his designs; Believe it (London), thy Miseries approach, they are like to be many, great, and grievous, and not to be diverted, unless thou seasonably crave Pardon of God for being Nurse to this present Rebellion, and speedily submit to thy Prince's Mercy; Which shall be the daily Prayer of Geo. Wharton."[7]

In the light of after events, it is probable that Wharton's stock as an astrologer was not greatly enhanced by this document, at least among members of the Royal family. Lilly's book, on the other hand, became a favorite with the Parliamentary army.

After the downfall and death of Napoleon there were unearthed many alleged authentic astrological documents foretelling his ruin. And on the death of George IV., in 1830, there appeared a document (unknown, as usual, until that time) purporting to foretell the death of the monarch to the day, and this without the astrologer knowing that his horoscope was being cast for a monarch. A full account of this prophecy is told, with full belief, by Roback, a nineteenth−century astrologer. He says:

"In the year 1828, a stranger of noble mien, advanced in life, but possessing the most bland manners, arrived at the abode of a celebrated astrologer in London," asking that the learned man foretell his future. "The astrologer complied with the request of the mysterious visitor, drew forth his tables, consulted his ephemeris, and cast the horoscope or celestial map for the hour and the moment of the inquiry, according to the established rules of his art.

"The elements of his calculation were adverse, and a feeling of gloom cast a shade of serious thought, if not dejection, over his countenance.

" 'You are of high rank,' said the astrologer, as he calculated and looked on the stranger, 'and of illustrious title.' The stranger made a graceful inclination of the head in token of acknowledgment of the complimentary remarks, and the astrologer proceeded with his mission.

"The celestial signs were ominous of calamity to the stranger, who, probably observing a sudden change in the countenance of the astrologer, eagerly inquired what evil or good fortune had been assigned him by the celestial orbs.

'To the first part of your inquiry,' said the astrologer, 'I can readily reply. You have been a favorite of fortune;

her smiles on you have been abundant, her frowns but few; you have had, perhaps now possess, wealth and power; the impossibility of their accomplishment is the only limit to the fulfilment of your desires.' "

" 'You have spoken truly of the past,' said the stranger. 'I have full faith in your revelations of the future: what say you of my pilgrimage in this life—is it short or long?'

" 'I regret,' replied the astrologer, in answer to this inquiry, 'to be the herald of ill, though TRUE, fortune; your sojourn on earth will be short.'

" 'How short?' eagerly inquired the excited and anxious stranger.

" 'Give me a momentary truce,' said the astrologer; 'I will consult the horoscope, and may possibly find some mitigating circumstances.'

"Having cast his eyes over the celestial map, and paused for some moments, he surveyed the countenance of the stranger with great sympathy, and said, 'I am sorry that I can find no planetary influences that oppose your destiny—your death will take place in two years.'

"The event justified the astrologic prediction: George IV. died on May 18, 1830, exactly two years from the day on which he had visited the astrologer."[8]

This makes a very pretty story, but it hardly seems like occult insight that an astrologer should have been able to predict an early death of a man nearly seventy years old, or to have guessed that his well−groomed visitor "had, perhaps now possesses, wealth and power." Here again, however, the point of view of each individual plays the governing part in determining the importance of such a document. To the scientist it proves nothing; to the believer in astrology, everything. The significant thing is that it appeared shortly AFTER the death of the monarch.

On the Continent astrologers were even more in favor than in England. Charlemagne, and some of his immediate successors, to be sure, attempted to exterminate them, but such rulers as Louis XI. and Catherine de' Medici patronized and encouraged them, and it was many years after the time of Copernicus before their influence was entirely stamped out even in official life. There can be no question that what gave the color of truth to many of the predictions was the fact that so many of the prophecies of sudden deaths and great conflagrations were known to have come true—in many instances were made to come true by the astrologer himself. And so it happened that when the prediction of a great conflagration at a certain time culminated in such a conflagration, many times a second but less−important burning took place, in which the ambitious astrologer, or his followers, took a central part about a stake, being convicted of incendiarism, which they had committed in order that their prophecies might be fulfilled.

But, on the other hand, these predictions were sometimes turned to account by interested friends to warn certain persons of approaching dangers.

For example, a certain astrologer foretold the death of Prince Alexander de' Medici. He not only foretold the death, but described so minutely the circumstances that would attend it, and gave such a correct description of the assassin who should murder the prince, that he was at once suspected of having a hand in the assassination. It developed later, however, that such was probably not the case; but that some friend of Prince Alexander, knowing of the plot to take his life, had induced the astrologer to foretell the event in order that the prince might have timely warning and so elude the conspirators.

The cause of the decline of astrology was the growing prevalence of the new spirit of experimental science. Doubtless the most direct blow was dealt by the Copernican theory. So soon as this was established, the recognition of the earth's subordinate place in the universe must have made it difficult for astronomers to be longer deceived by such coincidences as had sufficed to convince the observers of a more credulous generation. Tycho Brahe was, perhaps, the last astronomer of prominence who was a conscientious practiser of the art of the astrologer.

VII. FROM PARACELSUS TO HARVEY

PARACELSUS

In the year 1526 there appeared a new lecturer on the platform at the University at Basel—a small, beardless, effeminate−looking person—who had already inflamed all Christendom with his peculiar philosophy, his revolutionary methods of treating diseases, and his unparalleled success in curing them. A man who was to be remembered in after−time by some as the father of modern chemistry and the founder of modern medicine; by others as madman, charlatan, impostor; and by still others as a combination of all these. This soft−cheeked, effeminate, woman−hating man, whose very sex has been questioned, was Theophrastus von Hohenheim, better known as Paracelsus (1493−1541).

To appreciate his work, something must be known of the life of the man. He was born near Maria−Einsiedeln, in Switzerland, the son of a poor physician of the place. He began the study of medicine under the instruction of his father, and later on came under the instruction of several learned churchmen. At the age of sixteen he entered the University of Basel, but, soon becoming disgusted with the philosophical teachings of the time, he quitted the scholarly world of dogmas and theories and went to live among the miners in the Tyrol, in order that he might study nature and men at first hand. Ordinary methods of study were thrown aside, and he devoted his time to personal observation—the only true means of gaining useful knowledge, as he preached and practised ever after. Here he became familiar with the art of mining, learned the physical properties of minerals, ores, and metals, and acquired some knowledge of mineral waters. More important still, he came in contact with such diseases, wounds, and injuries as miners are subject to, and he tried his hand at the practical treatment of these conditions, untrammelled by the traditions of a profession in which his training had been so scant.

Having acquired some empirical skill in treating diseases, Paracelsus set out wandering from place to place all over Europe, gathering practical information as he went, and learning more and more of the medicinal virtues of plants and minerals. His wanderings covered a period of about ten years, at the end of which time he returned to Basel, where he was soon invited to give a course of lectures in the university.

These lectures were revolutionary in two respects—they were given in German instead of time−honored Latin, and they were based upon personal experience rather than upon the works of such writers as Galen and Avicenna. Indeed, the iconoclastic teacher spoke with open disparagement of these revered masters, and openly upbraided his fellow−practitioners for following their tenets. Naturally such teaching raised a storm of opposition among the older physicians, but for a time the unparalleled success of Paracelsus in curing diseases more than offset his unpopularity. Gradually, however, his bitter tongue and his coarse personality rendered him so unpopular, even among his patients, that, finally, his liberty and life being jeopardized, he was obliged to flee from Basel, and became a wanderer. He lived for brief periods in Colmar, Nuremberg, Appenzell, Zurich, Pfeffers, Augsburg, and several other cities, until finally at Salzburg his eventful life came to a close in 1541. His enemies said that he had died in a tavern from the effects of a protracted debauch; his supporters maintained that he had been murdered at the instigation of rival physicians and apothecaries.

But the effects of his teachings had taken firm root, and continued to spread after his death. He had shown the fallibility of many of the teachings of the hitherto standard methods of treating diseases, and had demonstrated the advantages of independent reasoning based on observation. In his Magicum he gives his reasons for breaking with tradition. "I did," he says, "embrace at the beginning these doctrines, as my adversaries (followers of Galen) have done, but since I saw that from their procedures nothing resulted but death, murder, stranglings, anchylosed limbs, paralysis, and so forth, that they held most diseases incurable. . . . therefore have I quitted this wretched art, and sought for truth in any other direction. I asked myself if there were no such thing as a teacher in medicine, where could I learn this art best? Nowhere better than the open book of nature, written with God's own finger." We shall see, however, that this "book of nature" taught Paracelsus some very strange lessons. Modesty was not one of these. "Now at this time," he declares, "I, Theophrastus Paracelsus, Bombast, Monarch of the Arcana, was endowed by God with special gifts for this end, that every searcher after this supreme philosopher's work may be forced to imitate and to follow me, be he Italian, Pole, Gaul, German, or whatsoever or whosoever he be. Come hither after me, all ye philosophers, astronomers, and spagirists. . . . I will show and open to you ... this corporeal regeneration."[1]

Paracelsus based his medical teachings on four "pillars" —philosophy, astronomy, alchemy, and virtue of the physician—a strange−enough equipment surely, and yet, properly interpreted, not quite so anomalous as it seems at first blush. Philosophy was the "gate of medicine," whereby the physician entered rightly upon the true course of learning; astronomy, the study of the stars, was all−important because "they (the stars) caused disease by their exhalations, as, for instance, the sun by excessive heat"; alchemy, as he interpreted it, meant the improvement of natural substances for man's benefit; while virtue in the physician was necessary since "only the virtuous are permitted to penetrate into the innermost nature of man and the universe."

All his writings aim to promote progress in medicine, and to hold before the physician a grand ideal of his profession. In this his views are wide and far−reaching, based on the relationship which man bears to nature as a whole; but in his sweeping condemnations he not only rejected Galenic therapeutics and Galenic anatomy, but condemned dissections of any kind. He laid the cause of all diseases at the door of the three mystic elements—salt, sulphur, and mercury. In health he supposed these to be mingled in the body so as to be indistinguishable; a slight separation of them produced disease; and death he supposed to be the result of their complete separation. The spiritual agencies of diseases, he said, had nothing to do with either angels or devils, but were the spirits of human beings.

He believed that all food contained poisons, and that the function of digestion was to separate the poisonous from the nutritious. In the stomach was an archaeus, or alchemist, whose duty was to make this separation. In digestive disorders the archaeus failed to do this, and the poisons thus gaining access to the system were "coagulated" and deposited in the joints and various other parts of the body. Thus the deposits in the kidneys and tartar on the teeth were formed; and the stony deposits of gout were particularly familiar examples of this. All this is visionary enough, yet it shows at least a groping after rational explanations of vital phenomena.

Like most others of his time, Paracelsus believed firmly in the doctrine of "signatures"—a belief that every organ and part of the body had a corresponding form in nature, whose function was to heal diseases of the organ it resembled. The vagaries of this peculiar doctrine are too numerous and complicated for lengthy discussion, and varied greatly from generation to generation. In general, however, the theory may be summed up in the words of Paracelsus: "As a woman is known by her shape, so are the medicines." Hence the physicians were constantly searching for some object of corresponding shape to an organ of the body. The most natural application of this doctrine would be the use of the organs of the lower animals for the treatment of the corresponding diseased organs in man. Thus diseases of the heart were to be treated with the hearts of animals, liver disorders with livers, and so on. But this apparently simple form of treatment had endless modifications and restrictions, for not all animals were useful. For example, it was useless to give the stomach of an ox in gastric diseases when the indication in such cases was really for the stomach of a rat. Nor were the organs of animals the only "signatures" in nature. Plants also played a very important role, and the herb−doctors devoted endless labor to searching for such plants. Thus the blood−root, with its red juice, was supposed to be useful in blood diseases, in stopping hemorrhage, or in subduing the redness of an inflammation.

Paracelsus's system of signatures, however, was so complicated by his theories of astronomy and alchemy that it is practically beyond comprehension. It is possible that he himself may have understood it, but it is improbable that any one else did—as shown by the endless discussions that have taken place about it. But with all the vagaries of his theories he was still rational in his applications, and he attacked to good purpose the complicated "shot−gun" prescriptions of his contemporaries, advocating more simple methods of treatment.

The ever−fascinating subject of electricity, or, more specifically, "magnetism," found great favor with him, and with properly adjusted magnets he claimed to be able to cure many diseases. In epilepsy and lockjaw, for example, one had but to fasten magnets to the four extremities of the body, and then, "when the proper medicines were given," the cure would be effected. The easy loop−hole for excusing failure on the ground of improper medicines is obvious, but Paracelsus declares that this one prescription is of more value than "all the humoralists have ever written or taught."

Since Paracelsus condemned the study of anatomy as useless, he quite naturally regarded surgery in the same light. In this he would have done far better to have studied some of his predecessors, such as Galen, Paul of Aegina, and Avicenna. But instead of "cutting men to pieces," he taught that surgeons would gain more by devoting their time to searching for the universal panacea which would cure all diseases, surgical as well as medical. In this we detect a taint of the popular belief in the philosopher's stone and the magic elixir of life, his belief in which have been stoutly denied by some of his followers. He did admit, however, that one operation alone was perhaps permissible—lithotomy, or the "cutting for stone."

His influence upon medicine rests undoubtedly upon his revolutionary attitude, rather than on any great or new discoveries made by him. It is claimed by many that he brought prominently into use opium and mercury, and if this were indisputably proven his services to medicine could hardly be overestimated. Unfortunately, however, there are good grounds for doubting that he was particularly influential in reintroducing these medicines. His chief influence may perhaps be summed up in a single phrase—he overthrew old traditions.

To Paracelsus's endeavors, however, if not to the actual products of his work, is due the credit of setting in motion the chain of thought that developed finally into scientific chemistry. Nor can the ultimate aim of the modern chemist seek a higher object than that of this sixteenth−century alchemist, who taught that "true alchemy has but one aim and object, to extract the quintessence of things, and to prepare arcana, tinctures, and elixirs which may restore to man the health and soundness he has lost."

THE GREAT ANATOMISTS

About the beginning of the sixteenth century, while Paracelsus was scoffing at the study of anatomy as useless, and using his influence against it, there had already come upon the scene the first of the great anatomists whose work was to make the century conspicuous in that branch of medicine.

The young anatomist Charles etienne (1503−1564) made one of the first noteworthy discoveries, pointing out for the first time that the spinal cord contains a canal, continuous throughout its length. He also made other minor discoveries of some importance, but his researches were completely overshadowed and obscured by the work of a young Fleming who came upon the scene a few years later, and who shone with such brilliancy in the medical world that he obscured completely the work of his contemporary until many years later. This young physician, who was destined to lead such an eventful career and meet such an untimely end as a martyr to science, was Andrew Vesalius (1514−1564), who is called the "greatest of anatomists." At the time he came into the field medicine was struggling against the dominating Galenic teachings and the theories of Paracelsus, but perhaps most of all against the superstitions of the time. In France human dissections were attended with such dangers that the young Vesalius transferred his field of labors to Italy, where such investigations were covertly permitted, if not openly countenanced.

From the very start the young Fleming looked askance at the accepted teachings of the day, and began a series of independent investigations based upon his own observations. The results of these investigations he gave in a treatise on the subject which is regarded as the first comprehensive and systematic work on human anatomy. This remarkable work was published in the author's twenty−eighth or twenty−ninth year. Soon after this Vesalius was invited as imperial physician to the court of Emperor Charles V. He continued to act in the same capacity at the court of Philip II., after the abdication of his patron. But in spite of this royal favor there was at work a factor more powerful than the influence of the monarch himself—an instrument that did so much to retard scientific progress, and by which so many lives were brought to a premature close.

Vesalius had received permission from the kinsmen of a certain grandee to perform an autopsy. While making his observations the heart of the outraged body was seen to palpitate—so at least it was reported. This was brought immediately to the attention of the Inquisition, and it was only by the intervention of the king himself that the anatomist escaped the usual fate of those accused by that tribunal. As it was, he was obliged to perform a pilgrimage to the Holy Land. While returning from this he was shipwrecked, and perished from hunger and exposure on the island of Zante.

At the very time when the anatomical writings of Vesalius were startling the medical world, there was living and working contemporaneously another great anatomist, Eustachius (died 1574), whose records of his anatomical investigations were ready for publication only nine years after the publication of the work of Vesalius. Owing to the unfortunate circumstances of the anatomist, however, they were never published during his lifetime—not, in fact, until 1714. When at last they were given to the world as Anatomical Engravings, they showed conclusively that Eustachius was equal, if not superior to Vesalius in his knowledge of anatomy. It has been said of this remarkable collection of engravings that if they had been published when they were made in the sixteenth century, anatomy would have been advanced by at least two centuries. But be this as it may, they certainly show that their author was a most careful dissector and observer.

Eustachius described accurately for the first time certain structures of the middle ear, and rediscovered the tube leading from the ear to the throat that bears his name. He also made careful studies of the teeth and the phenomena of first and second dentition. He was not baffled by the minuteness of structures and where he was unable to study them with the naked eye he used glasses for the purpose, and resorted to macerations and injections for the study of certain complicated structures. But while the fruit of his pen and pencil were lost for more than a century after his death, the effects of his teachings were not; and his two pupils, Fallopius and Columbus, are almost as well known to−day as their illustrious teacher. Columbus (1490−1559) did much in correcting the mistakes made in the anatomy of the bones as described by Vesalius. He also added much to the science by giving correct accounts of the shape and cavities of the heart, and made many other discoveries of minor importance. Fallopius (1523−1562) added considerably to the general knowledge of anatomy, made several discoveries in the anatomy of the ear, and also several organs in the abdominal cavity.

At this time a most vitally important controversy was in progress as to whether or not the veins of the bodies were supplied with valves, many anatomists being unable to find them. etienne had first described these structures, and Vesalius had confirmed his observations. It would seem as if there could be no difficulty in settling the question as to the fact of such valves being present in the vessels, for the demonstration is so simple that it is now made daily by medical students in all physiological laboratories and dissecting−rooms. But many of the great anatomists of the sixteenth century were unable to make this demonstration, even when it had been brought to their attention by such an authority as Vesalius. Fallopius, writing to Vesalius on the subject in 1562, declared that he was unable to find such valves. Others, however, such as Eustachius and Fabricius (1537−1619), were more successful, and found and described these structures. But the purpose served by these valves was entirely misinterpreted. That they act in preventing the backward flow of the blood in the veins on its way to the heart, just as the valves of the heart itself prevent regurgitation, has been known since the time of Harvey; but the best interpretation that could be given at that time, even by such a man as Fabricius, was that they acted in retarding the flow of the blood as it comes from the heart, and thus prevent its too rapid distribution throughout the body. The fact that the blood might have been going towards the heart, instead of coming from it, seems never to have been considered seriously until demonstrated so conclusively by Harvey.

Of this important and remarkable controversy over the valves in veins, Withington has this to say: "This is truly a marvellous story. A great Galenic anatomist is first to give a full and correct description of the valves and their function, but fails to see that any modification of the old view as to the motion of the blood is required. Two able dissectors carefully test their action by experiment, and come to a result. the exact reverse of the truth. Urged by them, the two foremost anatomists of the age make a special search for valves and fail to find them. Finally, passing over lesser peculiarities, an aged and honorable professor, who has lived through all this, calmly asserts that no anatomist, ancient or modern, has ever mentioned valves in veins till he discovered them in 1574!"[2]

Among the anatomists who probably discovered these valves was Michael Servetus (1511−1553); but if this is somewhat in doubt, it is certain that he discovered and described the pulmonary circulation, and had a very clear idea of the process of respiration as carried on in the lungs. The description was contained in a famous document sent to Calvin in 1545—a document which the reformer carefully kept for seven years in order that he might make use of some of the heretical statements it contained to accomplish his desire of bringing its writer to the stake. The awful fate of Servetus, the interesting character of the man, and the fact that he came so near to anticipating the discoveries of Harvey make him one of the most interesting figures in medical history.

In this document which was sent to Calvin, Servetus rejected the doctrine of natural, vital, and animal spirits, as contained in the veins, arteries, and nerves respectively, and made the all−important statement that the fluids contained in veins and arteries are the same. He showed also that the blood is "purged from fume" and purified by respiration in the lungs, and declared that there is a new vessel in the lungs, "formed out of vein and artery." Even at the present day there is little to add to or change in this description of Servetus's.

By keeping this document, pregnant with advanced scientific views, from the world, and in the end only using it as a means of destroying its author, the great reformer showed the same jealousy in retarding scientific progress as had his arch−enemies of the Inquisition, at whose dictates Vesalius became a martyr to science, and in whose dungeons etienne perished.

THE COMING OF HARVEY

The time was ripe for the culminating discovery of the circulation of the blood; but as yet no one had determined the all−important fact that there are two currents of blood in the body, one going to the heart, one coming from it. The valves in the veins would seem to show conclusively that the venous current did not come from the heart, and surgeons must have observed thousands of times the every−day phenomenon of congested veins at the distal extremity of a limb around which a ligature or constriction of any kind had been placed, and the simultaneous depletion of the vessels at the proximal points above the ligature. But it should be remembered that inductive science was in its infancy. This was the sixteenth, not the nineteenth century, and few men had learned to put implicit confidence in their observations and convictions when opposed to existing doctrines. The time was at hand, however, when such a man was to make his appearance, and, as in the case of so many revolutionary doctrines in science, this man was an Englishman. It remained for William Harvey (1578−1657) to solve the great mystery which had puzzled the medical world since the beginning of history; not only to solve it, but to prove his case so conclusively and so simply that for all time his little booklet must he handed down as one of the great masterpieces of lucid and almost faultless demonstration.

Harvey, the son of a prosperous Kentish yeoman, was born at Folkestone. His education was begun at the grammar−school of Canterbury, and later he became a pensioner of Caius College, Cambridge. Soon after taking his degree of B.A., at the age of nineteen, he decided upon the profession of medicine, and went to Padua as a pupil of Fabricius and Casserius. Returning to England at the age of twenty−four, he soon after (1609) obtained the reversion of the post of physician to St. Bartholomew's Hospital, his application being supported by James I. himself. Even at this time he was a popular physician, counting among his patients such men as Francis Bacon. In 1618 he was appointed physician extraordinary to the king, and, a little later, physician in ordinary. He was in attendance upon Charles I. at the battle of Edgehill, in 1642, where, with the young Prince of Wales and the Duke of York, after seeking shelter under a hedge, he drew a book out of his pocket and, forgetful of the battle, became absorbed in study, until finally the cannon−balls from the enemy's artillery made him seek a more sheltered position.

On the fall of Charles I. he retired from practice, and lived in retirement with his brother. He was then well along in years, but still pursued his scientific researches with the same vigor as before, directing his attention chiefly to the study of embryology. On June 3, 1657, he was attacked by paralysis and died, in his eightieth year. He had lived to see his theory of the circulation accepted, several years before, by all the eminent anatomists of the civilized world.

A keenness in the observation of facts, characteristic of the mind of the man, had led Harvey to doubt the truth of existing doctrines as to the phenomena of the circulation. Galen had taught that "the arteries are filled, like bellows, because they are expanded," but Harvey thought that the action of spurting blood from a severed vessel disproved this. For the spurting was remittant, "now with greater, now with less impetus," and its greater force always corresponded to the expansion (diastole), not the contraction (systole) of the vessel. Furthermore, it was evident that contraction of the heart and the arteries was not simultaneous, as was commonly taught, because in that case there would be no marked propulsion of the blood in any direction; and there was no gainsaying the fact that the blood was forcibly propelled in a definite direction, and that direction away from the heart.

Harvey's investigations led him to doubt also the accepted theory that there was a porosity in the septum of tissue that divides the two ventricles of the heart. It seemed unreasonable to suppose that a thick fluid like the blood could find its way through pores so small that they could not be demonstrated by any means devised by man. In evidence that there could be no such openings he pointed out that, since the two ventricles contract at the same time, this process would impede rather than facilitate such an intra−ventricular passage of blood. But what seemed the most conclusive proof of all was the fact that in the foetus there existed a demonstrable opening between the two ventricles, and yet this is closed in the fully developed heart. Why should Nature, if she intended that blood should pass between the two cavities, choose to close this opening and substitute microscopic openings in place of it? It would surely seem more reasonable to have the small perforations in the thin, easily permeable membrane of the foetus, and the opening in the adult heart, rather than the reverse. From all this Harvey drew his correct conclusions, declaring earnestly, "By Hercules, there ARE no such porosities, and they cannot be demonstrated."

Having convinced himself that no intra−ventricular opening existed, he proceeded to study the action of the heart itself, untrammelled by too much faith in established theories, and, as yet, with no theory of his own. He soon discovered that the commonly accepted theory of the heart striking against the chest−wall during the period of relaxation was entirely wrong, and that its action was exactly the reverse of this, the heart striking the chest−wall during contraction. Having thus disproved the accepted theory concerning the heart's action, he took up the subject of the action of arteries, and soon was able to demonstrate by vivisection that the contraction of the arteries was not simultaneous with contractions of the heart. His experiments demonstrated that these vessels were simply elastic tubes whose pulsations were "nothing else than the impulse of the blood within them." The reason that the arterial pulsation was not simultaneous with the heart−beat he found to be because of the time required to carry the impulse along the tube,

By a series of further careful examinations and experiments, which are too extended to be given here, he was soon able further to demonstrate the action and course of the blood during the contractions of the heart. His explanations were practically the same as those given to−day—first the contraction of the auricle, sending blood into the ventricle; then ventricular contraction, making the pulse, and sending the blood into the arteries. He had thus demonstrated what had not been generally accepted before, that the heart was an organ for the propulsion of blood. To make such a statement to−day seems not unlike the sober announcement that the earth is round or that the sun does not revolve about it. Before Harvey's time, however, it was considered as an organ that was "in some mysterious way the source of vitality and warmth, as an animated crucible for the concoction of blood and the generation of vital spirits."[3]

In watching the rapid and ceaseless contractions of the heart, Harvey was impressed with the fact that, even if a very small amount of blood was sent out at each pulsation, an enormous quantity must pass through the organ in a day, or even in an hour. Estimating the size of the cavities of the heart, and noting that at least a drachm must be sent out with each pulsation, it was evident that the two thousand beats given by a very slow human heart in an hour must send out some forty pounds of blood—more than twice the amount in the entire body. The question was, what became of it all? For it should be remembered that the return of the blood by the veins was unknown, and nothing like a "circulation" more than vaguely conceived even by Harvey himself. Once it could be shown that the veins were constantly returning blood to the heart, the discovery that the blood in some way passes from the arteries to the veins was only a short step. Harvey, by resorting to vivisections of lower animals and reptiles, soon demonstrated beyond question the fact that the veins do carry the return blood. "But this, in particular, can be shown clearer than daylight," says Harvey. "The vena cava enters the heart at an inferior portion, while the artery passes out above. Now if the vena cava be taken up with forceps or the thumb and finger, and the course of the blood intercepted for some distance below the heart, you will at once see it almost emptied between the fingers and the heart, the blood being exhausted by the heart's pulsation, the heart at the same time becoming much paler even in its dilatation, smaller in size, owing to the deficiency of blood, and at length languid in pulsation, as if about to die. On the other hand, when you release the vein the heart immediately regains its color and dimensions. After that, if you leave the vein free and tie and compress the arteries at some distance from the heart, you will see, on the contrary, their included portion grow excessively turgid, the heart becoming so beyond measure, assuming a dark−red color, even to lividity, and at length so overloaded with blood as to seem in danger of suffocation; but when the obstruction is removed it returns to its normal condition, in size, color, and movement."[4]

This conclusive demonstration that the veins return the blood to the heart must have been most impressive to Harvey, who had been taught to believe that the blood current in the veins pursued an opposite course, and must have tended to shake his faith in all existing doctrines of the day.

His next step was the natural one of demonstrating that the blood passes from the arteries to the veins. He demonstrated conclusively that this did occur, but for once his rejection of the ancient writers and one modern one was a mistake. For Galen had taught, and had attempted to demonstrate, that there are sets of minute vessels connecting the arteries and the veins; and Servetus had shown that there must be such vessels, at least in the lungs.

However, the little flaw in the otherwise complete demonstration of Harvey detracts nothing from the main issue at stake. It was for others who followed to show just how these small vessels acted in effecting the transfer of the blood from artery to vein, and the grand general statement that such a transfer does take place was, after all, the all−important one, and the exact method of how it takes place a detail. Harvey's experiments to demonstrate that the blood passes from the arteries to the veins are so simply and concisely stated that they may best be given in his own words.

"I have here to cite certain experiments," he wrote, "from which it seems obvious that the blood enters a limb by the arteries, and returns from it by the veins; that the arteries are the vessels carrying the blood from the heart, and the veins the returning channels of the blood to the heart; that in the limbs and extreme parts of the body the blood passes either by anastomosis from the arteries into the veins, or immediately by the pores of the flesh, or in both ways, as has already been said in speaking of the passage of the blood through the lungs; whence it appears manifest that in the circuit the blood moves from thence hither, and hence thither; from the centre to the extremities, to wit, and from the extreme parts back again to the centre. Finally, upon grounds of circulation, with the same elements as before, it will be obvious that the quantity can neither be accounted for by the ingesta, nor yet be held necessary to nutrition.

"Now let any one make an experiment on the arm of a man, either using such a fillet as is employed in blood−letting or grasping the limb tightly with his hand, the best subject for it being one who is lean, and who has large veins, and the best time after exercise, when the body is warm, the pulse is full, and the blood carried in large quantities to the extremities, for all then is more conspicuous; under such circumstances let a ligature be thrown about the extremity and drawn as tightly as can be borne: it will first be perceived that beyond the ligature neither in the wrist nor anywhere else do the arteries pulsate, that at the same time immediately above the ligature the artery begins to rise higher at each diastole, to throb more violently, and to swell in its vicinity with a kind of tide, as if it strove to break through and overcome the obstacle to its current; the artery here, in short, appears as if it were permanently full. The hand under such circumstances retains its natural color and appearances; in the course of time it begins to fall somewhat in temperature, indeed, but nothing is DRAWN into it.

"After the bandage has been kept on some short time in this way, let it be slackened a little, brought to the state or term of middling tightness which is used in bleeding, and it will be seen that the whole hand and arm will instantly become deeply suffused and distended, injected, gorged with blood, DRAWN, as it is said, by this middling ligature, without pain, or heat, or any horror of a vacuum, or any other cause yet indicated.

"As we have noted, in connection with the tight ligature, that the artery above the bandage was distended and pulsated, not below it, so, in the case of the moderately tight bandage, on the contrary, do we find that the veins below, never above, the fillet swell and become dilated, while the arteries shrink; and such is the degree of distention of the veins here that it is only very strong pressure that will force the blood beyond the fillet and cause any of the veins in the upper part of the arm to rise.

"From these facts it is easy for any careful observer to learn that the blood enters an extremity by the arteries; for when they are effectively compressed nothing is DRAWN to the member; the hand preserves its color; nothing flows into it, neither is it distended; but when the pressure is diminished, as it is with the bleeding fillet, it is manifest that the blood is instantly thrown in with force, for then the hand begins to swell; which is as much as to say that when the arteries pulsate the blood is flowing through them, as it is when the moderately tight ligature is applied; but when they do not pulsate, or when a tight ligature is used, they cease from transmitting anything; they are only distended above the part where the ligature is applied. The veins again being compressed, nothing can flow through them; the certain indication of which is that below the ligature they are much more tumid than above it, and than they usually appear when there is no bandage upon the arm.

"It therefore plainly appears that the ligature prevents the return of the blood through the veins to the parts above it, and maintains those beneath it in a state of permanent distention. But the arteries, in spite of the pressure, and under the force and impulse of the heart, send on the blood from the internal parts of the body to the parts beyond the bandage."[5]

This use of ligatures is very significant, because, as shown, a very tight ligature stops circulation in both arteries and veins, while a loose one, while checking the circulation in the veins, which lie nearer the surface and are not so directly influenced by the force of the heart, does not stop the passage of blood in the arteries, which are usually deeply imbedded in the tissues, and not so easily influenced by pressure from without.

The last step of Harvey's demonstration was to prove that the blood does flow along the veins to the heart, aided by the valves that had been the cause of so much discussion and dispute between the great sixteenth−century anatomists. Harvey not only demonstrated the presence of these valves, but showed conclusively, by simple experiments, what their function was, thus completing his demonstration of the phenomena of the circulation.

The final ocular demonstration of the passage of the blood from the arteries to the veins was not to be made until four years after Harvey's death. This process, which can be observed easily in the web of a frog's foot by the aid of a low−power lens, was first demonstrated by Marcello Malpighi (1628−1694) in 1661. By the aid of a lens he first saw the small "capillary" vessels connecting the veins and arteries in a piece of dried lung. Taking his cue from this, he examined the lung of a turtle, and was able to see in it the passage of the corpuscles through these minute vessels, making their way along these previously unknown channels from the arteries into the veins on their journey back to the heart. Thus the work of Harvey, all but complete, was made absolutely entire by the great Italian. And all this in a single generation.

LEEUWENHOEK DISCOVERS BACTERIA

The seventeenth century was not to close, however, without another discovery in science, which, when applied to the causation of disease almost two centuries later, revolutionized therapeutics more completely than any one discovery. This was the discovery of microbes, by Antonius von Leeuwenhoek (1632−1723), in 1683. Von Leeuwenhoek discovered that "in the white matter between his teeth" there were millions of microscopic "animals"—more, in fact, than "there were human beings in the united Netherlands," and all "moving in the most delightful manner." There can be no question that he saw them, for we can recognize in his descriptions of these various forms of little "animals" the four principal forms of microbes—the long and short rods of bacilli and bacteria, the spheres of micrococci, and the corkscrew spirillum.

The presence of these microbes in his mouth greatly annoyed Antonius, and he tried various methods of getting rid of them, such as using vinegar and hot coffee. In doing this he little suspected that he was anticipating modern antiseptic surgery by a century and three−quarters, and to be attempting what antiseptic surgery is now able to accomplish. For the fundamental principle of antisepsis is the use of medicines for ridding wounds of similar microscopic organisms. Von Leenwenhoek was only temporarily successful in his attempts, however, and took occasion to communicate his discovery to the Royal Society of England, hoping that they would be "interested in this novelty." Probably they were, but not sufficiently so for any member to pursue any protracted investigations or reach any satisfactory conclusions, and the whole matter was practically forgotten until the middle of the nineteenth century.

VIII. MEDICINE IN THE SIXTEENTH AND SEVENTEENTH CENTURIES

Of the half−dozen surgeons who were prominent in the sixteenth century, Ambroise Pare (1517−1590), called the father of French surgery, is perhaps the most widely known. He rose from the position of a common barber to that of surgeon to three French monarchs, Henry II., Francis II., and Charles IX. Some of his mottoes are still first principles of the medical man. Among others are: "He who becomes a surgeon for the sake of money, and not for the sake of knowledge, will accomplish nothing"; and "A tried remedy is better than a newly invented." On his statue is his modest estimate of his work in caring for the wounded, "Je le pansay, Dieu le guarit"—I dressed him, God cured him.

It was in this dressing of wounds on the battlefield that he accidentally discovered how useless and harmful was the terribly painful treatment of applying boiling oil to gunshot wounds as advocated by John of Vigo. It happened that after a certain battle, where there was an unusually large number of casualties, Pare found, to his horror, that no more boiling oil was available for the surgeons, and that he should be obliged to dress the wounded by other simpler methods. To his amazement the results proved entirely satisfactory, and from that day he discarded the hot−oil treatment.

As Pare did not understand Latin he wrote his treatises in French, thus inaugurating a custom in France that was begun by Paracelsus in Germany half a century before. He reintroduced the use of the ligature in controlling hemorrhage, introduced the "figure of eight" suture in the operation for hare−lip, improved many of the medico−legal doctrines, and advanced the practice of surgery generally. He is credited with having successfully performed the operation for strangulated hernia, but he probably borrowed it from Peter Franco (1505−1570), who published an account of this operation in 1556. As this operation is considered by some the most important operation in surgery, its discoverer is entitled to more than passing notice, although he was despised and ignored by the surgeons of his time.

Franco was an illiterate travelling lithotomist—a class of itinerant physicians who were very generally frowned down by the regular practitioners of medicine. But Franco possessed such skill as an operator, and appears to have been so earnest in the pursuit of what he considered a legitimate calling, that he finally overcame the popular prejudice and became one of the salaried surgeons of the republic of Bern. He was the first surgeon to perform the suprapubic lithotomy operation—the removal of stone through the abdomen instead of through the perineum. His works, while written in an illiterate style, give the clearest descriptions of any of the early modern writers.

As the fame of Franco rests upon his operation for prolonging human life, so the fame of his Italian contemporary, Gaspar Tagliacozzi (1545−1599), rests upon his operation for increasing human comfort and happiness by restoring amputated noses. At the time in which he lived amputation of the nose was very common, partly from disease, but also because a certain pope had fixed the amputation of that member as the penalty for larceny. Tagliacozzi probably borrowed his operation from the East; but he was the first Western surgeon to perform it and describe it. So great was the fame of his operations that patients flocked to him from all over Europe, and each "went away with as many noses as he liked." Naturally, the man who directed his efforts to restoring structures that bad been removed by order of the Church was regarded in the light of a heretic by many theologians; and though he succeeded in cheating the stake or dungeon, and died a natural death, his body was finally cast out of the church in which it had been buried.

In the sixteenth century Germany produced a surgeon, Fabricius Hildanes (1560−1639), whose work compares favorably with that of Pare, and whose name would undoubtedly have been much better known had not the circumstances of the time in which he lived tended to obscure his merits. The blind followers of Paracelsus could see nothing outside the pale of their master's teachings, and the disastrous Thirty Years' War tended to obscure and retard all scientific advances in Germany. Unlike many of his fellow−surgeons, Hildanes was well versed in Latin and Greek; and, contrary to the teachings of Paracelsus, he laid particular stress upon the necessity of the surgeon having a thorough knowledge of anatomy. He had a helpmate in his wife, who was also something of a surgeon, and she is credited with having first made use of the magnet in removing particles of metal from the eye. Hildanes tells of a certain man who had been injured by a small piece of steel in the cornea, which resisted all his efforts to remove it. After observing Hildanes' fruitless efforts for a time, it suddenly occurred to his wife to attempt to make the extraction with a piece of loadstone. While the physician held open the two lids, his wife attempted to withdraw the steel with the magnet held close to the cornea, and after several efforts she was successful—which Hildanes enumerates as one of the advantages of being a married man.

Hildanes was particularly happy in his inventions of surgical instruments, many of which were designed for locating and removing the various missiles recently introduced in warfare.

The seventeenth century, which was such a flourishing one for anatomy and physiology, was not as productive of great surgeons or advances in surgery as the sixteenth had been or the eighteenth was to be. There was a gradual improvement all along the line, however, and much of the work begun by such surgeons as Pare and Hildanes was perfected or improved. Perhaps the most progressive surgeon of the century was an Englishman, Richard Wiseman (1625−1686), who, like Harvey, enjoyed royal favor, being in the service of all the Stuart kings. He was the first surgeon to advocate primary amputation, in gunshot wounds, of the limbs, and also to introduce the treatment of aneurisms by compression; but he is generally rated as a conservative operator, who favored medication rather than radical operations, where possible.

In Italy, Marcus Aurelius Severinus (1580−1656) and Peter Marchettis (1589−1675) were the leading surgeons of their nation. Like many of his predecessors in Europe, Severinus ran amuck with the Holy Inquisition and fled from Naples. But the waning of the powerful arm of the Church is shown by the fact that he was brought back by the unanimous voice of the grateful citizens, and lived in safety despite the frowns of the theologians.

The sixteenth century cannot be said to have added much of importance in the field of practical medicine, and, as in the preceding and succeeding centuries, was at best only struggling along in the wake of anatomy, physiology, and surgery. In the seventeenth century, however, at least one discovery in therapeutics was made that has been an inestimable boon to humanity ever since. This was the introduction of cinchona bark (from which quinine is obtained) in 1640. But this century was productive of many medical SYSTEMS, and could boast of many great names among the medical profession, and, on the whole, made considerably more progress than the preceding century.

Of the founders of medical systems, one of the most widely known is Jan Baptista van Helmont (1578−1644), an eccentric genius who constructed a system of medicine of his own and for a time exerted considerable influence. But in the end his system was destined to pass out of existence, not very long after the death of its author. Van Helmont was not only a physician, but was master of all the other branches of learning of the time, taking up the study of medicine and chemistry as an after−thought, but devoting himself to them with the greatest enthusiasm once he had begun his investigations. His attitude towards existing doctrines was as revolutionary as that of Paracelsus, and he rejected the teachings of Galen and all the ancient writers, although retaining some of the views of Paracelsus. He modified the archaeus of Paracelsus, and added many complications to it. He believed the whole body to be controlled by an archaeus influus, the soul by the archaei insiti, and these in turn controlled by the central archeus. His system is too elaborate and complicated for full explanation, but its chief service to medicine was in introducing new chemical methods in the preparation of drugs. In this way he was indirectly connected with the establishment of the Iatrochemical school. It was he who first used the word "gas"—a word coined by him, along with many others that soon fell into disuse.

The principles of the Iatrochemical school were the use of chemical medicines, and a theory of pathology different from the prevailing "humoral" pathology. The founder of this school was Sylvius (Franz de le Boe, 1614−1672), professor of medicine at Leyden. He attempted to establish a permanent system of medicine based on the newly discovered theory of the circulation and the new chemistry, but his name is remembered by medical men because of the fissure in the brain (fissure of Sylvius) that bears it. He laid great stress on the cause of fevers and other diseases as originating in the disturbances of the process of fermentation in the stomach. The doctrines of Sylvius spread widely over the continent, but were not generally accepted in England until modified by Thomas Willis (1622−1675), whose name, like that of Sylvius, is perpetuated by a structure in the brain named after him, the circle of Willis. Willis's descriptions of certain nervous diseases, and an account of diabetes, are the first recorded, and added materially to scientific medicine. These schools of medicine lasted until the end of the seventeenth century, when they were finally overthrown by Sydenham.

The Iatrophysical school (also called iatromathematical, iatromechanical, or physiatric) was founded on theories of physiology, probably by Borelli, of Naples (1608−1679), although Sanctorius; Sanctorius, a professor at Padua, was a precursor, if not directly interested in establishing it. Sanctorius discovered the fact that an "insensible perspiration" is being given off by the body continually, and was amazed to find that loss of weight in this way far exceeded the loss of weight by all other excretions of the body combined. He made this discovery by means of a peculiar weighing−machine to which a chair was attached, and in which he spent most of his time. Very naturally he overestimated the importance of this discovery, but it was, nevertheless, of great value in pointing out the hygienic importance of the care of the skin. He also introduced a thermometer which he advocated as valuable in cases of fever, but the instrument was probably not his own invention, but borrowed from his friend Galileo.

Harvey's discovery of the circulation of the blood laid the foundation of the Iatrophysical school by showing that this vital process was comparable to a hydraulic system. In his On the Motive of Animals, Borelli first attempted to account for the phenomena of life and diseases on these principles. The iatromechanics held that the great cause of disease is due to different states of elasticity of the solids of the body interfering with the movements of the fluids, which are themselves subject to changes in density, one or both of these conditions continuing to cause stagnation or congestion. The school thus founded by Borelli was the outcome of the unbounded enthusiasm, with its accompanying exaggeration of certain phenomena with the corresponding belittling of others that naturally follows such a revolutionary discovery as that of Harvey. Having such a founder as the brilliant Italian Borelli, it was given a sufficient impetus by his writings to carry it some distance before it finally collapsed. Some of the exaggerated mathematical calculations of Borelli himself are worth noting. Each heart−beat, as he calculated it, overcomes a resistance equal to one hundred and eighty thousand pounds;—the modern physiologist estimates its force at from five to nine ounces!

THOMAS SYDENHAM

But while the Continent was struggling with these illusive "systems," and dabbling in mystic theories that were to scarcely outlive the men who conceived

them, there appeared in England—the "land of common−sense," as a German scientist has called it—"a cool, clear, and unprejudiced spirit," who in the golden age of systems declined "to be like the man who builds the chambers of the upper story of his house before he had laid securely the foundation walls."[1] This man was Thomas Sydenham (1624−1689), who, while the great Harvey was serving the king as surgeon, was fighting as a captain in the parliamentary army. Sydenham took for his guide the teachings of Hippocrates, modified to suit the advances that had been made in scientific knowledge since the days of the great Greek, and established, as a standard, observation and experience. He cared little for theory unless confirmed by practice, but took the Hippocratic view that nature cured diseases, assisted by the physician. He gave due credit, however, to the importance of the part played by the assistant. As he saw it, medicine could be advanced in three ways: (1) "By accurate descriptions or natural histories of diseases; (2) by establishing a fixed principle or method of treatment, founded upon experience; (3) by searching for specific remedies, which he believes must exist in considerable numbers, though he admits that the only one yet discovered is Peruvian bark."[2] As it happened, another equally specific remedy, mercury, when used in certain diseases, was already known to him, but he evidently did not recognize it as such.

The influence on future medicine of Sydenham's teachings was most pronounced, due mostly to his teaching of careful observation. To most physicians, however, he is now remembered chiefly for his introduction of the use of laudanum, still considered one of the most valuable remedies of modern pharmacopoeias. The German gives the honor of introducing this preparation to Paracelsus, but the English−speaking world will always believe that the credit should be given to Sydenham.

IX. PHILOSOPHER−SCIENTISTS AND NEW INSTITUTIONS OF LEARNING

We saw that in the old Greek days there was no sharp line of demarcation between the field of the philosopher and that of the scientist. In the Hellenistic epoch, however, knowledge became more specialized, and our recent chapters have shown us scientific investigators whose efforts were far enough removed from the intangibilities of the philosopher. It must not be overlooked, however, that even in the present epoch there were men whose intellectual efforts were primarily directed towards the subtleties of philosophy, yet who had also a penchant for strictly scientific imaginings, if not indeed for practical scientific experiments. At least three of these men were of sufficient importance in the history of the development of science to demand more than passing notice. These three are the Englishman Francis Bacon (1561−1626), the Frenchman Rene Descartes (1596−1650); and the German Gottfried Leibnitz (1646−1716). Bacon, as the earliest path−breaker, showed the way, theoretically at least, in which the sciences should be studied; Descartes, pursuing the methods pointed out by Bacon, carried the same line of abstract reason into practice as well; while Leibnitz, coming some years later, and having the advantage of the wisdom of his two great predecessors, was naturally influenced by both in his views of abstract scientific principles.

Bacon's career as a statesman and his faults and misfortunes as a man do not concern us here. Our interest in him begins with his entrance into Trinity College, Cambridge, where he took up the study of all the sciences taught there at that time. During the three years he became more and more convinced that science was not being studied in a profitable manner, until at last, at the end of his college course, he made ready to renounce the old Aristotelian methods of study and advance his theory of inductive study. For although he was a great admirer of Aristotle's work, he became convinced that his methods of approaching study were entirely wrong.

"The opinion of Aristotle," he says, in his De Argumentum Scientiarum, "seemeth to me a negligent opinion, that of those things which exist by nature nothing can be changed by custom; using for example, that if a stone be thrown ten thousand times up it will not learn to ascend; and that by often seeing or hearing we do not learn to see or hear better. For though this principle be true in things wherein nature is peremptory (the reason whereof we cannot now stand to discuss), yet it is otherwise in things wherein nature admitteth a latitude. For he might see that a straight glove will come more easily on with use; and that a wand will by use bend otherwise than it grew; and that by use of the voice we speak louder and stronger; and that by use of enduring heat or cold we endure it the better, and the like; which latter sort have a nearer resemblance unto that subject of manners he handleth than those instances which he allegeth."[1]

These were his opinions, formed while a young man in college, repeated at intervals through his maturer years, and reiterated and emphasized in his old age. Masses of facts were to be obtained by observing nature at first hand, and from such accumulations of facts deductions were to be made. In short, reasoning was to be from the specific to the general, and not vice versa.

It was by his teachings alone that Bacon thus contributed to the foundation of modern science; and, while he was constantly thinking and writing on scientific subjects, he contributed little in the way of actual discoveries. "I only sound the clarion," he said, "but I enter not the battle."

The case of Descartes, however, is different. He both sounded the clarion and entered into the fight. He himself freely acknowledges his debt to Bacon for his teachings of inductive methods of study, but modern criticism places his work on the same plane as that of the great Englishman. "If you lay hold of any characteristic product of modern ways of thinking," says Huxley, "either in the region of philosophy or in that of science, you find the spirit of that thought, if not its form, has been present in the mind of the great Frenchman."[2]

Descartes, the son of a noble family of France, was educated by Jesuit teachers. Like Bacon, he very early conceived the idea that the methods of teaching and studying science were wrong, but be pondered the matter well into middle life before putting into writing his ideas of philosophy and science. Then, in his Discourse Touching the Method of Using One's Reason Rightly and of Seeking Scientific Truth, he pointed out the way of seeking after truth. His central idea in this was to emphasize the importance of DOUBT, and avoidance of accepting as truth anything that does not admit of absolute and unqualified proof. In reaching these conclusions he had before him the striking examples of scientific deductions by Galileo, and more recently the discovery of the circulation of the blood by Harvey. This last came as a revelation to scientists, reducing this seemingly occult process, as it did, to the field of mechanical phenomena. The same mechanical laws that governed the heavenly bodies, as shown by Galileo, governed the action of the human heart, and, for aught any one knew, every part of the body, and even the mind itself.

Having once conceived this idea, Descartes began a series of dissections and experiments upon the lower animals, to find, if possible, further proof of this general law. To him the human body was simply a machine, a complicated mechanism, whose functions were controlled just as any other piece of machinery. He compared the human body to complicated machinery run by water−falls and complicated pipes. "The nerves of the machine which I am describing," he says, "may very well be compared to the pipes of these waterworks; its muscles and its tendons to the other various engines and springs which seem to move them; its animal spirits to the water which impels them, of which the heart is the fountain; while the cavities of the brain are the central office. Moreover, respiration and other such actions as are natural and usual in the body, and which depend on the course of the spirits, are like the movements of a clock, or a mill, which may be kept up by the ordinary flow of water."[3]

In such passages as these Descartes anticipates the ideas of physiology of the present time. He believed that the functions are performed by the various organs of the bodies of animals and men as a mechanism, to which in man was added the soul. This soul he located in the pineal gland, a degenerate and presumably functionless little organ in the brain. For years Descartes's idea of the function of this gland was held by many physiologists, and it was only the introduction of modern high−power microscopy that reduced this also to a mere mechanism, and showed that it is apparently the remains of a Cyclopean eye once common to man's remote ancestors.

Descartes was the originator of a theory of the movements of the universe by a mechanical process—the Cartesian theory of vortices—which for several decades after its promulgation reigned supreme in science. It is the ingenuity of this theory, not the truth of its assertions, that still excites admiration, for it has long since been supplanted. It was certainly the best hitherto advanced—the best "that the observations of the age admitted," according to D'Alembert.

According to this theory the infinite universe is full of matter, there being no such thing as a vacuum. Matter, as Descartes believed, is uniform in character throughout the entire universe, and since motion cannot take place in any part of a space completely filled, without simultaneous movement in all other parts, there are constant more or less circular movements, vortices, or whirlpools of particles, varying, of course, in size and velocity. As a result of this circular movement the particles of matter tend to become globular from contact with one another. Two species of matter are thus formed, one larger and globular, which continue their circular motion with a constant tendency to fly from the centre of the axis of rotation, the other composed of the clippings resulting from the grinding process. These smaller "filings" from the main bodies, becoming smaller and smaller, gradually lose their velocity and accumulate in the centre of the vortex. This collection of the smaller matter in the centre of the vortex constitutes the sun or star, while the spherical particles propelled in straight lines from the centre towards the circumference of the vortex produce the phenomenon of light radiating from the central star. Thus this matter becomes the atmosphere revolving around the accumulation at the centre. But the small particles being constantly worn away from the revolving spherical particles in the vortex, become entangled in their passage, and when they reach the edge of the inner strata of solar dust they settle upon it and form what we call sun−spots. These are constantly dissolved and reformed, until sometimes they form a crust round the central nucleus.

As the expansive force of the star diminishes in the course of time, it is encroached upon by neighboring vortices. If the part of the encroaching star be of a less velocity than the star which it has swept up, it will presently lose its hold, and the smaller star pass out of range, becoming a comet. But if the velocity of the vortex into which the incrusted star settles be equivalent to that of the surrounded vortex, it will hold it as a captive, still revolving and "wrapt in its own firmament." Thus the several planets of our solar system have been captured and held by the sun−vortex, as have the moon and other satellites.

But although these new theories at first created great enthusiasm among all classes of philosophers and scientists, they soon came under the ban of the Church. While no actual harm came to Descartes himself, his writings were condemned by the Catholic and Protestant churches alike. The spirit of philosophical inquiry he had engendered, however, lived on, and is largely responsible for modern philosophy.

In many ways the life and works of Leibnitz remind us of Bacon rather than Descartes. His life was spent in filling high political positions, and his philosophical and scientific writings were by−paths of his fertile mind. He was a theoretical rather than a practical scientist, his contributions to science being in the nature of philosophical reasonings rather than practical demonstrations. Had he been able to withdraw from public life and devote himself to science alone, as Descartes did, he would undoubtedly have proved himself equally great as a practical worker. But during the time of his greatest activity in philosophical fields, between the years 1690 and 1716, he was all the time performing extraordinary active duties in entirely foreign fields. His work may be regarded, perhaps, as doing for Germany in particular what Bacon's did for England and the rest of the world in general.

Only a comparatively small part of his philosophical writings concern us here. According to his theory of the ultimate elements of the universe, the entire universe is composed of individual centres, or monads. To these monads he ascribed numberless qualities by which every phase of nature may be accounted. They were supposed by him to be percipient, self−acting beings, not under arbitrary control of the deity, and yet God himself was the original monad from which all the rest are generated. With this conception as a basis, Leibnitz deduced his doctrine of pre−established harmony, whereby the numerous independent substances composing the world are made to form one universe. He believed that by virtue of an inward energy monads develop themselves spontaneously, each being independent of every other. In short, each monad is a kind of deity in itself—a microcosm representing all the great features of the macrocosm.

It would be impossible clearly to estimate the precise value of the stimulative influence of these philosophers upon the scientific thought of their time. There was one way, however, in which their influence was made very tangible—namely, in the incentive they gave to the foundation of scientific societies.

SCIENTIFIC SOCIETIES

At the present time, when the elements of time and distance are practically eliminated in the propagation of news, and when cheap printing has minimized the difficulties of publishing scientific discoveries, it is difficult to understand the isolated position of the scientific investigation of the ages that preceded steam and electricity. Shut off from the world and completely out of touch with fellow−laborers perhaps only a few miles away, the investigators were naturally seriously handicapped; and inventions and discoveries were not made with the same rapidity that they would undoubtedly have been had the same men been receiving daily, weekly, or monthly communications from fellow−laborers all over the world, as they do to−day. Neither did they have the advantage of public or semi−public laboratories, where they were brought into contact with other men, from whom to gather fresh trains of thought and receive the stimulus of their successes or failures. In the natural course of events, however, neighbors who were interested in somewhat similar pursuits, not of the character of the rivalry of trade or commerce, would meet more or less frequently and discuss their progress. The mutual advantages of such intercourse would be at once appreciated; and it would be but a short step from the casual meeting of two neighborly scientists to the establishment of "societies," meeting at fixed times, and composed of members living within reasonable travelling distance. There would, perhaps, be the weekly or monthly meetings of men in a limited area; and as the natural outgrowth of these little local societies, with frequent meetings, would come the formation of larger societies, meeting less often, where members travelled a considerable distance to attend. And, finally, with increased facilities for communication and travel, the great international societies of to−day would be produced—the natural outcome of the neighborly meetings of the primitive mediaeval investigators.

In Italy, at about the time of Galileo, several small societies were formed. One of the most important of these was the Lyncean Society, founded about the year 1611, Galileo himself being a member. This society was succeeded by the Accademia del Cimento, at Florence, in 1657, which for a time flourished, with such a famous scientist as Torricelli as one of its members.

In England an impetus seems to have been given by Sir Francis Bacon's writings in criticism and censure of the systern of teaching in colleges. It is supposed that his suggestions as to what should be the aims of a scientific society led eventually to the establishment of the Royal Society. He pointed out how little had really been accomplished by the existing institutions of learning in advancing science, and asserted that little good could ever come from them while their methods of teaching remained unchanged. He contended that the system which made the lectures and exercises of such a nature that no deviation from the established routine could be thought of was pernicious. But he showed that if any teacher had the temerity to turn from the traditional paths, the daring pioneer was likely to find insurmountable obstacles placed in the way of his advancement. The studies were "imprisoned" within the limits of a certain set of authors, and originality in thought or teaching was to be neither contemplated nor tolerated.

The words of Bacon, given in strong and unsparing terms of censure and condemnation, but nevertheless with perfect justification, soon bore fruit. As early as the year 1645 a small company of scientists had been in the habit of meeting at some place in London to discuss philosophical and scientific subjects for mental advancement. In 1648, owing to the political disturbances of the time, some of the members of these meetings removed to Oxford, among them Boyle, Wallis, and Wren, where the meetings were continued, as were also the meetings of those left in London. In 1662, however, when the political situation bad become more settled, these two bodies of men were united under a charter from Charles II., and Bacon's ideas were practically expressed in that learned body, the Royal Society of London. And it matters little that in some respects Bacon's views were not followed in the practical workings of the society, or that the division of labor in the early stages was somewhat different than at present. The aim of the society has always been one for the advancement of learning; and if Bacon himself could look over its records, he would surely have little fault to find with the aid it has given in carrying out his ideas for the promulgation of useful knowledge.

Ten years after the charter was granted to the Royal Society of London, Lord Bacon's words took practical effect in Germany, with the result that the Academia Naturae Curiosorum was founded, under the leadership of Professor J. C. Sturm. The early labors of this society were devoted to a repetition of the most notable experiments of the time, and the work of the embryo society was published in two volumes, in 1672 and 1685 respectively, which were practically text−books of the physics of the period. It was not until 1700 that Frederick I. founded the Royal Academy of Sciences at Berlin, after the elaborate plan of Leibnitz, who was himself the first president.

Perhaps the nearest realization of Bacon's ideal, however, is in the Royal Academy of Sciences at Paris, which was founded in 1666 under the administration of Colbert, during the reign of Louis XIV. This institution not only recognized independent members, but had besides twenty pensionnaires who received salaries from the government. In this way a select body of scientists were enabled to pursue their investigations without being obliged to "give thought to the morrow" for their sustenance. In return they were to furnish the meetings with scientific memoirs, and once a year give an account of the work they were engaged upon. Thus a certain number of the brightest minds were encouraged to devote their entire time to scientific research, "delivered alike from the temptations of wealth or the embarrassments of poverty." That such a plan works well is amply attested by the results emanating from the French academy. Pensionnaires in various branches of science, however, either paid by the state or by learned societies, are no longer confined to France.

Among the other early scientific societies was the Imperial Academy of Sciences at St. Petersburg, projected by Peter the Great, and established by his widow, Catharine I., in 1725; and also the Royal Swedish Academy, incorporated in 1781, and counting among its early members such men as the celebrated Linnaeus. But after the first impulse had resulted in a few learned societies, their manifest advantage was so evident that additional numbers increased rapidly, until at present almost every branch of every science is represented by more or less important bodies; and these are, individually and collectively, adding to knowledge and stimulating interest in the many fields of science, thus vindicating Lord Bacon's asseverations that knowledge could be satisfactorily promulgated in this manner.

X. THE SUCCESSORS OF GALILEO IN PHYSICAL SCIENCE

We have now to witness the diversified efforts of a company of men who, working for the most part independently, greatly added to the data of the physical sciences—such men as Boyle, Huygens, Von Gericke, and Hooke. It will be found that the studies of these men covered the whole field of physical sciences as then understood—the field of so−called natural philosophy. We shall best treat these successors of Galileo and precursors of Newton somewhat biographically, pointing out the correspondences and differences between their various accomplishments as we proceed. It will be noted in due course that the work of some of them was anticipatory of great achievements of a later century.

ROBERT BOYLE (1627−1691)

Some of Robert Boyle's views as to the possible structure of atmospheric air will be considered a little farther on in this chapter, but for the moment we will take up the consideration of some of his experiments upon that as well as other gases. Boyle was always much interested in alchemy, and carried on extensive experiments in attempting to accomplish the transmutation of metals; but he did not confine himself to these experiments, devoting himself to researches in all the fields of natural philosophy. He was associated at Oxford with a company of scientists, including Wallis and Wren, who held meetings and made experiments together, these gatherings being the beginning, as mentioned a moment ago, of what finally became the Royal Society. It was during this residence at Oxford that many of his valuable researches upon air were made, and during this time be invented his air−pump, now exhibited in the Royal Society rooms at Burlington House.[1]

His experiments to prove the atmospheric pressure are most interesting and conclusive. "Having three small, round glass bubbles, blown at the flame of a lamp, about the size of hazel−nuts," he says, "each of them with a short, slender stem, by means whereof they were so exactly poised in water that a very small change of weight would make them either emerge or sink; at a time when the atmosphere was of convenient weight, I put them into a wide−mouthed glass of common water, and leaving them in a quiet place, where they were frequently in my eye, I observed that sometimes they would be at the top of the water, and remain there for several days, or perhaps weeks, together, and sometimes fall to the bottom, and after having continued there for some time rise again. And sometimes they would rise or fall as the air was hot or cold."[2]

It was in the course of these experiments that the observations made by Boyle led to the invention of his "statical barometer," the mercurial barometer having been invented, as we have seen, by Torricelli, in 1643. In describing this invention he says: "Making choice of a large, thin, and light glass bubble, blown at the flame of a lamp, I counterpoised it with a metallic weight, in a pair of scales that were suspended in a frame, that would turn with the thirtieth part of a grain. Both the frame and the balance were then placed near a good barometer, whence I might learn the present weight of the atmosphere; when, though the scales were unable to show all the variations that appeared in the mercurial barometer, yet they gave notice of those that altered the height of the mercury half a quarter of an inch."[3] A fairly sensitive barometer, after all. This statical barometer suggested several useful applications to the fertile imagination of its inventor, among others the measuring of mountain−peaks, as with the mercurial barometer, the rarefication of the air at the top giving a definite ratio to the more condensed air in the valley.

Another of his experiments was made to discover the atmospheric pressure to the square inch. After considerable difficulty he determined that the relative weight of a cubic inch of water and mercury was about one to fourteen, and computing from other known weights he determined that "when a column of quicksilver thirty inches high is sustained in the barometer, as it frequently happens, a column of air that presses upon an inch square near the surface of the earth must weigh about fifteen avoirdupois pounds."[4] As the pressure of air at the sea−level is now estimated at 14.7304 pounds to the square inch, it will be seen that Boyle's calculation was not far wrong.

From his numerous experiments upon the air, Boyle was led to believe that there were many "latent qualities" due to substances contained in it that science had as yet been unable to fathom, believing that there is "not a more heterogeneous body in the world." He believed that contagious diseases were carried by the air, and suggested that eruptions of the earth, such as those made by earthquakes, might send up "venomous exhalations" that produced diseases. He suggested also that the air might play an important part in some processes of calcination, which, as we shall see, was proved to be true by Lavoisier late in the eighteenth century. Boyle's notions of the exact chemical action in these phenomena were of course vague and indefinite, but he had observed that some part was played by the air, and he was right in supposing that the air "may have a great share in varying the salts obtainable from calcined vitriol."[5]

Although he was himself such a painstaking observer of facts, he had the fault of his age of placing too much faith in hear−say evidence of untrained observers. Thus, from the numerous stories he heard concerning the growth of metals in previously exhausted mines, he believed that the air was responsible for producing this growth—in which he undoubtedly believed. The story of a tin−miner that, in his own time, after a lapse of only twenty−five years, a heap, of earth previously exhausted of its ore became again even more richly impregnated than before by lying exposed to the air, seems to have been believed by the philosopher.

As Boyle was an alchemist, and undoubtedly believed in the alchemic theory that metals have "spirits" and various other qualities that do not exist, it is not surprising that he was credulous in the matter of beliefs concerning peculiar phenomena exhibited by them. Furthermore, he undoubtedly fell into the error common to "specialists," or persons working for long periods of time on one subject—the error of over−enthusiasm in his subject. He had discovered so many remarkable qualities in the air that it is not surprising to find that he attributed to it many more that he could not demonstrate.

Boyle's work upon colors, although probably of less importance than his experiments and deductions upon air, show that he was in the van as far as the science of his day was concerned. As he points out, the schools of his time generally taught that "color is a penetrating quality, reaching to the innermost part of the substance," and, as an example of this, sealing−wax was cited, which could be broken into minute bits, each particle retaining the same color as its fellows or the original mass. To refute this theory, and to show instances to the contrary, Boyle, among other things, shows that various colors—blue, red, yellow—may be produced upon tempered steel, and yet the metal within "a hair's−breadth of its surface" have none of these colors. Therefore, he was led to believe that color, in opaque bodies at least, is superficial.

"But before we descend to a more particular consideration of our subject," he says, " 'tis proper to observe that colors may be regarded either as a quality residing in bodies to modify light after a particular manner, or else as light itself so modified as to strike upon the organs of sight, and cause the sensation we call color; and that this latter is the more proper acceptation of the word color will appear hereafter. And indeed it is the light itself, which after a certain manner, either mixed with shades or other−wise, strikes our eyes and immediately produces that motion in the organ which gives us the color of an object."[6]

In examining smooth and rough surfaces to determine the cause of their color, he made use of the microscope, and pointed out the very obvious example of the difference in color of a rough and a polished piece of the same block of stone. He used some striking illustrations of the effect of light and the position of the eye upon colors. "Thus the color of plush or velvet will appear various if you stroke part of it one way and part another, the posture of the particular threads in regard to the light, or the eye, being thereby varied. And 'tis observable that in a field of ripe corn, blown upon by the wind, there will appear waves of a color different from that of the rest of the corn, because the wind, by depressing some of the ears more than others, causes one to reflect more light from the lateral and strawy parts than another."[7] His work upon color, however, as upon light, was entirely overshadowed by the work of his great fellow−countryman Newton.

Boyle's work on electricity was a continuation of Gilbert's, to which he added several new facts. He added several substances to Gilbert's list of "electrics," experimented on smooth and rough surfaces in exciting of electricity, and made the important discovery that amber retained its attractive virtue after the friction that excited it bad ceased. "For the attrition having caused an intestine motion in its parts," he says, "the heat thereby excited ought not to cease as soon as ever the rubbing is over, but to continue capable of emitting effluvia for some time afterwards, longer or shorter according to the goodness of the electric and the degree of the commotion made; all which, joined together, may sometimes make the effect considerable; and by this means, on a warm day, I, with a certain body not bigger than a pea, but very vigorously attractive, moved a steel needle, freely poised, about three minutes after I had left off rubbing it."[8]

MARIOTTE AND VON GUERICKE

Working contemporaneously with Boyle, and a man whose name is usually associated with his as the propounder of the law of density of gases, was Edme Mariotte (died 1684), a native of Burgundy. Mariotte demonstrated that but for the resistance of the atmosphere, all bodies, whether light or heavy, dense or thin, would fall with equal rapidity, and he proved this by the well−known "guinea−and−feather" experiment. Having exhausted the air from a long glass tube in which a guinea piece and a feather had been placed, he showed that in the vacuum thus formed they fell with equal rapidity as often as the tube was reversed. From his various experiments as to the pressure of the atmosphere he deduced the law that the density and elasticity of the atmosphere are precisely proportional to the compressing force (the law of Boyle and Mariotte). He also ascertained that air existed in a state of mechanical mixture with liquids, "existing between their particles in a state of condensation." He made many other experiments, especially on the collision of bodies, but his most important work was upon the atmosphere.

But meanwhile another contemporary of Boyle and Mariotte was interesting himself in the study of the atmosphere, and had made a wonderful invention and a most striking demonstration. This was Otto von Guericke (1602−1686), Burgomaster of Magdeburg, and councillor to his "most serene and potent Highness" the elector of that place. When not engrossed with the duties of public office, he devoted his time to the study of the sciences, particularly pneumatics and electricity, both then in their infancy. The discoveries of Galileo, Pascal, and Torricelli incited him to solve the problem of the creation of a vacuum—a desideratum since before the days of Aristotle. His first experiments were with a wooden pump and a barrel of water, but he soon found that with such porous material as wood a vacuum could not be created or maintained. He therefore made use of a globe of copper, with pump and stop−cock; and with this he was able to pump out air almost as easily as water. Thus, in 1650, the air−pump was invented. Continuing his experiments upon vacuums and atmospheric pressure with his newly discovered pump, he made some startling discoveries as to the enormous pressure exerted by the air.

It was not his intention, however, to demonstrate his newly acquired knowledge by words or theories alone, nor by mere laboratory experiments; but he chose instead an open field, to which were invited Emperor Ferdinand III., and all the princes of the Diet at Ratisbon. When they were assembled he produced two hollow brass hemispheres about two feet in diameter, and placing their exactly fitting surfaces together, proceeded to pump out the air from their hollow interior, thus causing them to stick together firmly in a most remarkable way, apparently without anything holding them. This of itself was strange enough; but now the worthy burgomaster produced teams of horses, and harnessing them to either side of the hemispheres, attempted to pull the adhering brasses apart. Five, ten, fifteen teams—thirty horses, in all—were attached; but pull and tug as they would they could not separate the firmly clasped hemispheres. The enormous pressure of the atmosphere had been most strikingly demonstrated.

But it is one thing to demonstrate, another to convince; and many of the good people of Magdeburg shook their heads over this "devil's contrivance," and predicted that Heaven would punish the Herr Burgomaster, as indeed it had once by striking his house with lightning and injuring some of his infernal contrivances. They predicted his future punishment, but they did not molest him, for to his fellow−citizens, who talked and laughed, drank and smoked with him, and knew him for the honest citizen that he was, he did not seem bewitched at all. And so he lived and worked and added other facts to science, and his brass hemispheres were not destroyed by fanatical Inquisitors, but are still preserved in the royal library at Berlin.

In his experiments with his air−pump he discovered many things regarding the action of gases, among others, that animals cannot live in a vacuum. He invented the anemoscope and the air−balance, and being thus enabled to weight the air and note the changes that preceded storms and calms, he was able still further to dumfound his wondering fellow−Magde−burgers by more or less accurate predictions about the weather.

Von Guericke did not accept Gilbert's theory that the earth was a great magnet, but in his experiments along lines similar to those pursued by Gilbert, he not only invented the first electrical machine, but discovered electrical attraction and repulsion. The electrical machine which he invented consisted of a sphere of sulphur mounted on an iron axis to imitate the rotation of the earth, and which, when rubbed, manifested electrical reactions. When this globe was revolved and stroked with the dry hand it was found that it attached to it "all sorts of little fragments, like leaves of gold, silver, paper, etc." "Thus this globe," he says, "when brought rather near drops of water causes them to swell and puff up. It likewise attracts air, smoke, etc."[9] Before the time of Guericke's demonstrations, Cabaeus had noted that chaff leaped back from an "electric," but he did not interpret the phenomenon as electrical repulsion. Von Guericke, however, recognized it as such, and refers to it as what he calls "expulsive virtue." "Even expulsive virtue is seen in this globe," he says, "for it not only attracts, but also REPELS again from itself little bodies of this sort, nor does it receive them until they have touched something else." It will be observed from this that he was very close to discovering the discharge of the electrification of attracted bodies by contact with some other object, after which they are reattracted by the electric.

He performed a most interesting experiment with his sulphur globe and a feather, and in doing so came near anticipating Benjamin Franklin in his discovery of the effects of pointed conductors in drawing off the discharge. Having revolved and stroked his globe until it repelled a bit of down, he removed the globe from its rack and advancing it towards the now repellent down, drove it before him about the room. In this chase he observed that the down preferred to alight against "the points of any object whatsoever." He noticed that should the down chance to be driven within a few inches of a lighted candle, its attitude towards the globe suddenly changed, and instead of running away from it, it now "flew to it for protection" —the charge on the down having been dissipated by the hot air. He also noted that if one face of a feather had been first attracted and then repelled by the sulphur ball, that the surface so affected was always turned towards the globe; so that if the positions of the two were reversed, the sides of the feather reversed also.

Still another important discovery, that of electrical conduction, was made by Von Guericke. Until his discovery no one had observed the transference of electricity from one body to another, although Gilbert had some time before noted that a rod rendered magnetic at one end became so at the other. Von Guericke's experiments were made upon a linen thread with his sulphur globe, which, he says, "having been previously excited by rubbing, can exercise likewise its virtue through a linen thread an ell or more long, and there attract something." But this discovery, and his equally important one that the sulphur ball becomes luminous when rubbed, were practically forgotten until again brought to notice by the discoveries of Francis Hauksbee and Stephen Gray early in the eighteenth century. From this we may gather that Von Guericke himself did not realize the import of his discoveries, for otherwise he would certainly have carried his investigations still further. But as it was he turned his attention to other fields of research.

ROBERT HOOKE

A slender, crooked, shrivelled−limbed, cantankerous little man, with dishevelled hair and haggard countenance, bad−tempered and irritable, penurious and dishonest, at least in his claims for priority in discoveries—this is the picture usually drawn, alike by friends and enemies, of Robert Hooke (1635−1703), a man with an almost unparalleled genius for scientific discoveries in almost all branches of science. History gives few examples so striking of a man whose really great achievements in science would alone have made his name immortal, and yet who had the pusillanimous spirit of a charlatan—an almost insane mania, as it seems—for claiming the credit of discoveries made by others. This attitude of mind can hardly be explained except as a mania: it is certainly more charitable so to regard it. For his own discoveries and inventions were so numerous that a few more or less would hardly have added to his fame, as his reputation as a philosopher was well established. Admiration for his ability and his philosophical knowledge must always be marred by the recollection of his arrogant claims to the discoveries of other philosophers.

It seems pretty definitely determined that Hooke should be credited with the invention of the balance−spring for regulating watches; but for a long time a heated controversy was waged between Hooke and Huygens as to who was the real inventor. It appears that Hooke conceived the idea of the balance−spring, while to Huygens belongs the credit of having adapted the COILED spring in a working model. He thus made practical Hooke's conception, which is without value except as applied by the coiled spring; but, nevertheless, the inventor, as well as the perfector, should receive credit. In this controversy, unlike many others, the blame cannot be laid at Hooke's door.

Hooke was the first curator of the Royal Society, and when anything was to be investigated, usually invented the mechanical devices for doing so. Astronomical apparatus, instruments for measuring specific weights, clocks and chronometers, methods of measuring the velocity of falling bodies, freezing and boiling points, strength of gunpowder, magnetic instruments—in short, all kinds of ingenious mechanical devices in all branches of science and mechanics. It was he who made the famous air−pump of Robert Boyle, based on Boyle's plans. Incidentally, Hooke claimed to be the inventor of the first air−pump himself, although this claim is now entirely discredited.

Within a period of two years he devised no less than thirty different methods of flying, all of which, of course, came to nothing, but go to show the fertile imagination of the man, and his tireless energy. He experimented with electricity and made some novel suggestions upon the difference between the electric spark and the glow, although on the whole his contributions in this field are unimportant. He also first pointed out that the motions of the heavenly bodies must be looked upon as a mechanical problem, and was almost within grasping distance of the exact theory of gravitation, himself originating the idea of making use of the pendulum in measuring gravity. Likewise, he first proposed the wave theory of light; although it was Huygens who established it on its present foundation.

Hooke published, among other things, a book of plates and descriptions of his Microscopical Observations, which gives an idea of the advance that had already been made in microscopy in his time. Two of these plates are given here, which, even in this age of microscopy, are both interesting and instructive. These plates are made from prints of Hooke's original copper plates, and show that excellent lenses were made even at that time. They illustrate, also, how much might have been accomplished in the field of medicine if more attention had been given to microscopy by physicians. Even a century later, had physicians made better use of their microscopes, they could hardly have overlooked such an easily found parasite as the itch mite, which is quite as easily detected as the cheese mite, pictured in Hooke's book.

In justice to Hooke, and in extenuation of his otherwise inexcusable peculiarities of mind, it should be remembered that for many years he suffered from a painful and wasting disease. This may have affected his mental equilibrium, without appreciably affecting his ingenuity. In his own time this condition would hardly have been considered a disease; but to−day, with our advanced ideas as to mental diseases, we should be more inclined to ascribe his unfortunate attitude of mind to a pathological condition, rather than to any manifestation of normal mentality. From this point of view his mental deformity seems not unlike that of Cavendish's, later, except that in the case of Cavendish it manifested itself as an abnormal sensitiveness instead of an abnormal irritability.

CHRISTIAN HUYGENS

If for nothing else, the world is indebted to the man who invented the pendulum clock, Christian Huygens (1629−1695), of the Hague, inventor, mathematician, mechanician, astronomer, and physicist. Huygens was the descendant of a noble and distinguished family, his father, Sir Constantine Huygens, being a well−known poet and diplomatist. Early in life young Huygens began his career in the legal profession, completing his education in the juridical school at Breda; but his taste for mathematics soon led him to neglect his legal studies, and his aptitude for scientific researches was so marked that Descartes predicted great things of him even while he was a mere tyro in the field of scientific investigation.

One of his first endeavors in science was to attempt an improvement of the telescope. Reflecting upon the process of making lenses then in vogue, young Huygens and his brother Constantine attempted a new method of grinding and polishing, whereby they overcame a great deal of the spherical and chromatic aberration. With this new telescope a much clearer field of vision was obtained, so much so that Huygens was able to detect, among other things, a hitherto unknown satellite of Saturn. It was these astronomical researches that led him to apply the pendulum to regulate the movements of clocks. The need for some more exact method of measuring time in his observations of the stars was keenly felt by the young astronomer, and after several experiments along different lines, Huygens hit upon the use of a swinging weight; and in 1656 made his invention of the pendulum clock. The year following, his clock was presented to the states−general. Accuracy as to time is absolutely essential in astronomy, but until the invention of Huygens's clock there was no precise, nor even approximately precise, means of measuring short intervals.

Huygens was one of the first to adapt the micrometer to the telescope—a mechanical device on which all the nice determination of minute distances depends. He also took up the controversy against Hooke as to the superiority of telescopic over plain sights to quadrants, Hooke contending in favor of the plain. In this controversy, the subject of which attracted wide attention, Huygens was completely victorious; and Hooke, being unable to refute Huygens's arguments, exhibited such irritability that he increased his already general unpopularity. All of the arguments for and against the telescope sight are too numerous to be given here. In contending in its favor Huygens pointed out that the unaided eye is unable to appreciate an angular space in the sky less than about thirty seconds. Even in the best quadrant with a plain sight, therefore, the altitude must be uncertain by that quantity. If in place of the plain sight a telescope is substituted, even if it magnify only thirty times, it will enable the observer to fix the position to one second, with progressively increased accuracy as the magnifying power of the telescope is increased. This was only one of the many telling arguments advanced by

Huygens.

In the field of optics, also, Huygens has added considerably to science, and his work, Dioptrics, is said to have been a favorite book with Newton. During the later part of his life, however, Huygens again devoted himself to inventing and constructing telescopes, grinding the lenses, and devising, if not actually making, the frame for holding them. These telescopes were of enormous lengths, three of his object−glasses, now in possession of the Royal Society, being of 123, 180, and 210 feet focal length respectively. Such instruments, if constructed in the ordinary form of the long tube, were very unmanageable, and to obviate this Huygens adopted the plan of dispensing with the tube altogether, mounting his lenses on long poles manipulated by machinery. Even these were unwieldy enough, but the difficulties of manipulation were fully compensated by the results obtained.

It had been discovered, among other things, that in oblique refraction light is separated into colors. Therefore, any small portion of the convex lens of the telescope, being a prism, the rays proceed to the focus, separated into prismatic colors, which make the image thus formed edged with a fringe of color and indistinct. But, fortunately for the early telescope makers, the degree of this aberration is independent of the focal length of the lens; so that, by increasing this focal length and using the appropriate eye−piece, the image can be greatly magnified, while the fringe of colors remains about the same as when a less powerful lens is used. Hence the advantage of Huygens's long telescope. He did not confine his efforts to simply lengthening the focal length of his telescopes, however, but also added to their efficiency by inventing an almost perfect achromatic eye−piece.

In 1663 he was elected a fellow of the Royal Society of London, and in 1669 he gave to that body a concise statement of the laws governing the collision of elastic bodies. Although the same views had been given by Wallis and Wren a few weeks earlier, there is no doubt that Huygens's views were reached independently; and it is probable that he had arrived at his conclusions several years before. In the Philosophical Transactions for 1669 it is recorded that the society, being interested in the laws of the principles of motion, a request was made that M. Huygens, Dr. Wallis, and Sir Christopher Wren submit their views on the subject. Wallis submitted his paper first, November 15, 1668. A month later, December 17th, Wren imparted to the society his laws as to the nature of the collision of bodies. And a few days later, January 5, 1669, Huygens sent in his "Rules Concerning the Motion of Bodies after Mutual Impulse." Although Huygens's report was received last, he was anticipated by such a brief space of time, and his views are so clearly stated—on the whole rather more so than those of the other two—that we give them in part here:

"1. If a hard body should strike against a body equally hard at rest, after contact the former will rest and the latter acquire a velocity equal to that of the moving body.

"2. But if that other equal body be likewise in motion, and moving in the same direction, after contact they will move with reciprocal velocities.

"3. A body, however great, is moved by a body however small impelled with any velocity whatsoever.

"5. The quantity of motion of two bodies may be either increased or diminished by their shock; but the same quantity towards the same part remains, after subtracting the quantity of the contrary motion.

"6. The sum of the products arising from multiplying the mass of any hard body into the squares of its velocity is the same both before and after the stroke.

"7. A hard body at rest will receive a greater quantity of motion from another hard body, either greater or less than itself, by the interposition of any third body of a mean quantity, than if it was immediately struck by the body itself; and if the interposing body be a mean proportional between the other two, its action upon the quiescent body will be the greatest of all."[10]

This was only one of several interesting and important communications sent to the Royal Society during his lifetime. One of these was a report on what he calls "Pneumatical Experiments." "Upon including in a vacuum an insect resembling a beetle, but somewhat larger," he says, "when it seemed to be dead, the air was readmitted, and soon after it revived; putting it again in the vacuum, and leaving it for an hour, after which the air was readmitted, it was observed that the insect required a longer time to recover; including it the third time for two days, after which the air was admitted, it was ten hours before it began to stir; but, putting it in a fourth time, for eight days, it never afterwards recovered.... Several birds, rats, mice, rabbits, and cats were killed in a vacuum, but if the air was admitted before the engine was quite exhausted some of them would recover; yet none revived that had been in a perfect vacuum.... Upon putting the weight of eighteen grains of powder with a gauge into a receiver that held several pounds of water, and firing the powder, it raised the mercury an inch and a half; from which it appears that there is one−fifth of air in gunpowder, upon the supposition that air is about one thousand times lighter than water; for in this experiment the mercury rose to the eighteenth part of the height at which the air commonly sustains it, and consequently the weight of eighteen grains of powder yielded air enough to fill the eighteenth part of a receiver that contained seven pounds of water; now this eighteenth part contains forty−nine drachms of water; wherefore the air, that takes up an equal space, being a thousand times lighter, weighs one−thousandth part of forty−nine drachms, which is more than three grains and a half; it follows, therefore, that the weight of eighteen grains of powder contains more than three and a half of air, which is about one−fifth of eighteen grains...."

From 1665 to 1681, accepting the tempting offer made him through Colbert, by Louis XIV., Huygens pursued his studies at the Bibliotheque du Roi as a resident of France. Here he published his Horologium Oscillatorium, dedicated to the king, containing, among other things, his solution of the problem of the "centre of oscillation." This in itself was an important step in the history of mechanics. Assuming as true that the centre of gravity of any number of interdependent bodies cannot rise higher than the point from which it falls, he reached correct conclusions as to the general principle of the conservation of vis viva, although he did not actually prove his conclusions. This was the first attempt to deal with the dynamics of a system. In this work, also, was the true determination of the relation between the length of a pendulum and the time of its oscillation.

In 1681 he returned to Holland, influenced, it is believed, by the attitude that was being taken in France against his religion. Here he continued his investigations, built his immense telescopes, and, among other things, discovered "polarization," which is recorded in Traite de la Lumiere, published at Leyden in 1690. Five years later he died, bequeathing his manuscripts to the University of Leyden. It is interesting to note that he never accepted Newton's theory of gravitation as a universal property of matter.

XI. NEWTON AND THE COMPOSITION OF LIGHT

Galileo, that giant in physical science of the early seventeenth century, died in 1642. On Christmas day of the same year there was born in England another intellectual giant who was destined to carry forward the work of Copernicus, Kepler, and Galileo to a marvellous consummation through the discovery of the great unifying law in accordance with which the planetary motions are performed. We refer, of course, to the greatest of English physical scientists, Isaac Newton, the Shakespeare of the scientific world. Born thus before the middle of the seventeenth century, Newton lived beyond the first quarter of the eighteenth (1727). For the last forty years of that period his was the dominating scientific personality of the world. With full propriety that time has been spoken of as the "Age of Newton."

Yet the man who was to achieve such distinction gave no early premonition of future greatness. He was a sickly child from birth, and a boy of little seeming promise. He was an indifferent student, yet, on the other hand, he cared little for the common amusements of boyhood. He early exhibited, however, a taste for mechanical contrivances, and spent much time in devising windmills, water−clocks, sun−dials, and kites. While other boys were interested only in having kites that would fly, Newton—at least so the stories of a later time would have us understand—cared more for the investigation of the seeming principles involved, or for testing the best methods of attaching the strings, or the best materials to be used in construction.

Meanwhile the future philosopher was acquiring a taste for reading and study, delving into old volumes whenever he found an opportunity. These habits convinced his relatives that it was useless to attempt to make a farmer of the youth, as had been their intention. He was therefore sent back to school, and in the summer of 1661 he matriculated at Trinity College, Cambridge. Even at college Newton seems to have shown no unusual mental capacity, and in 1664, when examined for a scholarship by Dr. Barrow, that gentleman is said to have formed a poor opinion of the applicant. It is said that the knowledge of the estimate placed upon his abilities by his instructor piqued Newton, and led him to take up in earnest the mathematical studies in which he afterwards attained such distinction. The study of Euclid and Descartes's "Geometry" roused in him a latent interest in mathematics, and from that time forward his investigations were carried on with enthusiasm. In 1667 he was elected Fellow of Trinity College, taking the degree of M.A. the following spring.

It will thus appear that Newton's boyhood and early manhood were passed during that troublous time in British political annals which saw the overthrow of Charles I., the autocracy of Cromwell, and the eventual restoration of the Stuarts. His maturer years witnessed the overthrow of the last Stuart and the reign of the Dutchman, William of Orange. In his old age he saw the first of the Hanoverians mount the throne of England. Within a decade of his death such scientific path−finders as Cavendish, Black, and Priestley were born—men who lived on to the close of the eighteenth century. In a full sense, then, the age of Newton bridges the gap from that early time of scientific awakening under Kepler and Galileo to the time which we of the twentieth century think of as essentially modern.

THE COMPOSITION OF WHITE LIGHT

In December, 1672, Newton was elected a Fellow of the Royal Society, and at this meeting a paper describing his invention of the refracting telescope was read. A few days later he wrote to the secretary, making some inquiries as to the weekly meetings of the society, and intimating that he had an account of an interesting discovery that he wished to lay before the society. When this communication was made public, it proved to be an explanation of the discovery of the composition of white light. We have seen that the question as to the nature of color had commanded the attention of such investigators as Huygens, but that no very satisfactory solution of the question had been attained. Newton proved by demonstrative experiments that white light is composed of the blending of the rays of diverse colors, and that the color that we ascribe to any object is merely due to the fact that the object in question reflects rays of that color, absorbing the rest. That white light is really made up of many colors blended would seem incredible had not the experiments by which this composition is demonstrated become familiar to every one. The experiments were absolutely novel when Newton brought them forward, and his demonstration of the composition of light was one of the most striking expositions ever brought to the attention of the Royal Society. It is hardly necessary to add that, notwithstanding the conclusive character of Newton's work, his explanations did not for a long time meet with general acceptance.

Newton was led to his discovery by some experiments made with an ordinary glass prism applied to a hole in the shutter of a darkened room, the refracted rays of the sunlight being received upon the opposite wall and forming there the familiar spectrum. "It was a very pleasing diversion," he wrote, "to view the vivid and intense colors produced thereby; and after a time, applying myself to consider them very circumspectly, I became surprised to see them in varying form, which, according to the received laws of refraction, I expected should have been circular. They were terminated at the sides with straight lines, but at the ends the decay of light was so gradual that it was difficult to determine justly what was their figure, yet they seemed semicircular.

"Comparing the length of this colored spectrum with its breadth, I found it almost five times greater; a disproportion so extravagant that it excited me to a more than ordinary curiosity of examining from whence it might proceed. I could scarce think that the various thicknesses of the glass, or the termination with shadow or darkness, could have any influence on light to produce such an effect; yet I thought it not amiss, first, to examine those circumstances, and so tried what would happen by transmitting light through parts of the glass of divers thickness, or through holes in the window of divers bigness, or by setting the prism without so that the light might pass through it and be refracted before it was transmitted through the hole; but I found none of those circumstances material. The fashion of the colors was in all these cases the same.

"Then I suspected whether by any unevenness of the glass or other contingent irregularity these colors might be thus dilated. And to try this I took another prism like the former, and so placed it that the light, passing through them both, might be refracted contrary ways, and so by the latter returned into that course from which the former diverted it. For, by this means, I thought, the regular effects of the first prism would be destroyed by the second prism, but the irregular ones more augmented by the multiplicity of refractions. The event was that the light, which by the first prism was diffused into an oblong form, was by the second reduced into an orbicular one with as much regularity as when it did not all pass through them. So that, whatever was the cause of that length, 'twas not any contingent irregularity.

"I then proceeded to examine more critically what might be effected by the difference of the incidence of rays coming from divers parts of the sun; and to that end measured the several lines and angles belonging to the image. Its distance from the hole or prism was 22 feet; its utmost length 13 1/4 inches; its breadth 2 5/8; the diameter of the hole 1/4 of an inch; the angle which the rays, tending towards the middle of the image, made with those lines, in which they would have proceeded without refraction, was 44 degrees 56'; and the vertical angle of the prism, 63 degrees 12'. Also the refractions on both sides of the prism—that is, of the incident and emergent rays—were, as near as I could make them, equal, and consequently about 54 degrees 4'; and the rays fell perpendicularly upon the wall. Now, subducting the diameter of the hole from the length and breadth of the image, there remains 13 inches the length, and 2 3/8 the breadth, comprehended by those rays, which, passing through the centre of the said hole, which that breadth subtended, was about 31', answerable to the sun's diameter; but the angle which its length subtended was more than five such diameters, namely 2 degrees 49'.

"Having made these observations, I first computed from them the refractive power of the glass, and found it measured by the ratio of the sines 20 to 31. And then, by that ratio, I computed the refractions of two rays flowing from opposite parts of the sun's discus, so as to differ 31' in their obliquity of incidence, and found that the emergent rays should have comprehended an angle of 31', as they did, before they were incident.

"But because this computation was founded on the hypothesis of the proportionality of the sines of incidence and refraction, which though by my own experience I could not imagine to be so erroneous as to make that angle but 31', which in reality was 2 degrees 49', yet my curiosity caused me again to make my prism. And having placed it at my window, as before, I observed that by turning it a little about its axis to and fro, so as to vary its obliquity to the light more than an angle of 4 degrees or 5 degrees, the colors were not thereby sensibly translated from their place on the wall, and consequently by that variation of incidence the quantity of refraction was not sensibly varied. By this experiment, therefore, as well as by the former computation, it was evident that the difference of the incidence of rays flowing from divers parts of the sun could not make them after decussation diverge at a sensibly greater angle than that at which they before converged; which being, at most, but about 31' or 32', there still remained some other cause to be found out, from whence it could be 2 degrees 49'."

All this caused Newton to suspect that the rays, after their trajection through the prism, moved in curved rather than in straight lines, thus tending to be cast upon the wall at different places according to the amount of this curve. His suspicions were increased, also, by happening to recall that a tennis−ball sometimes describes such a curve when "cut" by a tennis−racket striking the ball obliquely.

"For a circular as well as a progressive motion being communicated to it by the stroke," he says, "its parts on that side where the motions conspire must press and beat the contiguous air more violently than on the other, and there excite a reluctancy and reaction of the air proportionately greater. And for the same reason, if the rays of light should possibly be globular bodies, and by their oblique passage out of one medium into another acquire a circulating motion, they ought to feel the greater resistance from the ambient ether on that side where the motions conspire, and thence be continually bowed to the other. But notwithstanding this plausible ground of suspicion, when I came to examine it I could observe no such curvity in them. And, besides (which was enough for my purpose), I observed that the difference 'twixt the length of the image and diameter of the hole through which the light was transmitted was proportionable to their distance.

"The gradual removal of these suspicions at length led me to the experimentum crucis, which was this: I took two boards, and, placing one of them close behind the prism at the window, so that the light must pass through a small hole, made in it for the purpose, and fall on the other board, which I placed at about twelve feet distance, having first made a small hole in it also, for some of the incident light to pass through. Then I placed another prism behind this second board, so that the light trajected through both the boards might pass through that also, and be again refracted before it arrived at the wall. This done, I took the first prism in my hands and turned it to and fro slowly about its axis, so much as to make the several parts of the image, cast on the second board, successively pass through the hole in it, that I might observe to what places on the wall the second prism would refract them. And I saw by the variation of these places that the light, tending to that end of the image towards which the refraction of the first prism was made, did in the second prism suffer a refraction considerably greater than the light tending to the other end. And so the true cause of the length of that image was detected to be no other than that LIGHT consists of RAYS DIFFERENTLY REFRANGIBLE, which, without any respect to a difference in their incidence, were, according to their degrees of refrangibility, transmitted towards divers parts of the wall."[1]

THE NATURE OF COLOR

Having thus proved the composition of light, Newton took up an exhaustive discussion as to colors, which cannot be entered into at length here. Some of his remarks on the subject of compound colors, however, may be stated in part. Newton's views are of particular interest in this connection, since, as we have already pointed out, the question as to what constituted color could not be agreed upon by the philosophers. Some held that color was an integral part of the substance; others maintained that it was simply a reflection from the surface; and no scientific explanation had been generally accepted. Newton concludes his paper as follows:

"I might add more instances of this nature, but I shall conclude with the general one that the colors of all natural bodies have no other origin than this, that they are variously qualified to reflect one sort of light in greater plenty than another. And this I have experimented in a dark room by illuminating those bodies with uncompounded light of divers colors. For by that means any body may be made to appear of any color. They have there no appropriate color, but ever appear of the color of the light cast upon them, but yet with this difference, that they are most brisk and vivid in the light of their own daylight color. Minium appeareth there of any color indifferently with which 'tis illustrated, but yet most luminous in red; and so Bise appeareth indifferently of any color with which 'tis illustrated, but yet most luminous in blue. And therefore Minium reflecteth rays of any color, but most copiously those indued with red; and consequently, when illustrated with daylight—that is, with all sorts of rays promiscuously blended—those qualified with red shall abound most in the reflected light, and by their prevalence cause it to appear of that color. And for the same reason, Bise, reflecting blue most copiously, shall appear blue by the excess of those rays in its reflected light; and the like of other bodies. And that this is the entire and adequate cause of their colors is manifest, because they have no power to change or alter the colors of any sort of rays incident apart, but put on all colors indifferently with which they are enlightened."[2]

This epoch−making paper aroused a storm of opposition. Some of Newton's opponents criticised his methods, others even doubted the truth of his experiments. There was one slight mistake in Newton's belief that all prisms would give a spectrum of exactly the same length, and it was some time before he corrected this error. Meanwhile he patiently met and answered the arguments of his opponents until he began to feel that patience was no longer a virtue. At one time he even went so far as to declare that, once he was "free of this business," he would renounce scientific research forever, at least in a public way. Fortunately for the world, however, he did not adhere to this determination, but went on to even greater discoveries—which, it may be added, involved still greater controversies.

In commenting on Newton's discovery of the composition of light, Voltaire said: "Sir Isaac Newton has demonstrated to the eye, by the bare assistance of a prism, that light is a composition of colored rays, which, being united, form white color. A single ray is by him divided into seven, which all fall upon a piece of linen or a sheet of white paper, in their order one above the other, and at equal distances. The first is red, the second orange, the third yellow, the fourth green, the fifth blue, the sixth indigo, the seventh a violet purple. Each of these rays transmitted afterwards by a hundred other prisms will never change the color it bears; in like manner as gold, when completely purged from its dross, will never change afterwards in the crucible."[3]

XII. NEWTON AND THE LAW OF GRAVITATION

We come now to the story of what is by common consent the greatest of scientific achievements. The law of universal gravitation is the most far−reaching principle as yet discovered. It has application equally to the minutest particle of matter and to the most distant suns in the universe, yet it is amazing in its very simplicity. As usually phrased, the law is this: That every particle of matter in the universe attracts every other particle with a force that varies directly with the mass of the particles and inversely as the squares of their mutual distance. Newton did not vault at once to the full expression of this law, though he had formulated it fully before he gave the results of his investigations to the world. We have now to follow the steps by which he reached this culminating achievement.

At the very beginning we must understand that the idea of universal gravitation was not absolutely original with Newton. Away back in the old Greek days, as we have seen, Anaxagoras conceived and clearly expressed the idea that the force which holds the heavenly bodies in their orbits may be the same that operates upon substances at the surface of the earth. With Anaxagoras this was scarcely more than a guess. After his day the idea seems not to have been expressed by any one until the seventeenth century's awakening of science. Then the consideration of Kepler's Third Law of planetary motion suggested to many minds perhaps independently the probability that the force hitherto mentioned merely as centripetal, through the operation of which the planets are held in their orbits is a force varying inversely as the square of the distance from the sun. This idea had come to Robert Hooke, to Wren, and perhaps to Halley, as well as to Newton; but as yet no one had conceived a method by which the validity of the suggestion might be tested. It was claimed later on by Hooke that he had discovered a method demonstrating the truth of the theory of inverse squares, and after the full announcement of Newton's discovery a heated controversy was precipitated in which Hooke put forward his claims with accustomed acrimony. Hooke, however, never produced his demonstration, and it may well be doubted whether he had found a method which did more than vaguely suggest the law which the observations of Kepler had partially revealed. Newton's great merit lay not so much in conceiving the law of inverse squares as in the demonstration of the law. He was led to this demonstration through considering the orbital motion of the moon. According to the familiar story, which has become one of the classic myths of science, Newton was led to take up the problem through observing the fall of an apple. Voltaire is responsible for the story, which serves as well as another; its truth or falsity need not in the least concern us. Suffice it that through pondering on the familiar fact of terrestrial gravitation, Newton was led to question whether this force which operates so tangibly here at the earth's surface may not extend its influence out into the depths of space, so as to include, for example, the moon. Obviously some force pulls the moon constantly towards the earth; otherwise that body would fly off at a tangent and never return. May not this so−called centripetal force be identical with terrestrial gravitation? Such was Newton's query. Probably many another man since Anaxagoras had asked the same question, but assuredly Newton was the first man to find an answer.

The thought that suggested itself to Newton's mind was this: If we make a diagram illustrating the orbital course of the moon for any given period, say one minute, we shall find that the course of the moon departs from a straight line during that period by a measurable distance—that: is to say, the moon has been virtually pulled towards the earth by an amount that is represented by the difference between its actual position at the end of the minute under observation and the position it would occupy had its course been tangential, as, according to the first law of motion, it must have been had not some force deflected it towards the earth. Measuring the deflection in question—which is equivalent to the so−called versed sine of the arc traversed—we have a basis for determining the strength of the deflecting force. Newton constructed such a diagram, and, measuring the amount of the moon's departure from a tangential rectilinear course in one minute, determined this to be, by his calculation, thirteen feet. Obviously, then, the force acting upon the moon is one that would cause that body to fall towards the earth to the distance of thirteen feet in the first minute of its fall. Would such be the force of gravitation acting at the distance of the moon if the power of gravitation varies inversely as the square of the distance? That was the tangible form in which the problem presented itself to Newton. The mathematical solution of the problem was simple enough. It is based on a comparison of the moon's distance with the length of the earth's radius. On making this calculation, Newton found that the pull of gravitation—if that were really the force that controls the moon—gives that body a fall of slightly over fifteen feet in the first minute, instead of thirteen feet. Here was surely a suggestive approximation, yet, on the other band, the discrepancy seemed to be too great to warrant him in the supposition that he had found the true solution. He therefore dismissed the matter from his mind for the time being, nor did he return to it definitely for some years.

{illustration caption = DIAGRAM TO ILLUSTRATE NEWTON'S LAW OF GRAVITATION (E represents the earth and A the moon. Were the earth's pull on the moon to cease, the moon's inertia would cause it to take the tangential course, AB. On the other hand, were the moon's motion to be stopped for an instant, the moon would fall directly towards the earth, along the line AD. The moon's actual orbit, resulting from these component forces, is AC. Let AC represent the actual flight of the moon in one minute. Then BC, which is obviously equal to AD, represents the distance which the moon virtually falls towards the earth in one minute. Actual computation, based on measurements of the moon's orbit, showed this distance to be about fifteen feet. Another computation showed that this is the distance that the moon would fall towards the earth under the influence of gravity, on the supposition that the force of gravity decreases inversely with the square of the distance; the basis of comparison being furnished by falling bodies at the surface of the earth. Theory and observations thus coinciding, Newton was justified in declaring that the force that pulls the moon towards the earth and keeps it in its orbit, is the familiar force of gravity, and that this varies inversely as the square of the distance.)}

It was to appear in due time that Newton's hypothesis was perfectly valid and that his method of attempted demonstration was equally so. The difficulty was that the earth's proper dimensions were not at that time known. A wrong estimate of the earth's size vitiated all the other calculations involved, since the measurement of the moon's distance depends upon the observation of the parallax, which cannot lead to a correct computation unless the length of the earth's radius is accurately known. Newton's first calculation was made as early as 1666, and it was not until 1682 that his attention was called to a new and apparently accurate measurement of a degree of the earth's meridian made by the French astronomer Picard. The new measurement made a degree of the earth's surface 69.10 miles, instead of sixty miles.

Learning of this materially altered calculation as to the earth's size, Newton was led to take up again his problem of the falling moon. As he proceeded with his computation, it became more and more certain that this time the result was to harmonize with the observed facts. As the story goes, he was so completely overwhelmed with emotion that he was forced to ask a friend to complete the simple calculation. That story may well be true, for, simple though the computation was, its result was perhaps the most wonderful demonstration hitherto achieved in the entire field of science. Now at last it was known that the force of gravitation operates at the distance of the moon, and holds that body in its elliptical orbit, and it required but a slight effort of the imagination to assume that the force which operates through such a reach of space extends its influence yet more widely. That such is really the case was demonstrated presently through calculations as to the moons of Jupiter and by similar computations regarding the orbital motions of the various planets. All results harmonizing, Newton was justified in reaching the conclusion that gravitation is a universal property of matter. It remained, as we shall see, for nineteenth−century scientists to prove that the same force actually operates upon the stars, though it should be added that this demonstration merely fortified a belief that had already found full acceptance.

Having thus epitomized Newton's discovery, we must now take up the steps of his progress somewhat in detail, and state his theories and their demonstration in his own words. Proposition IV., theorem 4, of his Principia is as follows:

"That the moon gravitates towards the earth and by the force of gravity is continually drawn off from a rectilinear motion and retained in its orbit.

"The mean distance of the moon from the earth, in the syzygies in semi−diameters of the earth, is, according to Ptolemy and most astronomers, 59; according to Vendelin and Huygens, 60; to Copernicus, 60 1/3; to Street, 60 2/3; and to Tycho, 56 1/2. But Tycho, and all that follow his tables of refractions, making the refractions of the sun and moon (altogether against the nature of light) to exceed the refractions of the fixed stars, and that by four or five minutes NEAR THE HORIZON, did thereby increase the moon's HORIZONTAL parallax by a like number of minutes, that is, by a twelfth or fifteenth part of the whole parallax. Correct this error and the distance will become about 60 1/2 semi−diameters of the earth, near to what others have assigned. Let us assume the mean distance of 60 diameters in the syzygies; and suppose one revolution of the moon, in respect to the fixed stars, to be completed in 27d. 7h. 43', as astronomers have determined; and the circumference of the earth to amount to 123,249,600 Paris feet, as the French have found by mensuration. And now, if we imagine the moon, deprived of all motion, to be let go, so as to descend towards the earth with the impulse of all that force by which (by Cor. Prop. iii.) it is retained in its orb, it will in the space of one minute of time describe in its fall 15 1/12 Paris feet. For the versed sine of that arc which the moon, in the space of one minute of time, would by its mean motion describe at the distance of sixty semi−diameters of the earth, is nearly 15 1/12 Paris feet, or more accurately 15 feet, 1 inch, 1 line 4/9. Wherefore, since that force, in approaching the earth, increases in the reciprocal−duplicate proportion of the distance, and upon that account, at the surface of the earth, is 60 x 60 times greater than at the moon, a body in our regions, falling with that force, ought in the space of one minute of time to describe 60 x 60 x 15 1/12 Paris feet; and in the space of one second of time, to describe 15 1/12 of those feet, or more accurately, 15 feet, 1 inch, 1 line 4/9. And with this very force we actually find that bodies here upon earth do really descend; for a pendulum oscillating seconds in the latitude of Paris will be 3 Paris feet, and 8 lines 1/2 in length, as Mr. Huygens has observed. And the space which a heavy body describes by falling in one second of time is to half the length of the pendulum in the duplicate ratio of the circumference of a circle to its diameter (as Mr. Huygens has also shown), and is therefore 15 Paris feet, 1 inch, 1 line 4/9. And therefore the force by which the moon is retained in its orbit is that very same force which we commonly call gravity; for, were gravity another force different from that, then bodies descending to the earth with the joint impulse of both forces would fall with a double velocity, and in the space of one second of time would describe 30 1/6 Paris feet; altogether against experience."[1]

All this is beautifully clear, and its validity has never in recent generations been called in question; yet it should be explained that the argument does not amount to an actually indisputable demonstration. It is at least possible that the coincidence between the observed and computed motion of the moon may be a mere coincidence and nothing more. This probability, however, is so remote that Newton is fully justified in disregarding it, and, as has been said, all subsequent generations have accepted the computation as demonstrative.

Let us produce now Newton's further computations as to the other planetary bodies, passing on to his final conclusion that gravity is a universal force.

"PROPOSITION V., THEOREM V.

"That the circumjovial planets gravitate towards Jupiter; the circumsaturnal towards Saturn; the circumsolar towards the sun; and by the forces of their gravity are drawn off from rectilinear motions, and retained in curvilinear orbits.

"For the revolutions of the circumjovial planets about Jupiter, of the circumsaturnal about Saturn, and of Mercury and Venus and the other circumsolar planets about the sun, are appearances of the same sort with the revolution of the moon about the earth; and therefore, by Rule ii., must be owing to the same sort of causes; especially since it has been demonstrated that the forces upon which those revolutions depend tend to the centres of Jupiter, of Saturn, and of the sun; and that those forces, in receding from Jupiter, from Saturn, and from the sun, decrease in the same proportion, and according to the same law, as the force of gravity does in receding from the earth.

"COR. 1.—There is, therefore, a power of gravity tending to all the planets; for doubtless Venus, Mercury, and the rest are bodies of the same sort with Jupiter and Saturn. And since all attraction (by Law iii.) is mutual, Jupiter will therefore gravitate towards all his own satellites, Saturn towards his, the earth towards the moon, and the sun towards all the primary planets.

"COR. 2.—The force of gravity which tends to any one planet is reciprocally as the square of the distance of places from the planet's centre.

"COR. 3.—All the planets do mutually gravitate towards one another, by Cor. 1 and 2, and hence it is that Jupiter and Saturn, when near their conjunction, by their mutual attractions sensibly disturb each other's motions. So the sun disturbs the motions of the moon; and both sun and moon disturb our sea, as we shall hereafter explain.

"SCHOLIUM

"The force which retains the celestial bodies in their orbits has been hitherto called centripetal force; but it being now made plain that it can be no other than a gravitating force, we shall hereafter call it gravity. For the cause of the centripetal force which retains the moon in its orbit will extend itself to all the planets by Rules i., ii., and iii.

"PROPOSITION VI., THEOREM VI.

"That all bodies gravitate towards every planet; and that the weights of the bodies towards any the same planet, at equal distances from the centre of the planet, are proportional to the quantities of matter which they severally contain.

"It has been now a long time observed by others that all sorts of heavy bodies (allowance being made for the inability of retardation which they suffer from a small power of resistance in the air) descend to the earth FROM EQUAL HEIGHTS in equal times; and that equality of times we may distinguish to a great accuracy by help of pendulums. I tried the thing in gold, silver, lead, glass, sand, common salt, wood, water, and wheat. I provided two wooden boxes, round and equal: I filled the one with wood, and suspended an equal weight of gold (as exactly as I could) in the centre of oscillation of the other. The boxes hanging by eleven feet, made a couple of pendulums exactly equal in weight and figure, and equally receiving the resistance of the air. And, placing the one by the other, I observed them to play together forward and backward, for a long time, with equal vibrations. And therefore the quantity of matter in gold was to the quantity of matter in the wood as the action of the motive force (or vis motrix) upon all the gold to the action of the same upon all the wood—that is, as the weight of the one to the weight of the other: and the like happened in the other bodies. By these experiments, in bodies of the same weight, I could manifestly have discovered a difference of matter less than the thousandth part of the whole, had any such been. But, without all doubt, the nature of gravity towards the planets is the same as towards the earth. For, should we imagine our terrestrial bodies removed to the orb of the moon, and there, together with the moon, deprived of all motion, to be let go, so as to fall together towards the earth, it is certain, from what we have demonstrated before, that, in equal times, they would describe equal spaces with the moon, and of consequence are to the moon, in quantity and matter, as their weights to its weight.

"Moreover, since the satellites of Jupiter perform their revolutions in times which observe the sesquiplicate proportion of their distances from Jupiter's centre, their accelerative gravities towards Jupiter will be reciprocally as the square of their distances from Jupiter's centre—that is, equal, at equal distances. And, therefore, these satellites, if supposed to fall TOWARDS JUPITER from equal heights, would describe equal spaces in equal times, in like manner as heavy bodies do on our earth. And, by the same argument, if the circumsolar planets were supposed to be let fall at equal distances from the sun, they would, in their descent towards the sun, describe equal spaces in equal times. But forces which equally accelerate unequal bodies must be as those bodies—that is to say, the weights of the planets (TOWARDS THE SUN must be as their quantities of matter. Further, that the weights of Jupiter and his satellites towards the sun are proportional to the several quantities of their matter, appears from the exceedingly regular motions of the satellites. For if some of these bodies were more strongly attracted to the sun in proportion to their quantity of matter than others, the motions of the satellites would be disturbed by that inequality of attraction. If at equal distances from the sun any satellite, in proportion to the quantity of its matter, did gravitate towards the sun with a force greater than Jupiter in proportion to his, according to any given proportion, suppose d to e; then the distance between the centres of the sun and of the satellite's orbit would be always greater than the distance between the centres of the sun and of Jupiter nearly in the subduplicate of that proportion: as by some computations I have found. And if the satellite did gravitate towards the sun with a force, lesser in the proportion of e to d, the distance of the centre of the satellite's orb from the sun would be less than the distance of the centre of Jupiter from the sun in the subduplicate of the same proportion. Therefore, if at equal distances from the sun, the accelerative gravity of any satellite towards the sun were greater or less than the accelerative gravity of Jupiter towards the sun by one−one−thousandth part of the whole gravity, the distance of the centre of the satellite's orbit from the sun would be greater or less than the distance of Jupiter from the sun by one one−two−thousandth part of the whole distance—that is, by a fifth part of the distance of the utmost satellite from the centre of Jupiter; an eccentricity of the orbit which would be very sensible. But the orbits of the satellites are concentric to Jupiter, and therefore the accelerative gravities of Jupiter and of all its satellites towards the sun, at equal distances from the sun, are as their several quantities of matter; and the weights of the moon and of the earth towards the sun are either none, or accurately proportional to the masses of matter which they contain.

"COR. 5.—The power of gravity is of a different nature from the power of magnetism; for the magnetic attraction is not as the matter attracted. Some bodies are attracted more by the magnet; others less; most bodies not at all. The power of magnetism in one and the same body may be increased and diminished; and is sometimes far stronger, for the quantity of matter, than the power of gravity; and in receding from the magnet decreases not in the duplicate, but almost in the triplicate proportion of the distance, as nearly as I could judge from some rude observations.

"PROPOSITION VII., THEOREM VII.

"That there is a power of gravity tending to all bodies, proportional to the several quantities of matter which they contain.

That all the planets mutually gravitate one towards another we have proved before; as well as that the force of gravity towards every one of them considered apart, is reciprocally as the square of the distance of places from the centre of the planet. And thence it follows, that the gravity tending towards all the planets is proportional to the matter which they contain.

"Moreover, since all the parts of any planet A gravitates towards any other planet B; and the gravity of every part is to the gravity of the whole as the matter of the part is to the matter of the whole; and to every action corresponds a reaction; therefore the planet B will, on the other hand, gravitate towards all the parts of planet A, and its gravity towards any one part will be to the gravity towards the whole as the matter of the part to the matter of the whole. Q.E.D.

"HENCE IT WOULD APPEAR THAT the force of the whole must arise from the force of the component parts."

Newton closes this remarkable Book iii. with the following words:

"Hitherto we have explained the phenomena of the heavens and of our sea by the power of gravity, but have not yet assigned the cause of this power. This is certain, that it must proceed from a cause that penetrates to the very centre of the sun and planets, without suffering the least diminution of its force; that operates not according to the quantity of the surfaces of the particles upon which it acts (as mechanical causes used to do), but according to the quantity of solid matter which they contain, and propagates its virtue on all sides to immense distances, decreasing always in the duplicate proportions of the distances. Gravitation towards the sun is made up out of the gravitations towards the several particles of which the body of the sun is composed; and in receding from the sun decreases accurately in the duplicate proportion of the distances as far as the orb of Saturn, as evidently appears from the quiescence of the aphelions of the planets; nay, and even to the remotest aphelions of the comets, if those aphelions are also quiescent. But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypothesis; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. . . . And 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."[2]

The very magnitude of the importance of the theory of universal gravitation made its general acceptance a matter of considerable time after the actual discovery. This opposition had of course been foreseen by Newton, and, much as be dreaded controversy, he was prepared to face it and combat it to the bitter end. He knew that his theory was right; it remained for him to convince the world of its truth. He knew that some of his contemporary philosophers would accept it at once; others would at first doubt, question, and dispute, but finally accept; while still others would doubt and dispute until the end of their days. This had been the history of other great discoveries; and this will probably be the history of most great discoveries for all time. But in this case the discoverer lived to see his theory accepted by practically all the great minds of his time.

Delambre is authority for the following estimate of Newton by Lagrange. "The celebrated Lagrange," he says, "who frequently asserted that Newton was the greatest genius that ever existed, used to add—'and the most fortunate, for we cannot find MORE THAN ONCE a system of the world to establish.' " With pardonable exaggeration the admiring followers of the great generalizer pronounced this epitaph:

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"Nature and Nature's laws lay hid in night;
 God said `Let Newton be!' and all was light."
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XIII. INSTRUMENTS OF PRECISION IN THE AGE OF NEWTON

During the Newtonian epoch there were numerous important inventions of scientific instruments, as well as many improvements made upon the older ones. Some of these discoveries have been referred to briefly in other places, but their importance in promoting scientific investigation warrants a fuller treatment of some of the more significant.

Many of the errors that had arisen in various scientific calculations before the seventeenth century may be ascribed to the crudeness and inaccuracy in the construction of most scientific instruments. Scientists had not as yet learned that an approach to absolute accuracy was necessary in every investigation in the field of science, and that such accuracy must be extended to the construction of the instruments used in these investigations and observations. In astronomy it is obvious that instruments of delicate exactness are most essential; yet Tycho Brahe, who lived in the sixteenth century, is credited with being the first astronomer whose instruments show extreme care in construction.

It seems practically settled that the first telescope was invented in Holland in 1608; but three men, Hans Lippershey, James Metius, and Zacharias Jansen, have been given the credit of the invention at different times. It would seem from certain papers, now in the library of the University of Leyden, and included in Huygens's papers, that Lippershey was probably the first to invent a telescope and to describe his invention. The story is told that Lippershey, who was a spectacle−maker, stumbled by accident upon the discovery that when two lenses are held at a certain distance apart, objects at a distance appear nearer and larger. Having made this discovery, be fitted two lenses with a tube so as to maintain them at the proper distance, and thus constructed the first telescope.

It was Galileo, however, as referred to in a preceding chapter, who first constructed a telescope based on his knowledge of the laws of refraction. In 1609, having heard that an instrument had been invented, consisting of two lenses fixed in a tube, whereby objects were made to appear larger and nearer, he set about constructing such an instrument that should follow out the known effects of refraction. His first telescope, made of two lenses fixed in a lead pipe, was soon followed by others of improved types, Galileo devoting much time and labor to perfecting lenses and correcting errors. In fact, his work in developing the instrument was so important that the telescope came gradually to be known as the "Galilean telescope."

In the construction of his telescope Galileo made use of a convex and a concave lens; but shortly after this Kepler invented an instrument in which both the lenses used were convex. This telescope gave a much larger field of view than the Galilean telescope, but did not give as clear an image, and in consequence did not come into general use until the middle of the seventeenth century. The first powerful telescope of this type was made by Huygens and his brother. It was of twelve feet focal length, and enabled Huygens to discover a new satellite of Saturn, and to determine also the true explanation of Saturn's ring.

It was Huygens, together with Malvasia and Auzout, who first applied the micrometer to the telescope, although the inventor of the first micrometer was William Gascoigne, of Yorkshire, about 1636. The micrometer as used in telescopes enables the observer to measure accurately small angular distances. Before the invention of the telescope such measurements were limited to the angle that could be distinguished by the naked eye, and were, of course, only approximately accurate. Even very careful observers, such as Tycho Brahe, were able to obtain only fairly accurate results. But by applying Gascoigne's invention to the telescope almost absolute accuracy became at once possible. The principle of Gascoigne's micrometer was that of two pointers lying parallel, and in this position pointing to zero. These were arranged so that the turning of a single screw separated or approximated them at will, and the angle thus formed could be determined with absolute accuracy.

Huygens's micrometer was a slip of metal of variable breadth inserted at the focus of the telescope. By observing at what point this exactly covered an object under examination, and knowing the focal length of the telescope and the width of the metal, he could then deduce the apparent angular breadth of the object. Huygens discovered also that an object placed in the common focus of the two lenses of a Kepler telescope appears distinct and clearly defined. The micrometers of Malvasia, and later of Auzout and Picard, are the development of this discovery. Malvasia's micrometer, which he described in 1662, consisted of fine silver wires placed at right−angles at the focus of his telescope.

As telescopes increased in power, however, it was found that even the finest wire, or silk filaments, were much too thick for astronomical observations, as they obliterated the image, and so, finally, the spider−web came into use and is still used in micrometers and other similar instruments. Before that time, however, the fine crossed wires had revolutionized astronomical observations. "We may judge how great was the improvement which these contrivances introduced into the art of observing," says Whewell, "by finding that Hevelius refused to adopt them because they would make all the old observations of no value. He had spent a laborious and active life in the exercise of the old methods, and could not bear to think that all the treasures which he had accumulated had lost their worth by the discovery of a new mine of richer ones."[1]

Until the time of Newton, all the telescopes in use were either of the Galilean or Keplerian type, that is, refractors. But about the year 1670 Newton constructed his first reflecting telescope, which was greatly superior to, although much smaller than, the telescopes then in use. He was led to this invention by his experiments with light and colors. In 1671 he presented to the Royal Society a second and somewhat larger telescope, which he had made; and this type of instrument was little improved upon until the introduction of the achromatic telescope, invented by Chester Moor Hall in 1733.

As is generally known, the element of accurate measurements of time plays an important part in the measurements of the movements of the heavenly bodies. In fact, one was scarcely possible without the other, and as it happened it was the same man, Huygens, who perfected Kepler's telescope and invented the pendulum clock. The general idea had been suggested by Galileo; or, better perhaps, the equal time occupied by the successive oscillations of the pendulum had been noted by him. He had not been able, however, to put this discovery to practical account. But in 1656 Huygens invented the necessary machinery for maintaining the motion of the pendulum and perfected several accurate clocks. These clocks were of invaluable assistance to the astronomers, affording as they did a means of keeping time "more accurate than the sun itself." When Picard had corrected the variation caused by heat and cold acting upon the pendulum rod by combining metals of different degrees of expansibility, a high degree of accuracy was possible.

But while the pendulum clock was an unequalled stationary time−piece, it was useless in such unstable situations as, for example, on shipboard. But here again Huygens played a prominent part by first applying the coiled balance−spring for regulating watches and marine clocks. The idea of applying a spring to the balance−wheel was not original with Huygens, however, as it had been first conceived by Robert Hooke; but Huygens's application made practical Hooke's idea. In England the importance of securing accurate watches or marine clocks was so fully appreciated that a reward of L20,000 sterling was offered by Parliament as a stimulus to the inventor of such a time−piece. The immediate incentive for this offer was the obvious fact that with such an instrument the determination of the longitude of places would be much simplified. Encouraged by these offers, a certain carpenter named Harrison turned his attention to the subject of watch−making, and, after many years of labor, in 1758 produced a spring time−keeper which, during a sea−voyage occupying one hundred and sixty−one days, varied only one minute and five seconds. This gained for Harrison a reward Of L5000 sterling at once, and a little later L10,000 more, from Parliament.

While inventors were busy with the problem of accurate chronometers, however, another instrument for taking longitude at sea had been invented. This was the reflecting quadrant, or sextant, as the improved instrument is now called, invented by John Hadley in 1731, and independently by Thomas Godfrey, a poor glazier of Philadelphia, in 1730. Godfrey's invention, which was constructed on the same principle as that of the Hadley instrument, was not generally recognized until two years after Hadley's discovery, although the instrument was finished and actually in use on a sea−voyage some months before Hadley reported his invention. The principle of the sextant, however, seems to have been known to Newton, who constructed an instrument not very unlike that of Hadley; but this invention was lost sight of until several years after the philosopher's death and some time after Hadley's invention.

The introduction of the sextant greatly simplified taking reckonings at sea as well as facilitating taking the correct longitude of distant places. Before that time the mariner was obliged to depend upon his compass, a cross−staff, or an astrolabe, a table of the sun's declination and a correction for the altitude of the polestar, and very inadequate and incorrect charts. Such were the instruments used by Columbus and Vasco da Gama and their immediate successors.

During the Newtonian period the microscopes generally in use were those constructed of simple lenses, for although compound microscopes were known, the difficulties of correcting aberration had not been surmounted, and a much clearer field was given by the simple instrument. The results obtained by the use of such instruments, however, were very satisfactory in many ways. By referring to certain plates in this volume, which reproduce illustrations from Robert Hooke's work on the microscope, it will be seen that quite a high degree of effectiveness had been attained. And it should be recalled that Antony von Leeuwenboek, whose death took place shortly before Newton's, had discovered such micro−organisms as bacteria, had seen the blood corpuscles in circulation, and examined and described other microscopic structures of the body.

XIV. PROGRESS IN ELECTRICITY FROM GILBERT AND VON GUERICKE TO FRANKLIN

We have seen how Gilbert, by his experiments with magnets, gave an impetus to the study of magnetism and electricity. Gilbert himself demonstrated some facts and advanced some theories, but the system of general laws was to come later. To this end the discovery of electrical repulsion, as well as attraction, by Von Guericke, with his sulphur ball, was a step forward; but something like a century passed after Gilbert's beginning before anything of much importance was done in the field of electricity.

In 1705, however, Francis Hauksbee began a series of experiments that resulted in some startling demonstrations. For many years it had been observed that a peculiar light was seen sometimes in the mercurial barometer, but Hauksbee and the other scientific investigators supposed the radiance to be due to the mercury in a vacuum, brought about, perhaps, by some agitation. That this light might have any connection with electricity did not, at first, occur to Hauksbee any more than it had to his predecessors. The problem that interested him was whether the vacuum in the tube of the barometer was essential to the light; and in experimenting to determine this, he invented his "mercurial fountain." Having exhausted the air in a receiver containing some mercury, he found that by allowing air to rush through the mercury the metal became a jet thrown in all directions against the sides of the vessel, making a great, flaming shower, "like flashes of lightning," as he said. But it seemed to him that there was a difference between this light and the glow noted in the barometer. This was a bright light, whereas the barometer light was only a glow. Pondering over this, Hauksbee tried various experiments, revolving pieces of amber, flint, steel, and other substances in his exhausted air−pump receiver, with negative, or unsatisfactory, results. Finally, it occurred to him to revolve an exhausted glass tube itself. Mounting such a globe of glass on an axis so that it could be revolved rapidly by a belt running on a large wheel, he found that by holding his fingers against the whirling globe a purplish glow appeared, giving sufficient light so that coarse print could be read, and the walls of a dark room sensibly lightened several feet away. As air was admitted to the globe the light gradually diminished, and it seemed to him that this diminished glow was very similar in appearance to the pale light seen in the mercurial barometer. Could it be that it was the glass, and not the mercury, that caused it? Going to a barometer he proceeded to rub the glass above the column of mercury over the vacuum, without disturbing the mercury, when, to his astonishment, the same faint light, to all appearances identical with the glow seen in the whirling globe, was produced.

Turning these demonstrations over in his mind, he recalled the well−known fact that rubbed glass attracted bits of paper, leaf−brass, and other light substances, and that this phenomenon was supposed to be electrical. This led him finally to determine the hitherto unsuspected fact, that the glow in the barometer was electrical as was also the glow seen in his whirling globe. Continuing his investigations, he soon discovered that solid glass rods when rubbed produced the same effects as the tube. By mere chance, happening to hold a rubbed tube to his cheek, he felt the effect of electricity upon the skin like "a number of fine, limber hairs," and this suggested to him that, since the mysterious manifestation was so plain, it could be made to show its effects upon various substances. Suspending some woollen threads over the whirling glass cylinder, he found that as soon as he touched the glass with his hands the threads, which were waved about by the wind of the revolution, suddenly straightened themselves in a peculiar manner, and stood in a radical position, pointing to the axis of the cylinder.

Encouraged by these successes, he continued his experiments with breathless expectancy, and soon made another important discovery, that of "induction," although the real significance of this discovery was not appreciated by him or, for that matter, by any one else for several generations following. This discovery was made by placing two revolving cylinders within an inch of each other, one with the air exhausted and the other unexhausted. Placing his hand on the unexhausted tube caused the light to appear not only upon it, but on the other tube as well. A little later he discovered that it is not necessary to whirl the exhausted tube to produce this effect, but simply to place it in close proximity to the other whirling cylinder.

These demonstrations of Hauksbee attracted wide attention and gave an impetus to investigators in the field of electricity; but still no great advance was made for something like a quarter of a century. Possibly the energies of the scientists were exhausted for the moment in exploring the new fields thrown open to investigation by the colossal work of Newton.

THE EXPERIMENTS OF STEPHEN GRAY

In 1729 Stephen Gray (died in 1736), an eccentric and irascible old pensioner of the Charter House in London, undertook some investigations along lines similar to those of Hauksbee. While experimenting with a glass tube for producing electricity, as Hauksbee had done, he noticed that the corks with which he had stopped the ends of the tube to exclude the dust, seemed to attract bits of paper and leaf−brass as well as the glass itself. He surmised at once that this mysterious electricity, or "virtue," as it was called, might be transmitted through other substances as it seemed to be through glass.

"Having by me an ivory ball of about one and three−tenths of an inch in diameter," he writes, "with a hole through it, this I fixed upon a fir−stick about four inches long, thrusting the other end into the cork, and upon rubbing the tube found that the ball attracted and repelled the feather with more vigor than the cork had done, repeating its attractions and repulsions for many times together. I then fixed the ball on longer sticks, first upon one of eight inches, and afterwards upon one of twenty−four inches long, and found the effect the same. Then I made use of iron, and then brass wire, to fix the ball on, inserting the other end of the wire in the cork, as before, and found that the attraction was the same as when the fir−sticks were made use of, and that when the feather was held over against any part of the wire it was attracted by it; but though it was then nearer the tube, yet its attraction was not so strong as that of the ball. When the wire of two or three feet long was used, its vibrations, caused by the rubbing of the tube, made it somewhat troublesome to be managed. This put me to thinking whether, if the ball was hung by a pack−thread and suspended by a loop on the tube, the electricity would not be carried down the line to the ball; I found it to succeed accordingly; for upon suspending the ball on the tube by a pack−thread about three feet long, when the tube had been excited by rubbing, the ivory ball attracted and repelled the leaf−brass over which it was held as freely as it had done when it was suspended on sticks or wire, as did also a ball of cork, and another of lead that weighed one pound and a quarter."

Gray next attempted to determine what other bodies would attract the bits of paper, and for this purpose he tried coins, pieces of metal, and even a tea−kettle, "both empty and filled with hot or cold water"; but he found that the attractive power appeared to be the same regardless of the substance used.

"I next proceeded," he continues, "to try at what greater distances the electric virtues might be carried, and having by me a hollow walking−cane, which I suppose was part of a fishing−rod, two feet seven inches long, I cut the great end of it to fit into the bore of the tube, into which it went about five inches; then when the cane was put into the end of the tube, and this excited, the cane drew the leaf−brass to the height of more than two inches, as did also the ivory ball, when by a cork and stick it had been fixed to the end of the cane.... With several pieces of Spanish cane and fir−sticks I afterwards made a rod, which, together with the tube, was somewhat more than eighteen feet long, which was the greatest length I could conveniently use in my chamber, and found the attraction very nearly, if not altogether, as strong as when the ball was placed on the shorter rods."

This experiment exhausted the capacity of his small room, but on going to the country a little later he was able to continue his experiments. "To a pole of eighteen feet there was tied a line of thirty−four feet in length, so that the pole and line together were fifty−two feet. With the pole and tube I stood in the balcony, the assistant below in the court, where he held the board with the leaf−brass on it. Then the tube being excited, as usual, the electric virtue passed from the tube up the pole and down the line to the ivory ball, which attracted the leaf−brass, and as the ball passed over it in its vibrations the leaf−brass would follow it till it was carried off the board."

Gray next attempted to send the electricity over a line suspended horizontally. To do this he suspended the pack−thread by pieces of string looped over nails driven into beams for that purpose. But when thus suspended he found that the ivory ball no longer excited the leaf−brass, and he guessed correctly that the explanation of this lay in the fact that "when the electric virtue came to the loop that was suspended on the beam it went up the same to the beam," none of it reaching the ball. As we shall see from what follows, however, Gray had not as yet determined that certain substances will conduct electricity while others will not. But by a lucky accident he made the discovery that silk, for example, was a poor conductor, and could be turned to account in insulating the conducting−cord.

A certain Mr. Wheler had become much interested in the old pensioner and his work, and, as a guest at the Wheler house, Gray had been repeating some of his former experiments with the fishing−rod, line, and ivory ball. He had finally exhausted the heights from which these experiments could be made by climbing to the clock−tower and exciting bits of leaf−brass on the ground below.

"As we had no greater heights here," he says, "Mr. Wheler was desirous to try whether we could not carry the electric virtue horizontally. I then told him of the attempt I had made with that design, but without success, telling him the method and materials made use of, as mentioned above. He then proposed a silk line to support the line by which the electric virtue was to pass. I told him it might do better upon account of its smallness; so that there would be less virtue carried from the line of communication.

"The first experiment was made in the matted gallery, July 2, 1729, about ten in the morning. About four feet from the end of the gallery there was a cross line that was fixed by its ends to each side of the gallery by two nails; the middle part of the line was silk, the rest at each end pack−thread; then the line to which the ivory ball was hung and by which the electric virtue was to be conveyed to it from the tube, being eighty and one−half feet in length, was laid on the cross silk line, so that the ball hung about nine feet below it. Then the other end of the line was by a loop suspended on the glass cane, and the leaf−brass held under the ball on a piece of white paper; when, the tube being rubbed, the ball attracted the leaf−brass, and kept it suspended on it for some time."

This experiment succeeded so well that the string was lengthened until it was some two hundred and ninety−three feet long; and still the attractive force continued, apparently as strong as ever. On lengthening the string still more, however, the extra weight proved too much for the strength of the silk suspending−thread. "Upon this," says Gray, "having brought with me both brass and iron wire, instead of the silk we put up small iron wire; but this was too weak to bear the weight of the line. We then took brass wire of a somewhat larger size than that of iron. This supported our line of communication; but though the tube was well rubbed, yet there was not the least motion or attraction given by the ball, neither with the great tube, which we made use of when we found the small solid cane to be ineffectual; by which we were now convinced that the success we had before depended upon the lines that supported the line of communication being silk, and not upon their being small, as before trial I had imagined it might be; the same effect happening here as it did when the line that is to convey the electric virtue is supported by pack−thread."

Soon after this Gray and his host suspended a pack−thread six hundred and sixty−six feet long on poles across a field, these poles being slightly inclined so that the thread could be suspended from the top by small silk cords, thus securing the necessary insulation. This pack−thread line, suspended upon poles along which Gray was able to transmit the electricity, is very suggestive of the modern telegraph, but the idea of signalling or making use of it for communicating in any way seems not to have occurred to any one at that time. Even the successors of Gray who constructed lines some thousands of feet long made no attempt to use them for anything but experimental purposes—simply to test the distances that the current could be sent. Nevertheless, Gray should probably be credited with the discovery of two of the most important properties of electricity—that it can be conducted and insulated, although, as we have seen, Gilbert and Von Guericke had an inkling of both these properties.

EXPERIMENTS OF CISTERNAY DUFAY

So far England had produced the two foremost workers in electricity. It was now France's turn to take a hand, and, through the efforts of Charles Francois de Cisternay Dufay, to advance the science of electricity very materially. Dufay was a highly educated savant, who had been soldier and diplomat betimes, but whose versatility and ability as a scientist is shown by the fact that he was the only man who had ever contributed to the annals of the academy investigations in every one of the six subjects admitted by that institution as worthy of recognition. Dufay upheld his reputation in this new field of science, making many discoveries and correcting many mistakes of former observers. In this work also he proved himself a great diplomat by remaining on terms of intimate friendship with Dr. Gray—a thing that few people were able to do.

Almost his first step was to overthrow the belief that certain bodies are "electrics" and others "non−electrics"—that is, that some substances when rubbed show certain peculiarities in attracting pieces of paper and foil which others do not. Dufay proved that all bodies possess this quality in a certain degree.

"I have found that all bodies (metallic, soft, or fluid ones excepted)," he says, "may be made electric by first heating them more or less and then rubbing them on any sort of cloth. So that all kinds of stones, as well precious as common, all kinds of wood, and, in general, everything that I have made trial of, became electric by beating and rubbing, except such bodies as grow soft by beat, as the gums, which dissolve in water, glue, and such like substances. 'Tis also to be remarked that the hardest stones or marbles require more chafing or heating than others, and that the same rule obtains with regard to the woods; so that box, lignum vitae, and such others must be chafed almost to the degree of browning, whereas fir, lime−tree, and cork require but a moderate heat.

"Having read in one of Mr. Gray's letters that water may be made electrical by holding the excited glass tube near it (a dish of water being fixed to a stand and that set on a plate of glass, or on the brim of a drinking−glass, previously chafed, or otherwise warmed), I have found, upon trial, that the same thing happened to all bodies without exception, whether solid or fluid, and that for that purpose 'twas sufficient to set them on a glass stand slightly warmed, or only dried, and then by bringing the tube near them they immediately became electrical. I made this experiment with ice, with a lighted wood−coal, and with everything that came into my mind; and I constantly remarked that such bodies of themselves as were least electrical had the greatest degree of electricity communicated to them at the approval of the glass tube."

His next important discovery was that colors had nothing to do with the conduction of electricity. "Mr. Gray says, towards the end of one of his letters," he writes, "that bodies attract more or less according to their colors. This led me to make several very singular experiments. I took nine silk ribbons of equal size, one white, one black, and the other seven of the seven primitive colors, and having hung them all in order in the same line, and then bringing the tube near them, the black one was first attracted, the white one next, and others in order successively to the red one, which was attracted least, and the last of them all. I afterwards cut out nine square pieces of gauze of the same colors with the ribbons, and having put them one after another on a hoop of wood, with leaf−gold under them, the leaf−gold was attracted through all the colored pieces of gauze, but not through the white or black. This inclined me first to think that colors contribute much to electricity, but three experiments convinced me to the contrary. The first, that by warming the pieces of gauze neither the black nor white pieces obstructed the action of the electrical tube more than those of the other colors. In like manner, the ribbons being warmed, the black and white are not more strongly attracted than the rest. The second is, the gauzes and ribbons being wetted, the ribbons are all attracted equally, and all the pieces of gauze equally intercept the action of electric bodies. The third is, that the colors of a prism being thrown on a white gauze, there appear no differences of attraction. Whence it proceeds that this difference proceeds, not from the color, as a color, but from the substances that are employed in the dyeing. For when I colored ribbons by rubbing them with charcoal, carmine, and such other substances, the differences no longer proved the same."

In connection with his experiments with his thread suspended on glass poles, Dufay noted that a certain amount of the current is lost, being given off to the surrounding air. He recommended, therefore, that the cords experimented with be wrapped with some non−conductor—that it should be "insulated" ("isolee"), as he said, first making use of this term.

DUFAY DISCOVERS VITREOUS AND RESINOUS ELECTRICITY

It has been shown in an earlier chapter how Von Guericke discovered that light substances like feathers, after being attracted to the sulphur−ball electric−machine, were repelled by it until they touched some object. Von Guericke noted this, but failed to explain it satisfactorily. Dufay, repeating Von Guericke's experiments, found that if, while the excited tube or sulphur ball is driving the repelled feather before it, the ball be touched or rubbed anew, the feather comes to it again, and is repelled alternately, as, the hand touches the ball, or is withdrawn. From this he concluded that electrified bodies first attract bodies not electrified, "charge" them with electricity, and then repel them, the body so charged not being attracted again until it has discharged its electricity by touching something.

"On making the experiment related by Otto von Guericke," he says, "which consists in making a ball of sulphur rendered electrical to repel a down feather, I perceived that the same effects were produced not only by the tube, but by all electric bodies whatsoever, and I discovered that which accounts for a great part of the irregularities and, if I may use the term, of the caprices that seem to accompany most of the experiments on electricity. This principle is that electric bodies attract all that are not so, and repel them as soon as they are become electric by the vicinity or contact of the electric body. Thus gold−leaf is first attracted by the tube, and acquires an electricity by approaching it, and of consequence is immediately repelled by it. Nor is it reattracted while it retains its electric quality. But if while it is thus sustained in the air it chance to light on some other body, it straightway loses its electricity, and in consequence is reattracted by the tube, which, after having given it a new electricity, repels it a second time, which continues as long as the tube keeps its electricity. Upon applying this principle to the various experiments of electricity, one will be surprised at the number of obscure and puzzling facts that it clears up. For Mr. Hauksbee's famous experiment of the glass globe, in which silk threads are put, is a necessary consequence of it. When these threads are arranged in the form of rays by the electricity of the sides of the globe, if the finger be put near the outside of the globe the silk threads within fly from it, as is well known, which happens only because the finger or any other body applied near the glass globe is thereby rendered electrical, and consequently repels the silk threads which are endowed with the same quality. With a little reflection we may in the same manner account for most of the other phenomena, and which seem inexplicable without attending to this principle.

"Chance has thrown in my way another principle, more universal and remarkable than the preceding one, and which throws a new light on the subject of electricity. This principle is that there are two distinct electricities, very different from each other, one of which I call vitreous electricity and the other resinous electricity. The first is that of glass, rock−crystal, precious stones, hair of animals, wool, and many other bodies. The second is that of amber, copal, gumsack, silk thread, paper, and a number of other substances. The characteristic of these two electricities is that a body of the vitreous electricity, for example, repels all such as are of the same electricity, and on the contrary attracts all those of the resinous electricity; so that the tube, made electrical, will repel glass, crystal, hair of animals, etc., when rendered electric, and will attract silk thread, paper, etc., though rendered electrical likewise. Amber, on the contrary, will attract electric glass and other substances of the same class, and will repel gum−sack, copal, silk thread, etc. Two silk ribbons rendered electrical will repel each other; two woollen threads will do the like; but a woollen thread and a silken thread will mutually attract each other. This principle very naturally explains why the ends of threads of silk or wool recede from each other, in the form of pencil or broom, when they have acquired an electric quality. From this principle one may with the same ease deduce the explanation of a great number of other phenomena; and it is probable that this truth will lead us to the further discovery of many other things.

"In order to know immediately to which of the two classes of electrics belongs any body whatsoever, one need only render electric a silk thread, which is known to be of the resinuous electricity, and see whether that body, rendered electrical, attracts or repels it. If it attracts it, it is certainly of the kind of electricity which I call VITREOUS; if, on the contrary, it repels it, it is of the same kind of electricity with the silk—that is, of the RESINOUS. I have likewise observed that communicated electricity retains the same properties; for if a ball of ivory or wood be set on a glass stand, and this ball be rendered electric by the tube, it will repel such substances as the tube repels; but if it be rendered electric by applying a cylinder of gum−sack near it, it will produce quite contrary effects—namely, precisely the same as gum−sack would produce. In order to succeed in these experiments, it is requisite that the two bodies which are put near each other, to find out the nature of their electricity, be rendered as electrical as possible, for if one of them was not at all or but weakly electrical, it would be attracted by the other, though it be of that sort that should naturally be repelled by it. But the experiment will always succeed perfectly well if both bodies are sufficiently electrical."[1]

As we now know, Dufay was wrong in supposing that there were two different kinds of electricity, vitreous and resinous. A little later the matter was explained by calling one "positive" electricity and the other "negative," and it was believed that certain substances produced only the one kind peculiar to that particular substance. We shall see presently, however, that some twenty years later an English scientist dispelled this illusion by producing both positive (or vitreous) and negative (or resinous) electricity on the same tube of glass at the same time.

After the death of Dufay his work was continued by his fellow−countryman Dr. Joseph Desaguliers, who was the first experimenter to electrify running water, and who was probably the first to suggest that clouds might be electrified bodies. But about, this time—that is, just before the middle of the eighteenth century—the field of greatest experimental activity was transferred to Germany, although both England and France were still active. The two German philosophers who accomplished most at this time were Christian August Hansen and George Matthias Bose, both professors in Leipsic. Both seem to have conceived the idea, simultaneously and independently, of generating electricity by revolving globes run by belt and wheel in much the same manner as the apparatus of Hauksbee.

With such machines it was possible to generate a much greater amount of electricity than Dufay had been able to do with the rubbed tube, and so equipped, the two German professors were able to generate electric sparks and jets of fire in a most startling manner. Bose in particular had a love for the spectacular, which he turned to account with his new electrical machine upon many occasions. On one of these occasions he prepared an elaborate dinner, to which a large number of distinguished guests were invited. Before the arrival of the company, however, Bose insulated the great banquet−table on cakes of pitch, and then connected it with a huge electrical machine concealed in another room. All being ready, and the guests in their places about to be seated, Bose gave a secret signal for starting this machine, when, to the astonishment of the party, flames of fire shot from flowers, dishes, and viands, giving a most startling but beautiful display.

To add still further to the astonishment of his guests, Bose then presented a beautiful young lady, to whom each of the young men of the party was introduced. In some mysterious manner she was insulated and connected with the concealed electrical machine, so that as each gallant touched her fingertips he received an electric shock that "made him reel." Not content with this, the host invited the young men to kiss the beautiful maid. But those who were bold enough to attempt it received an electric shock that nearly "knocked their teeth out," as the professor tells it.

LUDOLFF'S EXPERIMENT WITH THE ELECTRIC SPARK

But Bose was only one of several German scientists who were making elaborate experiments. While Bose was constructing and experimenting with his huge machine, another German, Christian Friedrich Ludolff, demonstrated that electric sparks are actual fire—a fact long suspected but hitherto unproved. Ludolff's discovery, as it chanced, was made in the lecture−hall of the reorganized Academy of Sciences at Berlin, before an audience of scientists and great personages, at the opening lecture in 1744.

In the course of this lecture on electricity, during which some of the well−known manifestations of electricity were being shown, it occurred to Ludolff to attempt to ignite some inflammable fluid by projecting an electric spark upon its surface with a glass rod. This idea was suggested to him while performing the familiar experiment of producing a spark on the surface of a bowl of water by touching it with a charged glass rod. He announced to his audience the experiment he was about to attempt, and having warmed a spoonful of sulphuric ether, he touched its surface with the glass rod, causing it to burst into flame. This experiment left no room for doubt that the electric spark was actual fire.

As soon as this experiment of Ludolff's was made known to Bose, he immediately claimed that he had previously made similar demonstrations on various inflammable substances, both liquid and solid; and it seems highly probable that he had done so, as he was constantly experimenting with the sparks, and must almost certainly have set certain substances ablaze by accident, if not by intent. At all events, he carried on a series of experiments along this line to good purpose, finally succeeding in exploding gun−powder, and so making the first forerunner of the electric fuses now so universally used in blasting, firing cannon, and other similar purposes. It was Bose also who, observing some of the peculiar manifestations in electrified tubes, and noticing their resemblance to "northern lights," was one of the first, if not the first, to suggest that the aurora borealis is of electric origin.

These spectacular demonstrations had the effect of calling public attention to the fact that electricity is a most wonderful and mysterious thing, to say the least, and kept both scientists and laymen agog with expectancy. Bose himself was aflame with excitement, and so determined in his efforts to produce still stronger electric currents, that he sacrificed the tube of his twenty−foot telescope for the construction of a mammoth electrical machine. With this great machine a discharge of electricity was generated powerful enough to wound the skin when it happened to strike it.

Until this time electricity had been little more than a plaything of the scientists—or, at least, no practical use had been made of it. As it was a practising physician, Gilbert, who first laid the foundation for experimenting with the new substance, so again it was a medical man who first attempted to put it to practical use, and that in the field of his profession. Gottlieb Kruger, a professor of medicine at Halle in 1743, suggested that electricity might be of use in some branches of medicine; and the year following Christian Gottlieb Kratzenstein made a first experiment to determine the effects of electricity upon the body. He found that "the action of the heart was accelerated, the circulation increased, and that muscles were made to contract by the discharge": and he began at once administering electricity in the treatment of certain diseases. He found that it acted beneficially in rheumatic affections, and that it was particularly useful in certain nervous diseases, such as palsies. This was over a century ago, and to−day about the most important use made of the particular kind of electricity with which he experimented (the static, or frictional) is for the treatment of diseases affecting the nervous system.

By the middle of the century a perfect mania for making electrical machines had spread over Europe, and the whirling, hand−rubbed globes were gradually replaced by great cylinders rubbed by woollen cloths or pads, and generating an "enormous power of electricity." These cylinders were run by belts and foot−treadles, and gave a more powerful, constant, and satisfactory current than known heretofore. While making experiments with one of these machines, Johann Heinrichs Winkler attempted to measure the speed at which electricity travels. To do this he extended a cord suspended on silk threads, with the end attached to the machine and the end which was to attract the bits of gold−leaf near enough together so that the operator could watch and measure the interval of time that elapsed between the starting of the current along the cord and its attracting the gold−leaf. The length of the cord used in this experiment was only a little over a hundred feet, and this was, of course, entirely inadequate, the current travelling that space apparently instantaneously.

The improved method of generating electricity that had come into general use made several of the scientists again turn their attention more particularly to attempt putting it to some practical account. They were stimulated to these efforts by the constant reproaches that were beginning to be heard on all sides that electricity was merely a "philosopher's plaything." One of the first to succeed in inventing something that approached a practical mechanical contrivance was Andrew Gordon, a Scotch Benedictine monk. He invented an electric bell which would ring automatically, and a little "motor," if it may be so called. And while neither of these inventions were of any practical importance in themselves, they were attempts in the right direction, and were the first ancestors of modern electric bells and motors, although the principle upon which they worked was entirely different from modern electrical machines. The motor was simply a wheel with several protruding metal points around its rim. These points were arranged to receive an electrical discharge from a frictional machine, the discharge causing the wheel to rotate. There was very little force given to this rotation, however, not enough, in fact, to make it possible to more than barely turn the wheel itself. Two more great discoveries, galvanism and electro−magnetic induction, were necessary before the practical motor became possible.

The sober Gordon had a taste for the spectacular almost equal to that of Bose. It was he who ignited a bowl of alcohol by turning a stream of electrified water upon it, thus presenting the seeming paradox of fire produced by a stream of water. Gordon also demonstrated the power of the electrical discharge by killing small birds and animals at a distance of two hundred ells, the electricity being conveyed that distance through small wires.

THE LEYDEN JAR DISCOVERED

As yet no one had discovered that electricity could be stored, or generated in any way other than by some friction device. But very soon two experimenters, Dean von Kleist, of Camin, Pomerania, and Pieter van Musschenbroek, the famous teacher of Leyden, apparently independently, made the discovery of what has been known ever since as the Leyden jar. And although Musschenbroek is sometimes credited with being the discoverer, there can be no doubt that Von Kleist's discovery antedated his by a few months at least.

Von Kleist found that by a device made of a narrow−necked bottle containing alcohol or mercury, into which an iron nail was inserted, be was able to retain the charge of electricity, after electrifying this apparatus with the frictional machine. He made also a similar device, more closely resembling the modern Leyden jar, from a thermometer tube partly filled with water and a wire tipped with a ball of lead. With these devices he found that he could retain the charge of electricity for several hours, and could produce the usual electrical manifestations, even to igniting spirits, quite as well as with the frictional machine. These experiments were first made in October, 1745, and after a month of further experimenting, Von Kleist sent the following account of them to several of the leading scientists, among others, Dr. Lieberkuhn, in Berlin, and Dr. Kruger, of Halle.

"When a nail, or a piece of thick brass wire, is put into a small apothecary's phial and electrified, remarkable effects follow; but the phial must be very dry, or warm. I commonly rub it over beforehand with a finger on which I put some pounded chalk. If a little mercury or a few drops of spirit of wine be put into it, the experiment succeeds better. As soon as this phial and nail are removed from the electrifying−glass, or the prime conductor, to which it has been exposed, is taken away, it throws out a pencil of flame so long that, with this burning machine in my hand, I have taken above sixty steps in walking about my room. When it is electrified strongly, I can take it into another room and there fire spirits of wine with it. If while it is electrifying I put my finger, or a piece of gold which I hold in my hand, to the nail, I receive a shock which stuns my arms and shoulders.

"A tin tube, or a man, placed upon electrics, is electrified much stronger by this means than in the common way. When I present this phial and nail to a tin tube, which I have, fifteen feet long, nothing but experience can make a person believe how strongly it is electrified. I am persuaded," he adds, "that in this manner Mr. Bose would not have taken a second electrical kiss. Two thin glasses have been broken by the shock of it. It appears to me very extraordinary, that when this phial and nail are in contact with either conducting or non−conducting matter, the strong shock does not follow. I have cemented it to wood, metal, glass, sealing−wax, etc., when I have electrified without any great effect. The human body, therefore, must contribute something to it. This opinion is confirmed by my observing that unless I hold the phial in my hand I cannot fire spirits of wine with it."[2]

But it seems that none of the men who saw this account were able to repeat the experiment and produce the effects claimed by Von Kleist, and probably for this reason the discovery of the obscure Pomeranian was for a time lost sight of.

Musschenbroek's discovery was made within a short time after Von Kleist's—in fact, only a matter of about two months later. But the difference in the reputations of the two discoverers insured a very different reception for their discoveries. Musschenbroek was one of the foremost teachers of Europe, and so widely known that the great universities vied with each other, and kings were bidding, for his services. Naturally, any discovery made by such a famous person would soon be heralded from one end of Europe to the other. And so when this professor of Leyden made his discovery, the apparatus came to be called the "Leyden jar," for want of a better name. There can be little doubt that Musschenbroek made his discovery entirely independently of any knowledge of Von Kleist's, or, for that matter, without ever having heard of the Pomeranian, and his actions in the matter are entirely honorable.

His discovery was the result of an accident. While experimenting to determine the strength of electricity he suspended a gun−barrel, which he charged with electricity from a revolving glass globe. From the end of the gun−barrel opposite the globe was a brass wire, which extended into a glass jar partly filled with water. Musschenbroek held in one hand this jar, while with the other he attempted to draw sparks from the barrel. Suddenly he received a shock in the hand holding the jar, that "shook him like a stroke of lightning," and for a moment made him believe that "he was done for." Continuing his experiments, nevertheless, he found that if the jar were placed on a piece of metal on the table, a shock would be received by touching this piece of metal with one hand and touching the wire with the other—that is, a path was made for the electrical discharge through the body. This was practically the same experiment as made by Von Kleist with his bottle and nail, but carried one step farther, as it showed that the "jar" need not necessarily be held in the hand, as believed by Von Kleist. Further experiments, continued by many philosophers at the time, revealed what Von Kleist had already pointed out, that the electrified jar remained charged for some time.

Soon after this Daniel Gralath, wishing to obtain stronger discharges than could be had from a single Leyden jar, conceived the idea of combining several jars, thus for the first time grouping the generators in a "battery" which produced a discharge strong enough to kill birds and small animals. He also attempted to measure the strength of the discharges, but soon gave it up in despair, and the solution of this problem was left for late nineteenth−century scientists.

The advent of the Leyden jar, which made it possible to produce strong electrical discharges from a small and comparatively simple device, was followed by more spectacular demonstrations of various kinds all over Europe. These exhibitions aroused the interest of the kings and noblemen, so that electricity no longer remained a "plaything of the philosophers" alone, but of kings as well. A favorite demonstration was that of sending the electrical discharge through long lines of soldiers linked together by pieces of wire, the discharge causing them to "spring into the air simultaneously" in a most astonishing manner. A certain monk in Paris prepared a most elaborate series of demonstrations for the amusement of the king, among other things linking together an entire regiment of nine hundred men, causing them to perform simultaneous springs and contortions in a manner most amusing to the royal guests. But not all the experiments being made were of a purely spectacular character, although most of them accomplished little except in a negative way. The famous Abbe Nollet, for example, combined useful experiments with spectacular demonstrations, thus keeping up popular interest while aiding the cause of scientific electricity.

WILLIAM WATSON

Naturally, the new discoveries made necessary a new nomenclature, new words and electrical terms being constantly employed by the various writers of that day. Among these writers was the English scientist William Watson, who was not only a most prolific writer but a tireless investigator. Many of the words coined by him are now obsolete, but one at least, "circuit," still remains in use.

In 1746, a French scientist, Louis Guillaume le Monnier, bad made a circuit including metal and water by laying a chain half−way around the edge of a pond, a man at either end holding it. One of these men dipped his free hand in the water, the other presenting a Leyden jar to a rod suspended on a cork float on the water, both men receiving a shock simultaneously. Watson, a year later, attempted the same experiment on a larger scale. He laid a wire about twelve hundred feet long across Westminster Bridge over the Thames, bringing the ends to the water's edge on the opposite banks, a man at one end holding the wire and touching the water. A second man on the opposite side held the wire and a Leyden jar; and a third touched the jar with one hand, while with the other he grasped a wire that extended into the river. In this way they not only received the shock, but fired alcohol as readily across the stream as could be done in the laboratory. In this experiment Watson discovered the superiority of wire over chain as a conductor, rightly ascribing this superiority to the continuity of the metal.

Watson continued making similar experiments over longer watercourses, some of them as long as eight thousand feet, and while engaged in making one of these he made the discovery so essential to later inventions, that the earth could be used as part of the circuit in the same manner as bodies of water. Lengthening his wires he continued his experiments until a circuit of four miles was made, and still the electricity seemed to traverse the course instantaneously, and with apparently undiminished force, if the insulation was perfect.

BENJAMIN FRANKLIN

Watson's writings now carried the field of active discovery across the Atlantic, and for the first time an American scientist appeared—a scientist who not only rivalled, but excelled, his European contemporaries. Benjamin Franklin, of Philadelphia, coming into possession of some of Watson's books, became so interested in the experiments described in them that he began at once experimenting with electricity. In Watson's book were given directions for making various experiments, and these assisted Franklin in repeating the old experiments, and eventually adding new ones. Associated with Franklin, and equally interested and enthusiastic, if not equally successful in making discoveries, were three other men, Thomas Hopkinson, Philip Sing, and Ebenezer Kinnersley. These men worked together constantly, although it appears to have been Franklin who made independently the important discoveries, and formulated the famous Franklinian theory.

Working steadily, and keeping constantly in touch with the progress of the European investigators, Franklin soon made some experiments which he thought demonstrated some hitherto unknown phases of electrical manifestation. This was the effect of pointed bodies "in DRAWING OFF and THROWING OFF the electrical fire." In his description of this phenomenon, Franklin writes:

"Place an iron shot of three or four inches diameter on the mouth of a clean, dry, glass bottle. By a fine silken thread from the ceiling right over the mouth of the bottle, suspend a small cork ball, about the bigness of a marble; the thread of such a length that the cork ball may rest against the side of the shot. Electrify the shot, and the ball will be repelled to the distance of four or five inches, more or less, according to the quantity of electricity. When in this state, if you present to the shot the point of a long, slender shaft−bodkin, at six or eight inches distance, the repellency is instantly destroyed, and the cork flies to the shot. A blunt body must be brought within an inch, and draw a spark, to produce the same effect.

"To prove that the electrical fire is DRAWN OFF by the point, if you take the blade of the bodkin out of the wooden handle and fix it in a stick of sealing−wax, and then present it at the distance aforesaid, or if you bring it very near, no such effect follows; but sliding one finger along the wax till you touch the blade, and the ball flies to the shot immediately. If you present the point in the dark you will see, sometimes at a foot distance, and more, a light gather upon it like that of a fire−fly or glow−worm; the less sharp the point, the nearer you must bring it to observe the light; and at whatever distance you see the light, you may draw off the electrical fire and destroy the repellency. If a cork ball so suspended be repelled by the tube, and a point be presented quick to it, though at a considerable distance, 'tis surprising to see how suddenly it flies back to the tube. Points of wood will do as well as those of iron, provided the wood is not dry; for perfectly dry wood will no more conduct electricity than sealing−wax.

"To show that points will THROW OFF as well as DRAW OFF the electrical fire, lay a long, sharp needle upon the shot, and you cannot electrify the shot so as to make it repel the cork ball. Or fix a needle to the end of a suspended gun−barrel or iron rod, so as to point beyond it like a little bayonet, and while it remains there, the gun−barrel or rod cannot, by applying the tube to the other end, be electrified so as to give a spark, the fire continually running out silently at the point. In the dark you may see it make the same appearance as it does in the case before mentioned."[3]

Von Guericke, Hauksbee, and Gray had noticed that pointed bodies attracted electricity in a peculiar manner, but this demonstration of the "drawing off" of "electrical fire" was original with Franklin. Original also was the theory that he now suggested, which had at least the merit of being thinkable even by non−philosophical minds. It assumes that electricity is like a fluid, that will flow along conductors and accumulate in proper receptacles, very much as ordinary fluids do. This conception is probably entirely incorrect, but nevertheless it is likely to remain a popular one, at least outside of scientific circles, or until something equally tangible is substituted.

FRANKLIN'S THEORY OF ELECTRICITY

According to Franklin's theory, electricity exists in all bodies as a "common stock," and tends to seek and remain in a state of equilibrium, just as fluids naturally tend to seek a level. But it may, nevertheless, be raised or lowered, and this equilibrium be thus disturbed. If a body has more electricity than its normal amount it is said to be POSITIVELY electrified; but if it has less, it is NEGATIVELY electrified. An over−electrified or "plus" body tends to give its surplus stock to a body containing the normal amount; while the "minus" or under−electrified body will draw electricity from one containing the normal amount.

Working along lines suggested by this theory, Franklin attempted to show that electricity is not created by friction, but simply collected from its diversified state, the rubbed glass globe attracting a certain quantity of "electrical fire," but ever ready to give it up to any body that has less. He explained the charged Leyden jar by showing that the inner coating of tin−foil received more than the ordinary quantity of electricity, and in consequence is POSITIVELY electrified, while the outer coating, having the ordinary quantity of electricity diminished, is electrified NEGATIVELY.

These studies of the Leyden jar, and the studies of pieces of glass coated with sheet metal, led Franklin to invent his battery, constructed of eleven large glass plates coated with sheets of lead. With this machine, after overcoming some defects, he was able to produce electrical manifestations of great force—a force that "knew no bounds," as he declared ("except in the matter of expense and of labor"), and which could be made to exceed "the greatest know effects of common lightning."

This reference to lightning would seem to show Franklin's belief, even at that time, that lightning is electricity. Many eminent observers, such as Hauksbee, Wall, Gray, and Nollet, had noticed the resemblance between electric sparks and lightning, but none of these had more than surmised that the two might be identical. In 1746, the surgeon, John Freke, also asserted his belief in this identity. Winkler, shortly after this time, expressed the same belief, and, assuming that they were the same, declared that "there is no proof that they are of different natures"; and still he did not prove that they were the same nature.

FRANKLIN INVENTS THE LIGHTNING−ROD

Even before Franklin proved conclusively the nature of lightning, his experiments in drawing off the electric charge with points led to some practical suggestions which resulted in the invention of the lightning−rod. In the letter of July, 1750, which he wrote on the subject, he gave careful instructions as to the way in which these rods might be constructed. In part Franklin wrote: "May not the knowledge of this power of points be of use to mankind in preserving houses, churches, ships, etc., from the stroke of lightning by directing us to fix on the highest parts of the edifices upright rods of iron made sharp as a needle, and gilt to prevent rusting, and from the foot of these rods a wire down the outside of the building into the grounds, or down round one of the shrouds of a ship and down her side till it reaches the water? Would not these pointed rods probably draw the electrical fire silently out of a cloud before it came nigh enough to strike, and thereby secure us from that most sudden and terrible mischief?

"To determine this question, whether the clouds that contain the lightning are electrified or not, I propose an experiment to be tried where it may be done conveniently. On the top of some high tower or steeple, place a kind of sentry−box, big enough to contain a man and an electrical stand. From the middle of the stand let an iron rod rise and pass, bending out of the door, and then upright twenty or thirty feet, pointed very sharp at the end. If the electrical stand be kept clean and dry, a man standing on it when such clouds are passing low might be electrified and afford sparks, the rod drawing fire to him from a cloud. If any danger to the man be apprehended (though I think there would be none), let him stand on the floor of his box and now and then bring near to the rod the loop of a wire that has one end fastened to the leads, he holding it by a wax handle; so the sparks, if the rod is electrified, will strike from the rod to the wire and not effect him."[4]

Not satisfied with all the evidence that he had collected pointing to the identity of lightning and electricity, he adds one more striking and very suggestive piece of evidence. Lightning was known sometimes to strike persons blind without killing them. In experimenting on pigeons and pullets with his electrical machine, Franklin found that a fowl, when not killed outright, was sometimes rendered blind. The report of these experiments were incorporated in this famous letter of the Philadelphia philosopher.

The attitude of the Royal Society towards this clearly stated letter, with its useful suggestions, must always remain as a blot on the record of this usually very receptive and liberal−minded body. Far from publishing it or receiving it at all, they derided the whole matter as too visionary for discussion by the society. How was it possible that any great scientific discovery could be made by a self−educated colonial newspaper editor, who knew nothing of European science except by hearsay, when all the great scientific minds of Europe had failed to make the discovery? How indeed! And yet it would seem that if any of the influential members of the learned society had taken the trouble to read over Franklin's clearly stated letter, they could hardly have failed to see that his suggestions were worthy of consideration. But at all events, whether they did or did not matters little. The fact remains that they refused to consider the paper seriously at the time; and later on, when its true value became known, were obliged to acknowledge their error by a tardy report on the already well−known document.

But if English scientists were cold in their reception of Franklin's theory and suggestions, the French scientists were not. Buffon, perceiving at once the importance of some of Franklin's experiments, took steps to have the famous letter translated into French, and soon not only the savants, but members of the court and the king himself were intensely interested. Two scientists, De Lor and D'Alibard, undertook to test the truth of Franklin's suggestions as to pointed rods "drawing off lightning." In a garden near Paris, the latter erected a pointed iron rod fifty feet high and an inch in diameter. As no thunder−clouds appeared for several days, a guard was stationed, armed with an insulated brass wire, who was directed to test the iron rods with it in case a storm came on during D'Alibard's absence. The storm did come on, and the guard, not waiting for his employer's arrival, seized the wire and touched the rod. Instantly there was a report. Sparks flew and the guard received such a shock that he thought his time had come. Believing from his outcry that he was mortally hurt, his friends rushed for a spiritual adviser, who came running through rain and hail to administer the last rites; but when he found the guard still alive and uninjured, he turned his visit to account by testing the rod himself several times, and later writing a report of his experiments to M. d'Alibard. This scientist at once reported the affair to the French Academy, remarking that "Franklin's idea was no longer a conjecture, but a reality."

FRANKLIN PROVES THAT LIGHTNING IS ELECTRICITY

Europe, hitherto somewhat sceptical of Franklin's views, was by this time convinced of the identity of lightning and electricity. It was now Franklin's turn to be sceptical. To him the fact that a rod, one hundred feet high, became electrified during a storm did not necessarily prove that the storm−clouds were electrified. A rod of that length was not really projected into the cloud, for even a very low thunder−cloud was more than a hundred feet above the ground. Irrefutable proof could only be had, as he saw it, by "extracting" the lightning with something actually sent up into the storm−cloud; and to accomplish this Franklin made his silk kite, with which he finally demonstrated to his own and the world's satisfaction that his theory was correct.

Taking his kite out into an open common on the approach of a thunder−storm, he flew it well up into the threatening clouds, and then, touching, the suspended key with his knuckle, received the electric spark; and a little later he charged a Leyden jar from the electricity drawn from the clouds with his kite.

In a brief but direct letter, he sent an account of his kite and his experiment to England:

"Make a small cross of two light strips of cedar," he wrote, "the arms so long as to reach to the four corners of a large, thin, silk handkerchief when extended; tie the corners of the handkerchief to the extremities of the cross so you have the body of a kite; which being properly accommodated with a tail, loop, and string, will rise in the air like those made of paper; but this being of silk is fitter to bear the wind and wet of a thunder−gust without tearing. To the top of the upright stick of the cross is to be fixed a very sharp−pointed wire, rising a foot or more above the wood. To the end of the twine, next the hand, is to be tied a silk ribbon; where the silk and twine join a key may be fastened. This kite is to be raised when a thunder−gust appears to be coming on, and the person who holds the string must stand within a door or window or under some cover, so that the silk ribbon may not be wet; and care must be taken that the twine does not touch the frame of the door or window. As soon as any of the thunder−clouds come over the kite, the pointed wire will draw the electric fire from them, and the kite, with all the twine, will be electrified and the loose filaments will stand out everywhere and be attracted by the approaching finger, and when the rain has wet the kite and twine so that it can conduct the electric fire freely, you will find it stream out plentifully from the key on the approach of your knuckle, and with this key the phial may be charged; and from electric fire thus obtained spirits may be kindled and all other electric experiments performed which are usually done by the help of a rubbed glass globe or tube, and thereby the sameness of the electric matter with that of lightning completely demonstrated."[5]

In experimenting with lightning and Franklin's pointed rods in Europe, several scientists received severe shocks, in one case with a fatal result. Professor Richman, of St. Petersburg, while experimenting during a thunder−storm, with an iron rod which he had erected on his house, received a shock that killed him instantly.

About 1733, as we have seen, Dufay had demonstrated that there were two apparently different kinds of electricity; one called VITREOUS because produced by rubbing glass, and the other RESINOUS because produced by rubbed resinous bodies. Dufay supposed that these two apparently different electricities could only be produced by their respective substances; but twenty years later, John Canton (1715−1772), an Englishman, demonstrated that under certain conditions both might be produced by rubbing the same substance. Canton's experiment, made upon a glass tube with a roughened surface, proved that if the surface of the tube were rubbed with oiled silk, vitreous or positive electricity was produced, but if rubbed with flannel, resinous electricity was produced. He discovered still further that both kinds could be excited on the same tube simultaneously with a single rubber. To demonstrate this he used a tube, one−half of which had a roughened the other a glazed surface. With a single stroke of the rubber he was able to excite both kinds of electricity on this tube. He found also that certain substances, such as glass and amber, were electrified positively when taken out of mercury, and this led to his important discovery that an amalgam of mercury and tin, when used on the surface of the rubber, was very effective in exciting glass.

XV. NATURAL HISTORY TO THE TIME OF LINNAeUS

Modern systematic botany and zoology are usually held to have their beginnings with Linnaeus. But there were certain precursors of the famous Swedish naturalist, some of them antedating him by more than a century, whose work must not be altogether ignored—such men as Konrad Gesner (1516−1565), Andreas Caesalpinus (1579−1603), Francisco Redi (1618−1676), Giovanni Alfonso Borelli (1608−1679), John Ray (1628−1705), Robert Hooke (1635−1703), John Swammerdam (1637−1680), Marcello Malpighi (1628−1694), Nehemiah Grew (1628−1711), Joseph Tournefort (1656−1708), Rudolf Jacob Camerarius (1665−1721), and Stephen Hales (1677−1761). The last named of these was, to be sure, a contemporary of Linnaeus himself, but Gesner and Caesalpinus belong, it will be observed, to so remote an epoch as that of Copernicus.

Reference has been made in an earlier chapter to the microscopic investigations of Marcello Malpighi, who, as there related, was the first observer who actually saw blood corpuscles pass through the capillaries. Another feat of this earliest of great microscopists was to dissect muscular tissue, and thus become the father of microscopic anatomy. But Malpighi did not confine his observations to animal tissues. He dissected plants as well, and he is almost as fully entitled to be called the father of vegetable anatomy, though here his honors are shared by the Englishman Grew. In 1681, while Malpighi's work, Anatomia plantarum, was on its way to the Royal Society for publication, Grew's Anatomy of Vegetables was in the hands of the publishers, making its appearance a few months earlier than the work of the great Italian. Grew's book was epoch−marking in pointing out the sex−differences in plants.

Robert Hooke developed the microscope, and took the first steps towards studying vegetable anatomy, publishing in 1667, among other results, the discovery of the cellular structure of cork. Hooke applied the name "cell" for the first time in this connection. These discoveries of Hooke, Malpighi, and Grew, and the discovery of the circulation of the blood by William Harvey shortly before, had called attention to the similarity of animal and vegetable structures. Hales made a series of investigations upon animals to determine the force of the blood pressure; and similarly he made numerous statical experiments to determine the pressure of the flow of sap in vegetables. His Vegetable Statics, published in 1727, was the first important work on the subject of vegetable physiology, and for this reason Hales has been called the father of this branch of science.

In botany, as well as in zoology, the classifications of Linnaeus of course supplanted all preceding classifications, for the obvious reason that they were much more satisfactory; but his work was a culmination of many similar and more or less satisfactory attempts of his predecessors. About the year 1670 Dr. Robert Morison (1620−1683), of Aberdeen, published a classification of plants, his system taking into account the woody or herbaceous structure, as well as the flowers and fruit. This classification was supplanted twelve years later by the classification of Ray, who arranged all known vegetables into thirty−three classes, the basis of this classification being the fruit. A few years later Rivinus, a professor of botany in the University of Leipzig, made still another classification, determining the distinguishing character chiefly from the flower, and Camerarius and Tournefort also made elaborate classifications. On the Continent Tournefort's classification was the most popular until the time of Linnaeus, his systematic arrangement including about eight thousand species of plants, arranged chiefly according to the form of the corolla.

Most of these early workers gave attention to both vegetable and animal kingdoms. They were called naturalists, and the field of their investigations was spoken of as "natural history." The specialization of knowledge had not reached that later stage in which botanist, zoologist, and physiologist felt their labors to be sharply divided. Such a division was becoming more and more necessary as the field of knowledge extended; but it did not become imperative until long after the time of Linnaeus. That naturalist himself, as we shall see, was equally distinguished as botanist and as zoologist. His great task of organizing knowledge was applied to the entire range of living things.

Carolus Linnaeus was born in the town of Rashult, in Sweden, on May 13, 1707. As a child he showed great aptitude in learning botanical names, and remembering facts about various plants as told him by his father. His eagerness for knowledge did not extend to the ordinary primary studies, however, and, aside from the single exception of the study of physiology, he proved himself an indifferent pupil. His backwardness was a sore trial to his father, who was desirous that his son should enter the ministry; but as the young Linnaeus showed no liking for that calling, and as he had acquitted himself well in his study of physiology, his father at last decided to allow him to take up the study of medicine. Here at last was a field more to the liking of the boy, who soon vied with the best of his fellow−students for first honors. Meanwhile he kept steadily at work in his study of natural history, acquiring considerable knowledge of ornithology, entomology, and botany, and adding continually to his collection of botanical specimens. In 1729 his botanical knowledge was brought to the attention of Olaf Rudbeck, professor of botany in the University of Upsala, by a short paper on the sexes of plants which Linnaeus had prepared. Rudbeck was so impressed by some of the ideas expressed in this paper that he appointed the author as his assistant the following year.

This was the beginning of Linnaes's career as a botanist. The academic gardens were thus thrown open to him, and he found time at his disposal for pursuing his studies between lecture hours and in the evenings. It was at this time that he began the preparation of his work the Systema naturae, the first of his great works, containing a comprehensive sketch of the whole field of natural history. When this work was published, the clearness of the views expressed and the systematic arrangement of the various classifications excited great astonishment and admiration, and placed Linaeus at once in the foremost rank of naturalists. This work was followed shortly by other publications, mostly on botanical subjects, in which, among other things, he worked out in detail his famous "system."

This system is founded on the sexes of plants, and is usually referred to as an "artificial method" of classification because it takes into account only a few marked characters of plants, without uniting them by more general natural affinities. At the present time it is considered only as a stepping−stone to the "natural" system; but at the time of its promulgation it was epoch−marking in its directness and simplicity, and therefore superiority, over any existing systems.

One of the great reforms effected by Linnaeus was in the matter of scientific terminology. Technical terms are absolutely necessary to scientific progress, and particularly so in botany, where obscurity, ambiguity, or prolixity in descriptions are fatally misleading. Linnaeus's work contains something like a thousand terms, whose meanings and uses are carefully explained. Such an array seems at first glance arbitrary and unnecessary, but the fact that it has remained in use for something like two centuries is indisputable evidence of its practicality. The descriptive language of botany, as employed by Linnaeus, still stands as a model for all other subjects.

Closely allied to botanical terminology is the subject of botanical nomenclature. The old method of using a number of Latin words to describe each different plant is obviously too cumbersome, and several attempts had been made prior to the time of Linnaeus to substitute simpler methods. Linnaeus himself made several unsatisfactory attempts before he finally hit upon his system of "trivial names," which was developed in his Species plantarum, and which, with some, minor alterations, remains in use to this day. The essence of the system is the introduction of binomial nomenclature—that is to say, the use of two names and no more to designate any single species of animal or plant. The principle is quite the same as that according to which in modern society a man has two names, let us say, John Doe, the one designating his family, the other being individual. Similarly each species of animal or plant, according to the Linnaeean system, received a specific or "trivial" name; while various species, associated according to their seeming natural affinities into groups called genera, were given the same generic name. Thus the generic name given all members of the cat tribe being Felis, the name Felis leo designates the lion; Felis pardus, the leopard; Felis domestica, the house cat, and so on. This seems perfectly simple and natural now, but to understand how great a reform the binomial nomenclature introduced we have but to consult the work of Linnaeus's predecessors. A single illustration will suffice. There is, for example, a kind of grass, in referring to which the naturalist anterior to Linnaeus, if he would be absolutely unambiguous, was obliged to use the following descriptive formula: Gramen Xerampelino, Miliacea, praetenuis ramosaque sparsa panicula, sive Xerampelino congener, arvense, aestivum; gramen minutissimo semine. Linnaeus gave to this plant the name Poa bulbosa—a name that sufficed, according to the new system, to distinguish this from every other species of vegetable. It does not require any special knowledge to appreciate the advantage of such a simplification.

While visiting Paris in 1738 Linnaeus met and botanized with the two botanists whose "natural method" of classification was later to supplant his own "artificial system." These were Bernard and Antoine Laurent de Jussieu. The efforts of these two scientists were directed towards obtaining a system which should aim at clearness, simplicity, and precision, and at the same time be governed by the natural affinities of plants. The natural system, as finally propounded by them, is based on the number of cotyledons, the structure of the seed, and the insertion of the stamens. Succeeding writers on botany have made various modifications of this system, but nevertheless it stands as the foundation−stone of modern botanical classification.

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[For notes and bibliography to vol. II. see vol. V.]