A History of Science, V3

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A
HISTORY OF SCIENCE
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BOOK III. MODERN DEVELOPMENT OF THE PHYSICAL SCIENCES

With the present book we enter the field of the distinctively modern. There is no precise date at which we take up each of the successive stories, but the main sweep of development has to do in each case with the nineteenth century. We shall see at once that this is a time both of rapid progress and of great differentiation. We have heard almost nothing hitherto of such sciences as paleontology, geology, and meteorology, each of which now demands full attention. Meantime, astronomy and what the workers of the elder day called natural philosophy become wonderfully diversified and present numerous phases that would have been startling enough to the star−gazers and philosophers of the earlier epoch.

Thus, for example, in the field of astronomy, Herschel is able, thanks to his perfected telescope, to discover a new planet and then to reach out into the depths of space and gain such knowledge of stars and nebulae as hitherto no one had more than dreamed of. Then, in rapid sequence, a whole coterie of hitherto unsuspected minor planets is discovered, stellar distances are measured, some members of the starry galaxy are timed in their flight, the direction of movement of the solar system itself is investigated, the spectroscope reveals the chemical composition even of suns that are unthinkably distant, and a tangible theory is grasped of the universal cycle which includes the birth and death of worlds.

Similarly the new studies of the earth's surface reveal secrets of planetary formation hitherto quite inscrutable. It becomes known that the strata of the earth's surface have been forming throughout untold ages, and that successive populations differing utterly from one another have peopled the earth in different geological epochs. The entire point of view of thoughtful men becomes changed in contemplating the history of the world in which we live—albeit the newest thought harks back to some extent to those days when the inspired thinkers of early Greece dreamed out the wonderful theories with which our earlier chapters have made our readers familiar.

In the region of natural philosophy progress is no less pronounced and no less striking. It suffices here, however, by way of anticipation, simply to name the greatest generalization of the century in physical science—the doctrine of the conservation of energy.

I. THE SUCCESSORS OF NEWTON IN ASTRONOMY

HEVELIUS AND HALLEY

STRANGELY enough, the decade immediately following Newton was one of comparative barrenness in scientific progress, the early years of the eighteenth century not being as productive of great astronomers as the later years of the seventeenth, or, for that matter, as the later years of the eighteenth century itself. Several of the prominent astronomers of the later seventeenth century lived on into the opening years of the following century, however, and the younger generation soon developed a coterie of astronomers, among whom Euler, Lagrange, Laplace, and Herschel, as we shall see, were to accomplish great things in this field before the century closed.

One of the great seventeenth−century astronomers, who died just before the close of the century, was Johannes Hevelius (1611−1687), of Dantzig, who advanced astronomy by his accurate description of the face and the spots of the moon. But he is remembered also for having retarded progress by his influence in refusing to use telescopic sights in his observations, preferring until his death the plain sights long before discarded by most other astronomers. The advantages of these telescope sights have been discussed under the article treating of Robert Hooke, but no such advantages were ever recognized by Hevelius. So great was Hevelius's reputation as an astronomer that his refusal to recognize the advantage of the telescope sights caused many astronomers to hesitate before accepting them as superior to the plain; and even the famous Halley, of whom we shall speak further in a moment, was sufficiently in doubt over the matter to pay the aged astronomer a visit to test his skill in using the old−style sights. Side by side, Hevelius and Halley made their observations, Hevelius with his old instrument and Halley with the new. The results showed slightly in the younger man's favor, but not enough to make it an entirely convincing demonstration. The explanation of this, however, did not lie in the lack of superiority of the telescopic instrument, but rather in the marvellous skill of the aged Hevelius, whose dexterity almost compensated for the defect of his instrument. What he might have accomplished could he have been induced to adopt the telescope can only be surmised.

Halley himself was by no means a tyro in matters astronomical at that time. As the only son of a wealthy soap−boiler living near London, he had been given a liberal education, and even before leaving college made such novel scientific observations as that of the change in the variation of the compass. At nineteen years of age he discovered a new method of determining the elements of the planetary orbits which was a distinct improvement over the old. The year following he sailed for the Island of St, Helena to make observations of the heavens in the southern hemisphere.

It was while in St. Helena that Halley made his famous observation of the transit of Mercury over the sun's disk, this observation being connected, indirectly at least, with his discovery of a method of determining the parallax of the planets. By parallax is meant the apparent change in the position of an object, due really to a change in the position of the observer. Thus, if we imagine two astronomers making observations of the sun from opposite sides of the earth at the same time, it is obvious that to these observers the sun will appear to be at two different points in the sky. Half the angle measuring this difference would be known as the sun's parallax. This would depend, then, upon the distance of the earth from the sun and the length of the earth's radius. Since the actual length of this radius has been determined, the parallax of any heavenly body enables the astronomer to determine its exact distance.

The parallaxes can be determined equally well, however, if two observers are separated by exactly known distances, several hundreds or thousands of miles apart. In the case of a transit of Venus across the sun's disk, for example, an observer at New York notes the image of the planet moving across the sun's disk, and notes also the exact time of this observation. In the same manner an observer at London makes similar observations. Knowing the distance between New York and London, and the different time of the passage, it is thus possible to calculate the difference of the parallaxes of the sun and a planet crossing its disk. The idea of thus determining the parallax of the planets originated, or at least was developed, by Halley, and from this phenomenon he thought it possible to conclude the dimensions of all the planetary orbits. As we shall see further on, his views were found to be correct by later astronomers.

In 1721 Halley succeeded Flamsteed as astronomer royal at the Greenwich Observatory. Although sixty− four years of age at that time his activity in astronomy continued unabated for another score of years. At Greenwich he undertook some tedious observations of the moon, and during those observations was first to detect the acceleration of mean motion. He was unable to explain this, however, and it remained for Laplace in the closing years of the century to do so, as we shall see later.

Halley's book, the Synopsis Astronomiae Cometicae, is one of the most valuable additions to astronomical literature since the time of Kepler. He was first to attempt the calculation of the orbit of a comet, having revived the ancient opinion that comets belong to the solar system, moving in eccentric orbits round the sun, and his calculation of the orbit of the comet of 1682 led him to predict correctly the return of that comet in 1758. Halley's Study of Meteors.

Like other astronomers of his time be was greatly puzzled over the well−known phenomena of shooting−stars, or meteors, making many observations himself, and examining carefully the observations of other astronomers. In 1714 he gave his views as to the origin and composition of these mysterious visitors in the earth's atmosphere. As this subject will be again referred to in a later chapter, Halley's views, representing the most advanced views of his age, are of interest.

"The theory of the air seemeth at present," he says, "to be perfectly well understood, and the differing densities thereof at all altitudes; for supposing the same air to occupy spaces reciprocally proportional to the quantity of the superior or incumbent air, I have elsewhere proved that at forty miles high the air is rarer than at the surface of the earth at three thousand times; and that the utmost height of the atmosphere, which reflects light in the Crepusculum, is not fully forty−five miles, notwithstanding which 'tis still manifest that some sort of vapors, and those in no small quantity, arise nearly to that height. An instance of this may be given in the great light the society had an account of (vide Transact. Sep., 1676) from Dr. Wallis, which was seen in very distant counties almost over all the south part of England. Of which though the doctor could not get so particular a relation as was requisite to determine the height thereof, yet from the distant places it was seen in, it could not but be very many miles high.

"So likewise that meteor which was seen in 1708, on the 31st of July, between nine and ten o'clock at night, was evidently between forty and fifty miles perpendicularly high, and as near as I can gather, over Shereness and the buoy on the Nore. For it was seen at London moving horizontally from east by north to east by south at least fifty degrees high, and at Redgrove, in Suffolk, on the Yarmouth road, about twenty miles from the east coast of England, and at least forty miles to the eastward of London, it appeared a little to the westward of the south, suppose south by west, and was seen about thirty degrees high, sliding obliquely downward. I was shown in both places the situation thereof, which was as described, but could wish some person skilled in astronomical matters bad seen it, that we might pronounce concerning its height with more certainty. Yet, as it is, we may securely conclude that it was not many more miles westerly than Redgrove, which, as I said before, is about forty miles more easterly than London. Suppose it, therefore, where perpendicular, to have been thirty−five miles east from London, and by the altitude it appeared at in London— viz., fifty degrees, its tangent will be forty−two miles, for the height of the meteor above the surface of the earth; which also is rather of the least, because the altitude of the place shown me is rather more than less than fifty degrees; and the like may be concluded from the altitude it appeared in at Redgrove, near seventy miles distant. Though at this very great distance, it appeared to move with an incredible velocity, darting, in a very few seconds of time, for about twelve degrees of a great circle from north to south, being very bright at its first appearance; and it died away at the east of its course, leaving for some time a pale whiteness in the place, with some remains of it in the track where it had gone; but no hissing sound as it passed, or bounce of an explosion were heard.

"It may deserve the honorable society's thoughts, how so great a quantity of vapor should be raised to the top of the atmosphere, and there collected, so as upon its ascension or otherwise illumination, to give a light to a circle of above one hundred miles diameter, not much inferior to the light of the moon; so as one might see to take a pin from the ground in the otherwise dark night. 'Tis hard to conceive what sort of exhalations should rise from the earth, either by the action of the sun or subterranean heat, so as to surmount the extreme cold and rareness of the air in those upper regions: but the fact is indisputable, and therefore requires a solution."

From this much of the paper it appears that there was a general belief that this burning mass was heated vapor thrown off from the earth in some mysterious manner, yet this is unsatisfactory to Halley, for after citing various other meteors that have appeared within his knowledge, he goes on to say:

"What sort of substance it must be, that could be so impelled and ignited at the same time; there being no Vulcano or other Spiraculum of subterraneous fire in the northeast parts of the world, that we ever yet heard of, from whence it might be projected.

"I have much considered this appearance, and think it one of the hardest things to account for that I have yet met with in the phenomena of meteors, and I am induced to think that it must be some collection of matter formed in the aether, as it were, by some fortuitous concourse of atoms, and that the earth met with it as it passed along in its orb, then but newly formed, and before it had conceived any great impetus of descent towards the sun. For the direction of it was exactly opposite to that of the earth, which made an angle with the meridian at that time of sixty−seven gr., that is, its course was from west southwest to east northeast, wherefore the meteor seemed to move the contrary way. And besides falling into the power of the earth's gravity, and losing its motion from the opposition of the medium, it seems that it descended towards the earth, and was extinguished in the Tyrrhene Sea, to the west southwest of Leghorn. The great blow being heard upon its first immersion into the water, and the rattling like the driving of a cart over stones being what succeeded upon its quenching; something like this is always heard upon quenching a very hot iron in water. These facts being past dispute, I would be glad to have the opinion of the learned thereon, and what objection can be reasonably made against the above hypothesis, which I humbly submit to their censure."[1]

These few paragraphs, coming as they do from a leading eighteenth−century astronomer, convey more clearly than any comment the actual state of the meteorological learning at that time. That this ball of fire, rushing "at a greater velocity than the swiftest cannon−ball," was simply a mass of heated rock passing through our atmosphere, did not occur to him, or at least was not credited. Nor is this surprising when we reflect that at that time universal gravitation had been but recently discovered; heat had not as yet been recognized as simply a form of motion; and thunder and lightning were unexplained mysteries, not to be explained for another three−quarters of a century. In the chapter on meteorology we shall see how the solution of this mystery that puzzled Halley and his associates all their lives was finally attained.

BRADLEY AND THE ABERRATION OF LIGHT

Halley was succeeded as astronomer royal by a man whose useful additions to the science were not to be recognized or appreciated fully until brought to light by the Prussian astronomer Bessel early in the nineteenth century. This was Dr. James Bradley, an ecclesiastic, who ranks as one of the most eminent astronomers of the eighteenth century. His most remarkable discovery was the explanation of a peculiar motion of the pole−star, first observed, but not explained, by Picard a century before. For many years a satisfactory explanation was sought unsuccessfully by Bradley and his fellow−astronomers, but at last he was able to demonstrate that the stary Draconis, on which he was making his observations, described, or appeared to describe, a small ellipse. If this observation was correct, it afforded a means of computing the aberration of any star at all times. The explanation of the physical cause of this aberration, as Bradley thought, and afterwards demonstrated, was the result of the combination of the motion of light with the annual motion of the earth. Bradley first formulated this theory in 1728, but it was not until 1748—twenty years of continuous struggle and observation by him—that he was prepared to communicate the results of his efforts to the Royal Society. This remarkable paper is thought by the Frenchman, Delambre, to entitle its author to a place in science beside such astronomers as Hipparcbus and Kepler.

Bradley's studies led him to discover also the libratory motion of the earth's axis. "As this appearance of g Draconis. indicated a diminution of the inclination of the earth's axis to the plane of the ecliptic," he says; "and as several astronomers have supposed THAT inclination to diminish regularly; if this phenomenon depended upon such a cause, and amounted to 18" in nine years, the obliquity of the ecliptic would, at that rate, alter a whole minute in thirty years; which is much faster than any observations, before made, would allow. I had reason, therefore, to think that some part of this motion at the least, if not the whole, was owing to the moon's action upon the equatorial parts of the earth; which, I conceived, might cause a libratory motion of the earth's axis. But as I was unable to judge, from only nine years observations, whether the axis would entirely recover the same position that it had in the year 1727, I found it necessary to continue my observations through a whole period of the moon's nodes; at the end of which I had the satisfaction to see, that the stars, returned into the same position again; as if there had been no alteration at all in the inclination of the earth's axis; which fully convinced me that I had guessed rightly as to the cause of the phenomena. This circumstance proves likewise, that if there be a gradual diminution of the obliquity of the ecliptic, it does not arise only from an alteration in the position of the earth's axis, but rather from some change in the plane of the ecliptic itself; because the stars, at the end of the period of the moon's nodes, appeared in the same places, with respect to the equator, as they ought to have done, if the earth's axis had retained the same inclination to an invariable plane."[2]

FRENCH ASTRONOMERS

Meanwhile, astronomers across the channel were by no means idle. In France several successful observers were making many additions to the already long list of observations of the first astronomer of the Royal Observatory of Paris, Dominic Cassini (1625−1712), whose reputation among his contemporaries was much greater than among succeeding generations of astronomers. Perhaps the most deserving of these successors was Nicolas Louis de Lacaille (1713−1762), a theologian who had been educated at the expense of the Duke of Bourbon, and who, soon after completing his clerical studies, came under the patronage of Cassini, whose attention had been called to the young man's interest in the sciences. One of Lacaille's first under−takings was the remeasuring of the French are of the meridian, which had been incorrectly measured by his patron in 1684. This was begun in 1739, and occupied him for two years before successfully completed. As a reward, however, he was admitted to the academy and appointed mathematical professor in Mazarin College.

In 1751 he went to the Cape of Good Hope for the purpose of determining the sun's parallax by observations of the parallaxes of Mars and Venus, and incidentally to make observations on the other southern hemisphere stars. The results of this undertaking were most successful, and were given in his Coelum australe stelligerum, etc., published in 1763. In this he shows that in the course of a single year he had observed some ten thousand stars, and computed the places of one thousand nine hundred and forty−two of them, measured a degree of the meridian, and made many observations of the moon—productive industry seldom equalled in a single year in any field. These observations were of great service to the astronomers, as they afforded the opportunity of comparing the stars of the southern hemisphere with those of the northern, which were being observed simultaneously by Lelande at Berlin.

Lacaille's observations followed closely upon the determination of an absorbing question which occupied the attention of the astronomers in the early part of the century. This question was as to the shape of the earth—whether it was actually flattened at the poles. To settle this question once for all the Academy of Sciences decided to make the actual measurement of the length of two degrees, one as near the pole as possible, the other at the equator. Accordingly, three astronomers, Godin, Bouguer, and La Condamine, made the journey to a spot on the equator in Peru, while four astronomers, Camus, Clairaut, Maupertuis, and Lemonnier, made a voyage to a place selected in Lapland. The result of these expeditions was the determination that the globe is oblately spheroidal.

A great contemporary and fellow−countryman of Lacaille was Jean Le Rond d'Alembert (1717−1783), who, although not primarily an astronomer, did so much with his mathematical calculations to aid that science that his name is closely connected with its progress during the eighteenth century. D'Alembert, who became one of the best−known men of science of his day, and whose services were eagerly sought by the rulers of Europe, began life as a foundling, having been exposed in one of the markets of Paris. The sickly infant was adopted and cared for in the family of a poor glazier, and treated as a member of the family. In later years, however, after the foundling had become famous throughout Europe, his mother, Madame Tencin, sent for him, and acknowledged her relationship. It is more than likely that the great philosopher believed her story, but if so he did not allow her the satisfaction of knowing his belief, declaring always that Madame Tencin could "not be nearer than a step−mother to him, since his mother was the wife of the glazier."

D'Alembert did much for the cause of science by his example as well as by his discoveries. By living a plain but honest life, declining magnificent offers of positions from royal patrons, at the same time refusing to grovel before nobility, he set a worthy example to other philosophers whose cringing and pusillanimous attitude towards persons of wealth or position had hitherto earned them the contempt of the upper classes.

His direct additions to astronomy are several, among others the determination of the mutation of the axis of the earth. He also determined the ratio of the attractive forces of the sun and moon, which he found to be about as seven to three. From this he reached the conclusion that the earth must be seventy times greater than the moon. The first two volumes of his Researches on the Systems of the World, published in 1754, are largely devoted to mathematical and astronomical problems, many of them of little importance now, but of great interest to astronomers at that time.

Another great contemporary of D'Alembert, whose name is closely associated and frequently confounded with his, was Jean Baptiste Joseph Delambre (1749− 1822). More fortunate in birth as also in his educational advantages, Delambre as a youth began his studies under the celebrated poet Delille. Later he was obliged to struggle against poverty, supporting himself for a time by making translations from Latin, Greek, Italian, and English, and acting as tutor in private families. The turning−point of his fortune came when the attention of Lalande was called to the young man by his remarkable memory, and Lalande soon showed his admiration by giving Delambre certain difficult astronomical problems to solve. By performing these tasks successfully his future as an astronomer became assured. At that time the planet Uranus had just been discovered by Herschel, and the Academy of Sciences offered as the subject for one of its prizes the determination of the planet's orbit. Delambre made this determination and won the prize—a feat that brought him at once into prominence.

By his writings he probably did as much towards perfecting modern astronomy as any one man. His History of Astronomy is not merely a narrative of progress of astronomy but a complete abstract of all the celebrated works written on the subject. Thus he became famous as an historian as well as an astronomer.

LEONARD EULER

Still another contemporary of D'Alembert and Delambre, and somewhat older than either of them, was Leonard Euler (1707−1783), of Basel, whose fame as a philosopher equals that of either of the great Frenchmen. He is of particular interest here in his capacity of astronomer, but astronomy was only one of the many fields of science in which he shone. Surely something out of the ordinary was to be expected of the man who could "repeat the AEneid of Virgil from the beginning to the end without hesitation, and indicate the first and last line of every page of the edition which he used." Something was expected, and he fulfilled these expectations.

In early life he devoted himself to the study of theology and the Oriental languages, at the request of his father, but his love of mathematics proved too strong, and, with his father's consent, he finally gave up his classical studies and turned to his favorite study, geometry. In 1727 he was invited by Catharine I. to reside in St. Petersburg, and on accepting this invitation he was made an associate of the Academy of Sciences. A little later he was made professor of physics, and in 1733 professor of mathematics. In 1735 he solved a problem in three days which some of the eminent mathematicians would not undertake under several months. In 1741 Frederick the Great invited him to Berlin, where he soon became a member of the Academy of Sciences and professor of mathematics; but in 1766 he returned to St. Petersburg. Towards the close of his life be became virtually blind, being obliged to dictate his thoughts, sometimes to persons entirely ignorant of the subject in hand. Nevertheless, his remarkable memory, still further heightened by his blindness, enabled him to carry out the elaborate computations frequently involved.

Euler's first memoir, transmitted to the Academy of Sciences of Paris in 1747, was on the planetary perturbations. This memoir carried off the prize that had been offered for the analytical theory of the motions of Jupiter and Saturn. Other memoirs followed, one in 1749 and another in 1750, with further expansions of the same subject. As some slight errors were found in these, such as a mistake in some of the formulae expressing the secular and periodic inequalities, the academy proposed the same subject for the prize of 1752. Euler again competed, and won this prize also. The contents of this memoir laid the foundation for the subsequent demonstration of the permanent stability of the planetary system by Laplace and Lagrange.

It was Euler also who demonstrated that within certain fixed limits the eccentricities and places of the aphelia of Saturn and Jupiter are subject to constant variation, and he calculated that after a lapse of about thirty thousand years the elements of the orbits of these two planets recover their original values.

II. THE PROGRESS OF MODERN ASTRONOMY

A NEW epoch in astronomy begins with the work of William Herschel, the Hanoverian, whom England made hers by adoption. He was a man with a positive genius for sidereal discovery. At first a mere amateur in astronomy, he snatched time from his duties as music−teacher to grind him a telescopic mirror, and began gazing at the stars. Not content with his first telescope, he made another and another, and he had such genius for the work that he soon possessed a better instrument than was ever made before. His patience in grinding the curved reflective surface was monumental. Sometimes for sixteen hours together he must walk steadily about the mirror, polishing it, without once removing his hands. Meantime his sister, always his chief lieutenant, cheered him with her presence, and from time to time put food into his mouth. The telescope completed, the astronomer turned night into day, and from sunset to sunrise, year in and year out, swept the heavens unceasingly, unless prevented by clouds or the brightness of the moon. His sister sat always at his side, recording his observations. They were in the open air, perched high at the mouth of the reflector, and sometimes it was so cold that the ink froze in the bottle in Caroline Herschel's hand; but the two enthusiasts hardly noticed a thing so common−place as terrestrial weather. They were living in distant worlds.

The results? What could they be? Such enthusiasm would move mountains. But, after all, the moving of mountains seems a liliputian task compared with what Herschel really did with those wonderful telescopes. He moved worlds, stars, a universe— even, if you please, a galaxy of universes; at least he proved that they move, which seems scarcely less wonderful; and he expanded the cosmos, as man conceives it, to thousands of times the dimensions it had before. As a mere beginning, he doubled the diameter of the solar system by observing the great outlying planet which we now call Uranus, but which he christened Georgium Sidus, in honor of his sovereign, and which his French contemporaries, not relishing that name, preferred to call Herschel.

This discovery was but a trifle compared with what Herschel did later on, but it gave him world−wide reputation none the less. Comets and moons aside, this was the first addition to the solar system that had been made within historic times, and it created a veritable furor of popular interest and enthusiasm. Incidentally King George was flattered at having a world named after him, and he smiled on the astronomer, and came with his court to have a look at his namesake. The inspection was highly satisfactory; and presently the royal favor enabled the astronomer to escape the thraldom of teaching music and to devote his entire time to the more congenial task of star−gazing.

Thus relieved from the burden of mundane embarrassments, he turned with fresh enthusiasm to the skies, and his discoveries followed one another in bewildering profusion. He found various hitherto unseen moons of our sister planets; be made special studies of Saturn, and proved that this planet, with its rings, revolves on its axis; he scanned the spots on the sun, and suggested that they influence the weather of our earth; in short, he extended the entire field of solar astronomy. But very soon this field became too small for him, and his most important researches carried him out into the regions of space compared with which the span of our solar system is a mere point. With his perfected telescopes he entered abysmal vistas which no human eve ever penetrated before, which no human mind had hitherto more than vaguely imagined. He tells us that his forty−foot reflector will bring him light from a distance of "at least eleven and three−fourths millions of millions of millions of miles"—light which left its source two million years ago. The smallest stars visible to the unaided eye are those of the sixth magnitude; this telescope, he thinks, has power to reveal stars of the 1342d magnitude.

But what did Herschel learn regarding these awful depths of space and the stars that people them? That was what the world wished to know. Copernicus, Galileo, Kepler, had given us a solar system, but the stars had been a mystery. What says the great reflector—are the stars points of light, as the ancients taught, and as more than one philosopher of the eighteenth century has still contended, or are they suns, as others hold? Herschel answers, they are suns, each and every one of all the millions—suns, many of them, larger than the one that is the centre of our tiny system. Not only so, but they are moving suns. Instead of being fixed in space, as has been thought, they are whirling in gigantic orbits about some common centre. Is our sun that centre? Far from it. Our sun is only a star like all the rest, circling on with its attendant satellites—our giant sun a star, no different from myriad other stars, not even so large as some; a mere insignificant spark of matter in an infinite shower of sparks.

Nor is this all. Looking beyond the few thousand stars that are visible to the naked eye, Herschel sees series after series of more distant stars, marshalled in galaxies of millions; but at last he reaches a distance beyond which the galaxies no longer increase. And yet—so he thinks—he has not reached the limits of his vision. What then? He has come to the bounds of the sidereal system—seen to the confines of the universe. He believes that he can outline this system, this universe, and prove that it has the shape of an irregular globe, oblately flattened to almost disklike proportions, and divided at one edge—a bifurcation that is revealed even to the naked eye in the forking of the Milky Way.

This, then, is our universe as Herschel conceives it— a vast galaxy of suns, held to one centre, revolving, poised in space. But even here those marvellous telescopes do not pause. Far, far out beyond the confines of our universe, so far that the awful span of our own system might serve as a unit of measure, are revealed other systems, other universes, like our own, each composed, as he thinks, of myriads of suns, clustered like our galaxy into an isolated system—mere islands of matter in an infinite ocean of space. So distant from our universe are these now universes of Herschel's discovery that their light reaches us only as a dim, nebulous glow, in most cases invisible to the unaided eye. About a hundred of these nebulae were known when Herschel began his studies. Before the close of the century he had discovered about two thousand more of them, and many of these had been resolved by his largest telescopes into clusters of stars. He believed that the farthest of these nebulae that he could see was at least three hundred thousand times as distant from us as the nearest fixed star. Yet that nearest star—so more recent studies prove—is so remote that its light, travelling one hundred and eighty thousand miles a second, requires three and one−half years to reach our planet.

As if to give the finishing touches to this novel scheme of cosmology, Herschel, though in the main very little given to unsustained theorizing, allows himself the privilege of one belief that he cannot call upon his telescope to substantiate. He thinks that all the myriad suns of his numberless systems are instinct with life in the human sense. Giordano Bruno and a long line of his followers had held that some of our sister planets may be inhabited, but Herschel extends the thought to include the moon, the sun, the stars—all the heavenly bodies. He believes that he can demonstrate the habitability of our own sun, and, reasoning from analogy, he is firmly convinced that all the suns of all the systems are "well supplied with inhabitants." In this, as in some other inferences, Herschel is misled by the faulty physics of his time. Future generations, working with perfected instruments, may not sustain him all along the line of his observations, even, let alone his inferences. But how one's egotism shrivels and shrinks as one grasps the import of his sweeping thoughts!

Continuing his observations of the innumerable nebulae, Herschel is led presently to another curious speculative inference. He notes that some star groups are much more thickly clustered than others, and he is led to infer that such varied clustering tells of varying ages of the different nebulae. He thinks that at first all space may have been evenly sprinkled with the stars and that the grouping has resulted from the action of gravitation.

"That the Milky Way is a most extensive stratum of stars of various sizes admits no longer of lasting doubt," he declares, "and that our sun is actually one of the heavenly bodies belonging to it is as evident. I have now viewed and gauged this shining zone in almost every direction and find it composed of stars whose number ... constantly increases and decreases in proportion to its apparent brightness to the naked eye.

"Let us suppose numberless stars of various sizes, scattered over an indefinite portion of space in such a manner as to be almost equally distributed throughout the whole. The laws of attraction which no doubt extend to the remotest regions of the fixed stars will operate in such a manner as most probably to produce the following effects:

"In the first case, since we have supposed the stars to be of various sizes, it will happen that a star, being considerably larger than its neighboring ones, will attract them more than they will be attracted by others that are immediately around them; by which means they will be, in time, as it were, condensed about a centre, or, in other words, form themselves into a cluster of stars of almost a globular figure, more or less regular according to the size and distance of the surrounding stars....

"The next case, which will also happen almost as frequently as the former, is where a few stars, though not superior in size to the rest, may chance to be rather nearer one another than the surrounding ones,... and this construction admits of the utmost variety of shapes. . . .

"From the composition and repeated conjunction of both the foregoing formations, a third may be derived when many large stars, or combined small ones, are spread in long, extended, regular, or crooked rows, streaks, or branches; for they will also draw the surrounding stars, so as to produce figures of condensed stars curiously similar to the former which gave rise to these condensations.

"We may likewise admit still more extensive combinations; when, at the same time that a cluster of stars is forming at the one part of space, there may be another collection in a different but perhaps not far− distant quarter, which may occasion a mutual approach towards their own centre of gravity.

"In the last place, as a natural conclusion of the former cases, there will be formed great cavities or vacancies by the retreating of the stars towards the various centres which attract them."[1]

Looking forward, it appears that the time must come when all the suns of a system will be drawn together and destroyed by impact at a common centre. Already, it seems to Herschel, the thickest clusters have "outlived their usefulness" and are verging towards their doom.

But again, other nebulae present an appearance suggestive of an opposite condition. They are not resolvable into stars, but present an almost uniform appearance throughout, and are hence believed to be composed of a shining fluid, which in some instances is seen to be condensed at the centre into a glowing mass. In such a nebula Herschel thinks he sees a sun in process of formation.

THE NEBULAR HYPOTHESIS OF KANT

Taken together, these two conceptions outline a majestic cycle of world formation and world destruction— a broad scheme of cosmogony, such as had been vaguely adumbrated two centuries before by Kepler and in more recent times by Wright and Swedenborg. This so−called "nebular hypothesis" assumes that in the beginning all space was uniformly filled with cosmic matter in a state of nebular or "fire−mist" diffusion, "formless and void." It pictures the condensation— coagulation, if you will—of portions of this mass to form segregated masses, and the ultimate development out of these masses of the sidereal bodies that we see.

Perhaps the first elaborate exposition of this idea was that given by the great German philosopher Immanuel Kant (born at Konigsberg in 1724, died in 1804), known to every one as the author of the Critique of Pure Reason. Let us learn from his own words how the imaginative philosopher conceived the world to have come into existence.

"I assume," says Kant, "that all the material of which the globes belonging to our solar system—all the planets and comets—consist, at the beginning of all things was decomposed into its primary elements, and filled the whole space of the universe in which the bodies formed out of it now revolve. This state of nature, when viewed in and by itself without any reference to a system, seems to be the very simplest that can follow upon nothing. At that time nothing has yet been formed. The construction of heavenly bodies at a distance from one another, their distances regulated by their attraction, their form arising out of the equilibrium of their collected matter, exhibit a later state.... In a region of space filled in this manner, a universal repose could last only a moment. The elements have essential forces with which to put each other in motion, and thus are themselves a source of life. Matter immediately begins to strive to fashion itself. The scattered elements of a denser kind, by means of their attraction, gather from a sphere around them all the matter of less specific gravity; again, these elements themselves, together with the material which they have united with them, collect in those points where the particles of a still denser kind are found; these in like manner join still denser particles, and so on. If we follow in imagination this process by which nature fashions itself into form through the whole extent of chaos, we easily perceive that all the results of the process would consist in the formation of divers masses which, when their formation was complete, would by the equality of their attraction be at rest and be forever unmoved.

"But nature has other forces in store which are specially exerted when matter is decomposed into fine particles. They are those forces by which these particles repel one another, and which, by their conflict with attractions, bring forth that movement which is, as it were, the lasting life of nature. This force of repulsion is manifested in the elasticity of vapors, the effluences of strong−smelling bodies, and the diffusion of all spirituous matters. This force is an uncontestable phenomenon of matter. It is by it that the elements, which may be falling to the point attracting them, are turned sideways promiscuously from their movement in a straight line; and their perpendicular fall thereby issues in circular movements, which encompass the centre towards which they were falling. In order to make the formation of the world more distinctly conceivable, we will limit our view by withdrawing it from the infinite universe of nature and directing it to a particular system, as the one which belongs to our sun. Having considered the generation of this system, we shall be able to advance to a similar consideration of the origin of the great world−systems, and thus to embrace the infinitude of the whole creation in one conception.

"From what has been said, it will appear that if a point is situated in a very large space where the attraction of the elements there situated acts more strongly than elsewhere, then the matter of the elementary particles scattered throughout the whole region will fall to that point. The first effect of this general fall is the formation of a body at this centre of attraction, which, so to speak, grows from an infinitely small nucleus by rapid strides; and in the proportion in which this mass increases, it also draws with greater force the surrounding particles to unite with it. When the mass of this central body has grown so great that the velocity with which it draws the particles to itself with great distances is bent sideways by the feeble degree of repulsion with which they impede one another, and when it issues in lateral movements which are capable by means of the centrifugal force of encompassing the central body in an orbit, then there are produced whirls or vortices of particles, each of which by itself describes a curved line by the composition of the attracting force and the force of revolution that had been bent sideways. These kinds of orbits all intersect one another, for which their great dispersion in this space gives place. Yet these movements are in many ways in conflict with one another, and they naturally tend to bring one another to a uniformity—that is, into a state in which one movement is as little obstructive to the other as possible. This happens in two ways: first by the particles limiting one another's movement till they all advance in one direction; and, secondly, in this way, that the particles limit their vertical movements in virtue of which they are approaching the centre of attraction, till they all move horizontally—i. e., in parallel circles round the sun as their centre, no longer intercept one another, and by the centrifugal force becoming equal with the falling force they keep themselves constantly in free circular orbits at the distance at which they move. The result, finally, is that only those particles continue to move in this region of space which have acquired by their fall a velocity, and through the resistance of the other particles a direction, by which they can continue to maintain a FREE CIRCULAR MOVEMENT....

"The view of the formation of the planets in this system has the advantage over every other possible theory in holding that the origin of the movements, and the position of the orbits in arising at that same point of time—nay, more, in showing that even the deviations from the greatest possible exactness in their determinations, as well as the accordances themselves, become clear at a glance. The planets are formed out of particles which, at the distance at which they move, have exact movements in circular orbits; and therefore the masses composed out of them will continue the same movements and at the same rate and in the same direction."[2]

It must be admitted that this explanation leaves a good deal to be desired. It is the explanation of a metaphysician rather than that of an experimental scientist. Such phrases as "matter immediately begins to strive to fashion itself," for example, have no place in the reasoning of inductive science. Nevertheless, the hypothesis of Kant is a remarkable conception; it attempts to explain along rational lines something which hitherto had for the most part been considered altogether inexplicable.

But there are various questions that at once suggest themselves which the Kantian theory leaves unanswered. How happens it, for example, that the cosmic mass which gave birth to our solar system was divided into several planetary bodies instead of remaining a single mass? Were the planets struck from the sun by the chance impact of comets, as Buffon has suggested? or thrown out by explosive volcanic action, in accordance with the theory of Dr. Darwin? or do they owe their origin to some unknown law? In any event, how chanced it that all were projected in nearly the same plane as we now find them?

LAPLACE AND THE NEBULAR HYPOTHESIS

It remained for a mathematical astronomer to solve these puzzles. The man of all others competent to take the subject in hand was the French astronomer Laplace. For a quarter of a century he had devoted his transcendent mathematical abilities to the solution of problems of motion of the heavenly bodies. Working in friendly rivalry with his countryman Lagrange, his only peer among the mathematicians of the age, he had taken up and solved one by one the problems that Newton left obscure. Largely through the efforts of these two men the last lingering doubts as to the solidarity of the Newtonian hypothesis of universal gravitation had been removed. The share of Lagrange was hardly less than that of his co−worker; but Laplace will longer be remembered, because he ultimately brought his completed labors into a system, and, incorporating with them the labors of his contemporaries, produced in the Mecanique Celeste the undisputed mathematical monument of the century, a fitting complement to the Principia of Newton, which it supplements and in a sense completes.

In the closing years of the eighteenth century Laplace took up the nebular hypothesis of cosmogony, to which we have just referred, and gave it definite proportions; in fact, made it so thoroughly his own that posterity will always link it with his name. Discarding the crude notions of cometary impact and volcanic eruption, Laplace filled up the gaps in the hypothesis with the aid of well−known laws of gravitation and motion. He assumed that the primitive mass of cosmic matter which was destined to form our solar system was revolving on its axis even at a time when it was still nebular in character, and filled all space to a distance far beyond the present limits of the system. As this vaporous mass contracted through loss of heat, it revolved more and more swiftly, and from time to time, through balance of forces at its periphery, rings of its substance were whirled off and left revolving there, subsequently to become condensed into planets, and in their turn whirl off minor rings that became moons. The main body of the original mass remains in the present as the still contracting and rotating body which we call the sun.

Let us allow Laplace to explain all this in detail:

"In order to explain the prime movements of the planetary system," he says, "there are the five following phenomena: The movement of the planets in the same direction and very nearly in the same plane; the movement of the satellites in the same direction as that of the planets; the rotation of these different bodies and the sun in the same direction as their revolution, and in nearly the same plane; the slight eccentricity of the orbits of the planets and of the satellites; and, finally, the great eccentricity of the orbits of the comets, as if their inclinations had been left to chance.

"Buffon is the only man I know who, since the discovery of the true system of the world, has endeavored to show the origin of the planets and their satellites. He supposes that a comet, in falling into the sun, drove from it a mass of matter which was reassembled at a distance in the form of various globes more or less large, and more or less removed from the sun, and that these globes, becoming opaque and solid, are now the planets and their satellites.

"This hypothesis satisfies the first of the five preceding phenomena; for it is clear that all the bodies thus formed would move very nearly in the plane which passed through the centre of the sun, and in the direction of the torrent of matter which was produced; but the four other phenomena appear to be inexplicable to me by this means. Indeed, the absolute movement of the molecules of a planet ought then to be in the direction of the movement of its centre of gravity; but it does not at all follow that the motion of the rotation of the planets should be in the same direction. Thus the earth should rotate from east to west, but nevertheless the absolute movement of its molecules should be from east to west; and this ought also to apply to the movement of the revolution of the satellites, in which the direction, according to the hypothesis which he offers, is not necessarily the same as that of the progressive movement of the planets.

"A phenomenon not only very difficult to explain under this hypothesis, but one which is even contrary to it, is the slight eccentricity of the planetary orbits. We know, by the theory of central forces, that if a body moves in a closed orbit around the sun and touches it, it also always comes back to that point at every revolution; whence it follows that if the planets were originally detached from the sun, they would touch it at each return towards it, and their orbits, far from being circular, would be very eccentric. It is true that a mass of matter driven from the sun cannot be exactly compared to a globe which touches its surface, for the impulse which the particles of this mass receive from one another and the reciprocal attractions which they exert among themselves, could, in changing the direction of their movements, remove their perihelions from the sun; but their orbits would be always most eccentric, or at least they would not have slight eccentricities except by the most extraordinary chance. Thus we cannot see, according to the hypothesis of Buffon, why the orbits of more than a hundred comets already observed are so elliptical. This hypothesis is therefore very far from satisfying the preceding phenomena. Let us see if it is possible to trace them back to their true cause.

"Whatever may be its ultimate nature, seeing that it has caused or modified the movements of the planets, it is necessary that this cause should embrace every body, and, in view of the enormous distances which separate them, it could only have been a fluid of immense extent. In order to have given them an almost circular movement in the same direction around the sun, it is necessary that this fluid should have enveloped the sun as in an atmosphere. The consideration of the planetary movements leads us then to think that, on account of excessive heat, the atmosphere of the sun originally extended beyond the orbits of all the planets, and that it was successively contracted to its present limits.

"In the primitive condition in which we suppose the sun to have been, it resembled a nebula such as the telescope shows is composed of a nucleus more or less brilliant, surrounded by a nebulosity which, on condensing itself towards the centre, forms a star. If it is conceived by analogy that all the stars were formed in this manner, it is possible to imagine their previous condition of nebulosity, itself preceded by other states in which the nebulous matter was still more diffused, the nucleus being less and less luminous. By going back as far as possible, we thus arrive at a nebulosity so diffused that its existence could hardly be suspected.

"For a long time the peculiar disposition of certain stars, visible to the unaided eye, has struck philosophical observers. Mitchell has already remarked how little probable it is that the stars in the Pleiades, for example, could have been contracted into the small space which encloses them by the fortuity of chance alone, and he has concluded that this group of stars, and similar groups which the skies present to us, are the necessary result of the condensation of a nebula, with several nuclei, and it is evident that a nebula, by continually contracting, towards these various nuclei, at length would form a group of stars similar to the Pleiades. The condensation of a nebula with two nuclei would form a system of stars close together, turning one upon the other, such as those double stars of which we already know the respective movements.

"But how did the solar atmosphere determine the movements of the rotation and revolution of the planets and satellites? If these bodies had penetrated very deeply into this atmosphere, its resistance would have caused them to fall into the sun. We can therefore conjecture that the planets were formed at their successive limits by the condensation of a zone of vapors which the sun, on cooling, left behind, in the plane of his equator.

"Let us recall the results which we have given in a preceding chapter. The atmosphere of the sun could not have extended indefinitely. Its limit was the point where the centrifugal force due to its movement of rotation balanced its weight. But in proportion as the cooling contracted the atmosphere, and those molecules which were near to them condensed upon the surface of the body, the movement of the rotation increased; for, on account of the Law of Areas, the sum of the areas described by the vector of each molecule of the sun and its atmosphere and projected in the plane of the equator being always the same, the rotation should increase when these molecules approach the centre of the sun. The centrifugal force due to this movement becoming thus larger, the point where the weight is equal to it is nearer the sun. Supposing, then, as it is natural to admit, that the atmosphere extended at some period to its very limits, it should, on cooling, leave molecules behind at this limit and at limits successively occasioned by the increased rotation of the sun. The abandoned molecules would continue to revolve around this body, since their centrifugal force was balanced by their weight. But this equilibrium not arising in regard to the atmospheric molecules parallel to the solar equator, the latter, on account of their weight, approached the atmosphere as they condensed, and did not cease to belong to it until by this motion they came upon the equator.

"Let us consider now the zones of vapor successively left behind. These zones ought, according to appearance, by the condensation and mutual attraction of their molecules, to form various concentric rings of vapor revolving around the sun. The mutual gravitational friction of each ring would accelerate some and retard others, until they had all acquired the same angular velocity. Thus the actual velocity of the molecules most removed from the sun would be the greatest. The following cause would also operate to bring about this difference of speed. The molecules farthest from the sun, and which by the effects of cooling and condensation approached one another to form the outer part of the ring, would have always described areas proportional to the time since the central force by which they were controlled has been constantly directed towards this body. But this constancy of areas necessitates an increase of velocity proportional to the distance. It is thus seen that the same cause would diminish the velocity of the molecules which form the inner part of the ring.

"If all the molecules of the ring of vapor continued to condense without disuniting, they would at length form a ring either solid or fluid. But this formation would necessitate such a regularity in every part of the ring, and in its cooling, that this phenomenon is extremely rare; and the solar system affords us, indeed, but one example—namely, in the ring of Saturn. In nearly every case the ring of vapor was broken into several masses, each moving at similar velocities, and continuing to rotate at the same distance around the sun. These masses would take a spheroid form with a rotatory movement in the direction of the revolution, because their inner molecules had less velocity than the outer. Thus were formed so many planets in a condition of vapor. But if one of them were powerful enough to reunite successively by its attraction all the others around its centre of gravity, the ring of vapor would be thus transformed into a single spheroidical mass of vapor revolving around the sun with a rotation in the direction of its revolution. The latter case has been that which is the most common, but nevertheless the solar system affords us an instance of the first case in the four small planets which move between Jupiter and Mars; at least, if we do not suppose, as does M. Olbers, that they originally formed a single planet which a mighty explosion broke up into several portions each moving at different velocities.

"According to our hypothesis, the comets are strangers to our planetary system. In considering them, as we have done, as minute nebulosities, wandering from solar system to solar system, and formed by the condensation of the nebulous matter everywhere existent in profusion in the universe, we see that when they come into that part of the heavens where the sun is all−powerful, he forces them to describe orbits either elliptical or hyperbolic, their paths being equally possible in all directions, and at all inclinations of the ecliptic, conformably to what has been observed. Thus the condensation of nebulous matter, by which we have at first explained the motions of the rotation and revolution of the planets and their satellites in the same direction, and in nearly approximate planes, explains also why the movements of the comets escape this general law."[3]

The nebular hypothesis thus given detailed completion by Laplace is a worthy complement of the grand cosmologic scheme of Herschel. Whether true or false, the two conceptions stand as the final contributions of the eighteenth century to the history of man's ceaseless efforts to solve the mysteries of cosmic origin and cosmic structure. The world listened eagerly and without prejudice to the new doctrines; and that attitude tells of a marvellous intellectual growth of our race. Mark the transition. In the year 1600, Bruno was burned at the stake for teaching that our earth is not the centre of the universe. In 1700, Newton was pronounced "impious and heretical" by a large school of philosophers for declaring that the force which holds the planets in their orbits is universal gravitation. In 1800, Laplace and Herschel are honored for teaching that gravitation built up the system which it still controls; that our universe is but a minor nebula, our sun but a minor star, our earth a mere atom of matter, our race only one of myriad races peopling an infinity of worlds. Doctrines which but the span of two human lives before would have brought their enunciators to the stake were now pronounced not impious, but sublime.

ASTEROIDS AND SATELLITES

The first day of the nineteenth century was fittingly signalized by the discovery of a new world. On the evening of January 1, 1801, an Italian astronomer, Piazzi, observed an apparent star of about the eighth magnitude (hence, of course, quite invisible to the unaided eye), which later on was seen to have moved, and was thus shown to be vastly nearer the earth than any true star. He at first supposed, as Herschel had done when he first saw Uranus, that the unfamiliar body was a comet; but later observation proved it a tiny planet, occupying a position in space between Mars and Jupiter. It was christened Ceres, after the tutelary goddess of Sicily.

Though unpremeditated, this discovery was not unexpected, for astronomers had long surmised the existence of a planet in the wide gap between Mars and Jupiter. Indeed, they were even preparing to make concerted search for it, despite the protests of philosophers, who argued that the planets could not possibly exceed the magic number seven, when Piazzi forestalled their efforts. But a surprise came with the sequel; for the very next year Dr. Olbers, the wonderful physician− astronomer of Bremen, while following up the course of Ceres, happened on another tiny moving star, similarly located, which soon revealed itself as planetary. Thus two planets were found where only one was expected.

The existence of the supernumerary was a puzzle, but Olbers solved it for the moment by suggesting that Ceres and Pallas, as he called his captive, might be fragments of a quondam planet, shattered by internal explosion or by the impact of a comet. Other similar fragments, he ventured to predict, would be found when searched for. William Herschel sanctioned this theory, and suggested the name asteroids for the tiny planets. The explosion theory was supported by the discovery of another asteroid, by Harding, of Lilienthal, in 1804, and it seemed clinched when Olbers himself found a fourth in 1807. The new−comers were named Juno and Vesta respectively.

There the case rested till 1845, when a Prussian amateur astronomer named Hencke found another asteroid, after long searching, and opened a new epoch of discovery. From then on the finding of asteroids became a commonplace. Latterly, with the aid of photography, the list has been extended to above four hundred, and as yet there seems no dearth in the supply, though doubtless all the larger members have been revealed. Even these are but a few hundreds of miles in diameter, while the smaller ones are too tiny for measurement. The combined bulk of these minor planets is believed to be but a fraction of that of the earth.

Olbers's explosion theory, long accepted by astronomers, has been proven open to fatal objections. The minor planets are now believed to represent a ring of cosmical matter, cast off from the solar nebula like the rings that went to form the major planets, but prevented from becoming aggregated into a single body by the perturbing mass of Jupiter.

The Discovery of Neptune

As we have seen, the discovery of the first asteroid confirmed a conjecture; the other important planetary discovery of the nineteenth century fulfilled a prediction. Neptune was found through scientific prophecy. No one suspected the existence of a trans−Uranian planet till Uranus itself, by hair−breadth departures from its predicted orbit, gave out the secret. No one saw the disturbing planet till the pencil of the mathematician, with almost occult divination, had pointed out its place in the heavens. The general predication of a trans−Uranian planet was made by Bessel, the great Konigsberg astronomer, in 1840; the analysis that revealed its exact location was undertaken, half a decade later, by two independent workers—John Couch Adams, just graduated senior wrangler at Cambridge, England, and U. J. J. Leverrier, the leading French mathematician of his generation.

Adams's calculation was first begun and first completed. But it had one radical defect—it was the work of a young and untried man. So it found lodgment in a pigeon−hole of the desk of England's Astronomer Royal, and an opportunity was lost which English astronomers have never ceased to mourn. Had the search been made, an actual planet would have been seen shining there, close to the spot where the pencil of the mathematician had placed its hypothetical counterpart. But the search was not made, and while the prophecy of Adams gathered dust in that regrettable pigeon−hole, Leverrier's calculation was coming on, his tentative results meeting full encouragement from Arago and other French savants. At last the laborious calculations proved satisfactory, and, confident of the result, Leverrier sent to the Berlin observatory, requesting that search be made for the disturber of Uranus in a particular spot of the heavens. Dr. Galle received the request September 23, 1846. That very night he turned his telescope to the indicated region, and there, within a single degree of the suggested spot, he saw a seeming star, invisible to the unaided eye, which proved to be the long−sought planet, henceforth to be known as Neptune. To the average mind, which finds something altogether mystifying about abstract mathematics, this was a feat savoring of the miraculous.

Stimulated by this success, Leverrier calculated an orbit for an interior planet from perturbations of Mercury, but though prematurely christened Vulcan, this hypothetical nursling of the sun still haunts the realm of the undiscovered, along with certain equally hypothetical trans−Neptunian planets whose existence has been suggested by "residual perturbations" of Uranus, and by the movements of comets. No other veritable additions of the sun's planetary family have been made in our century, beyond the finding of seven small moons, which chiefly attest the advance in telescopic powers. Of these, the tiny attendants of our Martian neighbor, discovered by Professor Hall with the great Washington refractor, are of greatest interest, because of their small size and extremely rapid flight. One of them is poised only six thousand miles from Mars, and whirls about him almost four times as fast as he revolves, seeming thus, as viewed by the Martian, to rise in the west and set in the east, and making the month only one−fourth as long as the day.

The Rings of Saturn

The discovery of the inner or crape ring of Saturn, made simultaneously in 1850 by William C. Bond, at the Harvard observatory, in America, and the Rev. W. R. Dawes in England, was another interesting optical achievement; but our most important advances in knowledge of Saturn's unique system are due to the mathematician. Laplace, like his predecessors, supposed these rings to be solid, and explained their stability as due to certain irregularities of contour which Herschel bad pointed out. But about 1851 Professor Peirce, of Harvard, showed the untenability of this conclusion, proving that were the rings such as Laplace thought them they must fall of their own weight. Then Professor J. Clerk−Maxwell, of Cambridge, took the matter in hand, and his analysis reduced the puzzling rings to a cloud of meteoric particles—a "shower of brickbats"—each fragment of which circulates exactly as if it were an independent planet, though of course perturbed and jostled more or less by its fellows. Mutual perturbations, and the disturbing pulls of Saturn's orthodox satellites, as investigated by Maxwell, explain nearly all the phenomena of the rings in a manner highly satisfactory.

After elaborate mathematical calculations covering many pages of his paper entitled "On the Stability of Saturn's Rings," he summarizes his deductions as follows:

"Let us now gather together the conclusions we have been able to draw from the mathematical theory of various kinds of conceivable rings.

"We found that the stability of the motion of a solid ring depended on so delicate an adjustment, and at the same time so unsymmetrical a distribution of mass, that even if the exact conditions were fulfilled, it could scarcely last long, and, if it did, the immense preponderance of one side of the ring would be easily observed, contrary to experience. These considerations, with others derived from the mechanical structure of so vast a body, compel us to abandon any theory of solid rings.

"We next examined the motion of a ring of equal satellites, and found that if the mass of the planet is sufficient, any disturbances produced in the arrangement of the ring will be propagated around it in the form of waves, and will not introduce dangerous confusion. If the satellites are unequal, the propagations of the waves will no longer be regular, but disturbances of the ring will in this, as in the former case, produce only waves, and not growing confusion. Supposing the ring to consist, not of a single row of large satellites, but a cloud of evenly distributed unconnected particles, we found that such a cloud must have a very small density in order to be permanent, and that this is inconsistent with its outer and inner parts moving with the same angular velocity. Supposing the ring to be fluid and continuous, we found that it will be necessarily broken up into small portions.

"We conclude, therefore, that the rings must consist of disconnected particles; these must be either solid or liquid, but they must be independent. The entire system of rings must, therefore, consist either of a series of many concentric rings each moving with its own velocity and having its own system of waves, or else of a confused multitude of revolving particles not arranged in rings and continually coming into collision with one another.

"Taking the first case, we found that in an indefinite number of possible cases the mutual perturbations of two rings, stable in themselves, might mount up in time to a destructive magnitude, and that such cases must continually occur in an extensive system like that of Saturn, the only retarding cause being the irregularity of the rings.

"The result of long−continued disturbance was found to be the spreading−out of the rings in breadth, the outer rings pressing outward, while the inner rings press inward.

"The final result, therefore, of the mechanical theory is that the only system of rings which can exist is one composed of an indefinite number of unconnected particles, revolving around the planet with different velocities, according to their respective distances. These particles may be arranged in series of narrow rings, or they may move through one another irregularly. In the first case the destruction of the system will be very slow, in the second case it will be more rapid, but there may be a tendency towards arrangement in narrow rings which may retard the process.

"We are not able to ascertain by observation the constitution of the two outer divisions of the system of rings, but the inner ring is certainly transparent, for the limb of Saturn has been observed through it. It is also certain that though the space occupied by the ring is transparent, it is not through the material parts of it that the limb of Saturn is seen, for his limb was observed without distortion; which shows that there was no refraction, and, therefore, that the rays did not pass through a medium at all, but between the solar or liquid particles of which the ring is composed. Here, then, we have an optical argument in favor of the theory of independent particles as the material of the rings. The two outer rings may be of the same nature, but not so exceedingly rare that a ray of light can pass through their whole thickness without encountering one of the particles.

"Finally, the two outer rings have been observed for two hundred years, and it appears, from the careful analysis of all the observations of M. Struve, that the second ring is broader than when first observed, and that its inner edge is nearer the planet than formerly. The inner ring also is suspected to be approaching the planet ever since its discovery in 1850. These appearances seem to indicate the same slow progress of the rings towards separation which we found to be the result of theory, and the remark that the inner edge of the inner ring is more distinct seems to indicate that the approach towards the planet is less rapid near the edge, as we had reason to conjecture. As to the apparent unchangeableness of the exterior diameter of the outer ring, we must remember that the outer rings are certainly far more dense than the inner one, and that a small change in the outer rings must balance a great change in the inner one. It is possible, however, that some of the observed changes may be due to the existence of a resisting medium. If the changes already suspected should be confirmed by repeated observations with the same instruments, it will be worth while to investigate more carefully whether Saturn's rings are permanent or transitory elements of the solar system, and whether in that part of the heavens we see celestial immutability or terrestrial corruption and generation, and the old order giving place to the new before our eyes."[4]

Studies of the Moon

But perhaps the most interesting accomplishments of mathematical astronomy—from a mundane standpoint, at any rate—are those that refer to the earth's own satellite. That seemingly staid body was long ago discovered to have a propensity to gain a little on the earth, appearing at eclipses an infinitesimal moment ahead of time. Astronomers were sorely puzzled by this act of insubordination; but at last Laplace and Lagrange explained it as due to an oscillatory change in the earth's orbit, thus fully exonerating the moon, and seeming to demonstrate the absolute stability of our planetary system, which the moon's misbehavior had appeared to threaten.

This highly satisfactory conclusion was an orthodox belief of celestial mechanics until 1853, when Professor Adams of Neptunian fame, with whom complex analyses were a pastime, reviewed Laplace's calculation, and discovered an error which, when corrected, left about half the moon's acceleration unaccounted for. This was a momentous discrepancy, which at first no one could explain. But presently Professor Helmholtz, the great German physicist, suggested that a key might be found in tidal friction, which, acting as a perpetual brake on the earth's rotation, and affecting not merely the waters but the entire substance of our planet, must in the long sweep of time have changed its rate of rotation. Thus the seeming acceleration of the moon might be accounted for as actual retardation of the earth's rotation—a lengthening of the day instead of a shortening of the month.

Again the earth was shown to be at fault, but this time the moon could not be exonerated, while the estimated stability of our system, instead of being re−established, was quite upset. For the tidal retardation is not an oscillatory change which will presently correct itself, like the orbital wobble, but a perpetual change, acting always in one direction. Unless fully counteracted by some opposing reaction, therefore (as it seems not to be), the effect must be cumulative, the ultimate consequences disastrous. The exact character of these consequences was first estimated by Professor G. H. Darwin in 1879. He showed that tidal friction, in retarding the earth, must also push the moon out from the parent planet on a spiral orbit. Plainly, then, the moon must formerly have been nearer the earth than at present. At some very remote period it must have actually touched the earth; must, in other words, have been thrown off from the then plastic mass of the earth, as a polyp buds out from its parent polyp. At that time the earth was spinning about in a day of from two to four hours.

Now the day has been lengthened to twenty−four hours, and the moon has been thrust out to a distance of a quarter−million miles; but the end is not yet. The same progress of events must continue, till, at some remote period in the future, the day has come to equal the month, lunar tidal action has ceased, and one face of the earth looks out always at the moon with that same fixed stare which even now the moon has been brought to assume towards her parent orb. Should we choose to take even greater liberties with the future, it may be made to appear (though some astronomers dissent from this prediction) that, as solar tidal action still continues, the day must finally exceed the month, and lengthen out little by little towards coincidence with the year; and that the moon meantime must pause in its outward flight, and come swinging back on a descending spiral, until finally, after the lapse of untold aeons, it ploughs and ricochets along the surface of the earth, and plunges to catastrophic destruction.

But even though imagination pause far short of this direful culmination, it still is clear that modern calculations, based on inexorable tidal friction, suffice to revolutionize the views formerly current as to the stability of the planetary system. The eighteenth−century mathematician looked upon this system as a vast celestial machine which had been in existence about six thousand years, and which was destined to run on forever. The analyst of to−day computes both the past and the future of this system in millions instead of thousands of years, yet feels well assured that the solar system offers no contradiction to those laws of growth and decay which seem everywhere to represent the immutable order of nature.

COMETS AND METEORS

Until the mathematician ferreted out the secret, it surely never could have been suspected by any one that the earth's serene attendant,

"That orbed maiden, with white fire laden,
Whom mortals call the moon,"

could be plotting injury to her parent orb. But there is another inhabitant of the skies whose purposes have not been similarly free from popular suspicion. Needless to say I refer to the black sheep of the sidereal family, that "celestial vagabond" the comet.

Time out of mind these wanderers have been supposed to presage war, famine, pestilence, perhaps the destruction of the world. And little wonder. Here is a body which comes flashing out of boundless space into our system, shooting out a pyrotechnic tail some hundreds of millions of miles in length; whirling, perhaps, through the very atmosphere of the sun at a speed of three or four hundred miles a second; then darting off on a hyperbolic orbit that forbids it ever to return, or an elliptical one that cannot be closed for hundreds or thousands of years; the tail meantime pointing always away from the sun, and fading to nothingness as the weird voyager recedes into the spatial void whence it came. Not many times need the advent of such an apparition coincide with the outbreak of a pestilence or the death of a Caesar to stamp the race of comets as an ominous clan in the minds of all superstitious generations.

It is true, a hard blow was struck at the prestige of these alleged supernatural agents when Newton proved that the great comet of 1680 obeyed Kepler's laws in its flight about the sun; and an even harder one when the same visitant came back in 1758, obedient to Halley's prediction, after its three−quarters of a century of voyaging but in the abyss of space. Proved thus to bow to natural law, the celestial messenger could no longer fully, sustain its role. But long−standing notoriety cannot be lived down in a day, and the comet, though proved a "natural" object, was still regarded as a very menacing one for another hundred years or so. It remained for the nineteenth century to completely unmask the pretender and show how egregiously our forebears had been deceived.

The unmasking began early in the century, when Dr. Olbers, then the highest authority on the subject, expressed the opinion that the spectacular tail, which had all along been the comet's chief stock−in−trade as an earth−threatener, is in reality composed of the most filmy vapors, repelled from the cometary body by the sun, presumably through electrical action, with a velocity comparable to that of light. This luminous suggestion was held more or less in abeyance for half a century. Then it was elaborated by Zollner, and particularly by Bredichin, of the Moscow observatory, into what has since been regarded as the most plausible of cometary theories. It is held that comets and the sun are similarly electrified, and hence mutually repulsive. Gravitation vastly outmatches this repulsion in the body of the comet, but yields to it in the case of gases, because electrical force varies with the surface, while gravitation varies only with the mass. From study of atomic weights and estimates of the velocity of thrust of cometary tails, Bredichin concluded that the chief components of the various kinds of tails are hydrogen, hydrocarbons, and the vapor of iron; and spectroscopic analysis goes far towards sustaining these assumptions.

But, theories aside, the unsubstantialness of the comet's tail has been put to a conclusive test. Twice during the nineteenth century the earth has actually plunged directly through one of these threatening appendages—in 1819, and again in 1861, once being immersed to a depth of some three hundred thousand miles in its substance. Yet nothing dreadful happened to us. There was a peculiar glow in the atmosphere, so the more imaginative observers thought, and that was all. After such fiascos the cometary train could never again pose as a world−destroyer.

But the full measure of the comet's humiliation is not yet told. The pyrotechnic tail, composed as it is of portions of the comet's actual substance, is tribute paid the sun, and can never be recovered. Should the obeisance to the sun be many times repeated, the train−forming material will be exhausted, and the comet's chiefest glory will have departed. Such a fate has actually befallen a multitude of comets which Jupiter and the other outlying planets have dragged into our system and helped the sun to hold captive here. Many of these tailless comets were known to the eighteenth− century astronomers, but no one at that time suspected the true meaning of their condition. It was not even known how closely some of them are enchained until the German astronomer Encke, in 1822, showed that one which he had rediscovered, and which has since borne his name, was moving in an orbit so contracted that it must complete its circuit in about three and a half years. Shortly afterwards another comet, revolving in a period of about six years, was discovered by Biela, and given his name. Only two more of these short−period comets were discovered during the first half of last century, but latterly they have been shown to be a numerous family. Nearly twenty are known which the giant Jupiter holds so close that the utmost reach of their elliptical tether does not let them go beyond the orbit of Saturn. These aforetime wanderers have adapted themselves wonderfully to planetary customs, for all of them revolve in the same direction with the planets, and in planes not wide of the ecliptic.

Checked in their proud hyperbolic sweep, made captive in a planetary net, deprived of their trains, these quondam free−lances of the heavens are now mere shadows of their former selves. Considered as to mere bulk, they are very substantial shadows, their extent being measured in hundreds of thousands of miles; but their actual mass is so slight that they are quite at the mercy of the gravitation pulls of their captors. And worse is in store for them. So persistently do sun and planets tug at them that they are doomed presently to be torn into shreds.

Such a fate has already overtaken one of them, under the very eyes of the astronomers, within the relatively short period during which these ill−fated comets have. been observed. In 1832 Biela's comet passed quite near the earth, as astronomers measure distance, and in doing so created a panic on our planet. It did no greater harm than that, of course, and passed on its way as usual. The very next time it came within telescopic hail it was seen to have broken into two fragments. Six years later these fragments were separated by many millions of miles; and in 1852, when the comet was due again, astronomers looked for it in vain. It had been completely shattered.

What had become of the fragments? At that time no one positively knew. But the question was to be answered presently. It chanced that just at this period astronomers were paying much attention to a class of bodies which they had hitherto somewhat neglected, the familiar shooting−stars, or meteors. The studies of Professor Newton, of Yale, and Professor Adams, of Cambridge, with particular reference to the great meteor−shower of November, 1866, which Professor Newton had predicted and shown to be recurrent at intervals of thirty−three years, showed that meteors are not mere sporadic swarms of matter flying at random, but exist in isolated swarms, and sweep about the sun in regular elliptical orbits.

Presently it was shown by the Italian astronomer Schiaparelli that one of these meteor swarms moves in the orbit of a previously observed comet, and other coincidences of the kind were soon forthcoming. The conviction grew that meteor swarms are really the debris of comets; and this conviction became a practical certainty when, in November, 1872, the earth crossed the orbit of the ill−starred Biela, and a shower of meteors came whizzing into our atmosphere in lieu of the lost comet.

And so at last the full secret was out. The awe− inspiring comet, instead of being the planetary body it had all along been regarded, is really nothing more nor less than a great aggregation of meteoric particles, which have become clustered together out in space somewhere, and which by jostling one another or through electrical action become luminous. So widely are the individual particles separated that the cometary body as a whole has been estimated to be thousands of times less dense than the earth's atmosphere at sea− level. Hence the ease with which the comet may be dismembered and its particles strung out into streaming swarms.

So thickly is the space we traverse strewn with this cometary dust that the earth sweeps up, according to Professor Newcomb's estimate, a million tons of it each day. Each individual particle, perhaps no larger than a millet seed, becomes a shooting−star, or meteor, as it burns to vapor in the earth's upper atmosphere. And if one tiny planet sweeps up such masses of this cosmic matter, the amount of it in the entire stretch of our system must be beyond all estimate. What a story it tells of the myriads of cometary victims that have fallen prey to the sun since first he stretched his planetary net across the heavens!

THE FIXED STARS

When Biela's comet gave the inhabitants of the earth such a fright in 1832, it really did not come within fifty millions of miles of us. Even the great comet through whose filmy tail the earth passed in 1861 was itself fourteen millions of miles away. The ordinary mind, schooled to measure space by the tiny stretches of a pygmy planet, cannot grasp the import of such distances; yet these are mere units of measure compared with the vast stretches of sidereal space. Were the comet which hurtles past us at a speed of, say, a hundred miles a second to continue its mad flight unchecked straight into the void of space, it must fly on its frigid way eight thousand years before it could reach the very nearest of our neighbor stars; and even then it would have penetrated but a mere arm's−length into the vistas where lie the dozen or so of sidereal residents that are next beyond. Even to the trained mind such distances are only vaguely imaginable. Yet the astronomer of our century has reached out across this unthinkable void and brought back many a secret which our predecessors thought forever beyond human grasp.

A tentative assault upon this stronghold of the stars was being made by Herschel at the beginning of the century. In 1802 that greatest of observing astronomers announced to the Royal Society his discovery that certain double stars had changed their relative positions towards one another since he first carefully charted them twenty years before. Hitherto it had been supposed that double stars were mere optical effects. Now it became clear that some of them, at any rate, are true "binary systems," linked together presumably by gravitation and revolving about one another. Halley had shown, three−quarters of a century before, that the stars have an actual or "proper" motion in space; Herschel himself had proved that the sun shares this motion with the other stars. Here was another shift of place, hitherto quite unsuspected, to be reckoned with by the astronomer in fathoming sidereal secrets.

Double Stars

When John Herschel, the only son and the worthy successor of the great astronomer, began star−gazing in earnest, after graduating senior wrangler at Cambridge, and making two or three tentative professional starts in other directions to which his versatile genius impelled him, his first extended work was the observation of his father's double stars. His studies, in which at first he had the collaboration of Mr. James South, brought to light scores of hitherto unrecognized pairs, and gave fresh data for the calculation of the orbits of those longer known. So also did the independent researches of F. G. W. Struve, the enthusiastic observer of the famous Russian observatory at the university of Dorpat, and subsequently at Pulkowa. Utilizing data gathered by these observers, M. Savary, of Paris, showed, in 1827, that the observed elliptical orbits of the double stars are explicable by the ordinary laws of gravitation, thus confirming the assumption that Newton's laws apply to these sidereal bodies. Henceforth there could be no reason to doubt that the same force which holds terrestrial objects on our globe pulls at each and every particle of matter throughout the visible universe.

The pioneer explorers of the double stars early found that the systems into which the stars are linked are by no means confined to single pairs. Often three or four stars are found thus closely connected into gravitation systems; indeed, there are all gradations between binary systems and great clusters containing hundreds or even thousands of members. It is known, for example, that the familiar cluster of the Pleiades is not merely an optical grouping, as was formerly supposed, but an actual federation of associated stars, some two thousand five hundred in number, only a few of which are visible to the unaided eve. And the more carefully the motions of the stars are studied, the more evident it becomes that widely separated stars are linked together into infinitely complex systems, as yet but little understood. At the same time, all instrumental advances tend to resolve more and more seemingly single stars into close pairs and minor clusters. The two Herschels between them discovered some thousands of these close multiple systems; Struve and others increased the list to above ten thousand; and Mr. S. W. Burnham, of late years the most enthusiastic and successful of double−star pursuers, added a thousand new discoveries while he was still an amateur in astronomy, and by profession the stenographer of a Chicago court. Clearly the actual number of multiple stars is beyond all present estimate.

The elder Herschel's early studies of double stars were undertaken in the hope that these objects might aid him in ascertaining the actual distance of a star, through measurement of its annual parallax—that is to say, of the angle which the diameter of the earth's orbit would subtend as seen from the star. The expectation was not fulfilled. The apparent shift of position of a star as viewed from opposite sides of the earth's orbit, from which the parallax might be estimated, is so extremely minute that it proved utterly inappreciable, even to the almost preternaturally acute vision of Herschel, with the aid of any instrumental means then at command. So the problem of star distance allured and eluded him to the end, and he died in 1822 without seeing it even in prospect of solution. His estimate of the minimum distance of the nearest star, based though it was on the fallacious test of apparent brilliancy, was a singularly sagacious one, but it was at best a scientific guess, not a scientific measurement.

The Distance of the Stars

Just about this time, however, a great optician came to the aid of the astronomers. Joseph Fraunhofer perfected the refracting telescope, as Herschel had perfected the reflector, and invented a wonderfully accurate "heliometer," or sun−measurer. With the aid of these instruments the old and almost infinitely difficult problem of star distance was solved. In 1838 Bessel announced from the Konigsberg observatory that he had succeeded, after months of effort, in detecting and measuring the parallax of a star. Similar claims had been made often enough before, always to prove fallacious when put to further test; but this time the announcement carried the authority of one of the greatest astronomers of the age, and scepticism was silenced.

Nor did Bessel's achievement long await corroboration. Indeed, as so often happens in fields of discovery, two other workers had almost simultaneously solved the same problem—Struve at Pulkowa, where the great Russian observatory, which so long held the palm over all others, had now been established; and Thomas Henderson, then working at the Cape of Good Hope, but afterwards the Astronomer Royal of Scotland. Henderson's observations had actual precedence in point of time, but Bessel's measurements were so much more numerous and authoritative that he has been uniformly considered as deserving the chief credit of the discovery, which priority of publication secured him.

By an odd chance, the star on which Henderson's observations were made, and consequently the first star the parallax of which was ever measured, is our nearest neighbor in sidereal space, being, indeed, some ten billions of miles nearer than the one next beyond. Yet even this nearest star is more than two hundred thousand times as remote from us as the sun. The sun's light flashes to the earth in eight minutes, and to Neptune in about three and a half hours, but it requires three and a half years to signal Alpha Centauri. And as for the great majority of the stars, had they been blotted out of existence before the Christian era, we of to−day should still receive their light and seem to see them just as we do. When we look up to the sky, we study ancient history; we do not see the stars as they ARE, but as they WERE years, centuries, even millennia ago.

The information derived from the parallax of a star by no means halts with the disclosure of the distance of that body. Distance known, the proper motion of the star, hitherto only to be reckoned as so many seconds of arc, may readily be translated into actual speed of progress; relative brightness becomes absolute lustre, as compared with the sun; and in the case of the double stars the absolute mass of the components may be computed from the laws of gravitation. It is found that stars differ enormously among themselves in all these regards. As to speed, some, like our sun, barely creep through space—compassing ten or twenty miles a second, it is true, yet even at that rate only passing through the equivalent of their own diameter in a day. At the other extreme, among measured stars, is one that moves two hundred miles a second; yet even this "flying star," as seen from the earth, seems to change its place by only about three and a half lunar diameters in a thousand years. In brightness, some stars yield to the sun, while others surpass him as the arc−light surpasses a candle. Arcturus, the brightest measured star, shines like two hundred suns; and even this giant orb is dim beside those other stars which are so distant that their parallax cannot be measured, yet which greet our eyes at first magnitude. As to actual bulk, of which apparent lustre furnishes no adequate test, some stars are smaller than the sun, while others exceed him hundreds or perhaps thousands of times. Yet one and all, so distant are they, remain mere disklike points of light before the utmost powers of the modern telescope.

Revelations of the Spectroscope

All this seems wonderful enough, but even greater things were in store. In 1859 the spectroscope came upon the scene, perfected by Kirchhoff and Bunsen, along lines pointed out by Fraunhofer almost half a century before. That marvellous instrument, by revealing the telltale lines sprinkled across a prismatic spectrum, discloses the chemical nature and physical condition of any substance whose light is submitted to it, telling its story equally well, provided the light be strong enough, whether the luminous substance be near or far—in the same room or at the confines of space. Clearly such an instrument must prove a veritable magic wand in the hands of the astronomer.

Very soon eager astronomers all over the world were putting the spectroscope to the test. Kirchhoff himself led the way, and Donati and Father Secchi in Italy, Huggins and Miller in England, and Rutherfurd in America, were the chief of his immediate followers. The results exceeded the dreams of the most visionary. At the very outset, in 1860, it was shown that such common terrestrial substances as sodium, iron, calcium, magnesium, nickel, barium, copper, and zinc exist in the form of glowing vapors in the sun, and very soon the stars gave up a corresponding secret. Since then the work of solar and sidereal analysis has gone on steadily in the hands of a multitude of workers (prominent among whom, in this country, are Professor Young of Princeton, Professor Langley of Washington, and Professor Pickering of Harvard), and more than half the known terrestrial elements have been definitely located in the sun, while fresh discoveries are in prospect.

It is true the sun also contains some seeming elements that are unknown on the earth, but this is no matter for surprise. The modern chemist makes no claim for his elements except that they have thus far resisted all human efforts to dissociate them; it would be nothing strange if some of them, when subjected to the crucible of the sun, which is seen to vaporize iron, nickel, silicon, should fail to withstand the test. But again, chemistry has by no means exhausted the resources of the earth's supply of raw material, and the substance which sends its message from a star may exist undiscovered in the dust we tread or in the air we breathe. In the year 1895 two new terrestrial elements were discovered; but one of these had for years been known to the astronomer as a solar and suspected as a stellar element, and named helium because of its abundance in the sun. The spectroscope had reached out millions of miles into space and brought back this new element, and it took the chemist a score of years to discover that he had all along had samples of the same substance unrecognized in his sublunary laboratory. There is hardly a more picturesque fact than that in the entire history of science.

But the identity in substance of earth and sun and stars was not more clearly shown than the diversity of their existing physical conditions. It was seen that sun and stars, far from being the cool, earthlike, habitable bodies that Herschel thought them (surrounded by glowing clouds, and protected from undue heat by other clouds), are in truth seething caldrons of fiery liquid, or gas made viscid by condensation, with lurid envelopes of belching flames. It was soon made clear, also, particularly by the studies of Rutherfurd and of Secchi, that stars differ among themselves in exact constitution or condition. There are white or Sirian stars, whose spectrum revels in the lines of hydrogen; yellow or solar stars (our sun being the type), showing various metallic vapors; and sundry red stars, with banded spectra indicative of carbon compounds; besides the purely gaseous stars of more recent discovery, which Professor Pickering had specially studied. Zollner's famous interpretation of these diversities, as indicative of varying stages of cooling, has been called in question as to the exact sequence it postulates, but the general proposition that stars exist under widely varying conditions of temperature is hardly in dispute.

The assumption that different star types mark varying stages of cooling has the further support of modern physics, which has been unable to demonstrate any way in which the sun's radiated energy may be restored, or otherwise made perpetual, since meteoric impact has been shown to be—under existing conditions, at any rate—inadequate. In accordance with the theory of Helmholtz, the chief supply of solar energy is held to be contraction of the solar mass itself; and plainly this must have its limits. Therefore, unless some means as yet unrecognized is restoring the lost energy to the stellar bodies, each of them must gradually lose its lustre, and come to a condition of solidification, seeming sterility, and frigid darkness. In the case of our own particular star, according to the estimate of Lord Kelvin, such a culmination appears likely to occur within a period of five or six million years.

The Astronomy of the Invisible

But by far the strongest support of such a forecast as this is furnished by those stellar bodies which even now appear to have cooled to the final stage of star development and ceased to shine. Of this class examples in miniature are furnished by the earth and the smaller of its companion planets. But there are larger bodies of the same type out in stellar space—veritable "dark stars"—invisible, of course, yet nowadays clearly recognized.

The opening up of this "astronomy of the invisible" is another of the great achievements of the nineteenth century, and again it is Bessel to whom the honor of discovery is due. While testing his stars for parallax; that astute observer was led to infer, from certain unexplained aberrations of motion, that various stars, Sirius himself among the number, are accompanied by invisible companions, and in 1840 he definitely predicated the existence of such "dark stars." The correctness of the inference was shown twenty years later, when Alvan Clark, Jr., the American optician, while testing a new lens, discovered the companion of Sirius, which proved thus to be faintly luminous. Since then the existence of other and quite invisible star companions has been proved incontestably, not merely by renewed telescopic observations, but by the curious testimony of the ubiquitous spectroscope.

One of the most surprising accomplishments of that instrument is the power to record the flight of a luminous object directly in the line of vision. If the luminous body approaches swiftly, its Fraunhofer lines are shifted from their normal position towards the violet end of the spectrum; if it recedes, the lines shift in the opposite direction. The actual motion of stars whose distance is unknown may be measured in this way. But in certain cases the light lines are seen to oscillate on the spectrum at regular intervals. Obviously the star sending such light is alternately approaching and receding, and the inference that it is revolving about a companion is unavoidable. From this extraordinary test the orbital distance, relative mass, and actual speed of revolution of the absolutely invisible body may be determined. Thus the spectroscope, which deals only with light, makes paradoxical excursions into the realm of the invisible. What secrets may the stars hope to conceal when questioned by an instrument of such necromantic power?

But the spectroscope is not alone in this audacious assault upon the strongholds of nature. It has a worthy companion and assistant in the photographic film, whose efficient aid has been invoked by the astronomer even more recently. Pioneer work in celestial photography was, indeed, done by Arago in France and by the elder Draper in America in 1839, but the results then achieved were only tentative, and it was not till forty years later that the method assumed really important proportions. In 1880, Dr. Henry Draper, at Hastings−on−the−Hudson, made the first successful photograph of a nebula. Soon after, Dr. David Gill, at the Cape observatory, made fine photographs of a comet, and the flecks of starlight on his plates first suggested the possibilities of this method in charting the heavens.

Since then star−charting with the film has come virtually to supersede the old method. A concerted effort is being made by astronomers in various parts of the world to make a complete chart of the heavens, and before the close of our century this work will be accomplished, some fifty or sixty millions of visible stars being placed on record with a degree of accuracy hitherto unapproachable. Moreover, other millions of stars are brought to light by the negative, which are too distant or dim to be visible with any telescopic powers yet attained—a fact which wholly discredits all previous inferences as to the limits of our sidereal system. Hence, notwithstanding the wonderful instrumental advances of the nineteenth century, knowledge of the exact form and extent of our universe seems more unattainable than it seemed a century ago.

The Structure of Nebulae

Yet the new instruments, while leaving so much untold, have revealed some vastly important secrets of cosmic structure. In particular, they have set at rest the long−standing doubts as to the real structure and position of the mysterious nebulae—those lazy masses, only two or three of them visible to the unaided eye, which the telescope reveals in almost limitless abundance, scattered everywhere among the stars, but grouped in particular about the poles of the stellar stream or disk which we call the Milky Way.

Herschel's later view, which held that some at least of the nebulae are composed of a "shining fluid," in process of condensation to form stars, was generally accepted for almost half a century. But in 1844, when Lord Rosse's great six−foot reflector—the largest telescope ever yet constructed—was turned on the nebulae, it made this hypothesis seem very doubtful. Just as Galileo's first lens had resolved the Milky Way into stars, just as Herschel had resolved nebulae that resisted all instruments but his own, so Lord Rosse's even greater reflector resolved others that would not yield to Herschel's largest mirror. It seemed a fair inference that with sufficient power, perhaps some day to be attained, all nebulae would yield, hence that all are in reality what Herschel had at first thought them— vastly distant "island universes," composed of aggregations of stars, comparable to our own galactic system.

But the inference was wrong; for when the spectroscope was first applied to a nebula in 1864, by Dr. Huggins, it clearly showed the spectrum not of discrete stars, but of a great mass of glowing gases, hydrogen among others. More extended studies showed, it is true, that some nebulae give the continuous spectrum of solids or liquids, but the different types intermingle and grade into one another. Also, the closest affinity is shown between nebulae and stars. Some nebulae are found to contain stars, singly or in groups, in their actual midst; certain condensed "planetary" nebulae are scarcely to be distinguished from stars of the gaseous type; and recently the photographic film has shown the presence of nebulous matter about stars that to telescopic vision differ in no respect from the generality of their fellows in the galaxy. The familiar stars of the Pleiades cluster, for example, appear on the negative immersed in a hazy blur of light. All in all, the accumulated impressions of the photographic film reveal a prodigality of nebulous matter in the stellar system not hitherto even conjectured.

And so, of course, all question of "island universes" vanishes, and the nebulae are relegated to their true position as component parts of the one stellar system—the one universe—that is open to present human inspection. And these vast clouds of world−stuff have been found by Professor Keeler, of the Lick observatory, to be floating through space at the starlike speed of from ten to thirty−eight miles per second.

The linking of nebulae with stars, so clearly evidenced by all these modern observations, is, after all, only the scientific corroboration of what the elder Herschel's later theories affirmed. But the nebulae have other affinities not until recently suspected; for the spectra of some of them are practically identical with the spectra of certain comets. The conclusion seems warranted that comets are in point of fact minor nebulae that are drawn into our system; or, putting it otherwise, that the telescopic nebulae are simply gigantic distant comets.

Lockyer's Meteoric Hypothesis

Following up the surprising clews thus suggested, Sir Norman Lockyer, of London, has in recent years elaborated what is perhaps the most comprehensive cosmogonic guess that has ever been attempted. His theory, known as the "meteoric hypothesis," probably bears the same relation to the speculative thought of our time that the nebular hypothesis of Laplace bore to that of the eighteenth century. Outlined in a few words, it is an attempt to explain all the major phenomena of the universe as due, directly or indirectly, to the gravitational impact of such meteoric particles, or specks of cosmic dust, as comets are composed of. Nebulae are vast cometary clouds, with particles more or less widely separated, giving off gases through meteoric collisions, internal or external, and perhaps glowing also with electrical or phosphorescent light. Gravity eventually brings the nebular particles into closer aggregations, and increased collisions finally vaporize the entire mass, forming planetary nebulae and gaseous stars. Continued condensation may make the stellar mass hotter and more luminous for a time, but eventually leads to its liquefaction, and ultimate consolidation— the aforetime nebulae becoming in the end a dark or planetary star.

The exact correlation which Lockyer attempts to point out between successive stages of meteoric condensation and the various types of observed stellar bodies does not meet with unanimous acceptance. Mr. Ranyard, for example, suggests that the visible nebulae may not be nascent stars, but emanations from stars, and that the true pre−stellar nebulae are invisible until condensed to stellar proportions. But such details aside, the broad general hypothesis that all the bodies of the universe are, so to speak, of a single species— that nebulae (including comets), stars of all types, and planets, are but varying stages in the life history of a single race or type of cosmic organisms—is accepted by the dominant thought of our time as having the highest warrant of scientific probability.

All this, clearly, is but an amplification of that nebular hypothesis which, long before the spectroscope gave us warrant to accurately judge our sidereal neighbors, had boldly imagined the development of stars out of nebulae and of planets out of stars. But Lockyer's hypothesis does not stop with this. Having traced the developmental process from the nebular to the dark star, it sees no cause to abandon this dark star to its fate by assuming, as the original speculation assumed, that this is a culminating and final stage of cosmic existence. For the dark star, though its molecular activities have come to relative stability and impotence, still retains the enormous potentialities of molar motion; and clearly, where motion is, stasis is not. Sooner or later, in its ceaseless flight through space, the dark star must collide with some other stellar body, as Dr. Croll imagines of the dark bodies which his "pre−nebular theory" postulates. Such collision may be long delayed; the dark star may be drawn in comet−like circuit about thousands of other stellar masses, and be hurtled on thousands of diverse parabolic or elliptical orbits, before it chances to collide—but that matters not: "billions are the units in the arithmetic of eternity," and sooner or later, we can hardly doubt, a collision must occur. Then without question the mutual impact must shatter both colliding bodies into vapor, or vapor combined with meteoric fragments; in short, into a veritable nebula, the matrix of future worlds. Thus the dark star, which is the last term of one series of cosmic changes, becomes the first term of another series—at once a post−nebular and a pre−nebular condition; and the nebular hypothesis, thus amplified, ceases to be a mere linear scale, and is rounded out to connote an unending series of cosmic cycles, more nearly satisfying the imagination.

In this extended view, nebulae and luminous stars are but the infantile and adolescent stages of the life history of the cosmic individual; the dark star, its adult stage, or time of true virility. Or we may think of the shrunken dark star as the germ−cell, the pollen−grain, of the cosmic organism. Reduced in size, as becomes a germ−cell, to a mere fraction of the nebular body from which it sprang, it yet retains within its seemingly non− vital body all the potentialities of the original organism, and requires only to blend with a fellow−cell to bring a new generation into being. Thus may the cosmic race, whose aggregate census makes up the stellar universe, be perpetuated—individual solar systems, such as ours, being born, and growing old, and dying to live again in their descendants, while the universe as a whole maintains its unified integrity throughout all these internal mutations—passing on, it may be, by infinitesimal stages, to a culmination hopelessly beyond human comprehension.

III. THE NEW SCIENCE OF PALEONTOLOGY. WILLIAM SMITH AND FOSSIL SHELLS

Ever since Leonardo da Vinci first recognized the true character of fossils, there had been here and there a man who realized that the earth's rocky crust is one gigantic mausoleum. Here and there a dilettante had filled his cabinets with relics from this monster crypt; here and there a philosopher had pondered over them—questioning whether perchance they had once been alive, or whether they were not mere abortive souvenirs of that time when the fertile matrix of the earth was supposed to have

"teemed at a birth
Innumerous living creatures, perfect forms,
Limbed and full grown."

Some few of these philosophers—as Robert Hooke and Steno in the seventeenth century, and Moro, Leibnitz, Buffon, Whitehurst, Werner, Hutton, and others in the eighteenth—had vaguely conceived the importance of fossils as records of the earth's ancient history, but the wisest of them no more suspected the full import of the story written in the rocks than the average stroller in a modern museum suspects the meaning of the hieroglyphs on the case of a mummy.

It was not that the rudiments of this story are so very hard to decipher—though in truth they are hard enough—but rather that the men who made the attempt had all along viewed the subject through an atmosphere of preconception, which gave a distorted image. Before this image could be corrected it was necessary that a man should appear who could see without prejudice, and apply sound common−sense to what he saw. And such a man did appear towards the close of the century, in the person of William Smith, the English surveyor. He was a self−taught man, and perhaps the more independent for that, and he had the gift, besides his sharp eyes and receptive mind, of a most tenacious memory. By exercising these faculties, rare as they are homely, he led the way to a science which was destined, in its later developments, to shake the structure of established thought to its foundations.

Little enough did William Smith suspect, however, that any such dire consequences were to come of his act when he first began noticing the fossil shells that here and there are to be found in the stratified rocks and soils of the regions over which his surveyor's duties led him. Nor, indeed, was there anything of such apparent revolutionary character in the facts which he unearthed; yet in their implications these facts were the most disconcerting of any that had been revealed since the days of Copernicus and Galileo. In its bald essence, Smith's discovery was simply this: that the fossils in the rocks, instead of being scattered haphazard, are arranged in regular systems, so that any given stratum of rock is labelled by its fossil population; and that the order of succession of such groups of fossils is always the same in any vertical series of strata in which they occur. That is to say, if fossil A underlies fossil B in any given region, it never overlies it in any other series; though a kind of fossils found in one set of strata may be quite omitted in another. Moreover, a fossil once having disappeared never reappears in any later stratum.

From these novel facts Smith drew the commonsense inference that the earth had had successive populations of creatures, each of which in its turn had become extinct. He partially verified this inference by comparing the fossil shells with existing species of similar orders, and found that such as occur in older strata of the rocks had no counterparts among living species. But, on the whole, being eminently a practical man, Smith troubled himself but little about the inferences that might be drawn from his facts. He was chiefly concerned in using the key he had discovered as an aid to the construction of the first geological map of England ever attempted, and he left to others the untangling of any snarls of thought that might seem to arise from his discovery of the succession of varying forms of life on the globe.

He disseminated his views far and wide, however, in the course of his journeyings—quite disregarding the fact that peripatetics went out of fashion when the printing−press came in—and by the beginning of the nineteenth century he had begun to have a following among the geologists of England. It must not for a moment be supposed, however, that his contention regarding the succession of strata met with immediate or general acceptance. On the contrary, it was most bitterly antagonized. For a long generation after the discovery was made, the generality of men, prone as always to strain at gnats and swallow camels, preferred to believe that the fossils, instead of being deposited in successive ages, had been swept all at once into their present positions by the current of a mighty flood—and that flood, needless to say, the Noachian deluge. Just how the numberless successive strata could have been laid down in orderly sequence to the depth of several miles in one such fell cataclysm was indeed puzzling, especially after it came to be admitted that the heaviest fossils were not found always at the bottom; but to doubt that this had been done in some way was rank heresy in the early days of the nineteenth century.

CUVIER AND FOSSIL VERTEBRATES

But once discovered, William Smith's unique facts as to the succession of forms in the rocks would not down. There was one most vital point, however, regarding which the inferences that seem to follow from these facts needed verification—the question, namely, whether the disappearance of a fauna from the register in the rocks really implies the extinction of that fauna. Everything really depended upon the answer to that question, and none but an accomplished naturalist could answer it with authority. Fortunately, the most authoritative naturalist of the time, George Cuvier, took the question in hand—not, indeed, with the idea of verifying any suggestion of Smith's, but in the course of his own original studies—at the very beginning of the century, when Smith's views were attracting general attention.

Cuvier and Smith were exact contemporaries, both men having been born in 1769, that "fertile year" which gave the world also Chateaubriand, Von Humboldt, Wellington, and Napoleon. But the French naturalist was of very different antecedents from the English surveyor. He was brilliantly educated, had early gained recognition as a scientist, and while yet a young man had come to be known as the foremost comparative anatomist of his time. It was the anatomical studies that led him into the realm of fossils. Some bones dug out of the rocks by workmen in a quarry were brought to his notice, and at once his trained eye told him that they were different from anything he had seen before. Hitherto such bones, when not entirely ignored, had been for the most part ascribed to giants of former days, or even to fallen angels. Cuvier soon showed that neither giants nor angels were in question, but elephants of an unrecognized species. Continuing his studies, particularly with material gathered from gypsum beds near Paris, he had accumulated, by the beginning of the nineteenth century, bones of about twenty−five species of animals that he believed to be different from any now living on the globe.

The fame of these studies went abroad, and presently fossil bones poured in from all sides, and Cuvier's conviction that extinct forms of animals are represented among the fossils was sustained by the evidence of many strange and anomalous forms, some of them of gigantic size. In 1816 the famous Ossements Fossiles, describing these novel objects, was published, and vertebrate paleontology became a science. Among other things of great popular interest the book contained the first authoritative description of the hairy elephant, named by Cuvier the mammoth, the remains of which bad been found embedded in a mass of ice in Siberia in 1802, so wonderfully preserved that the dogs of the Tungusian fishermen actually ate its flesh. Bones of the same species had been found in Siberia several years before by the naturalist Pallas, who had also found the carcass of a rhinoceros there, frozen in a mud−bank; but no one then suspected that these were members of an extinct population—they were supposed to be merely transported relics of the flood.

Cuvier, on the other hand, asserted that these and the other creatures he described had lived and died in the region where their remains were found, and that most of them have no living representatives upon the globe. This, to be sure, was nothing more than William Smith had tried all along to establish regarding lower forms of life; but flesh and blood monsters appeal to the imagination in a way quite beyond the power of mere shells; so the announcement of Cuvier's discoveries aroused the interest of the entire world, and the Ossements Fossiles was accorded a popular reception seldom given a work of technical science—a reception in which the enthusiastic approval of progressive geologists was mingled with the bitter protests of the conservatives.

"Naturalists certainly have neither explored all the continents," said Cuvier, "nor do they as yet even know all the quadrupeds of those parts which have been explored. New species of this class are discovered from time to time; and those who have not examined with attention all the circumstances belonging to these discoveries may allege also that the unknown quadrupeds, whose fossil bones have been found in the strata of the earth, have hitherto remained concealed in some islands not yet discovered by navigators, or in some of the vast deserts which occupy the middle of Africa, Asia, the two Americas, and New Holland.

"But if we carefully attend to the kind of quadrupeds that have been recently discovered, and to the circumstances of their discovery, we shall easily perceive that there is very little chance indeed of our ever finding alive those which have only been seen in a fossil state.

"Islands of moderate size, and at a considerable distance from the large continents, have very few quadrupeds. These must have been carried to them from other countries. Cook and Bougainville found no other quadrupeds besides hogs and dogs in the South Sea Islands; and the largest quadruped of the West India Islands, when first discovered, was the agouti, a species of the cavy, an animal apparently between the rat and the rabbit.

"It is true that the great continents, as Asia, Africa, the two Americas, and New Holland, have large quadrupeds, and, generally speaking, contain species common to each; insomuch, that upon discovering countries which are isolated from the rest of the world, the animals they contain of the class of quadruped were found entirely different from those which existed in other countries. Thus, when the Spaniards first penetrated into South America, they did not find it to contain a single quadruped exactly the same with those of Europe, Asia, and Africa. The puma, the jaguar, the tapir, the capybara, the llama, or glama, and vicuna, and the whole tribe of sapajous, were to them entirely new animals, of which they had not the smallest idea....

"If there still remained any great continent to be discovered, we might perhaps expect to be made acquainted with new species of large quadrupeds, among which some might be found more or less similar to those of which we find the exuviae in the bowels of the earth. But it is merely sufficient to glance the eye over the maps of the world and observe the innumerable directions in which navigators have traversed the ocean, in order to be satisfied that there does not remain any large land to be discovered, unless it may be situated towards the Antarctic Pole, where eternal ice necessarily forbids the existence of animal life."[1]

Cuvier then points out that the ancients were well acquainted with practically all the animals on the continents of Europe, Asia, and Africa now known to scientists. He finds little grounds, therefore, for belief in the theory that at one time there were monstrous animals on the earth which it was necessary to destroy in order that the present fauna and men might flourish. After reviewing these theories and beliefs in detail, he takes up his Inquiry Respecting the Fabulous Animals of the Ancients. "It is easy," he says, "to reply to the foregoing objections, by examining the descriptions that are left us by the ancients of those unknown animals, and by inquiring into their origins. Now that the greater number of these animals have an origin, the descriptions given of them bear the most unequivocal marks; as in almost all of them we see merely the different parts of known animals united by an unbridled imagination, and in contradiction to every established law of nature."[2]

Having shown how the fabulous monsters of ancient times and of foreign nations, such as the Chinese, were simply products of the imagination, having no prototypes in nature, Cuvier takes up the consideration of the difficulty of distinguishing the fossil bones of quadrupeds.

We shall have occasion to revert to this part of Cuvier's paper in another connection. Here it suffices to pass at once to the final conclusion that the fossil bones in question are the remains of an extinct fauna, the like of which has no present−day representation on the earth. Whatever its implications, this conclusion now seemed to Cuvier to be fully established.

In England the interest thus aroused was sent to fever−heat in 1821 by the discovery of abundant beds of fossil bones in the stalagmite−covered floor of a cave at Kirkdale, Yorkshire which went to show that England, too, had once had her share of gigantic beasts. Dr. Buckland, the incumbent of the chair of geology at Oxford, and the most authoritative English geologist of his day, took these finds in hand and showed that the bones belonged to a number of species, including such alien forms as elephants, rhinoceroses, hippopotami, and hyenas. He maintained that all of these creatures had actually lived in Britain, and that the caves in which their bones were found had been the dens of hyenas.

The claim was hotly disputed, as a matter of course. As late as 1827 books were published denouncing Buckland, doctor of divinity though he was, as one who had joined in an "unhallowed cause," and reiterating the old cry that the fossils were only remains of tropical species washed thither by the deluge. That they were found in solid rocks or in caves offered no difficulty, at least not to the fertile imagination of Granville Penn, the leader of the conservatives, who clung to the old idea of Woodward and Cattcut that the deluge had dissolved the entire crust of the earth to a paste, into which the relics now called fossils had settled. The caves, said Mr. Penn, are merely the result of gases given off by the carcasses during decomposition— great air−bubbles, so to speak, in the pasty mass, becoming caverns when the waters receded and the paste hardened to rocky consistency.

But these and such−like fanciful views were doomed even in the day of their utterance. Already in 1823 other gigantic creatures, christened ichthyosaurus and plesiosaurus by Conybeare, had been found in deeper strata of British rocks; and these, as well as other monsters whose remains were unearthed in various parts of the world, bore such strange forms that even the most sceptical could scarcely hope to find their counterparts among living creatures. Cuvier's contention that all the larger vertebrates of the existing age are known to naturalists was borne out by recent explorations, and there seemed no refuge from the conclusion that the fossil records tell of populations actually extinct. But if this were admitted, then Smith's view that there have been successive rotations of population could no longer be denied. Nor could it be in doubt that the successive faunas, whose individual remains have been preserved in myriads, representing extinct species by thousands and tens of thousands, must have required vast periods of time for the production and growth of their countless generations.

As these facts came to be generally known, and as it came to be understood in addition that the very matrix of the rock in which fossils are imbedded is in many cases one gigantic fossil, composed of the remains of microscopic forms of life, common−sense, which, after all, is the final tribunal, came to the aid of belabored science. It was conceded that the only tenable interpretation of the record in the rocks is that numerous populations of creatures, distinct from one another and from present forms, have risen and passed away; and that the geologic ages in which these creatures lived were of inconceivable length. The rank and file came thus, with the aid of fossil records, to realize the import of an idea which James Hutton, and here and there another thinker, had conceived with the swift intuition of genius long before the science of paleontology came into existence. The Huttonian proposition that time is long had been abundantly established, and by about the close of the first third of the last century geologists had begun to speak of "ages" and "untold aeons of time" with a familiarity which their predecessors had reserved for days and decades.

CHARLES LYELL COMBATS CATASTROPHISM

And now a new question pressed for solution. If the earth has been inhabited by successive populations of beings now extinct, how have all these creatures been destroyed? That question, however, seemed to present no difficulties. It was answered out of hand by the application of an old idea. All down the centuries, whatever their varying phases of cosmogonic thought, there had been ever present the idea that past times were not as recent times; that in remote epochs the earth had been the scene of awful catastrophes that have no parallel in "these degenerate days." Naturally enough, this thought, embalmed in every cosmogonic speculation of whatever origin, was appealed to in explanation of the destruction of these hitherto unimagined hosts, which now, thanks to science, rose from their abysmal slumber as incontestable, but also as silent and as thought−provocative, as Sphinx or pyramid. These ancient hosts, it was said, have been exterminated at intervals of odd millions of years by the recurrence of catastrophes of which the Mosaic deluge is the latest, but perhaps not the last.

This explanation had fullest warrant of scientific authority. Cuvier had prefaced his classical work with a speculative disquisition whose very title (Discours sur les Revolutions du Globe) is ominous of catastrophism, and whose text fully sustains the augury. And Buckland, Cuvier's foremost follower across the Channel, had gone even beyond the master, naming the work in which he described the Kirkdale fossils, Reliquiae Diluvianae, or Proofs of a Universal Deluge.

Both these authorities supposed the creatures whose remains they studied to have perished suddenly in the mighty flood whose awful current, as they supposed, gouged out the modern valleys and hurled great blocks of granite broadcast over the land. And they invoked similar floods for the extermination of previous populations.

It is true these scientific citations had met with only qualified approval at the time of their utterance, because then the conservative majority of mankind did not concede that there had been a plurality of populations or revolutions; but now that the belief in past geologic ages had ceased to be a heresy, the recurring catastrophes of the great paleontologists were accepted with acclaim. For the moment science and tradition were at one, and there was a truce to controversy, except indeed in those outlying skirmish−lines of thought whither news from headquarters does not permeate till it has become ancient history at its source.

The truce, however, was not for long. Hardly had contemporary thought begun to adjust itself to the conception of past ages of incomprehensible extent, each terminated by a catastrophe of the Noachian type, when a man appeared who made the utterly bewildering assertion that the geological record, instead of proving numerous catastrophic revolutions in the earth's past history, gives no warrant to the pretensions of any universal catastrophe whatever, near or remote.

This iconoclast was Charles Lyell, the Scotchman, who was soon to be famous as the greatest geologist of his time. As a young man he had become imbued with the force of the Huttonian proposition, that present causes are one with those that produced the past changes of the globe, and he carried that idea to what he conceived to be its logical conclusion. To his mind this excluded the thought of catastrophic changes in either inorganic or organic worlds.

But to deny catastrophism was to suggest a revolution in current thought. Needless to say, such revolution could not be effected without a long contest. For a score of years the matter was argued pro and con., often with most unscientific ardor. A mere outline of the controversy would fill a volume; yet the essential facts with which Lyell at last established his proposition, in its bearings on the organic world, may be epitomized in a few words. The evidence which seems to tell of past revolutions is the apparently sudden change of fossils from one stratum to another of the rocks. But Lyell showed that this change is not always complete. Some species live on from one alleged epoch into the next. By no means all the contemporaries of the mammoth are extinct, and numerous marine forms vastly more ancient still have living representatives.

Moreover, the blanks between strata in any particular vertical series are amply filled in with records in the form of thick strata in some geographically distant series. For example, in some regions Silurian rocks are directly overlaid by the coal measures; but elsewhere this sudden break is filled in with the Devonian rocks that tell of a great "age of fishes." So commonly are breaks in the strata in one region filled up in another that we are forced to conclude that the record shown by any single vertical series is of but local significance— telling, perhaps, of a time when that particular sea−bed oscillated above the water−line, and so ceased to receive sediment until some future age when it had oscillated back again. But if this be the real significance of the seemingly sudden change from stratum to stratum, then the whole case for catastrophism is hopelessly lost; for such breaks in the strata furnish the only suggestion geology can offer of sudden and catastrophic changes of wide extent.

Let us see how Lyell elaborates these ideas, particularly with reference to the rotation of species.[2]

"I have deduced as a corollary," he says, "that the species existing at any particular period must, in the course of ages, become extinct, one after the other. 'They must die out,' to borrow an emphatic expression from Buffon, 'because Time fights against them.' If the views which I have taken are just, there will be no difficulty in explaining why the habitations of so many species are now restrained within exceeding narrow limits. Every local revolution tends to circumscribe the range of some species, while it enlarges that of others; and if we are led to infer that new species originate in one spot only, each must require time to diffuse itself over a wide area. It will follow, therefore, from the adoption of our hypothesis that the recent origin of some species and the high antiquity of others are equally consistent with the general fact of their limited distribution, some being local because they have not existed long enough to admit of their wide dissemination; others, because circumstances in the animate or inanimate world have occurred to restrict the range within which they may once have obtained. . . .

"If the reader should infer, from the facts laid before him, that the successive extinction of animals and plants may be part of the constant and regular course of nature, he will naturally inquire whether there are any means provided for the repair of these losses? Is it possible as a part of the economy of our system that the habitable globe should to a certain extent become depopulated, both in the ocean and on the land, or that the variety of species should diminish until some new era arrives when a new and extraordinary effort of creative energy is to be displayed? Or is it possible that new species can be called into being from time to time, and yet that so astonishing a phenomenon can escape the naturalist?

"In the first place, it is obviously more easy to prove that a species once numerously represented in a given district has ceased to be than that some other which did not pre−exist had made its appearance—assuming always, for reasons before stated, that single stocks only of each animal and plant are originally created, and that individuals of new species did not suddenly start up in many different places at once.

"So imperfect has the science of natural history remained down to our own times that, within the memory of persons now living, the numbers of known animals and plants have doubled, or even quadrupled, in many classes. New and often conspicuous species are annually discovered in parts of the old continent long inhabited by the most civilized nations. Conscious, therefore, of the limited extent of our information, we always infer, when such discoveries are made, that the beings in question bad previously eluded our research, or had at least existed elsewhere, and only migrated at a recent period into the territories where we now find them.

"What kind of proofs, therefore, could we reasonably expect to find of the origin at a particular period of a new species?

"Perhaps, it may be said in reply, that within the last two or three centuries some forest tree or new quadruped might have been observed to appear suddenly in those parts of England or France which had been most thoroughly investigated—that naturalists might have been able to show that no such being inhabited any other region of the globe, and that there was no tradition of anything similar having been observed in the district where it had made its appearance.

"Now, although this objection may seem plausible, yet its force will be found to depend entirely on the rate of fluctuation which we suppose to prevail in the animal world, and on the proportions which such conspicuous subjects of the animal and vegetable kingdoms bear to those which are less known and escape our observation. There are perhaps more than a million species of plants and animals, exclusive of the microscopic and infusory animalcules, now inhabiting the terraqueous globe, so that if only one of these were to become extinct annually, and one new one were to be every year called into being, much more than a million of years might be required to bring about a complete revolution of organic life.

"I am not hazarding at present any hypothesis as to the probable rate of change, but none will deny that when the annual birth and the annual death of one species on the globe is proposed as a mere speculation, this, at least, is to imagine no slight degree of instability in the animate creation. If we divide the surface of the earth into twenty regions of equal area, one of these might comprehend a space of land and water about equal in dimensions to Europe, and might contain a twentieth part of the million of species which may be assumed to exist in the animal kingdom. In this region one species only could, according to the rate of mortality before assumed, perish in twenty years, or only five out of fifty thousand in the course of a century. But as a considerable portion of the whole world belongs to the aquatic classes, with which we have a very imperfect acquaintance, we must exclude them from our consideration, and, if they constitute half of the entire number, then one species only might be lost in forty years among the terrestrial tribes. Now the mammalia, whether terrestrial or aquatic, bear so small a proportion to other classes of animals, forming less, perhaps, than a thousandth part of a whole, that, if the longevity of species in the different orders were equal, a vast period must elapse before it would come to the turn of this conspicuous class to lose one of their number. If one species only of the whole animal kingdom died out in forty years, no more than one mammifer might disappear in forty thousand years, in a region of the dimensions of Europe.

"It is easy, therefore, to see that in a small portion of such an area, in countries, for example, of the size of England and France, periods of much greater duration must elapse before it would be possible to authenticate the first appearance of one of the larger plants or animals, assuming the annual birth and death of one species to be the rate of vicissitude in the animal creation throughout the world."[3]

In a word, then, said Lyell, it becomes clear that the numberless species that have been exterminated in the past have died out one by one, just as individuals of a species die, not in vast shoals; if whole populations have passed away, it has been not by instantaneous extermination, but by the elimination of a species now here, now there, much as one generation succeeds another in the life history of any single species. The causes which have brought about such gradual exterminations, and in the long lapse of ages have resulted in rotations of population, are the same natural causes that are still in operation. Species have died out in the past as they are dying out in the present, under influence of changed surroundings, such as altered climate, or the migration into their territory of more masterful species. Past and present causes are one—natural law is changeless and eternal.

Such was the essence of the Huttonian doctrine, which Lyell adopted and extended, and with which his name will always be associated. Largely through his efforts, though of course not without the aid of many other workers after a time, this idea—the doctrine of uniformitarianism, it came to be called—became the accepted dogma of the geologic world not long after the middle of the nineteenth century. The catastrophists, after clinging madly to their phantom for a generation, at last capitulated without terms: the old heresy became the new orthodoxy, and the way was paved for a fresh controversy.

THE ORIGIN OF SPECIES

The fresh controversy followed quite as a matter of course. For the idea of catastrophism had not concerned the destruction of species merely, but their introduction as well. If whole faunas had been extirpated suddenly, new faunas had presumably been introduced with equal suddenness by special creation; but if species die out gradually, the introduction of new species may be presumed to be correspondingly gradual. Then may not the new species of a later geological epoch be the modified lineal descendants of the extinct population of an earlier epoch?

The idea that such might be the case was not new. It had been suggested when fossils first began to attract conspicuous attention; and such sagacious thinkers as Buffon and Kant and Goethe and Erasmus Darwin had been disposed to accept it in the closing days of the eighteenth century. Then, in 1809, it had been contended for by one of the early workers in systematic paleontology—Jean Baptiste Lamarck, who had studied the fossil shells about Paris while Cuvier studied the vertebrates, and who had been led by these studies to conclude that there had been not merely a rotation but a progression of life on the globe. He found the fossil shells—the fossils of invertebrates, as he himself had christened them—in deeper strata than Cuvier's vertebrates; and he believed that there had been long ages when no higher forms than these were in existence, and that in successive ages fishes, and then reptiles, had been the highest of animate creatures, before mammals, including man, appeared. Looking beyond the pale of his bare facts, as genius sometimes will, he had insisted that these progressive populations had developed one from another, under influence of changed surroundings, in unbroken series.

Of course such a thought as this was hopelessly misplaced in a generation that doubted the existence of extinct species, and hardly less so in the generation that accepted catastrophism; but it had been kept alive by here and there an advocate like Geoffrey Saint−Hilaire, and now the banishment of catastrophism opened the way for its more respectful consideration. Respectful consideration was given it by Lyell in each recurring edition of his Principles, but such consideration led to its unqualified rejection. In its place Lyell put forward a modified hypothesis of special creation. He assumed that from time to time, as the extirpation of a species had left room, so to speak, for a new species, such new species had been created de novo; and he supposed that such intermittent, spasmodic impulses of creation manifest themselves nowadays quite as frequently as at any time in the past. He did not say in so many words that no one need be surprised to−day were he to see a new species of deer, for example, come up out of the ground before him, "pawing to get free," like Milton's lion, but his theory implied as much. And that theory, let it be noted, was not the theory of Lyell alone, but of nearly all his associates in the geologic world. There is perhaps no other fact that will bring home to one so vividly the advance in thought of our own generation as the recollection that so crude, so almost unthinkable a conception could have been the current doctrine of science less than half a century ago.

This theory of special creation, moreover, excluded the current doctrine of uniformitarianism as night excludes day, though most thinkers of the time did not seem to be aware of the incompatibility of the two ideas. It may be doubted whether even Lyell himself fully realized it. If he did, he saw no escape from the dilemma, for it seemed to him that the record in the rocks clearly disproved the alternative Lamarckian hypothesis. And almost with one accord the paleontologists of the time sustained the verdict. Owen, Agassiz, Falconer, Barrande, Pictet, Forbes, repudiated the idea as unqualifiedly as their great predecessor Cuvier had done in the earlier generation. Some of them did, indeed, come to believe that there is evidence of a progressive development of life in the successive ages, but no such graded series of fossils had been discovered as would give countenance to the idea that one species had ever been transformed into another. And to nearly every one this objection seemed insuperable.

But in 1859 appeared a book which, though not dealing primarily with paleontology, yet contained a chapter that revealed the geological record in an altogether new light. The book was Charles Darwin's Origin of Species, the chapter that wonderful citation of the "Imperfections of the Geological Record." In this epoch−making chapter Darwin shows what conditions must prevail in any given place in order that fossils shall be formed, how unusual such conditions are, and how probable it is that fossils once imbedded in sediment of a sea−bed will be destroyed by metamorphosis of the rocks, or by denudation when the strata are raised above the water−level. Add to this the fact that only small territories of the earth have been explored geologically, he says, and it becomes clear that the paleontological record as we now possess it shows but a mere fragment of the past history of organisms on the earth. It is a history "imperfectly kept and written in a changing dialect. Of this history we possess the last volume alone, relating only to two or three countries. Of this volume only here and there a short chapter has been preserved, and of each page only here and there a few lines." For a paleontologist to dogmatize from such a record would be as rash, he thinks, as "for a naturalist to land for five minutes on a barren point of Australia and then discuss the number and range of its productions."

This citation of observations, which when once pointed out seemed almost self−evident, came as a revelation to the geological world. In the clarified view now possible old facts took on a new meaning. It was recalled that

Cuvier had been obliged to establish a new order for some of the first fossil creatures he examined, and that Buckland had noted that the nondescript forms were intermediate in structure between allied existing orders. More recently such intermediate forms had been discovered over and over; so that, to name but one example, Owen had been able, with the aid of extinct species, to "dissolve by gradations the apparently wide interval between the pig and the camel." Owen, moreover, had been led to speak repeatedly of the "generalized forms" of extinct animals, and Agassiz had called them "synthetic or prophetic types," these terms clearly implying "that such forms are in fact intermediate or connecting links." Darwin himself had shown some years before that the fossil animals of any continent are closely related to the existing animals of that continent—edentates predominating, for example, in South America, and marsupials in Australia. Many observers had noted that recent strata everywhere show a fossil fauna more nearly like the existing one than do more ancient strata; and that fossils from any two consecutive strata are far more closely related to each other than are the fossils of two remote formations, the fauna of each geological formation being, indeed, in a wide view, intermediate between preceding and succeeding faunas.

So suggestive were all these observations that Lyell, the admitted leader of the geological world, after reading Darwin's citations, felt able to drop his own crass explanation of the introduction of species and adopt the transmutation hypothesis, thus rounding out the doctrine of uniformitarianism to the full proportions in which Lamarck had conceived it half a century before. Not all paleontologists could follow him at once, of course; the proof was not yet sufficiently demonstrative for that; but all were shaken in the seeming security of their former position, which is always a necessary stage in the progress of thought. And popular interest in the matter was raised to white heat in a twinkling.

So, for the third time in this first century of its existence, paleontology was called upon to play a leading role in a controversy whose interest extended far beyond the bounds of staid truth−seeking science. And the controversy waged over the age of the earth had not been more bitter, that over catastrophism not more acrimonious, than that which now raged over the question of the transmutation of species. The question had implications far beyond the bounds of paleontology, of course. The main evidence yet presented had been drawn from quite other fields, but by common consent the record in the rocks might furnish a crucial test of the truth or falsity of the hypothesis. "He who rejects this view of the imperfections of the geological record," said Darwin, "will rightly reject the whole theory."

With something more than mere scientific zeal, therefore, paleontologists turned anew to the records in the rocks, to inquire what evidence in proof or refutation might be found in unread pages of the "great stone book." And, as might have been expected, many minds being thus prepared to receive new evidence, such evidence was not long withheld.

FOSSIL MAN

Indeed, at the moment of Darwin's writing a new and very instructive chapter of the geologic record was being presented to the public—a chapter which for the first time brought man into the story. In 1859 Dr. Falconer, the distinguished British paleontologist, made a visit to Abbeville, in the valley of the Somme, incited by reports that for a decade before bad been sent out from there by M. Boucher de Perthes. These reports had to do with the alleged finding of flint implements, clearly the work of man, in undisturbed gravel− beds, in the midst of fossil remains of the mammoth and other extinct animals. What Falconer saw there and what came of his visit may best be told in his own words:

"In September of 1856 I made the acquaintance of my distinguished friend M. Boucher de Perthes," wrote Dr. Falconer, "on the introduction of M. Desnoyers at Paris, when he presented to me the earlier volume of his Antiquites celtiques, etc., with which I thus became acquainted for the first time. I was then fresh from the examination of the Indian fossil remains of the valley of the Jumna; and the antiquity of the human race being a subject of interest to both, we conversed freely about it, each from a different point of view. M. de Perthes invited me to visit Abbeville, in order to examine his antediluvian collection, fossil and geological, gleaned from the valley of the Somme. This I was unable to accomplish then, but I reserved it for a future occasion.

"In October, 1856, having determined to proceed to Sicily, I arranged by correspondence with M. Boucher de Perthes to visit Abbeville on my journey through France. I was at the time in constant communication with Mr. Prestwich about the proofs of the antiquity of the human race yielded by the Broxham Cave, in which he took a lively interest; and I engaged to communicate to him the opinions at which I should arrive, after my examination of the Abbeville collection. M. de Perthes gave me the freest access to his materials, with unreserved explanations of all the facts of the case that had come under his observation; and having considered his Menchecourt Section, taken with such scrupulous care, and identified the molars of elephas primigenius, which he had exhumed with his own hands deep in that section, along with flint weapons, presenting the same character as some of those found in the Broxham Cave, I arrived at the conviction that they were of contemporaneous age, although I was not prepared to go along with M. de Perthes in all his inferences regarding the hieroglyphics and in an industrial interpretation of the various other objects which he had met with."[4]

That Dr. Falconer was much impressed by the collection of M. de Perthes is shown in a communication which he sent at once to his friend Prestwich:

"I have been richly rewarded," he exclaims. "His collection of wrought flint implements, and of the objects of every description associated with them, far exceeds everything I expected to have seen, especially from a single locality. He has made great additions, since the publication of his first volume, in the second, which I now have by me. He showed me flint hatchets which HE HAD DUG UP with his own hands, mixed INDISCRIMINATELY with molars of elephas primigenius. I examined and identified plates of the molars and the flint objects which were got along with them. Abbeville is an out−of−the−way place, very little visited; and the French savants who meet him in Paris laugh at Monsieur de Perthes and his researches. But after devoting the greater part of a day to his vast collection, I am perfectly satisfied that there is a great deal of fair presumptive evidence in favor of many of his speculations regarding the remote antiquity of these industrial objects and their association with animals now extinct. M. Boucher's hotel is, from the ground floor to garret, a continued museum, filled with pictures, mediaeval art, and Gaulish antiquities, including antediluvian flint−knives, fossil−bones, etc. If, during next summer, you should happen to be paying a visit to France, let me strongly recommend you to come to Abbeville. I am sure you would be richly rewarded."[5]

This letter aroused the interest of the English geologists, and in the spring of 1859 Prestwich and Mr. (afterwards Sir John) Evans made a visit to Abbeville to see the specimens and examine at first hand the evidences as pointed out by Dr. Falconer. "The evidence yielded by the valley of the Somme," continues Falconer, in speaking of this visit, "was gone into with the scrupulous care and severe and exhaustive analysis which are characteristic of Mr. Prestwich's researches. The conclusions to which he was conducted were communicated to the Royal Society on May 12, 1859, in his celebrated memoir, read on May 26th and published in the Philosophical Transactions of 1860, which, in addition to researches made in the valley of the Somme, contained an account of similar phenomena presented by the valley of the Waveney, near Hoxne, in Suffolk. Mr. Evans communicated to the Society of Antiquaries a memoir on the character and geological position of the 'Flint Implements in the Drift,' which appeared in the Archaeologia for 1860. The results arrived at by Mr. Prestwich were expressed as follows:

"First. That the flint implements are the result of design and the work of man.

"Second. That they are found in beds of gravel, sand, and clay, which have never been artificially disturbed.

"Third. That they occur associated with the remains of land, fresh−water, and marine testacea, of species now living, and most of them still common in the same neighborhood, and also with the remains of various mammalia—a few species now living, but more of extinct forms.

"Fourth. That the period at which their entombment took place was subsequent to the bowlder−clay period, and to that extent post−glacial; and also that it was among the latest in geological time—one apparently anterior to the surface assuming its present form, so far as it regards some of the minor features."[6]

These reports brought the subject of the very significant human fossils at Abbeville prominently before the public; whereas the publications of the original discoverer, Boucher de Perthes, bearing date of 1847, had been altogether ignored. A new aspect was thus given to the current controversy.

As Dr. Falconer remarked, geology was now passing through the same ordeal that astronomy passed in the age of Galileo. But the times were changed since the day when the author of the Dialogues was humbled before the Congregation of the Index, and now no Index Librorum Prohibitorum could avail to hide from eager human eyes such pages of the geologic story as Nature herself had spared. Eager searchers were turning the leaves with renewed zeal everywhere, and with no small measure of success. In particular, interest attached just at this time to a human skull which Dr. Fuhlrott had discovered in a cave at Neanderthal two or three years before—a cranium which has ever since been famous as the Neanderthal skull, the type specimen of what modern zoologists are disposed to regard as a distinct species of man, Homo neanderthalensis. Like others of the same type since discovered at Spy, it is singularly simian in character—low−arched, with receding forehead and enormous, protuberant eyebrows. When it was first exhibited to the scientists at Berlin by Dr. Fuhlrott, in 1857, its human character was doubted by some of the witnesses; of that, however, there is no present question.

This interesting find served to recall with fresh significance some observations that had been made in France and Belgium a long generation earlier, but whose bearings had hitherto been ignored. In 1826 MM. Tournal and Christol had made independent discoveries of what they believed to be human fossils in the caves of the south of France; and in 1827 Dr. Schmerling had found in the cave of Engis, in Westphalia, fossil bones of even greater significance. Schmerling's explorations had been made with the utmost care, and patience. At Engis he had found human bones, including skulls, intermingled with those of extinct mammals of the mammoth period in a way that left no doubt in his mind that all dated from the same geological epoch. He bad published a full account of his discoveries in an elaborate monograph issued in 1833.

But at that time, as it chanced, human fossils were under a ban as effectual as any ever pronounced by canonical index, though of far different origin. The oracular voice of Cuvier had declared against the authenticity of all human fossils. Some of the bones brought him for examination the great anatomist had pettishly pitched out of the window, declaring them fit only for a cemetery, and that had settled the matter for a generation: the evidence gathered by lesser workers could avail nothing against the decision rendered at the Delphi of Science. But no ban, scientific or canonical, can longer resist the germinative power of a fact, and so now, after three decades of suppression, the truth which Cuvier had buried beneath the weight of his ridicule burst its bonds, and fossil man stood revealed, if not as a flesh−and−blood, at least as a skeletal entity.

The reception now accorded our prehistoric ancestor by the progressive portion of the scientific world amounted to an ovation; but the unscientific masses, on the other hand, notwithstanding their usual fondness for tracing remote genealogies, still gave the men of Engis and Neanderthal the cold shoulder. Nor were all of the geologists quite agreed that the contemporaneity of these human fossils with the animals whose remains had been mingled with them had been fully established. The bare possibility that the bones of man and of animals that long preceded him had been swept together into the eaves in successive ages, and in some mysterious way intermingled there, was clung to by the conservatives as a last refuge. But even this small measure of security was soon to be denied them, for in 1865 two associated workers, M. Edouard Lartet and Mr. Henry Christy, in exploring the caves of Dordogne, unearthed a bit of evidence against which no such objection could be urged. This momentous exhibit was a bit of ivory, a fragment of the tusk of a mammoth, on which was scratched a rude but unmistakable outline portrait of the mammoth itself. If all the evidence as to man's antiquity before presented was suggestive merely, here at last was demonstration; for the cave−dwelling man could not well have drawn the picture of the mammoth unless he had seen that animal, and to admit that man and the mammoth had been contemporaries was to concede the entire case. So soon, therefore, as the full import of this most instructive work of art came to be realized, scepticism as to man's antiquity was silenced for all time to come.

In the generation that has elapsed since the first drawing of the cave−dweller artist was discovered, evidences of the wide−spread existence of man in an early epoch have multiplied indefinitely, and to−day the paleontologist traces the history of our race back beyond the iron and bronze ages, through a neolithic or polished−stone age, to a paleolithic or rough−stone age, with confidence born of unequivocal knowledge. And he looks confidently to the future explorer of the earth's fossil records to extend the history back into vastly more remote epochs, for it is little doubted that paleolithic man, the most ancient of our recognized progenitors, is a modern compared to those generations that represented the real childhood of our race.

THE FOSSIL−BEDS OF AMERICA

Coincidently with the discovery of these highly suggestive pages of the geologic story, other still more instructive chapters were being brought to light in America. It was found that in the Rocky Mountain region, in strata found in ancient lake beds, records of the tertiary period, or age of mammals, had been made and preserved with fulness not approached in any other region hitherto geologically explored. These records were made known mainly by Professors Joseph Leidy, O. C. Marsh, and E. D. Cope, working independently, and more recently by numerous younger paleontologists.

The profusion of vertebrate remains thus brought to light quite beggars all previous exhibits in point of mere numbers. Professor Marsh, for example, who was first in the field, found three hundred new tertiary species between the years 1870 and 1876. Meanwhile, in cretaceous strata, he unearthed remains of about two hundred birds with teeth, six hundred pterodactyls, or flying dragons, some with a spread of wings of twenty− five feet, and one thousand five hundred mosasaurs of the sea−serpent type, some of them sixty feet or more in length. In a single bed of Jurassic rock, not larger than a good−sized lecture−room, he found the remains of one hundred and sixty individuals of mammals, representing twenty species and nine genera; while beds of the same age have yielded three hundred reptiles, varying from the size of a rabbit to sixty or eighty feet in length.

But the chief interest of these fossils from the West is not their number but their nature; for among them are numerous illustrations of just such intermediate types of organisms as must have existed in the past if the succession of life on the globe has been an unbroken lineal succession. Here are reptiles with bat−like wings, and others with bird−like pelves and legs adapted for bipedal locomotion. Here are birds with teeth, and other reptilian characters. In short, what with reptilian birds and birdlike reptiles, the gap between modern reptiles and birds is quite bridged over. In a similar way, various diverse mammalian forms, as the tapir, the rhinoceros, and the horse, are linked together by fossil progenitors. And, most important of all, Professor Marsh has discovered a series of mammalian remains, occurring in successive geological epochs, which are held to represent beyond cavil the actual line of descent of the modern horse; tracing the lineage of our one−toed species back through two and three toed forms, to an ancestor in the eocene or early tertiary that had four functional toes and the rudiment of a fifth. This discovery is too interesting and too important not to be detailed at length in the words of the discoverer.

Marsh Describes the Fossil Horse

"It is a well−known fact," says Professor Marsh, "that the Spanish discoverers of America discovered no horses on this continent, and that the modern horse (Equus caballus, Linn.) was subsequently introduced from the Old World. It is, however, not so generally known that these animals had formerly been abundant here, and that long before, in tertiary time, near relatives of the horse, and probably his ancestors, existed in the far West in countless numbers and in a marvellous variety of forms. The remains of equine mammals, now known from the tertiary and quaternary deposits of this country, already represent more than double the number of genera and species hitherto found in the strata of the eastern hemisphere, and hence afford most important aid in tracing out the genealogy of the horses still existing.

"The animals of this group which lived in America during the three diversions of the tertiary period were especially numerous in the Rocky Mountain regions, and their remains are well preserved in the old lake basins which then covered so much of that country. The most ancient of these lakes—which extended over a considerable part of the present territories of Wyoming and Utah—remained so long in eocene times that the mud and sand, slowly deposited in it, accumulated to more than a mile in vertical thickness. In these deposits vast numbers of tropical animals were entombed, and here the oldest equine remains occur, four species of which have been described. These belong to the genus Orohippus (Marsh), and are all of a diminutive size, hardly bigger than a fox. The skeletons of these animals resemble that of the horse in many respects, much more indeed than any other existing species, but, instead of the single toe on each foot, so characteristic of all modern equines, the various species of Orohippus had four toes before and three behind, all of which reached the ground. The skull, too, was proportionately shorter, and the orbit was not enclosed behind by a bridge of bone. There were fifty four teeth in all, and the premolars were larger than the molars. The crowns of these teeth were very short. The canine teeth were developed in both sexes, and the incisors did not have the "mark" which indicates the age of the modern horse. The radius and ulna were separate, and the latter was entire through the whole length. The tibia and fibula were distinct. In the forefoot all the digits except the pollex, or first, were well developed. The third digit is the largest, and its close resemblance to that of the horse is clearly marked. The terminal phalanx, or coffin−bone, has a shallow median bone in front, as in many species of this group in the later tertiary. The fourth digit exceeds the second in size, and the second is much the shortest of all. Its metacarpal bone is considerably curved outward. In the hind−foot of this genus there are but three digits. The fourth metatarsal is much larger than the second.

"The larger number of equine mammals now known from the tertiary deposits of this country, and their regular distributions through the subdivisions of this formation, afford a good opportunity to ascertain the probable descent of the modern horse. The American representative of the latter is the extinct Equus fraternus (Leidy), a species almost, if not wholly, identical with the Old World Equus caballus (Linnaeus), to which our recent horse belongs. Huxley has traced successfully the later genealogy of the horse through European extinct forms, but the line in America was probably a more direct one, and the record is more complete. Taking, then, as the extreme of a series, Orohippus agilis (Marsh), from the eocene, and Equus fraternus (Leidy), from the quaternary, intermediate forms may be intercalated with considerable certainty from thirty or more well−marked species that lived in the intervening periods. The natural line of descent would seem to be through the following genera: Orohippus, of the eocene; Miohippus and Anchitherium, of the miocene; Anchippus, Hipparion, Protohippus, Phohippus, of the pliocene; and Equus, quaternary and recent.

The most marked changes undergone by the successive equine genera are as follows: First, increase in size; second, increase in speed, through concentration of limb bones; third, elongation of head and neck, and modifications of skull. The eocene Orohippus was the size of a fox. Miohippus and Anchitherium, from the miocene, were about as large as a sheep. Hipparion and Pliohippus, of the pliocene, equalled the ass in height; while the size of the quaternary Equus was fully up to that of a modern horse.

"The increase of speed was equally well marked, and was a direct result of the gradual formation of the limbs. The latter were slowly concentrated by the reduction of their lateral elements and enlargement of the axial bone, until the force exerted by each limb came to act directly through its axis in the line of motion. This concentration is well seen—e.g., in the fore−limb. There was, first, a change in the scapula and humerus, especially in the latter, which facilitated motion in one line only; second, an expansion of the radius and reduction of the ulna, until the former alone remained entire and effective; third, a shortening of all the carpal bones and enlargement of the median ones, insuring a firmer wrist; fourth, an increase of size of the third digit, at the expense of those of each side, until the former alone supported the limb.

"Such is, in brief, a general outline of the more marked changes that seemed to have produced in America the highly specialized modern Equus from his diminutive four−toed predecessor, the eocene Orohippus. The line of descent appears to have been direct, and the remains now known supply every important intermediate form. It is, of course, impossible to say with certainty through which of the three−toed genera of the pliocene that lived together the succession came. It is not impossible that the latter species, which appear generically identical, are the descendants of more distinct pliocene types, as the persistent tendency in all the earlier forms was in the same direction. Considering the remarkable development of the group through the tertiary period, and its existence even later, it seems very strange that none of the species should have survived, and that we are indebted for our present horse to the Old World."[7]

PALEONTOLOGY OF EVOLUTION

These and such−like revelations have come to light in our own time—are, indeed, still being disclosed. Needless to say, no index of any sort now attempts to conceal them; yet something has been accomplished towards the same end by the publication of the discoveries in Smithsonian bulletins and in technical memoirs of government surveys. Fortunately, however, the results have been rescued from that partial oblivion by such interpreters as Professors Huxley and Cope, so the unscientific public has been allowed to gain at least an inkling of the wonderful progress of paleontology in our generation.

The writings of Huxley in particular epitomize the record. In 1862 he admitted candidly that the paleontological record as then known, so far as it bears on the doctrine of progressive development, negatives that doctrine. In 1870 he was able to "soften somewhat the Brutus−like severity" of his former verdict, and to assert that the results of recent researches seem "to leave a clear balance in favor of the doctrine of the evolution of living forms one from another." Six years later, when reviewing the work of Marsh in America and of Gaudry in Pikermi, he declared that, "on the evidence of paleontology, the evolution of many existing forms of animal life from their predecessors is no longer an hypothesis, but an historical fact." In 1881 he asserted that the evidence gathered in the previous decade had been so unequivocal that, had the transmutation hypothesis not existed, "the paleontologist would have had to invent it."

Since then the delvers after fossils have piled proof on proof in bewildering profusion. The fossil−beds in the "bad lands" of western America seem inexhaustible. And in the Connecticut River Valley near relatives of the great reptiles which Professor Marsh and others have found in such profusion in the West left their tracks on the mud−flats—since turned to sandstone; and a few skeletons also have been found. The bodies of a race of great reptiles that were the lords of creation of their day have been dissipated to their elements, while the chance indentations of their feet as they raced along the shores, mere footprints on the sands, have been preserved among the most imperishable of the memory−tablets of the world.

Of the other vertebrate fossils that have been found in the eastern portions of America, among the most abundant and interesting are the skeletons of mastodons. Of these one of the largest and most complete is that which was unearthed in the bed of a drained lake near Newburg, New York, in 1845. This specimen was larger than the existing elephants, and had tusks eleven feet in length. It was mounted and described by Dr. John C. Warren, of Boston, and has been famous for half a century as the "Warren mastodon."

But to the student of racial development as recorded by the fossils all these sporadic finds have but incidental interest as compared with the rich Western fossil− beds to which we have already referred. From records here unearthed, the racial evolution of many mammals has in the past few years been made out in greater or less detail. Professor Cope has traced the ancestry of the camels (which, like the rhinoceroses, hippopotami, and sundry other forms now spoken of as "Old World," seem to have had their origin here) with much completeness.

A lemuroid form of mammal, believed to be of the type from which man has descended, has also been found in these beds. It is thought that the descendants of this creature, and of the other "Old−World" forms above referred to, found their way to Asia, probably, as suggested by Professor Marsh, across a bridge at Bering Strait, to continue their evolution on the other hemisphere, becoming extinct in the land of their nativity. The ape−man fossil found in the tertiary strata of the island of Java in 1891 by the Dutch surgeon Dr. Eugene Dubois, and named Pithecanthropus erectus, may have been a direct descendant of the American tribe of primitive lemurs, though this is only a conjecture.

Not all the strange beasts which have left their remains in our "bad lands" are represented by living descendants. The titanotheres, or brontotheridae, for example, a gigantic tribe, offshoots of the same stock which produced the horse and rhinoceros, represented the culmination of a line of descent. They developed rapidly in a geological sense, and flourished about the middle of the tertiary period; then, to use Agassiz's phrase," time fought against them." The story of their evolution has been worked out by Professors Leidy, Marsh, Cope, and H. F. Osborne.

A recent bit of paleontological evidence bearing on the question of the introduction of species is that presented by Dr. J. L. Wortman in connection with the fossil lineage of the edentates. It was suggested by Marsh, in 1877, that these creatures, whose modern representatives are all South American, originated in North America long before the two continents had any land connection. The stages of degeneration by which these animals gradually lost the enamel from their teeth, coming finally to the unique condition of their modern descendants of the sloth tribe, are illustrated by strikingly graded specimens now preserved in the American Museum of Natural History, as shown by Dr. Wortman.

All these and a multitude of other recent observations that cannot be even outlined here tell the same story. With one accord paleontologists of our time regard the question of the introduction of new species as solved. As Professor Marsh has said, "to doubt evolution today is to doubt science; and science is only another name for truth."

Thus the third great battle over the meaning of the fossil records has come to a conclusion. Again there is a truce to controversy, and it may seem to the casual observer that the present stand of the science of fossils is final and impregnable. But does this really mean that a full synopsis of the story of paleontology has been told? Or do we only await the coming of the twentieth−century Lamarck or Darwin, who shall attack the fortified knowledge of to−day with the batteries of a new generalization?

IV. THE ORIGIN AND DEVELOPMENT OF MODERN GEOLOGY

JAMES HUTTON

One might naturally suppose that the science of the earth which lies at man's feet would at least have kept pace with the science of the distant stars. But perhaps the very obviousness of the phenomena delayed the study of the crust of the earth. It is the unattainable that allures and mystifies and enchants the developing mind. The proverbial child spurns its toys and cries for the moon.

So in those closing days of the eighteenth century, when astronomers had gone so far towards explaining the mysteries of the distant portions of the universe, we find a chaos of opinion regarding the structure and formation of the earth. Guesses were not wanting to explain the formation of the world, it is true, but, with one or two exceptions, these are bizarre indeed. One theory supposed the earth to have been at first a solid mass of ice, which became animated only after a comet had dashed against it. Other theories conceived the original globe as a mass of water, over which floated vapors containing the solid elements, which in due time were precipitated as a crust upon the waters. In a word, the various schemes supposed the original mass to have been ice, or water, or a conglomerate of water and solids, according to the random fancies of the theorists; and the final separation into land and water was conceived to have taken place in all the ways which fancy, quite unchecked by any tenable data, could invent.

Whatever important changes in the general character of the surface of the globe were conceived to have taken place since its creation were generally associated with the Mosaic: deluge, and the theories which attempted to explain this catastrophe were quite on a par with those which dealt with a remoter period of the earth's history. Some speculators, holding that the interior of the globe is a great abyss of waters, conceived that the crust had dropped into this chasm and had thus been inundated. Others held that the earth had originally revolved on a vertical axis, and that the sudden change to its present position bad caused the catastrophic shifting of its oceans. But perhaps the favorite theory was that which supposed a comet to have wandered near the earth, and in whirling about it to have carried the waters, through gravitation, in a vast tide over the continents.

Thus blindly groped the majority of eighteenth−century philosophers in their attempts to study what we now term geology. Deluded by the old deductive methods, they founded not a science, but the ghost of a science, as immaterial and as unlike anything in nature as any other phantom that could be conjured from the depths of the speculative imagination. And all the while the beckoning earth lay beneath the feet of these visionaries; but their eyes were fixed in air.

At last, however, there came a man who had the penetration to see that the phantom science of geology needed before all else a body corporeal, and who took to himself the task of supplying it. This was Dr. James Hutton, of Edinburgh, physician, farmer, and manufacturing chemist—patient, enthusiastic, level−headed devotee of science. Inspired by his love of chemistry to study the character of rocks and soils, Hutton had not gone far before the earth stood revealed to him in a new light. He saw, what generations of predecessors had blindly refused to see, that the face of nature everywhere, instead of being rigid and immutable, is perennially plastic, and year by year is undergoing metamorphic changes. The solidest rocks are day by day disintegrated slowly, but none the less surely, by wind and rain and frost, by mechanical attrition and chemical decomposition, to form the pulverized earth and clay. This soil is being swept away by perennial showers, and carried off to the oceans. The oceans themselves beat on their shores, and eat insidiously into the structure of sands and rocks. Everywhere, slowly but surely, the surface of the land is being worn away; its substance is being carried to burial in the seas.

Should this denudation continue long enough, thinks Hutton, the entire surface of the continents must be worn away. Should it be continued LONG ENOUGH! And with that thought there flashes on his mind an inspiring conception—the idea that solar time is long, indefinitely long. That seems a simple enough thought —almost a truism—to the twentieth−century mind; but it required genius to conceive it in the eighteenth. Hutton pondered it, grasped its full import, and made it the basis of his hypothesis, his "theory of the earth."

MODERN GEOLOGY

The hypothesis is this—that the observed changes of the surface of the earth, continued through indefinite lapses of time, must result in conveying all the land at last to the sea; in wearing continents away till the oceans overflow them. What then? Why, as the continents wear down, the oceans are filling up. Along their bottoms the detritus of wasted continents is deposited in strata, together with the bodies of marine animals and vegetables. Why might not this debris solidify to form layers of rocks—the basis of new continents? Why not, indeed?

But have we any proof that such formation of rocks in an ocean−bed has, in fact, occurred? To be sure we have. It is furnished by every bed of limestone, every outcropping fragment of fossil−bearing rock, every stratified cliff. How else than through such formation in an ocean−bed came these rocks to be stratified? How else came they to contain the shells of once living organisms imbedded in their depths? The ancients, finding fossil shells imbedded in the rocks, explained them as mere freaks of "nature and the stars." Less superstitious generations had repudiated this explanation, but had failed to give a tenable solution of the mystery. To Hutton it is a mystery no longer. To him it seems clear that the basis of the present continents was laid in ancient sea−beds, formed of the detritus of continents yet more ancient.

But two links are still wanting to complete the chain of Hutton's hypothesis. Through what agency has the ooze of the ocean−bed been transformed into solid rock? and through what agency has this rock been lifted above the surface of the water to form new continents? Hutton looks about him for a clew, and soon he finds it. Everywhere about us there are outcropping rocks that are not stratified, but which give evidence to the observant eye of having once been in a molten state. Different minerals are mixed together; pebbles are scattered through masses of rock like plums in a pudding; irregular crevices in otherwise solid masses of rock—so−called veinings—are seen to be filled with equally solid granite of a different variety, which can have gotten there in no conceivable way, so Hutton thinks, but by running in while molten, as liquid metal is run into the moulds of the founder. Even the stratified rocks, though they seemingly have not been melted, give evidence in some instances of having been subjected to the action of heat. Marble, for example, is clearly nothing but calcined limestone.

With such evidence before him, Hutton is at no loss to complete his hypothesis. The agency which has solidified the ocean−beds, he says, is subterranean heat. The same agency, acting excessively, has produced volcanic cataclysms, upheaving ocean−beds to form continents. The rugged and uneven surfaces of mountains, the tilted and broken character of stratified rocks everywhere, are the standing witnesses of these gigantic upheavals.

And with this the imagined cycle is complete. The continents, worn away and carried to the sea by the action of the elements, have been made over into rocks again in the ocean−beds, and then raised once more into continents. And this massive cycle, In Hutton's scheme, is supposed to have occurred not once only, but over and over again, times without number. In this unique view ours is indeed a world without beginning and without end; its continents have been making and unmaking in endless series since time began.

Hutton formulated his hypothesis while yet a young man, not long after the middle of the century. He first gave it publicity in 1781, in a paper before the Royal Society of Edinburgh:

"A solid body of land could not have answered the purpose of a habitable world," said Hutton, "for a soil is necessary to the growth of plants, and a soil is nothing but the material collected from the destruction of the solid land. Therefore the surface of this land inhabited by man, and covered by plants and animals, is made by nature to decay, in dissolving from that hard and compact state in which it is found; and this soil is necessarily washed away by the continual circulation of the water running from the summits of the mountains towards the general receptacle of that fluid.

"The heights of our land are thus levelled with our shores, our fertile plains are formed from the ruins of the mountains; and those travelling materials are still pursued by the moving water, and propelled along the inclined surface of the earth. These movable materials, delivered into the sea, cannot, for a long continuance, rest upon the shore, for by the agitation of the winds, the tides, and the currents every movable thing is carried farther and farther along the shelving bottom of the sea, towards the unfathomable regions of the ocean.

"If the vegetable soil is thus constantly removed from the surface of the land, and if its place is then to be supplied from the dissolution of the solid earth as here represented, we may perceive an end to this beautiful machine; an end arising from no error in its constitution as a world, but from that destructibility of its land which is so necessary in the system of the globe, in the economy of life and vegetation.

"The immense time necessarily required for the total destruction of the land must not be opposed to that view of future events which is indicated by the surest facts and most approved principles. Time, which measures everything in our idea, and is often deficient to our schemes, is to nature endless and as nothing; it cannot limit that by which alone it has existence; and as the natural course of time, which to us seems infinite, cannot be bounded by any operation that may have an end, the progress of things upon this globe that in the course of nature cannot be limited by time must proceed in a continual succession. We are, therefore, to consider as inevitable the destruction of our land, so far as effected by those operations which are necessary in the purpose of the globe, considered as a habitable world, and so far as we have not examined any other part of the economy of nature, in which other operations and a different intention might appear.

"We have now considered the globe of this earth as a machine, constructed upon chemical as well as mechanical principles, by which its different parts are all adapted, in form, in quality, and quantity, to a certain end—an end attained with certainty of success, and an end from which we may perceive wisdom in contemplating the means employed.

"But is this world to be considered thus merely as a machine, to last no longer than its parts retain their present position, their proper forms and qualities? Or may it not be also considered as an organized body such as has a constitution, in which the necessary decay of the machine is naturally repaired in the exertion of those productive powers by which it has been formed?

"This is the view in which we are now to examine the globe; to see if there be, in the constitution of the world, a reproductive operation by which a ruined constitution may be again repaired and a duration of stability thus procured to the machine considered as a world containing plants and animals.

"If no such reproductive power, or reforming operation, after due inquiry, is to be found in the constitution of this world, we should have reason to conclude that the system of this earth has either been intentionally made imperfect or has not been the work of infinite power and wisdom."[1]

This, then, was the important question to be answered—the question of the constitution of the globe. To accomplish this, it was necessary, first of all, to examine without prejudice the material already in hand, adding such new discoveries from time to time as might be made, but always applying to the whole unvarying scientific principles and inductive methods of reasoning.

"If we are to take the written history of man for the rule by which we should judge of the time when the species first began," said Hutton, "that period would be but little removed from the present state of things. The Mosaic history places this beginning of man at no great distance; and there has not been found, in natural history, any document by which high antiquity might be attributed to the human race. But this is not the case with regard to the inferior species of animals, particularly those which inhabit the ocean and its shores. We find in natural history monuments which prove that those animals had long existed; and we thus procure a measure for the computation of a period of time extremely remote, though far from being precisely ascertained.

"In examining things present, we have data from which to reason with regard to what has been; and from what actually has been we have data for concluding with regard to that which is to happen hereafter. Therefore, upon the supposition that the operations of nature are equable and steady, we find, in natural appearances, means for concluding a certain portion of time to have necessarily elapsed in the production of those events of which we see the effects.

"It is thus that, in finding the relics of sea animals of every kind in the solid body of our earth, a natural history of those animals is formed, which includes a certain portion of time; and for the ascertaining this portion of time we must again have recourse to the regular operations of this world. We shall thus arrive at facts which indicate a period to which no other species of chronology is able to remount.

"We find the marks of marine animals in the most solid parts of the earth, consequently those solid parts have been formed after the ocean was inhabited by those animals which are proper to that fluid medium. If, therefore, we knew the natural history of these solid parts, and could trace the operations of the globe by which they have been formed, we would have some means for computing the time through which those species of animals have continued to live. But how shall we describe a process which nobody has seen performed and of which no written history gives any account? This is only to be investigated, first, in examining the nature of those solid bodies the history of which we want to know; and, secondly, in examining the natural operations of the globe, in order to see if there now exist such operations as, from the nature of the solid bodies, appear to have been necessary for their formation.

"There are few beds of marble or limestone in which may not be found some of those objects which indicate the marine object of the mass. If, for example, in a mass of marble taken from a quarry upon the top of the Alps or

Andes there shall be found one cockle−shell or piece of coral, it must be concluded that this bed of stone has been originally formed at the bottom of the sea, as much as another bed which is evidently composed almost altogether of cockle−shells and coral. If one bed of limestone is thus found to have been of marine origin, every concomitant bed of the same kind must be also concluded to have been formed in the same manner.

"In those calcareous strata, which are evidently of marine origin, there are many parts which are of sparry structure—that is to say, the original texture of those beds in such places has been dissolved, and a new structure has been assumed which is peculiar to a certain state of the calcareous earth. This change is produced by crystallization, in consequence of a previous state of fluidity, which has so disposed the concerting parts as to allow them to assume a regular shape and structure proper to that substance. A body whose external form has been modified by this process is called a CRYSTAL; one whose internal arrangement of parts is determined by it is said to be of a SPARRY STRUCTURE, and this is known from its fracture.

"There are, in all the regions of the earth, huge masses of calcareous matter in that crystalline form or sparry state in which, perhaps, no vestige can be found of any organized body, nor any indication that such calcareous matter has belonged to animals; but as in other masses this sparry structure or crystalline state is evidently assumed by the marine calcareous substances in operations which are natural to the globe, and which are necessary to the consolidation of the strata, it does not appear that the sparry masses in which no figured body is formed have been originally different from other masses, which, being only crystallized in part, and in part still retaining their original form, have ample evidence of their marine origin.

"We are led, in this manner, to conclude that all the strata of the earth, not only those consisting of such calcareous masses, but others superincumbent upon these, have had their origin at the bottom of the sea.

"The general amount of our reasoning is this, that nine−tenths, perhaps, or ninety−nine−hundredths, of this earth, so far as we see, have been formed by natural operations of the globe in collecting loose materials and depositing them at the bottom of the sea; consolidating those collections in various degrees, and either elevating those consolidated masses above the level on which they were formed or lowering the level of that sea.

"Let us now consider how far the other proposition of strata being elevated by the power of heat above the level of the sea may be confirmed from the examination of natural appearances. The strata formed at the bottom of the ocean are necessarily horizontal in their position, or nearly so, and continuous in their horizontal direction or extent. They may be changed and gradually assume the nature of each other, so far as concerns the materials of which they are formed, but there cannot be any sudden change, fracture, or displacement naturally in the body of a stratum. But if the strata are cemented by the heat of fusion, and erected with an expansive power acting below, we may expect to find every species of fracture, dislocation, and contortion in those bodies and every degree of departure from a horizontal towards a vertical position.

"The strata of the globe are actually found in every possible position: for from horizontal they are frequently found vertical; from continuous they are broken and separated in every possible direction; and from a plane they are bent and doubled. It is impossible that they could have originally been formed, by the known laws of nature, in their present state and position; and the power that has been necessarily required for their change has not been inferior to that which might have been required for their elevation from the place in which they have been formed."[2]

From all this, therefore, Hutton reached the conclusion that the elevation of the bodies of land above the water on the earth's surface had been effected by the same force which had acted in consolidating the strata and giving them stability. This force he conceived to be exerted by the expansion of heated matter.

"We have," he said, "been now supposing that the beginning of our present earth had been laid in the bottom of the ocean, at the completion of the former land, but this was only for the sake of distinctness. The just view is this, that when the former land of the globe had been complete, so as to begin to waste and be impaired by the encroachment of the sea, the present land began to appear above the surface of the ocean. In this manner we suppose a due proportion to be always preserved of land and water upon the surface of the globe, for the purpose of a habitable world such as this which we possess. We thus also allow time and opportunity for the translation of animals and plants to occupy the earth.

"But if the earth on which we live began to appear in the ocean at the time when the LAST began to be resolved, it could not be from the materials of the continent immediately preceding this which we examine that the present earth has been constructed; for the bottom of the ocean must have been filled with materials before land could be made to appear above its surface.

"Let us suppose that the continent which is to succeed our land is at present beginning to appear above the water in the middle of the Pacific Ocean; it must be evident that the materials of this great body, which is formed and ready to be brought forth, must have been collected from the destruction of an earth which does not now appear. Consequently, in this true statement of the case there is necessarily required the destruction of an animal and vegetable earth prior to the former land; and the materials of that earth which is first in our account must have been collected at the bottom of the ocean, and begun to be concocted for the production of the present earth, when the land immediately preceding the present had arrived at its full extent.

"We have now got to the end of our reasoning; we have no data further to conclude immediately from that which actually is; but we have got enough; we have the satisfaction to find that in nature there are wisdom, system, and consistency. For having in the natural history of the earth seen a succession of worlds, we may from this conclude that there is a system in nature; in like manner as, from seeing revolutions of the planets, it is concluded that there is a system by which they are intended to continue those revolutions. But if the succession of worlds is established in the system of nature, it is in vain to look for anything higher in the origin of the earth. The result, therefore, of our present inquiry is that we find no vestige of a beginning—no prospect of an end."

Altogether remarkable as this paper seems in the light of later knowledge, neither friend nor foe deigned to notice it at the moment. It was not published in book form until the last decade of the century, when Hutton had lived with and worked over his theory for almost fifty years. Then it caught the eye of the world. A school of followers expounded the Huttonian doctrines; a rival school under Werner in Germany opposed some details of the hypothesis, and the educated world as a whole viewed the disputants askance. The very novelty of the new views forbade their immediate acceptance. Bitter attacks were made upon the "heresies," and that was meant to be a soberly tempered judgment which in 1800 pronounced Hutton's theories "not only hostile to sacred history, but equally hostile to the principles of probability, to the results of the ablest observations on the mineral kingdom, and to the dictates of rational philosophy." And all this because Hutton's theory presupposed the earth to have been in existence more than six thousand years.

Thus it appears that though the thoughts of men had widened, in those closing days of the eighteenth century, to include the stars, they had not as yet expanded to receive the most patent records that are written everywhere on the surface of the earth. Before Hutton's views could be accepted, his pivotal conception that time is long must be established by convincing proofs. The evidence was being gathered by William Smith, Cuvier, and other devotees of the budding science of paleontology in the last days of the century, but their labors were not brought to completion till a subsequent epoch.

NEPTUNISTS VERSUS PLUTONISTS

In the mean time, James Hutton's theory that continents wear away and are replaced by volcanic upheaval gained comparatively few adherents. Even the lucid Illustrations of the Huttonian Theory, which Playfair, the pupil and friend of the great Scotchman, published in 1802, did not at once prove convincing. The world had become enamoured of the rival theory of Hutton's famous contemporary, Werner of Saxony —the theory which taught that "in the beginning" all the solids of the earth's present crust were dissolved in the heated waters of a universal sea. Werner affirmed that all rocks, of whatever character, had been formed by precipitation from this sea as the waters cooled; that even veins have originated in this way; and that mountains are gigantic crystals, not upheaved masses. In a word, he practically ignored volcanic action, and denied in toto the theory of metamorphosis of rocks through the agency of heat.

The followers of Werner came to be known as Neptunists; the Huttonians as Plutonists. The history of geology during the first quarter of the nineteenth century is mainly a recital of the intemperate controversy between these opposing schools; though it should not be forgotten that, meantime, the members of the Geological Society of London were making an effort to hunt for facts and avoid compromising theories. Fact and theory, however, were too closely linked to be thus divorced.

The brunt of the controversy settled about the unstratified rocks—granites and their allies—which the Plutonists claimed as of igneous origin. This contention had the theoretical support of the nebular hypothesis, then gaining ground, which supposed the earth to be a cooling globe. The Plutonists laid great stress, too, on the observed fact that the temperature of the earth increases at a pretty constant ratio as descent towards its centre is made in mines. But in particular they appealed to the phenomena of volcanoes.

The evidence from this source was gathered and elaborated by Mr. G. Poulett Scrope, secretary of the Geological Society of England, who, in 1823, published a classical work on volcanoes in which he claimed that volcanic mountains, including some of the highest− known peaks, are merely accumulated masses of lava belched forth from a crevice in the earth's crust.

"Supposing the globe to have had any irregular shape when detached from the sun," said Scrope, "the vaporization of its surface, and, of course, of its projecting angles, together with its rotatory motion on its axis and the liquefaction of its outer envelope, would necessarily occasion its actual figure of an oblate spheroid. As the process of expansion proceeded in depth, the original granitic beds were first partially disaggregated, next disintegrated, and more or less liquefied, the crystals being merged in the elastic vehicle produced by the vaporization of the water contained between the laminae.

"Where this fluid was produced in abundance by great dilatation—that is, in the outer and highly disintegrated strata, the superior specific gravity of the crystals forced it to ooze upward, and thus a great quantity of aqueous vapor was produced on the surface of the globe. As this elastic fluid rose into outer space, its continually increasing expansion must have proportionately lowered its temperature; and, in consequence, a part was recondensed into water and sank back towards the more solid surface of the globe.

"And in this manner, for a certain time, a violent reciprocation of atmospheric phenomena must have continued—torrents of vapor rising outwardly, while equally tremendous torrents of condensed vapor, or rain, fell towards the earth. The accumulation of the latter on the yet unstable and unconsolidated surface of the globe constituted the primeval ocean. The surface of this ocean was exposed to continued vaporization owing to intense heat; but this process, abstracting caloric from the stratum of the water below, by partially cooling it, tended to preserve the remainder in a liquid form. The ocean will have contained, both in solution and suspension, many of the matters carried upward from the granitic bed in which the vapors from whose condensation it proceeded were produced, and which they had traversed in their rise. The dissolved matters will have been silex, carbonates, and sulphates of lime, and those other mineral substances which water at an intense temperature and under such circumstances was enabled to hold in solution. The suspended substances will have been all the lighter and finer particles of the upper beds where the disintegration had been extreme; and particularly their mica, which, owing to the tenuity of its plate−shaped crystals, would be most readily carried up by the ascending fluid, and will have remained longest in suspension.

"But as the torrents of vapor, holding these various matters in solution and suspension, were forced upward, the greater part of the disintegrated crystals by degrees subsided; those of felspar and quartz first, the mica being, as observed above, from the form of its plates, of peculiar buoyancy, and therefore held longest in suspension.

"The crystals of felspar and quartz as they subsided, together with a small proportion of mica, would naturally arrange themselves so as to have their longest dimensions more or less parallel to the surface on which they rest; and this parallelism would be subsequently increased, as we shall see hereafter, by the pressure of these beds sustained between the weight of the supported column of matter and the expansive force beneath them. These beds I conceive, when consolidated, to constitute the gneiss formation.

"The farther the process of expansion proceeded in depth, the more was the column of liquid matter lengthened, which, gravitating towards the centre of the globe, tended to check any further expansion. It is, therefore, obvious that after the globe settled into its actual orbit, and thenceforward lost little of its enveloping matter, the whole of which began from that moment to gravitate towards its centre, the progress of expansion inwardly would continually increase in rapidity; and a moment must have at length arrived hen the forces of expansion and repression had reached an equilibrium and the process was stopped from progressing farther inwardly by the great pressure of the gravitating column of liquid.

This column may be considered as consisting of different strata, though the passage from one extremity of complete solidity to the other of complete expansion, in reality, must have been perfectly gradual. The lowest stratum, immediately above the extreme limit of expansion, will have been granite barely DISAGGREGATED, and rendered imperfectly liquid by the partial vaporization of its contained water.

"The second stratum was granite DISINTEGRATED; aqueous vapor, having been produced in such abundance as to be enabled to rise upward, partially disintegrating the crystals of felspar and mica, and superficially dissolving those of quartz. This mass would reconsolidate into granite, though of a smaller grain than the preceding rock.

"The third stratum was so disintegrated that a greater part of the mica had been carried up by the escaping vapor IN SUSPENSION, and that of quartz in solution; the felspar crystals, with the remaining quartz and mica, SUBSIDING by their specific gravity and arranging themselves in horizontal planes.

"The consolidation of this stratum produced the gneiss formation.

"The fourth zone will have been composed of the ocean of turbid and heated water, holding mica, etc., in suspension, and quartz, carbonate of lime, etc., in solution, and continually traversed by reciprocating bodies of heated water rising from below, and of cold fluid sinking from the surface, by reason of their specific gravities.

"The disturbance thus occasioned will have long retarded the deposition of the suspended particles. But this must by degrees have taken place, the quartz grains and the larger and coarser plates of mica subsiding first and the finest last.

"But the fragments of quartz and mica were not deposited alone; a great proportion of the quartz held in SOLUTION must have been precipitated at the same time as the water cooled, and therefore by degrees lost its faculty of so much in solution. Thus was gradually produced the formation of mica−schist, the mica imperfectly recrystallizing or being merely aggregated together in horizontal plates, between which the quartz either spread itself generally in minute grains or unified into crystalline nuclei. On other spots, instead of silex, carbonate of lime was precipitated, together with more or less of the nucaceous sediment, and gave rise to saccharoidal limestones. At a later period, when the ocean was yet further cooled down, rock−salt and sulphate of lime were locally precipitated in a similar mode.

"The fifth stratum was aeriform, and consisted in great part of aqueous vapors; the remainder being a compound of other elastic fluids (permanent gases) which had been formed probably from the volatilization of some of the substances contained in the primitive granite and carried upward with the aqueous vapor from below. These gases will have been either mixed together or otherwise disposed, according to their different specific gravities or chemical affinities, and this stratum constituted the atmosphere or aerial envelope of the globe.

"When, in this manner, the general and positive expansion of the globe, occasioned by the sudden reduction of outward pressure, had ceased (in consequence of the REPRESSIVE FORCE, consisting of the weight of its fluid envelope, having reached an equilibrium with the EXPANSIVE FORCE, consisting of the caloric of the heated nucleus), the rapid superficial evaporation of the ocean continued; and, by gradually reducing its temperature, occasioned the precipitation of a proportionate quantity of the minerals it held in solution, particularly its silex. These substances falling to the bottom, accompanied by a large proportion of the matters held in solution, particularly the mica, in consequence of the greater comparative tranquillity of the ocean, agglomerated these into more or less compact beds of rock (the mica−schist formation), producing the first crust or solid envelope of the globe. Upon this, other stratified rocks, composed sometimes of a mixture, sometimes of an alternation of precipitations, sediments, and occasionally of conglomerates, were by degrees deposited, giving rise to the TRANSITION formations.

"Beneath this crust a new process now commenced. The outer zones of crystalline matter having been suddenly refrigerated by the rapid vaporization and partial escape of the water they contained, abstracted caloric from the intensely heated nucleus of the globe. These crystalline zones were of unequal density, the expansion they had suffered diminishing from above downward.

"Their expansive force was, however, equal at all points, their temperature everywhere bearing an inverse ratio to their density. But when by the accession of caloric from the inner and unliquefied nucleus the temperature, and consequently the expansive force of the lower strata of dilated crystalline matter, was augmented, it acted upon the upper and more liquefied strata. These being prevented from yielding OUTWARDLY by the tenacity and weight of the solid involucrum of precipitated and sedimental deposits which overspread them, sustained a pressure out of proportion to their expansive force, and were in consequence proportionately condensed, and by the continuance of the process, where the overlying strata were sufficiently resistant, finally consolidated.

"This process of consolidation must have progressed from above downward, with the increase of the expansive force in the lower strata, commencing from the upper surface, which, its temperature being lowest, offered the least resistance to the force of compression.

"By this process the upper zone of crystalline matter, which had intumesced so far as to allow of the escape of its aqueous vapor and of much of its mica and quartz, was resolidified, the component crystals arranging themselves in planes perpendicular to the direction of the pressure by which the mass was consolidated—that is, to the radius of the globe. The gneiss formation, as already observed, was the result.

"The inferior zone of barely disintegrated granite, from which only a part of the steam and quartz and none of the mica had escaped, reconsolidated in a confused or granitoidal manner; but exhibits marks of the process it had undergone in its broken crystals of felspar and mica, its rounded and superficially dissolved grains of quartz, its imbedded fragments (broken from the more solid parts of the mass, as it rose, and enveloped by the softer parts), its concretionary nodules and new minerals, etc.

"Beneath this, the granite which had been simply disintegrated was again solidified, and returned in all respects to its former condition. The temperature, however, and with it the expansive force of the inferior zone, was continually on the increase, the caloric of the interior of the globe still endeavoring to put itself in equilibrio by passing off towards the less−intensely heated crust.

"This continually increasing expansive force must at length have overcome the resistance opposed by the tenacity and weight of the overlying consolidated strata. It is reasonable to suppose that this result took place contemporaneously, or nearly so, on many spots, wherever accidental circumstances in the texture or composition of the oceanic deposits led them to yield more readily; and in this manner were produced those original fissures in the primeval crust of the earth through some of which (fissures of elevation) were intruded portions of interior crystalline zones in a solid or nearly solid state, together with more or less of the intumescent granite, in the manner above described; while others (fissures of eruption) gave rise to extravasations of the heated crystalline matter, in the form of lavas—that is, still further liquefied by the greater comparative reduction of the pressure they endured."[3]

The Neptunists stoutly contended for the aqueous origin of volcanic as of other mountains. But the facts were with Scrope, and as time went on it came to be admitted that not merely volcanoes, but many "trap" formations not taking the form of craters, had been made by the obtrusion of molten rock through fissures in overlying strata. Such, for example, to cite familiar illustrations, are Mount Holyoke, in Massachusetts, and the well−known formation of the Palisades along the Hudson.

But to admit the "Plutonic" origin of such widespread formations was practically to abandon the Neptunian hypothesis. So gradually the Huttonian explanation of the origin of granites and other "igneous" rocks, whether massed or in veins, came to be accepted. Most geologists then came to think of the earth as a molten mass, on which the crust rests as a mere film. Some, indeed, with Lyell, preferred to believe that the molten areas exist only as lakes in a solid crust, heated to melting, perhaps, by electrical or chemical action, as Davy suggested. More recently a popular theory attempts to reconcile geological facts with the claim of the physicists, that the earth's entire mass is at least as rigid as steel, by supposing that a molten film rests between the observed solid crust and the alleged solid nucleus. But be that as it may, the theory that subterranean heat has been instrumental in determining the condition of "primary" rocks, and in producing many other phenomena of the earth's crust, has never been in dispute since the long controversy between the Neptunists and the Plutonists led to its establishment.

LYELL AND UNIFORMITARIANISM

If molten matter exists beneath the crust of the earth, it must contract in cooling, and in so doing it must disturb the level of the portion of the crust already solidified. So a plausible explanation of the upheaval of continents and mountains was supplied by the Plutonian theory, as Hutton had from the first alleged. But now an important difference of opinion arose as to the exact rationale of such upheavals. Hutton himself, and practically every one else who accepted his theory, had supposed that there are long periods of relative repose, during which the level of the crust is undisturbed, followed by short periods of active stress, when continents are thrown up with volcanic suddenness, as by the throes of a gigantic earthquake. But now came Charles Lyell with his famous extension of the "uniformitarian" doctrine, claiming that past changes of the earth's surface have been like present changes in degree as well as in kind. The making of continents and mountains, he said, is going on as rapidly to−day as at any time in the past. There have been no gigantic cataclysmic upheavals at any time, but all changes in level of the strata as a whole have been gradual, by slow oscillation, or at most by repeated earthquake shocks such as are still often experienced.

In support of this very startling contention Lyell gathered a mass of evidence of the recent changes in level of continental areas. He corroborated by personal inspection the claim which had been made by Playfair in 1802, and by Von Buch in 1807, that the coast−line of Sweden is rising at the rate of from a few inches to several feet in a century. He cited Darwin's observations going to prove that Patagonia is similarly rising, and Pingel's claim that Greenland is slowly sinking. Proof as to sudden changes of level of several feet, over large areas, due to earthquakes, was brought forward in abundance. Cumulative evidence left it no longer open to question that such oscillatory changes of level, either upward or downward, are quite the rule, and it could not be denied that these observed changes, if continued long enough in one direction, would produce the highest elevations. The possibility that the making of even the highest ranges of mountains had been accomplished without exaggerated catastrophic action came to be freely admitted.

It became clear that the supposedly stable−land surfaces are in reality much more variable than the surface of the "shifting sea"; that continental masses, seemingly so fixed, are really rising and falling in billows thousands of feet in height, ages instead of moments being consumed in the sweep between crest and hollow.

These slow oscillations of land surfaces being understood, many geological enigmas were made clear— such as the alternation of marine and fresh−water formations in a vertical series, which Cuvier and Brongniart had observed near Paris; or the sandwiching of layers of coal, of subaerial formation, between layers of subaqueous clay or sandstone, which may be observed everywhere in the coal measures. In particular, the extreme thickness of the sedimentary strata as a whole, many times exceeding the depth of the deepest known sea, was for the first time explicable when it was understood that such strata had formed in slowly sinking ocean−beds.

All doubt as to the mode of origin of stratified rocks being thus removed, the way was opened for a more favorable consideration of that other Huttonian doctrine of the extremely slow denudation of land surfaces. The enormous amount of land erosion will be patent to any one who uses his eyes intelligently in a mountain district. It will be evident in any region where the strata are tilted—as, for example, the Alleghanies— that great folds of strata which must once have risen miles in height have in many cases been worn entirely away, so that now a valley marks the location of the former eminence. Where the strata are level, as in the case of the mountains of Sicily, the Scotch Highlands, and the familiar Catskills, the evidence of denudation is, if possible, even more marked; for here it is clear that elevation and valley have been carved by the elements out of land that rose from the sea as level plateaus.

But that this herculean labor of land−sculpturing could have been accomplished by the slow action of wind and frost and shower was an idea few men could grasp within the first half−century after Hutton propounded it; nor did it begin to gain general currency until Lyell's crusade against catastrophism, begun about 1830, had for a quarter of a century accustomed geologists to the thought of slow, continuous changes producing final results of colossal proportions. And even long after that it was combated by such men as Murchison, Director−General of the Geological Survey of Great Britain, then accounted the foremost field−geologist of his time, who continued to believe that the existing valleys owe their main features to subterranean forces of upheaval. Even Murchison, however, made some recession from the belief of the Continental authorities, Elie de Beaumont and Leopold von Buch, who contended that the mountains had sprung up like veritable jacks−in−the−box. Von Buch, whom his friend and fellow−pupil Von Humboldt considered the foremost geologist of the time, died in 1853, still firm in his early faith that the erratic bowlders found high on the Jura had been hurled there, like cannon−balls, across the valley of Geneva by the sudden upheaval of a neighboring mountain−range.

AGASSIZ AND THE GLACIAL THEORY

The bowlders whose presence on the crags of the Jura the old Gerinan accounted for in a manner so theatrical had long been a source of contention among geologists. They are found not merely on the Jura, but on numberless other mountains in all north−temperate latitudes, and often far out in the open country, as many a farmer who has broken his plough against them might testify. The early geologists accounted for them, as for nearly everything else, with their supposititious Deluge. Brongniart and Cuvier and Buckland and their contemporaries appeared to have no difficulty in conceiving that masses of granite weighing hundreds of tons had been swept by this current scores or hundreds of miles from their source. But, of course, the uniformitarian faith permitted no such explanation, nor could it countenance the projection idea; so Lyell was bound to find some other means of transportation for the puzzling erratics.

The only available medium was ice, but, fortunately, this one seemed quite sufficient. Icebergs, said Lyell, are observed to carry all manner of debris, and deposit it in the sea−bottoms. Present land surfaces have often been submerged beneath the sea. During the latest of these submergences icebergs deposited the bowlders now scattered here and there over the land. Nothing could be simpler or more clearly uniformitarian. And even the catastrophists, though they met Lyell amicably on almost no other theoretical ground, were inclined to admit the plausibility of his theory of erratics. Indeed, of all Lyell's nonconformist doctrines, this seemed the one most likely to meet with general acceptance.

Yet, even as this iceberg theory loomed large and larger before the geological world, observations were making in a different field that were destined to show its fallacy. As early as 1815 a sharp−eyed chamois− hunter of the Alps, Perraudin by name, had noted the existence of the erratics, and, unlike most of his companion hunters, had puzzled his head as to how the bowlders got where he saw them. He knew nothing of submerged continents or of icebergs, still less of upheaving mountains; and though he doubtless had heard of the Flood, he had no experience of heavy rocks floating like corks in water. Moreover, he had never observed stones rolling uphill and perching themselves on mountain−tops, and he was a good enough uniformitarian (though he would have been puzzled indeed had any one told him so) to disbelieve that stones in past times had disported themselves differently in this regard from stones of the present. Yet there the stones are. How did they get there?

The mountaineer thought that he could answer that question. He saw about him those gigantic serpent− like streams of ice called glaciers, "from their far fountains slow rolling on," carrying with them blocks of granite and other debris to form moraine deposits. If these glaciers had once been much more extensive than they now are, they might have carried the bowlders and left them where we find them. On the other hand, no other natural agency within the sphere of the chamois−hunter's knowledge could have accomplished this, ergo the glaciers must once have been more extensive. Perraudin would probably have said that common−sense drove him to this conclusion; but be that as it may, he had conceived one of the few truly original and novel ideas of which the nineteenth century can boast.

Perraudin announced his idea to the greatest scientist in his little world—Jean de Charpentier, director of the mines at Bex, a skilled geologist who had been a fellow−pupil of Von Buch and Von Humboldt under Werner at the Freiberg School of Mines. Charpentier laughed at the mountaineer's grotesque idea, and thought no more about it. And ten years elapsed before Perraudin could find any one who treated his notion with greater respect. Then he found a listener in M. Venetz, a civil engineer, who read a paper on the novel glacial theory before a local society in 1823. This brought the matter once more to the attention of De Charpentier, who now felt that there might be something in it worth investigation.

A survey of the field in the light of the new theory soon convinced Charpentier that the chamois−hunter had all along been right. He became an enthusiastic supporter of the idea that the Alps had once been imbedded in a mass of ice, and in 1836 he brought the notion to the attention of Louis Agassiz, who was spending the summer in the Alps. Agassiz was sceptical at first, but soon became a convert.

In 1840 Agassiz published a paper in which the results of his Alpine studies were elaborated.

"Let us consider," he says, "those more considerable changes to which glaciers are subject, or rather, the immense extent which they had in the prehistoric period. This former immense extension, greater than any that tradition has preserved, is proved, in the case of nearly every valley in the Alps, by facts which are both many and well established. The study of these facts is even easy if the student is looking out for them, and if he will seize the least indication of their presence; and, if it were a long time before they were observed and connected with glacial action, it is because the evidences are often isolated and occur at places more or less removed from the glacier which originated them. If it be true that it is the prerogative of the scientific observer to group in the field of his mental vision those facts which appear to be without connection to the vulgar herd, it is, above all, in such a case as this that he is called upon to do so. I have often compared these feeble effects, produced by the glacial action of former ages, with the appearance of the markings upon a lithographic stone, prepared for the purpose of preservation, and upon which one cannot see the lines of the draughtsman's work unless it is known beforehand where and how to search for them.

"The fact of the former existence of glaciers which have now disappeared is proved by the survival of the various phenomena which always accompany them, and which continue to exist even after the ice has melted. These phenomena are as follows:

"1. Moraines.—The disposition and composition of moraines enable them to be always recognized, even when they are no longer adjacent to a glacier nor immediately surround its lower extremities. I may remark that lateral and terminal moraines alone enable us to recognize with certainty the limits of glacial extension, because they can be easily distinguished from the dikes and irregularly distributed stones carried down by the Alpine torrents, The lateral moraines deposited upon the sides of valleys are rarely affected by the larger torrents, but they are, however, often cut by the small streams which fall down the side of a mountain, and which, by interfering with their continuity, make them so much more difficult to recognize.

"2. The Perched Bowlders.—It often happens that glaciers encounter projecting points of rock, the sides of which become rounded, and around which funnel− like cavities are formed with more or less profundity. When glaciers diminish and retire, the blocks which have fallen into these funnels often remain perched upon the top of the projecting rocky point within it, in such a state of equilibrium that any idea of a current of water as the cause of their transportation is completely inadmissible on account of their position. When such points of rock project above the surface of the glacier or appear as a more considerable islet in the midst of its mass (such as is the case in the Jardin of the Mer de Glace, above Montavert), such projections become surrounded on all sides by stones which ultimately form a sort of crown around the summit whenever the glaciers decrease or retire completely. Water currents never produce anything like this; but, on the contrary, whenever a stream breaks itself against a projecting rock, the stones which it carries down are turned aside and form a more or less regular trail. Never, under such circumstances, can the stones remain either at the top or at the sides of the rock, for, if such a thing were possible, the rapidity of the current would be accelerated by the increased resistance, and the moving bowlders would be carried beyond the obstruction before they were finally deposited.

"3. The polished and striated rocks, such as have been described in Chapter XIV., afford yet further evidence of the presence of a glacier; for, as has been said already, neither a current nor the action of waves upon an extensive beach produces such effects. The general direction of the channels and furrows indicates the direction of the general movement of the glacier, and the streaks which vary more or less from this direction are produced by the local effects of oscillation and retreat, as we shall presently see.

"4. The Lapiaz, or Lapiz, which the inhabitants of German Switzerland call Karrenfelder, cannot always be distinguished from erosions, because, both produced as they are by water, they do not differ in their exterior characteristics, but only in their positions. Erosions due to torrents are always found in places more or less depressed, and never occur upon large inclined surfaces. The Lapiaz, on the contrary, are frequently found upon the projecting parts of the sides of valleys in places where it is not possible to suppose that water has ever formed a current. Some geologists, in their embarrassment to explain these phenomena, have supposed that they were due to the infiltration of acidulated water, but this hypothesis is purely gratuitous.

"We will now describe the remains of these various phenomena as they are found in the Alps outside the actual glacial limits, in order to prove that at a certain epoch glaciers were much larger than they are to−day.

"The ancient moraines, situated as they are at a great distance from those of the present day, are nowhere so distinct or so frequent as in Valais, where MM. Venetz and J. de Charpentier noticed them for the first time; but as their observations are as yet unpublished, and they themselves gave me the information, it would be an appropriation of their discovery if I were to describe them here in detail. I will limit myself to say that there can be found traces, more or less distinct, of ancient terminal moraines in the form of vaulted dikes at the foot of every glacier, at a distance of a few minutes' walk, a quarter of an hour, a half−hour, an hour, and even of several leagues from their present extremities. These traces become less distinct in proportion to their distance from the glacier, and, since they are also often traversed by torrents, they are not as continuous as the moraines which are nearer to the glaciers. The farther these ancient moraines are removed from the termination of a glacier, the higher up they reach upon the sides of the valley, which proves to us that the thickness of the glacier must have been greater when its size was larger. At the same time, their number indicates so many stopping−places in the retreat of the glacier, or so many extreme limits of its extension—limits which were never reached again after it had retired. I insist upon this point, because if it is true that all these moraines demonstrate a larger extent of the glacier, they also prove that their retreat into their present boundaries, far from having been catastrophic, was marked on the contrary by periods of repose more or less frequent, which caused the formation of a series of concentric moraines which even now indicate their retrogression.

"The remains of longitudinal moraines are less frequent, less distinct, and more difficult to investigate, because, indicating as they do the levels to which the edges of the glacier reached at different epochs, it is generally necessary to look for them above the line of the paths along the escarpments of the valleys, and hence it is not always possible to follow them along a valley. Often, also, the sides of a valley which enclosed a glacier are so steep that it is only here and there that the stones have remained in place. They are, nevertheless, very distinct in the lower part of the valley of the Rhone, between Martigny and the Lake of Geneva, where several parallel ridges can be observed, one above the other, at a height of one thousand, one thousand two hundred, and even one thousand five hundred feet above the Rhone. It is between St. Maurice and the cascade of Pissevache, close to the hamlet of Chaux−Fleurie, that they are most accessible, for at this place the sides of the valley at different levels ascend in little terraces, upon which the moraines have been preserved. They are also very distinct above the Bains de Lavey, and above the village of Monthey at the entrance of the Val d'Illiers, where the sides of the valley are less inclined than in many other places.

"The perched bowlders which are found in the Alpine valleys, at considerable distances from the glaciers, occupy at times positions so extraordinary that they excite in a high degree the curiosity of those who see them. For instance, when one sees an angular stone perched upon the top of an isolated pyramid, or resting in some way in a very steep locality, the first inquiry of the mind is, When and how have these stones been placed in such positions, where the least shock would seem to turn them over? But this phenomenon is not in the least astonishing when it is seen to occur also within the limits of actual glaciers, and it is recalled by what circumstances it is occasioned.

"The most curious examples of perched stones which can be cited are those which command the northern part of the cascade of Pissevache, close to Chaux−Fleurie, and those above the Bains de Lavey, close to the village of Morcles; and those, even more curious, which I have seen in the valley of St. Nicolas and Oberhasli. At Kirchet, near Meiringen, can be seen some very remarkable crowns of bowlders around several domes of rock which appear to have been projected above the surface of the glacier which surrounded them. Something very similar can be seen around the top of the rock of St. Triphon.

"The extraordinary phenomenon of perched stones could not escape the observing eye of De Saussure, who noticed several at Saleve, of which he described the positions in the following manner: 'One sees,' said he, 'upon the slope of an inclined meadow, two of these great bowlders of granite, elevated one upon the other, above the grass at a height of two or three feet, upon a base of limestone rock on which both rest. This base is a continuation of the horizontal strata of the mountain, and is even united with it visibly on its lower face, being cut perpendicularly upon the other sides, and is not larger than the stone which it supports.' But seeing that the entire mountain is composed of the same limestone, De Saussure naturally concluded that it would be absurd to think that it was elevated precisely and only beneath the blocks of granite. But, on the other hand, since he did not know the manner in which these perched stones are deposited in our days by glacial action, he had recourse to another explanation: He supposes that the rock was worn away around its base by the continual erosion of water and air, while the portion of the rock which served as the base for the granite had been protected by it. This explanation, although very ingenious, could no longer be admitted after the researches of M. Elie de Beaumont had proved that the action of atmospheric agencies was not by a good deal so destructive as was theretofore supposed. De Saussure speaks also of a detached bowlder, situated upon the opposite side of the Tete−Noire, 'which is,' he says, 'of so great a size that one is tempted to believe that it was formed in the place it occupies; and it is called Barme russe, because it is worn away beneath in the form of a cave which can afford accommodation for more than thirty persons at a time."[4]

But the implications of the theory of glaciers extend, so Agassiz has come to believe, far beyond the Alps. If the Alps had been covered with an ice sheet, so had many other regions of the northern hemisphere. Casting abroad for evidences of glacial action, Agassiz found them everywhere in the form of transported erratics, scratched and polished outcropping rocks, and moraine−like deposits. Finally, he became convinced that the ice sheet that covered the Alps had spread over the whole of the higher latitudes of the northern hemisphere, forming an ice cap over the globe. Thus the common−sense induction of the chamois− hunter blossomed in the mind of Agassiz into the conception of a universal ice age.

In 1837 Agassiz had introduced his theory to the world, in a paper read at Neuchatel, and three years later he published his famous Etudes sur les Glaciers, from which we have just quoted. Never did idea make a more profound disturbance in the scientific world. Von Buch treated it with alternate ridicule, contempt, and rage; Murchison opposed it with customary vigor; even Lyell, whose most remarkable mental endowment was an unfailing receptiveness to new truths, could not at once discard his iceberg theory in favor of the new claimant. Dr. Buckland, however, after Agassiz had shown him evidence of former glacial action in his own Scotland, became a convert—the more readily, perhaps, as it seemed to him to oppose the uniformitarian idea. Gradually others fell in line, and after the usual imbittered controversy and the inevitable full generation of probation, the idea of an ice age took its place among the accepted tenets of geology. All manner of moot points still demanded attention—the cause of the ice age, the exact extent of the ice sheet, the precise manner in which it produced its effects, and the exact nature of these effects; and not all of these have even yet been determined. But, details aside, the ice age now has full recognition from geologists as an historical period. There may have been many ice ages, as Dr. Croll contends; there was surely one; and the conception of such a period is one of the very few ideas of our century that no previous century had even so much as faintly adumbrated.

THE GEOLOGICAL AGES

But, for that matter, the entire subject of historical geology is one that had but the barest beginning before our century. Until the paleontologist found out the key to the earth's chronology, no one—not even Hutton— could have any definite idea as to the true story of the earth's past. The only conspicuous attempt to classify the strata was that made by Werner, who divided the rocks into three systems, based on their supposed order of deposition, and called primary, transition, and secondary.

Though Werner's observations were confined to the small province of Saxony, he did not hesitate to affirm that all over the world the succession of strata would be found the same as there, the concentric layers, according to this conception, being arranged about the earth with the regularity of layers on an onion. But in this Werner was as mistaken as in his theoretical explanation of the origin of the "primary" rocks. It required but little observation to show that the exact succession of strata is never precisely the same in any widely separated regions. Nevertheless, there was a germ of truth in Werner's system. It contained the idea, however faultily interpreted, of a chronological succession of strata; and it furnished a working outline for the observers who were to make out the true story of geological development. But the correct interpretation of the observed facts could only be made after the Huttonian view as to the origin of strata had gained complete acceptance.

When William Smith, having found the true key to this story, attempted to apply it, the territory with which he had to deal chanced to be one where the surface rocks are of that later series which Werner termed secondary. He made numerous subdivisions within this system, based mainly on the fossils. Meantime it was found that, judged by the fossils, the strata that Brongniart and Cuvier studied near Paris were of a still more recent period (presumed at first to be due to the latest deluge), which came to be spoken of as tertiary. It was in these beds, some of which seemed to have been formed in fresh−water lakes, that many of the strange mammals which Cuvier first described were found.

But the "transition" rocks, underlying the "secondary" system that Smith studied, were still practically unexplored when, along in the thirties, they were taken in hand by Roderick Impey Murchison, the reformed fox−hunter and ex−captain, who had turned geologist to such notable advantage, and Adam Sedgwick, the brilliant Woodwardian professor at Cambridge.

Working together, these two friends classified the

transition rocks into chronological groups, since familiar to every one in the larger outlines as the Silurian system (age of invertebrates) and the Devonian system (age of fishes)—names derived respectively from the country of the ancient Silures, in Wales and Devonshire, England. It was subsequently discovered that these systems of strata, which crop out from beneath newer rocks in restricted areas in Britain, are spread out into broad, undisturbed sheets over thousands of miles in continental Europe and in America. Later on Murchison studied them in Russia, and described them, conjointly with Verneuil and Von Kerserling, in a ponderous and classical work. In America they were studied by Hall, Newberry, Whitney, Dana, Whitfield, and other pioneer geologists, who all but anticipated their English contemporaries.

The rocks that are of still older formation than those studied by Murchison and Sedgwick (corresponding in location to the "primary" rocks of Werner's conception) are the surface feature of vast areas in Canada, and were first prominently studied there by William I. Logan, of the Canadian Government Survey, as early as 1846, and later on by Sir William Dawson. These rocks —comprising the Laurentian system—were formerly supposed to represent parts of the original crust of the earth, formed on first cooling from a molten state; but they are now more generally regarded as once−stratified deposits metamorphosed by the action of heat.

Whether "primitive" or metamorphic, however, these Canadian rocks, and analogous ones beneath the fossiliferous strata of other countries, are the oldest portions of the earth's crust of which geology has any present knowledge. Mountains of this formation, as the Adirondacks and the Storm King range, overlooking the Hudson near West Point, are the patriarchs of their kind, beside which Alleghanies and Sierra Nevadas are recent upstarts, and Rockies, Alps, and Andes are mere parvenus of yesterday.

The Laurentian rocks were at first spoken of as representing "Azoic" time; but in 1846 Dawson found a formation deep in their midst which was believed to b e the fossil relic of a very low form of life, and after that it became customary to speak of the system as "Eozoic." Still more recently the title of Dawson's supposed fossil to rank as such has been questioned, and Dana's suggestion that the early rocks be termed merely Archman has met with general favor. Murchison and Sedgwick's Silurian, Devonian, and Carboniferous groups (the ages of invertebrates, of fishes, and of coal plants, respectively) are together spoken of as representing Paleozoic time. William Smith's system of strata, next above these, once called "secondary," represents Mesozoic time, or the age of reptiles. Still higher, or more recent, are Cuvier and Brongniart's tertiary rocks, representing the age of mammals. Lastly, the most recent formations, dating back, however, to a period far enough from recent in any but a geological sense, are classed as quaternary, representing the age of man.

It must not be supposed, however, that the successive "ages" of the geologist are shut off from one another in any such arbitrary way as this verbal classification might seem to suggest. In point of fact, these "ages" have no better warrant for existence than have the "centuries" and the "weeks" of every−day computation. They are convenient, and they may even stand for local divisions in the strata, but they are bounded by no actual gaps in the sweep of terrestrial events.

Moreover, it must be understood that the "ages" of different continents, though described under the same name, are not necessarily of exact contemporaneity. There is no sure test available by which it could be shown that the Devonian age, for instance, as outlined in the strata of Europe, did not begin millions of years earlier or later than the period whose records are said to represent the Devonian age in America. In attempting to decide such details as this, mineralogical data fail us utterly. Even in rocks of adjoining regions identity of structure is no proof of contemporaneous origin; for the veritable substance of the rock of one age is ground up to build the rocks of subsequent ages. Furthermore, in seas where conditions change but little the same form of rock may be made age after age. It is believed that chalk−beds still forming in some of our present seas may form one continuous mass dating back to earliest geologic ages. On the other hand, rocks different in character maybe formed at the same time in regions not far apart—say a sandstone along shore, a coral limestone farther seaward, and a chalk−bed beyond. This continuous stratum, broken in the process of upheaval, might seem the record of three different epochs.

Paleontology, of course, supplies far better chronological tests, but even these have their limitations. There has been no time since rocks now in existence were formed, if ever, when the earth had a uniform climate and a single undiversified fauna over its entire land surface, as the early paleontologists supposed. Speaking broadly, the same general stages have attended the evolution of organic forms everywhere, but there is nothing to show that equal periods of time witnessed corresponding changes in diverse regions, but quite the contrary. To cite but a single illustration, the marsupial order, which is the dominant mammalian type of the living fauna of Australia to−day, existed in Europe and died out there in the tertiary age. Hence a future geologist might think the Australia of to−day contemporaneous with a period in Europe which in reality antedated it by perhaps millions of years.

All these puzzling features unite to render the subject of historical geology anything but the simple matter the fathers of the science esteemed it. No one would now attempt to trace the exact sequence of formation of all the mountains of the globe, as Elie de Beaumont did a half−century ago. Even within the limits of a single continent, the geologist must proceed with much caution in attempting to chronicle the order in which its various parts rose from the matrix of the sea. The key to this story is found in the identification of the strata that are the surface feature in each territory. If Devonian rocks are at the surface in any given region, for example, it would appear that this region became a land surface in the Devonian age, or just afterwards. But a moment's consideration shows that there is an element of uncertainty about this, due to the steady denudation that all land surfaces undergo. The Devonian rocks may lie at the surface simply because the thousands of feet of carboniferous strata that once lay above them have been worn away. All that the cautious geologist dare assert, therefore, is that the region in question did not become permanent land surface earlier than the Devonian age.

But to know even this is much—sufficient, indeed, to establish the chronological order of elevation, if not its exact period, for all parts of any continent that have been geologically explored—understanding always that there must be no scrupling about a latitude of a few millions or perhaps tens of millions of years here and there.

Regarding our own continent, for example, we learn through the researches of a multitude of workers that in the early day it was a mere archipelago. Its chief island—the backbone of the future continent—was a great V−shaped area surrounding what is now Hudson Bay, an area built tip, perhaps, through denudation of a yet more ancient polar continent, whose existence is only conjectured. To the southeast an island that is now the Adirondack Mountains, and another that is now the Jersey Highlands rose above the waste of waters, and far to the south stretched probably a line of islands now represented by the Blue Ridge Mountains. Far off to the westward another line of islands foreshadowed our present Pacific border. A few minor islands in the interior completed the archipelago.

From this bare skeleton the continent grew, partly by the deposit of sediment from the denudation of the original islands (which once towered miles, perhaps, where now they rise thousands of feet), but largely also by the deposit of organic remains, especially in the interior sea, which teemed with life. In the Silurian ages, invertebrates—brachiopods and crinoids and cephalopods—were the dominant types. But very early—no one knows just when—there came fishes of many strange forms, some of the early ones enclosed in turtle−like shells. Later yet, large spaces within the interior sea having risen to the surface, great marshes or forests of strange types of vegetation grew and deposited their remains to form coal−beds. Many times over such forests were formed, only to be destroyed by the oscillations of the land surface. All told, the strata of this Paleozoic period aggregate several miles in thickness, and the time consumed in their formation stands to all later time up to the present, according to Professor Dana's estimate, as three to one.

Towards the close of this Paleozoic era the Appalachian Mountains were slowly upheaved in great convoluted folds, some of them probably reaching three or four miles above the sea−level, though the tooth of time has since gnawed them down to comparatively puny limits. The continental areas thus enlarged were peopled during the ensuing Mesozoic time with multitudes of strange reptiles, many of them gigantic in size. The waters, too, still teeming with invertebrates and fishes, had their quota of reptilian monsters; and in the air were flying reptiles, some of which measured twenty− five feet from tip to tip of their batlike wings. During this era the Sierra Nevada Mountains rose. Near the eastern border of the forming continent the strata were perhaps now too thick and stiff to bend into mountain folds, for they were rent into great fissures, letting out floods of molten lava, remnants of which are still in evidence after ages of denudation, as the Palisades along the Hudson, and such elevations as Mount Holyoke in western Massachusetts.

Still there remained a vast interior sea, which later on, in the tertiary age, was to be divided by the slow uprising of the land, which only yesterday—that is to say, a million, or three or five or ten million, years ago—became the Rocky Mountains. High and erect these young mountains stand to this day, their sharp angles and rocky contours vouching for their youth, in strange contrast with the shrunken forms of the old Adirondacks, Green Mountains, and Appalachians, whose lowered heads and rounded shoulders attest the weight of ages. In the vast lakes which still remained on either side of the Rocky range, tertiary strata were slowly formed to the ultimate depth of two or three miles, enclosing here and there those vertebrate remains which were to be exposed again to view by denudation when the land rose still higher, and then, in our own time, to tell so wonderful a story to the paleontologist.

Finally, the interior seas were filled, and the shore lines of the continent assumed nearly their present outline.

Then came the long winter of the glacial epoch—perhaps of a succession of glacial epochs. The ice sheet extended southward to about the fortieth parallel, driving some animals before it, and destroying those that were unable to migrate. At its fulness, the great ice mass lay almost a mile in depth over New England, as attested by the scratched and polished rock surfaces and deposited erratics in the White Mountains. Such a mass presses down with a weight of about one hundred and twenty−five tons to the square foot, according to Dr. Croll's estimate. It crushed and ground everything beneath it more or less, and in some regions planed off hilly surfaces into prairies. Creeping slowly forward, it carried all manner of debris with it. When it melted away its terminal moraine built up the nucleus of the land masses now known as Long Island and Staten Island; other of its deposits formed the "drumlins" about Boston famous as Bunker and Breed's hills; and it left a long, irregular line of ridges of "till" or bowlder clay and scattered erratics clear across the country at about the latitude of New York city.

As the ice sheet slowly receded it left minor moraines all along its course. Sometimes its deposits dammed up river courses or inequalities in the surface, to form the lakes which everywhere abound over Northern territories.

Some glacialists even hold the view first suggested by Ramsey, of the British Geological Survey, that the great glacial sheets scooped out the basins of many lakes, including the system that feeds the St. Lawrence. At all events, it left traces of its presence all along the line of its retreat, and its remnants exist to this day as mountain glaciers and the polar ice cap. Indeed, we live on the border of the last glacial epoch, for with the closing of this period the long geologic past merges into the present.

PAST, PRESENT, AND FUTURE

And the present, no less than the past, is a time of change. This is the thought which James Hutton conceived more than a century ago, but which his contemporaries and successors were so very slow to appreciate. Now, however, it has become axiomatic—one can hardly realize that it was ever doubted. Every new scientific truth, says Agassiz, must pass through three stages —first, men say it is not true; then they declare it hostile to religion; finally, they assert that every one has known it always. Hutton's truth that natural law is changeless and eternal has reached this final stage. Nowhere now could you find a scientist who would dispute the truth of that text which Lyell, quoting from Playfair's Illustrations of the Huttonian Theory, printed on the title−page of his Principles: "Amid all the revolutions of the globe the economy of Nature has been uniform, and her laws are the only things that have resisted the general movement. The rivers and the rocks, the seas and the continents, have been changed in all their parts; but the laws which direct those changes, and the rules to which they are subject, have remained invariably the same."

But, on the other hand, Hutton and Playfair, and in particular Lyell, drew inferences from this principle which the modern physicist can by no means admit. To them it implied that the changes on the surface of the earth have always been the same in degree as well as in kind, and must so continue while present forces hold their sway. In other words, they thought of the world as a great perpetual−motion machine. But the modern physicist, given truer mechanical insight by the doctrines of the conservation and the dissipation of energy, will have none of that. Lord Kelvin, in particular, has urged that in the periods of our earth's in fancy and adolescence its developmental changes must have been, like those of any other infant organism, vastly more rapid and pronounced than those of a later day; and to every clear thinker this truth also must now seem axiomatic.

Whoever thinks of the earth as a cooling globe can hardly doubt that its crust, when thinner, may have heaved under strain of the moon's tidal pull—whether or not that body was nearer—into great billows, daily rising and falling, like waves of the present seas vastly magnified.

Under stress of that same lateral pressure from contraction which now produces the slow depression of the Jersey coast, the slow rise of Sweden, the occasional belching of an insignificant volcano, the jetting of a geyser, or the trembling of an earthquake, once large areas were rent in twain, and vast floods of lava flowed over thousands of square miles of the earth's surface, perhaps, at a single jet; and, for aught we know to the contrary, gigantic mountains may have heaped up their contorted heads in cataclysms as spasmodic as even the most ardent catastrophist of the elder day of geology could have imagined.

The atmosphere of that early day, filled with vast volumes of carbon, oxygen, and other chemicals that have since been stored in beds of coal, limestone, and granites, may have worn down the rocks on the one hand and built up organic forms on the other, with a rapidity that would now seem hardly conceivable.

And yet while all these anomalous things went on, the same laws held sway that now are operative; and a true doctrine of uniformitarianism would make no unwonted concession in conceding them all—though most of the imbittered geological controversies of the middle of the nineteenth century were due to the failure of both parties to realize that simple fact.

And as of the past and present, so of the future. The same forces will continue to operate; and under operation of these unchanging forces each day will differ from every one that has preceded it. If it be true, as every physicist believes, that the earth is a cooling globe, then, whatever its present stage of refrigeration, the time must come when its surface contour will assume a rigidity of level not yet attained. Then, just as surely, the slow action of the elements will continue to wear away the land surfaces, particle by particle, and transport them to the ocean, as it does to−day, until, compensation no longer being afforded by the upheaval of the continents, the last foot of dry land will sink for the last time beneath the water, the last mountain− peak melting away, and our globe, lapsing like any other organism into its second childhood, will be on the surface—as presumably it was before the first continent rose—one vast "waste of waters." As puny man conceives time and things, an awful cycle will have lapsed; in the sweep of the cosmic life, a pulse− beat will have throbbed.

V. THE NEW SCIENCE OF METEOROLOGY

METEORITES

"An astonishing miracle has just occurred in our district," wrote M. Marais, a worthy if undistinguished citizen of France, from his home at L'Aigle, under date of "the 13th Floreal, year 11"—a date which outside of France would be interpreted as meaning May 3, 1803. This "miracle" was the appearance of a "fireball" in broad daylight—"perhaps it was wildfire," says the naive chronicle—which "hung over the meadow," being seen by many people, and then exploded with a loud sound, scattering thousands of stony fragments over the surface of a territory some miles in extent.

Such a "miracle" could not have been announced at a more opportune time. For some years the scientific world had been agog over the question whether such a form of lightning as that reported—appearing in a clear sky, and hurling literal thunderbolts—had real existence. Such cases had been reported often enough, it is true. The "thunderbolts" themselves were exhibited as sacred relics before many an altar, and those who doubted their authenticity had been chided as having "an evil heart of unbelief." But scientific scepticism had questioned the evidence, and late in the eighteenth century a consensus of opinion in the French Academy had declined to admit that such stones had been "conveyed to the earth by lightning," let alone any more miraculous agency.

In 1802, however, Edward Howard had read a paper before the Royal Society in which, after reviewing the evidence recently put forward, he had reached the conclusion that the fall of stones from the sky, sometimes or always accompanied by lightning, must be admitted as an actual phenomenon, however inexplicable. So now, when the great stone−fall at L'Aigle was announced, the French Academy made haste to send the brilliant young physicist Jean Baptiste Biot to investigate it, that the matter might, if possible, be set finally at rest. The investigation was in all respects successful, and Biot's report transferred the stony or metallic lightning−bolt—the aerolite or meteorite—from the realm of tradition and conjecture to that of accepted science.

But how explain this strange phenomenon? At once speculation was rife. One theory contended that the stony masses had not actually fallen, but had been formed from the earth by the action of the lightning; but this contention was early abandoned. The chemists were disposed to believe that the aerolites had been formed by the combination of elements floating in the upper atmosphere. Geologists, on the other hand, thought them of terrestrial origin, urging that they might have been thrown up by volcanoes. The astronomers, as represented by Olbers and Laplace, modified this theory by suggesting that the stones might, indeed, have been cast out by volcanoes, but by volcanoes situated not on the earth, but on the moon.

And one speculator of the time took a step even more daring, urging that the aerolites were neither of telluric nor selenitic origin, nor yet children of the sun, as the old Greeks had, many of them, contended, but that they are visitants from the depths of cosmic space. This bold speculator was the distinguished German physicist Ernst F. F. Chladni, a man of no small repute in his day. As early as 1794 he urged his cosmical theory of meteorites, when the very existence of meteorites was denied by most scientists. And he did more: he declared his belief that these falling stones were really one in origin and kind with those flashing meteors of the upper atmosphere which are familiar everywhere as "shooting−stars."

Each of these coruscating meteors, he affirmed, must tell of the ignition of a bit of cosmic matter entering the earth's atmosphere. Such wandering bits of matter might be the fragments of shattered worlds, or, as Chladni thought more probable, merely aggregations of "world stuff" never hitherto connected with any large planetary mass.

Naturally enough, so unique a view met with very scant favor. Astronomers at that time saw little to justify it; and the non−scientific world rejected it with fervor as being "atheistic and heretical," because its acceptance would seem to imply that the universe is not a perfect mechanism.

Some light was thrown on the moot point presently by the observations of Brandes and Benzenberg, which tended to show that falling−stars travel at an actual speed of from fifteen to ninety miles a second. This observation tended to discredit the selenitic theory, since an object, in order to acquire such speed in falling merely from the moon, must have been projected with an initial velocity not conceivably to be given by any lunar volcanic impulse. Moreover, there was a growing conviction that there are no active volcanoes on the moon, and other considerations of the same tenor led to the complete abandonment of the selenitic theory.

But the theory of telluric origin of aerolites was by no means so easily disposed of. This was an epoch when electrical phenomena were exciting unbounded and universal interest, and there was a not unnatural tendency to appeal to electricity in explanation of every obscure phenomenon; and in this case the seeming similarity between a lightning flash and the flash of an aerolite lent color to the explanation. So we find Thomas Forster, a meteorologist of repute, still adhering to the atmospheric theory of formation of aerolites in his book published in 1823; and, indeed, the prevailing opinion of the time seemed divided between various telluric theories, to the neglect of any cosmical theory whatever.

But in 1833 occurred a phenomenon which set the matter finally at rest. A great meteoric shower occurred in November of that year, and in observing it Professor Denison Olmstead, of Yale, noted that all the stars of the shower appeared to come from a single centre or vanishing−point in the heavens, and that this centre shifted its position with the stars, and hence was not telluric. The full significance of this observation was at once recognized by astronomers; it demonstrated beyond all cavil the cosmical origin of the shooting−stars. Some conservative meteorologists kept up the argument for the telluric origin for some decades to come, as a matter of course—such a band trails always in the rear of progress. But even these doubters were silenced when the great shower of shooting− stars appeared again in 1866, as predicted by Olbers and Newton, radiating from the same point of the heavens as before.

Since then the spectroscope has added its confirmatory evidence as to the identity of meteorite and shooting−star, and, moreover, has linked these atmospheric meteors with such distant cosmic residents as comets and nebulae. Thus it appears that Chladni's daring hypothesis of 1794 has been more than verified, and that the fragments of matter dissociated from planetary connection—which be postulated and was declared atheistic for postulating—have been shown to be billions of times more numerous than any larger cosmic bodies of which we have cognizance—so widely does the existing universe differ from man's preconceived notions as to what it should be.

Thus also the "miracle" of the falling stone, against which the scientific scepticism of yesterday presented "an evil heart of unbelief," turns out to be the most natural phenomena, inasmuch as it is repeated in our atmosphere some millions of times each day.

THE AURORA BOREALIS

If fire−balls were thought miraculous and portentous in days of yore, what interpretation must needs have been put upon that vastly more picturesque phenomenon, the aurora? "Through all the city," says the Book of Maccabees, "for the space of almost forty days, there were seen horsemen running in the air, in cloth of gold, armed with lances, like a band of soldiers: and troops of horsemen in array encountering and running one against another, with shaking of shields and multitude of pikes, and drawing of swords, and casting of darts, and glittering of golden ornaments and harness." Dire omens these; and hardly less ominous the aurora seemed to all succeeding generations that observed it down well into the eighteenth century—as witness the popular excitement in England in 1716 over the brilliant aurora of that year, which became famous through Halley's description.

But after 1752, when Franklin dethroned the lightning, all spectacular meteors came to be regarded as natural phenomena, the aurora among the rest. Franklin explained the aurora—which was seen commonly enough in the eighteenth century, though only recorded once in the seventeenth—as due to the accumulation of electricity on the surface of polar snows, and its discharge to the equator through the upper atmosphere. Erasmus Darwin suggested that the luminosity might be due to the ignition of hydrogen, which was supposed by many philosophers to form the upper atmosphere. Dalton, who first measured the height of the aurora, estimating it at about one hundred miles, thought the phenomenon due to magnetism acting on ferruginous particles in the air, and his explanation was perhaps the most popular one at the beginning of the last century.

Since then a multitude of observers have studied the aurora, but the scientific grasp has found it as elusive in fact as it seems to casual observation, and its exact nature is as undetermined to−day as it was a hundred years ago. There has been no dearth of theories concerning it, however. Blot, who studied it in the Shetland Islands in 1817, thought it due to electrified ferruginous dust, the origin of which he ascribed to Icelandic volcanoes. Much more recently the idea of ferruginous particles has been revived, their presence being ascribed not to volcanoes, but to the meteorites constantly being dissipated in the upper atmosphere. Ferruginous dust, presumably of such origin, has been found on the polar snows, as well as on the snows of mountain−tops, but whether it could produce the phenomena of auroras is at least an open question.

Other theorists have explained the aurora as due to the accumulation of electricity on clouds or on spicules of ice in the upper air. Yet others think it due merely to the passage of electricity through rarefied air itself. Humboldt considered the matter settled in yet another way when Faraday showed, in 1831, that magnetism may produce luminous effects. But perhaps the prevailing theory of to−day assumes that the aurora is due to a current of electricity generated at the equator and passing through upper regions of space, to enter the earth at the magnetic poles—simply reversing the course which Franklin assumed.

The similarity of the auroral light to that generated in a vacuum bulb by the passage of electricity lends support to the long−standing supposition that the aurora is of electrical origin, but the subject still awaits complete elucidation. For once even that mystery− solver the spectroscope has been baffled, for the line it sifts from the aurora is not matched by that of any recognized substance. A like line is found in the zodiacal light, it is true, but this is of little aid, for the zodiacal light, though thought by some astronomers to be due to meteor swarms about the sun, is held to be, on the whole, as mysterious as the aurora itself.

Whatever the exact nature of the aurora, it has long been known to be intimately associated with the phenomena of terrestrial magnetism. Whenever a brilliant aurora is visible, the world is sure to be visited with what Humboldt called a magnetic storm—a "storm" which manifests itself to human senses in no way whatsoever except by deflecting the magnetic needle and conjuring with the electric wire. Such magnetic storms are curiously associated also with spots on the sun—just how no one has explained, though the fact itself is unquestioned. Sun−spots, too, seem directly linked with auroras, each of these phenomena passing through periods of greatest and least frequency in corresponding cycles of about eleven years' duration.

It was suspected a full century ago by Herschel that the variations in the number of sun−spots had a direct effect upon terrestrial weather, and he attempted to demonstrate it by using the price of wheat as a criterion of climatic conditions, meantime making careful observation of the sun−spots. Nothing very definite came of his efforts in this direction, the subject being far too complex to be determined without long periods of observation. Latterly, however, meteorologists, particularly in the tropics, are disposed to think they find evidence of some such connection between sun−spots and the weather as Herschel suspected. Indeed, Mr. Meldrum declares that there is a positive coincidence between periods of numerous sun−spots and seasons of excessive rain in India.

That some such connection does exist seems intrinsically probable. But the modern meteorologist, learning wisdom of the past, is extremely cautious about ascribing casual effects to astronomical phenomena. He finds it hard to forget that until recently all manner of climatic conditions were associated with phases of the moon; that not so very long ago showers of falling−stars were considered "prognostic" of certain kinds of weather; and that the "equinoctial storm" had been accepted as a verity by every one, until the unfeeling hand of statistics banished it from the earth.

Yet, on the other hand, it is easily within the possibilities that the science of the future may reveal associations between the weather and sun−spots, auroras, and terrestrial magnetism that as yet are hardly dreamed of. Until such time, however, these phenomena must feel themselves very grudgingly admitted to the inner circle of meteorology. More and more this science concerns itself, in our age of concentration and specialization, with weather and climate. Its votaries no longer concern themselves with stars or planets or comets or shooting−stars—once thought the very essence of guides to weather wisdom; and they are even looking askance at the moon, and asking her to show cause why she also should not be excluded from their domain. Equally little do they care for the interior of the earth, since they have learned that the central emanations of heat which Mairan imagined as a main source of aerial warmth can claim no such distinction. Even such problems as why the magnetic pole does not coincide with the geographical, and why the force of terrestrial magnetism decreases from the magnetic poles to the magnetic equator, as Humboldt first discovered that it does, excite them only to lukewarm interest; for magnetism, they say, is not known to have any connection whatever with climate or weather.

EVAPORATION, CLOUD FORMATION, AND DEW

There is at least one form of meteor, however, of those that interested our forebears whose meteorological importance they did not overestimate. This is the vapor of water. How great was the interest in this familiar meteor at the beginning of the century is attested by the number of theories then extant regarding it; and these conflicting theories bear witness also to the difficulty with which the familiar phenomenon of the evaporation of water was explained.

Franklin had suggested that air dissolves water much as water dissolves salt, and this theory was still popular, though Deluc had disproved it by showing that water evaporates even more rapidly in a vacuum than in air. Deluc's own theory, borrowed from earlier chemists, was that evaporation is the chemical union of particles of water with particles of the supposititious element heat. Erasmus Darwin combined the two theories, suggesting that the air might hold a variable quantity of vapor in mere solution, and in addition a permanent moiety in chemical combination with caloric.

Undisturbed by these conflicting views, that strangely original genius, John Dalton, afterwards to be known as perhaps the greatest of theoretical chemists, took the question in hand, and solved it by showing that water exists in the air as an utterly independent gas. He reached a partial insight into the matter in 1793, when his first volume of meteorological essays was published; but the full elucidation of the problem came to him in 1801. The merit of his studies was at once recognized, but the tenability of his hypothesis was long and ardently disputed.

While the nature of evaporation was in dispute, as a matter of course the question of precipitation must be equally undetermined. The most famous theory of the period was that formulated by Dr. Hutton in a paper read before the Royal Society of Edinburgh, and published in the volume of transactions which contained also the same author's epoch−making paper on geology. This "theory of rain" explained precipitation as due to the cooling of a current of saturated air by contact with a colder current, the assumption being that the surplusage of moisture was precipitated in a chemical sense, just as the excess of salt dissolved in hot water is precipitated when the water cools. The idea that the cooling of the saturated air causes the precipitation of its moisture is the germ of truth that renders this paper of Hutton's important. All correct later theories build on this foundation.

"Let us suppose the surface of this earth wholly covered with water," said Hutton, "and that the sun were stationary, being always vertical in one place; then, from the laws of heat and rarefaction, there would be formed a circulation in the atmosphere, flowing from the dark and cold hemisphere to the heated and illuminated place, in all directions, towards the place of the greatest cold.

"As there is for the atmosphere of this earth a constant cooling cause, this fluid body could only arrive at a certain degree of heat; and this would be regularly decreasing from the centre of illumination to the opposite point of the globe, most distant from the light and heat. Between these two regions of extreme heat and cold there would, in every place, be found two streams of air following in opposite directions. If those streams of air, therefore, shall be supposed as both sufficiently saturated with humidity, then, as they are of different temperatures, there would be formed a continual condensation of aqueous vapor, in some middle region of the atmosphere, by the commixtion of part of those two opposite streams.

"Hence there is reason to believe that in this supposed case there would be formed upon the surface of the globe three different regions—the torrid region, the temperate, and the frigid. These three regions would continue stationary; and the operations of each would be continual. In the torrid region, nothing but evaporation and heat would take place; no cloud could be formed, because in changing the transparency of the atmosphere to opacity it would be heated immediately by the operation of light, and thus the condensed water would be again evaporated. But this power of the sun would have a termination; and it is these that would begin the region of temperate heat and of continual rain. It is not probable that the region of temperance would reach far beyond the region of light; and in the hemisphere of darkness there would be found a region of extreme cold and perfect dryness.

"Let us now suppose the earth as turning on its axis in the equinoctial situation. The torrid region would thus be changed into a zone, in which there would be night and day; consequently, here would be much temperance, compared with the torrid region now considered; and here perhaps there would be formed periodical condensation and evaporation of humidity, corresponding to the seasons of night and day. As temperance would thus be introduced into the region of torrid extremity, so would the effect of this change be felt over all the globe, every part of which would now be illuminated, consequently heated in some degree. Thus we would have a line of great heat and evaporation, graduating each way into a point of great cold and congelation. Between these two extremes of heat and cold there would be found in each hemisphere a region of much temperance, in relation to heat, but of much humidity in the atmosphere, perhaps of continual rain and condensation.

"The supposition now formed must appear extremely unfit for making this globe a habitable world in every part; but having thus seen the effect of night and day in temperating the effects of heat and cold in every place, we are now prepared to contemplate the effects of supposing this globe to revolve around the sun with a certain inclination of its axis. By this beautiful contrivance, that comparatively uninhabited globe is now divided into two hemispheres, each of which is thus provided with a summer and a winter season. But our present view is limited to the evaporation and condensation of humidity; and, in this contrivance of the seasons, there must appear an ample provision for those alternate operations in every part; for as the place of the vertical sun is moved alternately from one tropic to the other, heat and cold, the original causes of evaporation and condensation, must be carried over all the globe, producing either annual seasons of rain or diurnal seasons of condensation and evaporation, or both these seasons, more or less—that is, in some degree.

"The original cause of motion in the atmosphere is the influence of the sun heating the surface of the earth exposed to that luminary. We have not supposed that surface to have been of one uniform shape and similar substance; from whence it has followed that the annual propers of the sun, perhaps also the diurnal propers, would produce a regular condensation of rain in certain regions, and the evaporation of humidity in others; and this would have a regular progress in certain determined seasons, and would not vary. But nothing can be more distant from this supposition, that is the natural constitution of the earth; for the globe is composed of sea and land, in no regular shape or mixture, while the surface of the land is also irregular with respect to its elevations and depressions, and various with regard to the humidity and dryness of that part which is exposed to heat as the cause of evaporation. Hence a source of the most valuable motions in the fluid atmosphere with aqueous vapor, more or less, so far as other natural operations will admit; and hence a source of the most irregular commixture of the several parts of this elastic fluid, whether saturated or not with aqueous vapor.

"According to the theory, nothing is required for the production of rain besides the mixture of portions of the atmosphere with humidity, and of mixing the parts that are in different degrees of heat. But we have seen the causes of saturating every portion of the atmosphere with humidity and of mixing the parts which are in different degrees of heat. Consequently, over all the surface of the globe there should happen occasionally rain and evaporation, more or less; and also, in every place, those vicissitudes should be observed to take place with some tendency to regularity, which, however, may be so disturbed as to be hardly distinguishable upon many occasions. Variable winds and variable rains should be found in proportion as each place is situated in an irregular mixture of land and water; whereas regular winds should be found in proportion to the uniformity of the surface; and regular rains in proportion to the regular changes of those winds by which the mixture of the atmosphere necessary to the rain may be produced. But as it will be acknowledged that this is the case in almost all this earth where rain appears according to the conditions here specified, the theory is found to be thus in conformity with nature, and natural appearances are thus explained by the theory."[1]

The next ambitious attempt to explain the phenomena of aqueous meteors was made by Luke Howard, in his remarkable paper on clouds, published in the Philosophical Magazine in 1803—the paper in which the names cirrus, cumulus, stratus, etc., afterwards so universally adopted, were first proposed. In this paper Howard acknowledges his indebtedness to Dalton for the theory of evaporation; yet he still clings to the idea that the vapor, though independent of the air, is combined with particles of caloric. He holds that clouds are composed of vapor that has previously risen from the earth, combating the opinions of those who believe that they are formed by the union of hydrogen and oxygen existing independently in the air; though he agrees with these theorists that electricity has entered largely into the modus operandi of cloud formation. He opposes the opinion of Deluc and De Saussure that clouds are composed of particles of water in the form of hollow vesicles (miniature balloons, in short, perhaps filled with hydrogen), which untenable opinion was a revival of the theory as to the formation of all vapor which Dr. Halley had advocated early in the eighteenth century.

Of particular interest are Howard's views as to the formation of dew, which he explains as caused by the particles of caloric forsaking the vapor to enter the cool body, leaving the water on the surface. This comes as near the truth, perhaps, as could be expected while the old idea as to the materiality of heat held sway. Howard believed, however, that dew is usually formed in the air at some height, and that it settles to the surface, opposing the opinion, which had gained vogue in France and in America (where Noah Webster prominently advocated it), that dew ascends from the earth.

The complete solution of the problem of dew formation— which really involved also the entire question of precipitation of watery vapor in any form—was made by Dr. W. C. Wells, a man of American birth, whose life, however, after boyhood, was spent in Scotland (where as a young man he enjoyed the friendship of David Hume) and in London. Inspired, no doubt, by the researches of Mack, Hutton, and their confreres of that Edinburgh school, Wells made observations on evaporation and precipitation as early as 1784, but other things claimed his attention; and though he asserts that the subject was often in his mind, he did not take it up again in earnest until about 1812.

Meantime the observations on heat of Rumford and Davy and Leslie had cleared the way for a proper interpretation of the facts—about the facts themselves there had long been practical unanimity of opinion. Dr. Black, with his latent−heat observations, had really given the clew to all subsequent discussions of the subject of precipitation of vapor; and from this time on it had been known that heat is taken up when water evaporates, and given out again when it condenses. Dr. Darwin had shown in 1788, in a paper before the Royal Society, that air gives off heat on contracting and takes it up on expanding; and Dalton, in his essay of 1793, had explained this phenomenon as due to the condensation and vaporization of the water contained in the air.

But some curious and puzzling observations which Professor Patrick Wilson, professor of astronomy in the University of Glasgow, had communicated to the Royal Society of Edinburgh in 1784, and some similar ones made by Mr. Six, of Canterbury, a few years later, had remained unexplained. Both these gentlemen observed that the air is cooler where dew is forming than the air a few feet higher, and they inferred that the dew in forming had taken up heat, in apparent violation of established physical principles.

It remained for Wells, in his memorable paper of 1816, to show that these observers had simply placed the cart before the horse. He made it clear that the air is not cooler because the dew is formed, but that the dew is formed because the air is cooler—having become so through radiation of heat from the solids on which the dew forms. The dew itself, in forming, gives out its latent heat, and so tends to equalize the temperature.

Wells's paper is so admirable an illustration of the lucid presentation of clearly conceived experiments and logical conclusions that we should do it injustice not to present it entire. The author's mention of the observations of Six and Wilson gives added value to his own presentation.

Dr. Wells's Essay on Dew

"I was led in the autumn of 1784, by the event of a rude experiment, to think it probable that the formation of dew is attended with the production of cold. In 1788, a paper on hoar−frost, by Mr. Patrick Wilson, of Glasgow, was published in the first volume of the Transactions of the Royal Society of Edinburgh, by which it appeared that this opinion bad been entertained by that gentleman before it had occurred to myself. In the course of the same year, Mr. Six, of Canterbury, mentioned in a paper communicated to the Royal Society that on clear and dewy nights he always found the mercury lower in a thermometer laid upon the ground in a meadow in his neighborhood than it was in a similar thermometer suspended in the air six feet above the former; and that upon one night the difference amounted to five degrees of Fahrenheit's scale. Mr. Six, however, did not suppose, agreeably to the opinion of Mr. Wilson and myself, that the cold was occasioned by the formation of dew, but imagined that it proceeded partly from the low temperature of the air, through which the dew, already formed in the atmosphere, had descended, and partly from the evaporation of moisture from the ground, on which his thermometer had been placed. The conjecture of Mr. Wilson and the observations of Mr. Six, together with many facts which I afterwards learned in the course of reading, strengthened my opinion; but I made no attempt, before the autumn of 1811, to ascertain by experiment if it were just, though it had in the mean time almost daily occurred to my thoughts. Happening, in that season, to be in that country in a clear and calm night, I laid a thermometer upon grass wet with dew, and suspended a second in the air, two feet above the other. An hour afterwards the thermometer on the grass was found to be eight degrees lower, by Fahrenheit's division, than the one in the air. Similar results having been obtained from several similar experiments, made during the same autumn, I determined in the next spring to prosecute the subject with some degree of steadiness, and with that view went frequently to the house of one of my friends who lives in Surrey.

At the end of two months I fancied that I had collected information worthy of being published; but, fortunately, while preparing an account of it I met by accident with a small posthumous work by Mr. Six, printed at Canterbury in 1794, in which are related differences observed on dewy nights between thermometers placed upon grass and others in the air that are much greater than those mentioned in the paper presented by him to the Royal Society in 1788. In this work, too, the cold of the grass is attributed, in agreement with the opinion of Mr. Wilson, altogether to the dew deposited upon it. The value of my own observations appearing to me now much diminished, though they embraced many points left untouched by Mr. Six, I gave up my intentions of making them known. Shortly after, however, upon considering the subject more closely, I began to suspect that Mr.

Wilson, Mr. Six, and myself had all committed an error regarding the cold which accompanies dew as an effect of the formation of that fluid. I therefore resumed my experiments, and having by means of them, I think, not only established the justness of my suspicions, but ascertained the real cause both of dew and of several other natural appearances which have hitherto received no sufficient explanation, I venture now to submit to the consideration of the learned an account of some of my labors, without regard to the order of time in which they were performed, and of various conclusions which may be drawn from them, mixed with facts and opinions already published by others:

"There are various occurrences in nature which seem to me strictly allied to dew, though their relation to it be not always at first sight perceivable. The statement and explanation of several of these will form the concluding part of the present essay.

"1. I observed one morning, in winter, that the insides of the panes of glass in the windows of my bedchamber were all of them moist, but that those which had been covered by an inside shutter during the night were much more so than the others which had been uncovered. Supposing that this diversity of appearance depended upon a difference of temperature, I applied the naked bulbs of two delicate thermometers to a covered and uncovered pane; on which I found that the former was three degrees colder than the latter. The air of the chamber, though no fire was kept in it, was at this time eleven and one−half degrees warmer than that without. Similar experiments were made on many other mornings, the results of which were that the warmth of the internal air exceeded that of the external from eight to eighteen degrees, the temperature of the covered panes would be from one to five degrees less than the uncovered; that the covered were sometimes dewed, while the uncovered were dry; that at other times both were free from moisture; that the outsides of the covered and uncovered panes had similar differences with respect to heat, though not so great as those of the inner surfaces; and that no variation in the quantity of these differences was occasioned by the weather's being cloudy or fair, provided the heat of the internal air exceeded that of the external equally in both of those states of the atmosphere.

"The remote reason of these differences did not immediately present itself. I soon, however, saw that the closed shutter shielded the glass which it covered from the heat that was radiated to the windows by the walls and furniture of the room, and thus kept it nearer to the temperature of the external air than those parts could be which, from being uncovered, received the heat emitted to them by the bodies just mentioned.

"In making these experiments, I seldom observed the inside of any pane to be more than a little damped, though it might be from eight to twelve degrees colder than the general mass of the air in the room; while, in the open air, I had often found a great dew to form on substances only three or four degrees colder than the atmosphere. This at first surprised me; but the cause now seems plain. The air of the chamber had once been a portion of the external atmosphere, and had afterwards been heated, when it could receive little accessories to its original moisture. It constantly required being cooled considerably before it was even brought back to its former nearness to repletion with water; whereas the whole external air is commonly, at night, nearly replete with moisture, and therefore readily precipitates dew on bodies only a little colder than itself.

"When the air of a room is warmer than the external atmosphere, the effect of an outside shutter on the temperature of the glass of the window will be directly opposite to what has just been stated; since it must prevent the radiation, into the atmosphere, of the heat of the chamber transmitted through the glass.

"2. Count Rumford appears to have rightly conjectured that the inhabitants of certain hot countries, who sleep at nights on the tops of their houses, are cooled during this exposure by the radiation of their heat to the sky; or, according to his manner of expression, by receiving frigorific rays from the heavens. Another fact of this kind seems to be the greater chill which we often experience upon passing at night from the cover of a house into the air than might have been expected from the cold of the external atmosphere. The cause, indeed, is said to be the quickness of transition from one situation to another. But if this were the whole reason, an equal chill would be felt in the day, when the difference, in point of heat, between the internal and external air was the same as at night, which is not the case. Besides, if I can trust my own observation, the feeling of cold from this cause is more remarkable in a clear than in a cloudy night, and in the country than in towns. The following appears to be the manner in which these things are chiefly to be explained:

"During the day our bodies while in the open air, although not immediately exposed to the sun's rays, are yet constantly deriving heat from them by means of the reflection of the atmosphere. This heat, though it produces little change on the temperature of the air which it traverses, affords us some compensation for the heat which we radiate to the heavens. At night, also, if the sky be overcast, some compensation will be made to us, both in the town and in the country, though in a less degree than during the day, as the clouds will remit towards the earth no inconsiderable quantity of heat. But on a clear night, in an open part of the country, nothing almost can be returned to us from above in place of the heat which we radiate upward. In towns, however, some compensation will be afforded even on the clearest nights for the heat which we lose in the open air by that which is radiated to us from the sun round buildings.

To our loss of heat by radiation at times that we derive little compensation from the radiation of other bodies is probably to be attributed a great part of the hurtful effects of the night air. Descartes says that these are not owing to dew, as was the common opinion of his contemporaries, but to the descent of certain noxious vapors which have been exhaled from the earth during the heat of the day, and are afterwards condensed by the cold of a serene night. The effects in question certainly cannot be occasioned by dew, since that fluid does not form upon a healthy human body in temperate climates; but they may, notwithstanding, arise from the same cause that produces dew on those substances which do not, like the human body, possess the power of generating heat for the supply of what they lose by radiation or any other means."[2]

This explanation made it plain why dew forms on a clear night, when there are no clouds to reflect the radiant heat. Combined with Dalton's theory that vapor is an independent gas, limited in quantity in any given space by the temperature of that space, it solved the problem of the formation of clouds, rain, snow, and hoar−frost. Thus this paper of Wells's closed the epoch of speculation regarding this field of meteorology, as Hutton's paper of 1784 had opened it. The fact that the volume containing Hutton's paper contained also his epoch−making paper on geology finds curiously a duplication in the fact that Wells's volume contained also his essay on Albinism, in which the doctrine of natural selection was for the first time formulated, as Charles Darwin freely admitted after his own efforts had made the doctrine famous.

ISOTHERMS AND OCEAN CURRENTS

The very next year after Dr. Wells's paper was published there appeared in France the third volume of the Memoires de Physique et de Chimie de la Societe d'Arcueil, and a new epoch in meteorology was inaugurated. The society in question was numerically an inconsequential band, listing only a dozen members; but every name was a famous one: Arago, Berard, Berthollet, Biot, Chaptal, De Candolle, Dulong, Gay−Lussac, Humboldt, Laplace, Poisson, and Thenard—rare spirits every one. Little danger that the memoirs of such a band would be relegated to the dusty shelves where most proceedings of societies belong—no milk−for−babes fare would be served to such a company.

The particular paper which here interests us closes this third and last volume of memoirs. It is entitled "Des Lignes Isothermes et de la Distribution de la Chaleursurle Globe." The author is Alexander Humboldt. Needless to say, the topic is handled in a masterly manner. The distribution of heat on the surface of the globe, on the mountain−sides, in the interior of the earth; the causes that regulate such distribution; the climatic results—these are the topics discussed. But what gives epochal character to the paper is the introduction of those isothermal lines circling the earth in irregular course, joining together places having the same mean annual temperature, and thus laying the foundation for a science of comparative climatology.

It is true the attempt to study climates comparatively was not new. Mairan had attempted it in those papers in which he developed his bizarre ideas as to central emanations of heat. Euler had brought his profound mathematical genius to bear on the topic, evolving the "extraordinary conclusion that under the equator at midnight the cold ought to be more rigorous than at the poles in winter." And in particular Richard Kirwan, the English chemist, had combined the mathematical and the empirical methods and calculated temperatures for all latitudes. But Humboldt differs from all these predecessors in that he grasps the idea that the basis of all such computations should be not theory, but fact. He drew his isothermal lines not where some occult calculation would locate them on an ideal globe, but where practical tests with the thermometer locate them on our globe as it is. London, for example, lies in the same latitude as the southern extremity of Hudson Bay; but the isotherm of London, as Humboldt outlines it, passes through Cincinnati.

Of course such deviations of climatic conditions between places in the same latitude had long been known. As Humboldt himself observes, the earliest settlers of America were astonished to find themselves subjected to rigors of climate for which their European experience had not at all prepared them. Moreover, sagacious travellers, in particular Cook's companion on his second voyage, young George Forster, had noted as a general principle that the western borders of continents in temperate regions are always warmer than corresponding latitudes of their eastern borders; and of course the general truth of temperatures being milder in the vicinity of the sea than in the interior of continents had long been familiar. But Humboldt's isothermal lines for the first time gave tangibility to these ideas, and made practicable a truly scientific study of comparative climatology.

In studying these lines, particularly as elaborated by further observations, it became clear that they are by no means haphazard in arrangement, but are dependent upon geographical conditions which in most cases are not difficult to determine. Humboldt himself pointed out very clearly the main causes that tend to produce deviations from the average—or, as Dove later on called it, the normal—temperature of any given latitude. For example, the mean annual temperature of a region (referring mainly to the northern hemisphere) is raised by the proximity of a western coast; by a divided configuration of the continent into peninsulas; by the existence of open seas to the north or of radiating continental surfaces to the south; by mountain ranges to shield from cold winds; by the infrequency of swamps to become congealed; by the absence of woods in a dry, sandy soil; and by the serenity of sky in the summer months and the vicinity of an ocean current bringing water which is of a higher temperature than that of the surrounding sea.

Conditions opposite to these tend, of course, correspondingly to lower the temperature. In a word, Humboldt says the climatic distribution of heat depends on the relative distribution of land and sea, and on the "hypsometrical configuration of the continents"; and he urges that "great meteorological phenomena cannot be comprehended when considered independently of geognostic relations"—a truth which, like most other general principles, seems simple enough once it is pointed out.

With that broad sweep of imagination which characterized him, Humboldt speaks of the atmosphere as the "aerial ocean, in the lower strata and on the shoals of which we live," and he studies the atmospheric phenomena always in relation to those of that other ocean of water. In each of these oceans there are vast permanent currents, flowing always in determinate directions, which enormously modify the climatic conditions of every zone. The ocean of air is a vast maelstrom, boiling up always under the influence of the sun's heat at the equator, and flowing as an upper current towards either pole, while an undercurrent from the poles, which becomes the trade−winds, flows towards the equator to supply its place.

But the superheated equatorial air, becoming chilled, descends to the surface in temperate latitudes, and continues its poleward journey as the anti−trade−winds. The trade−winds are deflected towards the west, because in approaching the equator they constantly pass over surfaces of the earth having a greater and greater velocity of rotation, and so, as it were, tend to lag behind— an explanation which Hadley pointed out in 1735, but which was not accepted until Dalton independently worked it out and promulgated it in 1793. For the opposite reason, the anti−trades are deflected towards the east; hence it is that the western, borders of continents in temperate zones are bathed in moist sea−breezes, while their eastern borders lack this cold− dispelling influence.

In the ocean of water the main currents run as more sharply circumscribed streams—veritable rivers in the sea. Of these the best known and most sharply circumscribed is the familiar Gulf Stream, which has its origin in an equatorial current, impelled westward by trade−winds, which is deflected northward in the main at Cape St. Roque, entering the Caribbean Sea and Gulf of Mexico, to emerge finally through the Strait of Florida, and journey off across the Atlantic to warm the shores of Europe.

Such, at least, is the Gulf Stream as Humboldt understood it. Since his time, however, ocean currents in general, and this one in particular, have been the subject of no end of controversy, it being hotly disputed whether either causes or effects of the Gulf Stream are just what Humboldt, in common with others of his time, conceived them to be. About the middle of the century Lieutenant M. F. Maury, the distinguished American hydrographer and meteorologist, advocated a theory of gravitation as the chief cause of the currents, claiming that difference in density, due to difference in temperature and saltness, would sufficiently account for the oceanic circulation. This theory gained great popularity through the wide circulation of Maury's Physical Geography of the Sea, which is said to have passed through more editions than any other scientific book of the period; but it was ably and vigorously combated by Dr. James Croll, the Scottish geologist, in his Climate and Time, and latterly the old theory that ocean currents are due to the trade−winds has again come into favor. Indeed, very recently a model has been constructed, with the aid of which it is said to have been demonstrated that prevailing winds in the direction of the actual trade−winds would produce such a current as the Gulf Stream.

Meantime, however, it is by no means sure that gravitation does not enter into the case to the extent of producing an insensible general oceanic circulation, independent of the Gulf Stream and similar marked currents, and similar in its larger outlines to the polar− equatorial circulation of the air. The idea of such oceanic circulation was first suggested in detail by Professor Lenz, of St. Petersburg, in 1845, but it was not generally recognized until Dr. Carpenter independently hit upon the idea more than twenty years later. The plausibility of the conception is obvious; yet the alleged fact of such circulation has been hotly disputed, and the question is still sub judice.

But whether or not such general circulation of ocean water takes place, it is beyond dispute that the recognized currents carry an enormous quantity of heat from the tropics towards the poles. Dr. Croll, who has perhaps given more attention to the physics of the subject than almost any other person, computes that the Gulf Stream conveys to the North Atlantic one− fourth as much heat as that body receives directly from the sun, and he argues that were it not for the transportation of heat by this and similar Pacific currents, only a narrow tropical region of the globe would be warm enough for habitation by the existing faunas. Dr. Croll argues that a slight change in the relative values of northern and southern trade−winds (such as he believes has taken place at various periods in the past) would suffice to so alter the equatorial current which now feeds the Gulf Stream that its main bulk would be deflected southward instead of northward, by the angle of Cape St. Roque. Thus the Gulf Stream would be nipped in the bud, and, according to Dr. Croll's estimates, the results would be disastrous for the northern hemisphere. The anti−trades, which now are warmed by the Gulf Stream, would then blow as cold winds across the shores of western Europe, and in all probability a glacial epoch would supervene throughout the northern hemisphere.

The same consequences, so far as Europe is concerned at least, would apparently ensue were the Isthmus of Panama to settle into the sea, allowing the Caribbean current to pass into the Pacific. But the geologist tells us that this isthmus rose at a comparatively recent geological period, though it is hinted that there had been some time previously a temporary land connection between the two continents. Are we to infer, then, that the two Americas in their unions and disunions have juggled with the climate of the other hemisphere? Apparently so, if the estimates made of the influence of the Gulf Stream be tenable. It is a far cry from Panama to Russia. Yet it seems within the possibilities that the meteorologist may learn from the geologist of Central America something that will enable him to explain to the paleontologist of Europe how it chanced that at one time the mammoth and rhinoceros roamed across northern Siberia, while at another time the reindeer and musk−ox browsed along the shores of the Mediterranean.

Possibilities, I said, not probabilities. Yet even the faint glimmer of so alluring a possibility brings home to one with vividness the truth of Humboldt's perspicuous observation that meteorology can be properly comprehended only when studied in connection with the companion sciences. There are no isolated phenomena in nature.

CYCLONES AND ANTI−CYCLONES

Yet, after all, it is not to be denied that the chief concern of the meteorologist must be with that other medium, the "ocean of air, on the shoals of which we live." For whatever may be accomplished by water currents in the way of conveying heat, it is the wind currents that effect the final distribution of that heat. As Dr. Croll has urged, the waters of the Gulf Stream do not warm the shores of Europe by direct contact, but by warming the anti−trade−winds, which subsequently blow across the continent. And everywhere the heat accumulated by water becomes effectual in modifying climate, not so much by direct radiation as by diffusion through the medium of the air.

This very obvious importance of aerial currents led to their practical study long before meteorology had any title to the rank of science, and Dalton's explanation of the trade−winds had laid the foundation for a science of wind dynamics before the beginning of the nineteenth century. But no substantial further advance in this direction was effected until about 1827, when Heinrich W. Dove, of Konigsberg, afterwards to be known as perhaps the foremost meteorologist of his generation, included the winds among the subjects of his elaborate statistical studies in climatology.

Dove classified the winds as permanent, periodical, and variable. His great discovery was that all winds, of whatever character, and not merely the permanent winds, come under the influence of the earth's rotation in such a way as to be deflected from their course, and hence to take on a gyratory motion—that, in short, all local winds are minor eddies in the great polar−equatorial whirl, and tend to reproduce in miniature the character of that vast maelstrom. For the first time, then, temporary or variable winds were seen to lie within the province of law.

A generation later, Professor William Ferrel, the American meteorologist, who had been led to take up the subject by a perusal of Maury's discourse on ocean winds, formulated a general mathematical law, to the effect that any body moving in a right line along the surface of the earth in any direction tends to have its course deflected, owing to the earth's rotation, to the right hand in the northern and to the left hand in the southern hemisphere. This law had indeed been stated as early as 1835 by the French physicist Poisson, but no one then thought of it as other than a mathematical curiosity; its true significance was only understood after Professor Ferrel had independently rediscovered it (just as Dalton rediscovered Hadley's forgotten law of the trade−winds) and applied it to the motion of wind currents.

Then it became clear that here is a key to the phenomena of atmospheric circulation, from the great polar−equatorial maelstrom which manifests itself in the trade−winds to the most circumscribed riffle which is announced as a local storm. And the more the phenomena were studied, the more striking seemed the parallel between the greater maelstrom and these lesser eddies. Just as the entire atmospheric mass of each hemisphere is seen, when viewed as a whole, to be carried in a great whirl about the pole of that hemisphere, so the local disturbances within this great tide are found always to take the form of whirls about a local storm−centre—which storm−centre, meantime, is carried along in the major current, as one often sees a little whirlpool in the water swept along with the main current of the stream. Sometimes, indeed, the local eddy, caught as it were in an ancillary current of the great polar stream, is deflected from its normal course and may seem to travel against the stream; but such deviations are departures from the rule. In the great majority of cases, for example, in the north temperate zone, a storm−centre (with its attendant local whirl) travels to the northeast, along the main current of the anti−trade−wind, of which it is a part; and though exceptionally its course may be to the southeast instead, it almost never departs so widely from the main channel as to progress to the westward. Thus it is that storms sweeping over the United States can be announced, as a rule, at the seaboard in advance of their coming by telegraphic communication from the interior, while similar storms come to Europe off the ocean unannounced. Hence the more practical availability of the forecasts of weather bureaus in the former country.

But these local whirls, it must be understood, are local only in a very general sense of the word, inasmuch as a single one may be more than a thousand miles in diameter, and a small one is two or three hundred miles across. But quite without regard to the size of the whirl, the air composing it conducts itself always in one of two ways. It never whirls in concentric circles; it always either rushes in towards the centre in a descending spiral, in which case it is called a cyclone, or it spreads out from the centre in a widening spiral, in which case it is called an anti−cyclone. The word cyclone is associated in popular phraseology with a terrific storm, but it has no such restriction in technical usage. A gentle zephyr flowing towards a "storm− centre" is just as much a cyclone to the meteorologist as is the whirl constituting a West−Indian hurricane. Indeed, it is not properly the wind itself that is called the cyclone in either case, but the entire system of whirls—including the storm−centre itself, where there may be no wind at all.

What, then, is this storm−centre? Merely an area of low barometric pressure—an area where the air has become lighter than the air of surrounding regions. Under influence of gravitation the air seeks its level just as water does; so the heavy air comes flowing in from all sides towards the low−pressure area, which thus becomes a "storm−centre." But the inrushing currents never come straight to their mark. In accordance with Ferrel's law, they are deflected to the right, and the result, as will readily be seen, must be a vortex current, which whirls always in one direction—namely, from left to right, or in the direction opposite to that of the hands of a watch held with its face upward. The velocity of the cyclonic currents will depend largely upon the difference in barometric pressure between the storm−centre and the confines of the cyclone system. And the velocity of the currents will determine to some extent the degree of deflection, and hence the exact path of the descending spiral in which the wind approaches the centre. But in every case and in every part of the cyclone system it is true, as Buys Ballot's famous rule first pointed out, that a person standing with his back to the wind has the storm−centre at his left.

The primary cause of the low barometric pressure which marks the storm−centre and establishes the cyclone is expansion of the air through excess of temperature. The heated air, rising into cold upper regions, has a portion of its vapor condensed into clouds, and now a new dynamic factor is added, for each particle of vapor, in condensing, gives up its modicum of latent heat. Each pound of vapor thus liberates, according to Professor Tyndall's estimate, enough heat to melt five pounds of cast iron; so the amount given out where large masses of cloud are forming must enormously add to the convection currents of the air, and hence to the storm−developing power of the forming cyclone. Indeed, one school of meteorologists, of whom Professor Espy was the leader, has held that, without such added increment of energy constantly augmenting the dynamic effects, no storm could long continue in violent action. And it is doubted whether any storm could ever attain, much less continue, the terrific force of that most dreaded of winds of temperate zones, the tornado—a storm which obeys all the laws of cyclones, but differs from ordinary cyclones in having a vortex core only a few feet or yards in diameter—without the aid of those great masses of condensing vapor which always accompany it in the form of storm−clouds.

The anti−cyclone simply reverses the conditions of the cyclone. Its centre is an area of high pressure, and the air rushes out from it in all directions towards surrounding regions of low pressure. As before, all parts of the current will be deflected towards the right, and the result, clearly, is a whirl opposite in direction to that of the cyclone. But here there is a tendency to dissipation rather than to concentration of energy, hence, considered as a storm−generator, the anti− cyclone is of relative insignificance.

In particular the professional meteorologist who conducts a "weather bureau"—as, for example, the chief of the United States signal−service station in New York—is so preoccupied with the observation of this phenomenon that cyclone−hunting might be said to be his chief pursuit. It is for this purpose, in the main, that government weather bureaus or signal− service departments have been established all over the world. Their chief work is to follow up cyclones, with the aid of telegraphic reports, mapping their course and recording the attendant meteorological conditions. Their so−called predictions or forecasts are essentially predications, gaining locally the effect of predictions because the telegraph outstrips the wind.

At only one place on the globe has it been possible as yet for the meteorologist to make long−time forecasts meriting the title of predictions. This is in the middle Ganges Valley of northern India. In this country the climatic conditions are largely dependent upon the periodical winds called monsoons, which blow steadily landward from April to October, and seaward from October to April. The summer monsoons bring the all−essential rains; if they are delayed or restricted in extent, there will be drought and consequent famine. And such restriction of the monsoon is likely to result when there has been an unusually deep or very late snowfall on the Himalayas, because of the lowering of spring temperature by the melting snow. Thus here it is possible, by observing the snowfall in the mountains, to predict with some measure of success the average rainfall of the following summer. The drought of 1896, with the consequent famine and plague that devastated India the following winter, was thus predicted some months in advance.

This is the greatest present triumph of practical meteorology. Nothing like it is yet possible anywhere in temperate zones. But no one can say what may not be possible in times to come, when the data now being gathered all over the world shall at last be co−ordinated, classified, and made the basis of broad inductions. Meteorology is pre−eminently a science of the future.

VI. MODERN THEORIES OF HEAT AND LIGHT

THE eighteenth−century philosopher made great strides in his studies of the physical properties of matter and the application of these properties in mechanics, as the steam−engine, the balloon, the optic telegraph, the spinning−jenny, the cotton−gin, the chronometer, the perfected compass, the Leyden jar, the lightning−rod, and a host of minor inventions testify. In a speculative way he had thought out more or less tenable conceptions as to the ultimate nature of matter, as witness the theories of Leibnitz and Boscovich and Davy, to which we may recur. But he had not as yet conceived the notion of a distinction between matter and energy, which is so fundamental to the physics of a later epoch. He did not speak of heat, light, electricity, as forms of energy or "force"; he conceived them as subtile forms of matter—as highly attenuated yet tangible fluids, subject to gravitation and chemical attraction; though he had learned to measure none of them but heat with accuracy, and this one he could test only within narrow limits until late in the century, when Josiah Wedgwood, the famous potter, taught him to gauge the highest temperatures with the clay pyrometer.

He spoke of the matter of heat as being the most universally distributed fluid in nature; as entering in some degree into the composition of nearly all other substances; as being sometimes liquid, sometimes condensed or solid, and as having weight that could be detected with the balance. Following Newton, he spoke of light as a "corpuscular emanation" or fluid, composed of shining particles which possibly are transmutable into particles of heat, and which enter into chemical combination with the particles of other forms of matter. Electricity he considered a still more subtile kind of matter−perhaps an attenuated form of light. Magnetism, "vital fluid," and by some even a "gravic fluid," and a fluid of sound were placed in the same scale; and, taken together, all these supposed subtile forms of matter were classed as "imponderables."

This view of the nature of the "imponderables" was in some measure a retrogression, for many seventeenth−century philosophers, notably Hooke and Huygens and Boyle, had held more correct views; but the materialistic conception accorded so well with the eighteenth− century tendencies of thought that only here and there a philosopher like Euler called it in question, until well on towards the close of the century. Current speech referred to the materiality of the "imponderables " unquestioningly. Students of meteorology—a science that was just dawning—explained atmospheric phenomena on the supposition that heat, the heaviest imponderable, predominated in the lower atmosphere, and that light, electricity, and magnetism prevailed in successively higher strata. And Lavoisier, the most philosophical chemist of the century, retained heat and light on a par with oxygen, hydrogen, iron, and the rest, in his list of elementary substances.

COUNT RUMFORD AND THE VIBRATORY THEORY OF HEAT

But just at the close of the century the confidence in the status of the imponderables was rudely shaken in the minds of philosophers by the revival of the old idea of Fra Paolo and Bacon and Boyle, that heat, at any rate, is not a material fluid, but merely a mode of motion or vibration among the particles of "ponderable" matter. The new champion of the old doctrine as to the nature of heat was a very distinguished philosopher and diplomatist of the time, who, it may be worth recalling, was an American. He was a sadly expatriated American, it is true, as his name, given all the official appendages, will amply testify; but he had been born and reared in a Massachusetts village none the less, and he seems always to have retained a kindly interest in the land of his nativity, even though he lived abroad in the service of other powers during all the later years of his life, and was knighted by England, ennobled by Bavaria, and honored by the most distinguished scientific bodies of Europe. The American, then, who championed the vibratory theory of heat, in opposition to all current opinion, in this closing era of the eighteenth century, was Lieutenant−General Sir Benjamin Thompson, Count Rumford, F.R.S.

Rumford showed that heat may be produced in indefinite quantities by friction of bodies that do not themselves lose any appreciable matter in the process, and claimed that this proves the immateriality of heat. Later on he added force to the argument by proving, in refutation of the experiments of Bowditch, that no body either gains or loses weight in virtue of being heated or cooled. He thought he had proved that heat is only a form of motion.

His experiment for producing indefinite quantities of heat by friction is recorded by him in his paper entitled, "Inquiry Concerning the Source of Heat Excited by Friction."

"Being engaged, lately, in superintending the boring of cannon in the workshops of the military arsenal at Munich," he says, "I was struck with the very considerable degree of heat which a brass gun acquires in a short time in being bored; and with the still more intense heat (much greater than that of boiling water, as I found by experiment) of the metallic chips separated from it by the borer.

"Taking a cannon (a brass six−pounder), cast solid, and rough, as it came from the foundry, and fixing it horizontally in a machine used for boring, and at the same time finishing the outside of the cannon by turning, I caused its extremity to be cut off; and by turning down the metal in that part, a solid cylinder was formed, 7 3/4 inches in diameter and 9 8/10 inches long; which, when finished, remained joined to the rest of the metal (that which, properly speaking, constituted the cannon) by a small cylindrical neck, only 2 1/5 inches in diameter and 3 8/10 inches long.

"This short cylinder, which was supported in its horizontal position, and turned round its axis by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer used in boring cannon.

"This cylinder being designed for the express purpose of generating heat by friction, by having a blunt borer forced against its solid bottom at the same time that it should be turned round its axis by the force of horses, in order that the heat accumulated in the cylinder might from time to time be measured, a small, round hole 0.37 of an inch only in diameter and 4.2 inches in depth, for the purpose of introducing a small cylindrical mercurial thermometer, was made in it, on one side, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the solid part of the metal which formed the bottom of the bore.

"At the beginning of the experiment, the temperature of the air in the shade, as also in the cylinder, was just sixty degrees Fahrenheit. At the end of thirty minutes, when the cylinder had made 960 revolutions about its axis, the horses being stopped, a cylindrical mercury thermometer, whose bulb was 32/100 of an inch in diameter and 3 1/4 inches in length, was introduced into the hole made to receive it in the side of the cylinder, when the mercury rose almost instantly to one hundred and thirty degrees.

"In order, by one decisive experiment, to determine whether the air of the atmosphere had any part or not in the generation of the heat, I contrived to repeat the experiment under circumstances in which it was evidently impossible for it to produce any effect whatever. By means of a piston exactly fitted to the mouth of the bore of the cylinder, through the middle of which piston the square iron bar, to the end of which the blunt steel borer was fixed, passed in a square hole made perfectly air−tight, the excess of the external air, to the inside of the bore of the cylinder, was effectually prevented. I did not find, however, by this experiment that the exclusion of the air diminished in the smallest degree the quantity of heat excited by the friction.

"There still remained one doubt, which, though it appeared to me to be so slight as hardly to deserve any attention, I was, however, desirous to remove. The piston which choked the mouth of the bore of the cylinder, in order that it might be air−tight, was fitted into it with so much nicety, by means of its collars of leather, and pressed against it with so much force, that, notwithstanding its being oiled, it occasioned a considerable degree of friction when the hollow cylinder was turned round its axis. Was not the heat produced, or at least some part of it, occasioned by this friction of the piston? and, as the external air had free access to the extremity of the bore, where it came into contact with the piston, is it not possible that this air may have had some share in the generation of the heat produced?

"A quadrangular oblong deal box, water−tight, being provided with holes or slits in the middle of each of its ends, just large enough to receive, the one the square iron rod to the end of which the blunt steel borer was fastened, the other the small cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occasionally closed above by a wooden cover or lid moving on hinges) was put into its place— that is to say, when, by means of the two vertical opening or slits in its two ends, the box was fixed to the machinery in such a manner that its bottom being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder, it is evident, from the description, that the hollow, metallic cylinder would occupy the middle of the box, without touching it on either side; and that, on pouring water into the box and filling it to the brim, the cylinder would be completely covered and surrounded on every side by that fluid. And, further, as the box was held fast by the strong, square iron rod which passed in a square hole in the centre of one of its ends, while the round or cylindrical neck which joined the hollow cylinder to the end of the cannon could turn round freely on its axis in the round hole in the centre of the other end of it, it is evident that the machinery could be put in motion without the least danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus."

Everything being thus ready, the box was filled with cold water, having been made water−tight by means of leather collars, and the machinery put in motion. "The result of this beautiful experiment," says Rumford, "was very striking, and the pleasure it afforded me amply repaid me for all the trouble I had had in contriving and arranging the complicated machinery used in making it. The cylinder, revolving at the rate of thirty−two times in a minute, had been in motion but a short time when I perceived, by putting my hand into the water and touching the outside of the cylinder, that heat was generated, and it was not long before the water which surrounded the cylinder began to be sensibly warm.

"At the end of one hour I found, by plunging a thermometer into the box, . . . that its temperature had been raised no less than forty−seven degrees Fahrenheit, being now one hundred and seven degrees Fahrenheit. ... One hour and thirty minutes after the machinery had been put in motion the heat of the water in the box was one hundred and forty−two degrees. At the end of two hours ... it was raised to one hundred and seventy−eight degrees; and at two hours and thirty minutes it ACTUALLY BOILED!

"It would be difficult to describe the surprise and astonishment expressed in the countenances of the bystanders on seeing so large a quantity of cold water heated, and actually made to boil, without any fire. Though there was, in fact, nothing that could justly be considered as a surprise in this event, yet I acknowledge fairly that it afforded me a degree of childish pleasure which, were I ambitious of the reputation of a GRAVE PHILOSOPHER, I ought most certainly rather to hide than to discover...."

Having thus dwelt in detail on these experiments, Rumford comes now to the all−important discussion as to the significance of them—the subject that had been the source of so much speculation among the philosophers—the question as to what heat really is, and if there really is any such thing (as many believed) as an igneous fluid, or a something called caloric.

"From whence came this heat which was continually given off in this manner, in the foregoing experiments?" asks Rumford. "Was it furnished by the small particles of metal detached from the larger solid masses on their being rubbed together? This, as we have already seen, could not possibly have been the case.

"Was it furnished by the air? This could not have been the case; for, in three of the experiments, the machinery being kept immersed in water, the access of the air of the atmosphere was completely prevented.

"Was it furnished by the water which surrounded the machinery? That this could not have been the case is evident: first, because this water was continually RECEIVING heat from the machinery, and could not, at the same time, be GIVING TO and RECEIVING HEAT FROM the same body; and, secondly, because there was no chemical decomposition of any part of this water. Had any such decomposition taken place (which, indeed, could not reasonably have been expected), one of its component elastic fluids (most probably hydrogen) must, at the same time, have been set at liberty, and, in making its escape into the atmosphere, would have been detected; but, though I frequently examined the water to see if any air−bubbles rose up through it, and had even made preparations for catching them if they should appear, I could perceive none; nor was there any sign of decomposition of any kind whatever, or other chemical process, going on in the water.

"Is it possible that the heat could have been supplied by means of the iron bar to the end of which the blunt steel borer was fixed? Or by the small neck of gun−metal by which the hollow cylinder was united to the cannon? These suppositions seem more improbable even than either of the before−mentioned; for heat was continually going off, or OUT OF THE MACHINERY, by both these passages during the whole time the experiment lasted.

"And in reasoning on this subject we must not forget to consider that most remarkable circumstance, that the source of the heat generated by friction in these experiments appeared evidently to be INEXHAUSTIBLE.

"It is hardly necessary to add that anything which any INSULATED body, or system of bodies, can continue to furnish WITHOUT LIMITATION cannot possibly be a MATERIAL substance; and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated, in the manner the heat was excited and communicated in these experiments, except in MOTION."[1]

THOMAS YOUNG AND THE WAVE THEORY OF LIGHT

But contemporary judgment, while it listened respectfully to Rumford, was little minded to accept his verdict. The cherished beliefs of a generation are not to be put down with a single blow. Where many minds have a similar drift, however, the first blow may precipitate a general conflict; and so it was here. Young Humphry Davy had duplicated Rumford's experiments, and reached similar conclusions; and soon others fell into line. Then, in 1800, Dr. Thomas Young— "Phenomenon Young" they called him at Cambridge, because he was reputed to know everything—took up the cudgels for the vibratory theory of light, and it began to be clear that the two "imponderables," heat and light, must stand or fall together; but no one as yet made a claim against the fluidity of electricity.

Before we take up the details of the assault made by Young upon the old doctrine of the materiality of light, we must pause to consider the personality of Young himself. For it chanced that this Quaker physician was one of those prodigies who come but few times in a century, and the full list of whom in the records of history could be told on one's thumbs and fingers. His biographers tell us things about him that read like the most patent fairy−tales. As a mere infant in arms he had been able to read fluently. Before his fourth birthday came he had read the Bible twice through, as well as Watts's Hymns—poor child!—and when seven or eight he had shown a propensity to absorb languages much as other children absorb nursery tattle and Mother Goose rhymes. When he was fourteen, a young lady visiting the household of his tutor patronized the pretty boy by asking to see a specimen of his penmanship. The pretty boy complied readily enough, and mildly rebuked his interrogator by rapidly writing some sentences for her in fourteen languages, including such as, Arabian, Persian, and Ethiopic.

Meantime languages had been but an incident in the education of the lad. He seems to have entered every available field of thought—mathematics, physics, botany, literature, music, painting, languages, philosophy, archaeology, and so on to tiresome lengths—and once he had entered any field he seldom turned aside until he had reached the confines of the subject as then known and added something new from the recesses of his own genius. He was as versatile as Priestley, as profound as Newton himself. He had the range of a mere dilettante, but everywhere the full grasp of the master. He took early for his motto the saying that what one man has done, another man may do. Granting that the other man has the brain of a Thomas Young, it is a true motto.

Such, then, was the young Quaker who came to London to follow out the humdrum life of a practitioner of medicine in the year 1801. But incidentally the young physician was prevailed upon to occupy the interims of early practice by fulfilling the duties of the chair of Natural Philosophy at the Royal Institution, which Count Rumford had founded, and of which Davy was then Professor of Chemistry—the institution whose glories have been perpetuated by such names as Faraday and Tyndall, and which the Briton of to−day speaks of as the "Pantheon of Science." Here it was that Thomas Young made those studies which have insured him a niche in the temple of fame not far removed from that of Isaac Newton.

As early as 1793, when he was only twenty, Young had begun to Communicate papers to the Royal Society of London, which were adjudged worthy to be printed in full in the Philosophical Transactions; so it is not strange that he should have been asked to deliver the Bakerian lecture before that learned body the very first year after he came to London. The lecture was delivered November 12, 1801. Its subject was "The Theory of Light and Colors," and its reading marks an epoch in physical science; for here was brought forward for the first time convincing proof of that undulatory theory of light with which every student of modern physics is familiar—the theory which holds that light is not a corporeal entity, but a mere pulsation in the substance of an all−pervading ether, just as sound is a pulsation in the air, or in liquids or solids.

Young had, indeed, advocated this theory at an earlier date, but it was not until 1801 that he hit upon the idea which enabled him to bring it to anything approaching a demonstration. It was while pondering over the familiar but puzzling phenomena of colored rings into which white light is broken when reflected from thin films—Newton's rings, so called—that an explanation occurred to him which at once put the entire undulatory theory on a new footing. With that sagacity of insight which we call genius, he saw of a sudden that the phenomena could be explained by supposing that when rays of light fall on a thin glass, part of the rays being reflected from the upper surface, other rays, reflected from the lower surface, might be so retarded in their course through the glass that the two sets would interfere with one another, the forward pulsation of one ray corresponding to the backward pulsation of another, thus quite neutralizing the effect. Some of the component pulsations of the light being thus effaced by mutual interference, the remaining rays would no longer give the optical effect of white light; hence the puzzling colors.

Here is Young's exposition of the subject:

Of the Colors of Thin Plates

"When a beam of light falls upon two refracting surfaces, the partial reflections coincide perfectly in direction;

and in this case the interval of retardation taken between the surfaces is to their radius as twice the cosine of the angle of refraction to the radius.

"Let the medium between the surfaces be rarer than the surrounding mediums; then the impulse reflected at the second surface, meeting a subsequent undulation at the first, will render the particles of the rarer medium capable of wholly stopping the motion of the denser and destroying the reflection, while they themselves will be more strongly propelled than if they had been at rest, and the transmitted light will be increased. So that the colors by reflection will be destroyed, and those by transmission rendered more vivid, when the double thickness or intervals of retardation are any multiples of the whole breadth of the undulations; and at intermediate thicknesses the effects will be reversed according to the Newtonian observation.

"If the same proportions be found to hold good with respect to thin plates of a denser medium, which is, indeed, not improbable, it will be necessary to adopt the connected demonstrations of Prop. IV., but, at any rate, if a thin plate be interposed between a rarer and a denser medium, the colors by reflection and transmission may be expected to change places.

Of the Colors of Thick Plates

"When a beam of light passes through a refracting surface, especially if imperfectly polished, a portion of it is irregularly scattered, and makes the surface visible in all directions, but most conspicuously in directions not far distant from that of the light itself; and if a reflecting surface be placed parallel to the refracting surface, this scattered light, as well as the principal beam, will be reflected, and there will be also a new dissipation of light, at the return of the beam through the refracting surface. These two portions of scattered light will coincide in direction; and if the surfaces be of such a form as to collect the similar effects, will exhibit rings of colors. The interval of retardation is here the difference between the paths of the principal beam and of the scattered light between the two surfaces; of course, wherever the inclination of the scattered light is equal to that of the beam, although in different planes, the interval will vanish and all the undulations will conspire. At other inclinations, the interval will be the difference of the secants from the secant of the inclination, or angle of refraction of the principal beam. From these causes, all the colors of concave mirrors observed by Newton and others are necessary consequences; and it appears that their production, though somewhat similar, is by no means as Newton imagined, identical with the production of thin plates."[2]

By following up this clew with mathematical precision, measuring the exact thickness of the plate and the space between the different rings of color, Young was able to show mathematically what must be the length of pulsation for each of the different colors of the spectrum. He estimated that the undulations of red light, at the extreme lower end of the visible spectrum, must number about thirty−seven thousand six hundred and forty to the inch, and pass any given spot at a rate of four hundred and sixty−three millions of millions of undulations in a second, while the extreme violet numbers fifty−nine thousand seven hundred and fifty undulations to the inch, or seven hundred and thirty−five millions of millions to the second.

The Colors of Striated Surfaces

Young similarly examined the colors that are produced by scratches on a smooth surface, in particular testing the light from "Mr. Coventry's exquisite micrometers," which consist of lines scratched on glass at measured intervals. These microscopic tests brought the same results as the other experiments. The colors were produced at certain definite and measurable angles, and the theory of interference of undulations explained them perfectly, while, as Young affirmed with confidence, no other hypothesis hitherto advanced would explain them at all. Here are his words:

"Let there be in a given plane two reflecting points very near each other, and let the plane be so situated that the reflected image of a luminous object seen in it may appear to coincide with the points; then it is obvious that the length of the incident and reflected ray, taken together, is equal with respect to both points, considering them as capable of reflecting in all directions. Let one of the points be now depressed below the given plane; then the whole path of the light reflected from it will be lengthened by a line which is to the depression of the point as twice the cosine of incidence to the radius.

"If, therefore, equal undulations of given dimensions be reflected from two points, situated near enough to appear to the eye but as one, whenever this line is equal to half the breadth of a whole undulation the reflection from the depressed point will so interfere with the reflection from the fixed point that the progressive motion of the one will coincide with the retrograde motion of the other, and they will both be destroyed; but when this line is equal to the whole breadth of an undulation, the effect will be doubled, and when to a breadth and a half, again destroyed; and thus for a considerable number of alternations, and if the reflected undulations be of a different kind, they will be variously affected, according to their proportions to the various length of the line which is the difference between the lengths of their two paths, and which may be denominated the interval of a retardation.

"In order that the effect may be the more perceptible, a number of pairs of points must be united into two parallel lines; and if several such pairs of lines be placed near each other, they will facilitate the observation. If one of the lines be made to revolve round the other as an axis, the depression below the given plane will be as the sine of the inclination; and while the eye and the luminous object remain fixed the difference of the length of the paths will vary as this sine.

"The best subjects for the experiment are Mr. Coventry's exquisite micrometers; such of them as consist of parallel lines drawn on glass, at a distance of one− five−hundredth of an inch, are the most convenient. Each of these lines appears under a microscope to consist of two or more finer lines, exactly parallel, and at a distance of somewhat more than a twentieth more than the adjacent lines. I placed one of these so as to reflect the sun's light at an angle of forty−five degrees, and fixed it in such a manner that while it revolved round one of the lines as an axis, I could measure its angular motion; I found that the longest red color occurred at the inclination 10 1/4 degrees, 20 3/4 degrees, 32 degrees, and 45 degrees; of which the sines are as the numbers 1, 2, 3, and 4. At all other angles also, when the sun's light was reflected from the surface, the color vanished with the inclination, and was equal at equal inclinations on either side.

This experiment affords a very strong confirmation of the theory. It is impossible to deduce any explanation of it from any hypothesis hitherto advanced; and I believe it would be difficult to invent any other that would account for it. There is a striking analogy between this separation of colors and the production of a musical note by successive echoes from equidistant iron palisades, which I have found to correspond pretty accurately with the known velocity of sound and the distances of the surfaces.

"It is not improbable that the colors of the integuments of some insects, and of some other natural bodies, exhibiting in different lights the most beautiful versatility, may be found to be of this description, and not to be derived from thin plates. In some cases a single scratch or furrow may produce similar effects, by the reflection of its opposite edges."[3]

This doctrine of interference of undulations was the absolutely novel part of Young's theory. The all−compassing genius of Robert Hooke had, indeed, very nearly apprehended it more than a century before, as Young himself points out, but no one else bad so much as vaguely conceived it; and even with the sagacious Hooke it was only a happy guess, never distinctly outlined in his own mind, and utterly ignored by all others. Young did not know of Hooke's guess until he himself had fully formulated the theory, but he hastened then to give his predecessor all the credit that could possibly be adjudged his due by the most disinterested observer. To Hooke's contemporary, Huygens, who was the originator of the general doctrine of undulation as the explanation of light, Young renders full justice also. For himself he claims only the merit of having demonstrated the theory which these and a few others of his predecessors had advocated without full proof.

The following year Dr. Young detailed before the Royal Society other experiments, which threw additional light on the doctrine of interference; and in 1803 he cited still others, which, he affirmed, brought the doctrine to complete demonstration. In applying this demonstration to the general theory of light, he made the striking suggestion that "the luminiferous ether pervades the substance of all material bodies with little or no resistance, as freely, perhaps, as the wind passes through a grove of trees." He asserted his belief also that the chemical rays which Ritter had discovered beyond the violet end of the visible spectrum are but still more rapid undulations of the same character as those which produce light. In his earlier lecture he had affirmed a like affinity between the light rays and the rays of radiant heat which Herschel detected below the red end of the spectrum, suggesting that "light differs from heat only in the frequency of its undulations or vibrations—those undulations which are within certain limits with respect to frequency affecting the optic nerve and constituting light, and those which are slower and probably stronger constituting heat only." From the very outset he had recognized the affinity between sound and light; indeed, it had been this affinity that led him on to an appreciation of the undulatory theory of light.

But while all these affinities seemed so clear to the great co−ordinating brain of Young, they made no such impression on the minds of his contemporaries. The immateriality of light had been substantially demonstrated, but practically no one save its author accepted the demonstration. Newton's doctrine of the emission of corpuscles was too firmly rooted to be readily dislodged, and Dr. Young had too many other interests to continue the assault unceasingly. He occasionally wrote something touching on his theory, mostly papers contributed to the Quarterly Review and similar periodicals, anonymously or under pseudonym, for he had conceived the notion that too great conspicuousness in fields outside of medicine would injure his practice as a physician. His views regarding light (including the original papers from the Philosophical Transactions of the Royal Society) were again given publicity in full in his celebrated volume on natural philosophy, consisting in part of his lectures before the Royal Institution, published in 1807; but even then they failed to bring conviction to the philosophic world. Indeed, they did not even arouse a controversial spirit, as his first papers had done.

ARAGO AND FRESNEL CHAMPION THE WAVE THEORY

So it chanced that when, in 1815, a young French military engineer, named Augustin Jean Fresnel, returning from the Napoleonic wars, became interested in the phenomena of light, and made some experiments concerning diffraction which seemed to him to controvert the accepted notions of the materiality of light, he was quite unaware that his experiments had been anticipated by a philosopher across the Channel. He communicated his experiments and results to the French Institute, supposing them to be absolutely novel. That body referred them to a committee, of which, as good fortune would have it, the dominating member was Dominique Francois Arago, a man as versatile as Young himself, and hardly less profound, if perhaps not quite so original. Arago at once recognized the merit of Fresnel's work, and soon became a convert to the theory. He told Fresnel that Young had anticipated him as regards the general theory, but that much remained to be done, and he offered to associate himself with Fresnel in prosecuting the investigation. Fresnel was not a little dashed to learn that his original ideas had been worked out by another while he was a lad, but he bowed gracefully to the situation and went ahead with unabated zeal.

The championship of Arago insured the undulatory theory a hearing before the French Institute, but by no means sufficed to bring about its general acceptance. On the contrary, a bitter feud ensued, in which Arago was opposed by the "Jupiter Olympus of the Academy," Laplace, by the only less famous Poisson, and by the younger but hardly less able Biot. So bitterly raged the feud that a life−long friendship between Arago and Biot was ruptured forever. The opposition managed to delay the publication of Fresnel's papers, but Arago continued to fight with his customary enthusiasm and pertinacity, and at last, in 1823, the Academy yielded, and voted Fresnel into its ranks, thus implicitly admitting the value of his work.

It is a humiliating thought that such controversies as this must mar the progress of scientific truth; but fortunately the story of the introduction of the undulatory theory has a more pleasant side. Three men, great both in character and in intellect, were concerned in pressing its claims—Young, Fresnel, and Arago—and the relations of these men form a picture unmarred by any of those petty jealousies that so often dim the lustre of great names. Fresnel freely acknowledged Young's priority so soon as his attention was called to it; and Young applauded the work of the Frenchman, and aided with his counsel in the application of the undulatory theory to the problems of polarization of light, which still demanded explanation, and which Fresnel's fertility of experimental resource and profundity of mathematical insight sufficed in the end to conquer.

After Fresnel's admission to the Institute in 1823 the opposition weakened, and gradually the philosophers came to realize the merits of a theory which Young had vainly called to their attention a full quarter− century before. Now, thanks largely to Arago, both Young and Fresnel received their full meed of appreciation. Fresnel was given the Rumford medal of the Royal Society of England in 1825, and chosen one of the foreign members of the society two years later, while Young in turn was elected one of the eight foreign members of the French Academy. As a fitting culmination of the chapter of felicities between the three friends, it fell to the lot of Young, as Foreign Secretary of the Royal Society, to notify Fresnel of the honors shown him by England's representative body of scientists; while Arago, as Perpetual Secretary of the French Institute, conveyed to Young in the same year the notification that he had been similarly honored by the savants of France.

A few months later Fresnel was dead, and Young survived him only two years. Both died prematurely, but their great work was done, and the world will remember always and link together these two names in connection with a theory which in its implications and importance ranks little below the theory of universal gravitation.

VII. THE MODERN DEVELOPMENT OF ELECTRICITY AND MAGNETISM

GALVANI AND VOLTA

The full importance of Young's studies of light might perhaps have gained earlier recognition had it not chanced that, at the time when they were made, the attention of the philosophic world was turned with the fixity and fascination of a hypnotic stare upon another field, which for a time brooked no rival. How could the old, familiar phenomenon, light, interest any one when the new agent, galvanism, was in view? As well ask one to fix attention on a star while a meteorite blazes across the sky.

Galvanism was so called precisely as the Roentgen ray was christened at a later day—as a safe means of begging the question as to the nature of the phenomena involved. The initial fact in galvanism was the discovery of Luigi Galvani (1737−1798), a physician of Bologna, in 1791, that by bringing metals in contact with the nerves of a frog's leg violent muscular contractions are produced. As this simple little experiment led eventually to the discovery of galvanic electricity and the invention of the galvanic battery, it may be regarded as the beginning of modern electricity.

The story is told that Galvani was led to his discovery while preparing frogs' legs to make a broth for his invalid wife. As the story runs, he had removed the skins from several frogs' legs, when, happening to touch the exposed muscles with a scalpel which had lain in close proximity to an electrical machine, violent muscular action was produced. Impressed with this phenomenon, he began a series of experiments which finally resulted in his great discovery. But be this story authentic or not, it is certain that Galvani experimented for several years upon frogs' legs suspended upon wires and hooks, until he finally constructed his arc of two different metals, which, when arranged so that one was placed in contact with a nerve and the other with a muscle, produced violent contractions.

These two pieces of metal form the basic principle of the modern galvanic battery, and led directly to Alessandro Volta's invention of his "voltaic pile," the immediate ancestor of the modern galvanic battery. Volta's experiments were carried on at the same time as those of Galvani, and his invention of his pile followed close upon Galvani's discovery of the new form of electricity. From these facts the new form of electricity was sometimes called "galvanic" and sometimes "voltaic" electricity, but in recent years the term "galvanism" and "galvanic current" have almost entirely supplanted the use of the term voltaic.

It was Volta who made the report of Galvani's wonderful discovery to the Royal Society of London, read on January 31, 1793. In this letter he describes Galvani's experiments in detail and refers to them in glowing terms of praise. He calls it one of the "most beautiful and important discoveries," and regarded it as the germ or foundation upon which other discoveries were to be made. The prediction proved entirely correct, Volta himself being the chief discoverer.

Working along lines suggested by Galvani's discovery, Volta constructed an apparatus made up of a number of disks of two different kinds of metal, such as tin and silver, arranged alternately, a piece of some moist, porous substance, like paper or felt, being interposed between each pair of disks. With this "pile," as it was called, electricity was generated, and by linking together several such piles an electric battery could be formed.

This invention took the world by storm. Nothing like the enthusiasm it created in the philosophic world had been known since the invention of the Leyden jar, more than half a century before. Within a few weeks after Volta's announcement, batteries made according to his plan were being experimented with in every important laboratory in Europe.

As the century closed, half the philosophic world was speculating as to whether "galvanic influence" were a new imponderable, or only a form of electricity; and the other half was eagerly seeking to discover what new marvels the battery might reveal. The least imaginative man could see that here was an invention that would be epoch−making, but the most visionary dreamer could not even vaguely adumbrate the real measure of its importance.

It was evident at once that almost any form of galvanic battery, despite imperfections, was a more satisfactory instrument for generating electricity than the frictional machine hitherto in use, the advantage lying in the fact that the current from the galvanic battery could be controlled practically at will, and that the apparatus itself was inexpensive and required comparatively little attention. These advantages were soon made apparent by the practical application of the electric current in several fields.

It will be recalled that despite the energetic endeavors of such philosophers as Watson, Franklin, Galvani, and many others, the field of practical application of electricity was very limited at the close of the eighteenth century. The lightning−rod had come into general use, to be sure, and its value as an invention can hardly be overestimated. But while it was the result of extensive electrical discoveries, and is a most practical instrument, it can hardly be called one that puts electricity to practical use, but simply acts as a means of warding off the evil effects of a natural manifestation of electricity. The invention, however, had all the effects of a mechanism which turned electricity to practical account. But with the advent of the new kind of electricity the age of practical application began.

DAVY AND ELECTRIC LIGHT

Volta's announcement of his pile was scarcely two months old when two Englishmen, Messrs. Nicholson and Carlisle, made the discovery that the current from the galvanic battery had a decided effect upon certain chemicals, among other things decomposing water into its elements, hydrogen and oxygen. On May 7, 1800, these investigators arranged the ends of two brass wires connected with the poles of a voltaic pile, composed of alternate silver and zinc plates, so that the current coming from the pile was discharged through a small quantity of "New River water." "A fine stream of minute bubbles immediately began to flow from the point of the lower wire in the tube which communicated with the silver," wrote Nicholson, "and the opposite point of the upper wire became tarnished, first deep orange and then black. . . ." The product of gas during two hours and a half was two−thirtieths of a cubic inch. "It was then mixed with an equal quantity of common air," continues Nicholson, "and exploded by the application of a lighted waxen thread."

This demonstration was the beginning of the very important science of electro−chemistry.

The importance of this discovery was at once recognized by Sir Humphry Davy, who began experimenting immediately in this new field. He constructed a series of batteries in various combinations, with which he attacked the "fixed alkalies," the composition of which was then unknown. Very shortly he was able to decompose potash into bright metallic globules, resembling quicksilver. This new substance he named "potassium." Then in rapid succession the elementary substances sodium, calcium, strontium, and magnesium were isolated.

It was soon discovered, also, that the new electricity, like the old, possessed heating power under certain conditions, even to the fusing of pieces of wire. This observation was probably first made by Frommsdorff, but it was elaborated by Davy, who constructed a battery of two thousand cells with which he produced a bright light from points of carbon—the prototype of the modern arc lamp. He made this demonstration before the members of the Royal Institution in 1810. But the practical utility of such a light for illuminating purposes was still a thing of the future. The expense of constructing and maintaining such an elaborate battery, and the rapid internal destruction of its plates, together with the constant polarization, rendered its use in practical illumination out of the question. It was not until another method of generating electricity was discovered that Davy's demonstration could be turned to practical account.

In Davy's own account of his experiment he says:

"When pieces of charcoal about an inch long and one−sixth of an inch in diameter were brought near each other (within the thirtieth or fortieth of an inch), a bright spark was produced, and more than half the volume of the charcoal became ignited to whiteness; and, by withdrawing the points from each other, a constant discharge took place through the heated air, in a space equal to at least four inches, producing a most brilliant ascending arch of light, broad and conical in form in the middle. When any substance was introduced into this arch, it instantly became ignited; platina melted as readily in it as wax in a common candle; quartz, the sapphire, magnesia, lime, all entered into fusion; fragments of diamond and points of charcoal and plumbago seemed to evaporate in it, even when the connection was made in the receiver of an air−pump; but there was no evidence of their having previously undergone fusion. When the communication between the points positively and negatively electrified was made in the air rarefied in the receiver of the air−pump, the distance at which the discharge took place increased as the exhaustion was made; and when the atmosphere in the vessel supported only one− fourth of an inch of mercury in the barometrical gauge, the sparks passed through a space of nearly half an inch; and, by withdrawing the points from each other, the discharge was made through six or seven inches, producing a most brilliant coruscation of purple light; the charcoal became intensely ignited, and some platina wire attached to it fused with brilliant scintillations and fell in large globules upon the plate of the pump. All the phenomena of chemical decomposition were produced with intense rapidity by this combination."[1]

But this experiment demonstrated another thing besides the possibility of producing electric light and chemical decomposition, this being the heating power capable of being produced by the electric current. Thus Davy's experiment of fusing substances laid the foundation of the modern electric furnaces, which are of paramount importance in several great commercial industries.

While some of the results obtained with Davy's batteries were practically as satisfactory as could be obtained with modern cell batteries, the batteries themselves were anything but satisfactory. They were expensive, required constant care and attention, and, what was more important from an experimental standpoint at least, were not constant in their action except for a very limited period of time, the current soon "running down." Numerous experimenters, therefore, set about devising a satisfactory battery, and when, in 1836, John Frederick Daniell produced the cell that bears his name, his invention was epoch− making in the history of electrical progress. The Royal Society considered it of sufficient importance to bestow the Copley medal upon the inventor, whose device is the direct parent of all modern galvanic cells. From the time of the advent of the Daniell cell experiments in electricity were rendered comparatively easy. In the mean while, however, another great discovery was made.

ELECTRICITY AND MAGNETISM

For many years there had been a growing suspicion, amounting in many instances to belief in the close relationship existing between electricity and magnetism. Before the winter of 1815, however, it was a belief that was surmised but not demonstrated. But in that year it occurred to Jean Christian Oersted, of Denmark, to pass a current of electricity through a wire held parallel with, but not quite touching, a suspended magnetic needle. The needle was instantly deflected and swung out of its position.

"The first experiments in connection with the subject which I am undertaking to explain," wrote Oersted, "were made during the course of lectures which I held last winter on electricity and magnetism. From those experiments it appeared that the magnetic needle could be moved from its position by means of a galvanic battery—one with a closed galvanic circuit. Since, however, those experiments were made with an apparatus of small power, I undertook to repeat and increase them with a large galvanic battery.

"Let us suppose that the two opposite ends of the galvanic apparatus are joined by a metal wire. This I shall always call the conductor for the sake of brevity. Place a rectilinear piece of this conductor in a horizontal position over an ordinary magnetic needle so that it is parallel to it. The magnetic needle will be set in motion and will deviate towards the west under that part of the conductor which comes from the negative pole of the galvanic battery. If the wire is not more than four−fifths of an inch distant from the middle of this needle, this deviation will be about forty−five degrees. At a greater distance the angle of deviation becomes less. Moreover, the deviation varies according to the strength of the battery. The conductor can be moved towards the east or west, so long as it remains parallel to the needle, without producing any other result than to make the deviation smaller.

"The conductor can consist of several combined wires or metal coils. The nature of the metal does not alter the result except, perhaps, to make it greater or less. We have used wires of platinum, gold, silver, brass, and iron, and coils of lead, tin, and quicksilver with the same result. If the conductor is interrupted by water, all effect is not cut off, unless the stretch of water is several inches long.

"The conductor works on the magnetic needle through glass, metals, wood, water, and resin, through clay vessels and through stone, for when we placed a glass plate, a metal plate, or a board between the conductor and the needle the effect was not cut off; even the three together seemed hardly to weaken the effect, and the same was the case with an earthen vessel, even when it was full of water. Our experiments also demonstrated that the said effects were not altered when we used a magnetic needle which was in a brass case full of water.

"When the conductor is placed in a horizontal plane under the magnetic needle all the effects we have described take place in precisely the same way, but in the opposite direction to what took place when the conductor was in a horizontal plane above the needle.

"If the conductor is moved in a horizontal plane so that it gradually makes ever−increasing angles with the magnetic meridian, the deviation of the magnetic needle from the magnetic meridian is increased when the wire is turned towards the place of the needle; it decreases, on the other hand, when it is turned away from that place.

"A needle of brass which is hung in the same way as the magnetic needle is not set in motion by the influence of the conductor. A needle of glass or rubber likewise remains static under similar experiments. Hence the electrical conductor affects only the magnetic parts of a substance. That the electrical current is not confined to the conducting wire, but is comparatively widely diffused in the surrounding space, is sufficiently demonstrated from the foregoing observations."[2]

The effect of Oersted's demonstration is almost incomprehensible. By it was shown the close relationship between magnetism and electricity. It showed the way to the establishment of the science of electrodynamics; although it was by the French savant Andre Marie Ampere (1775−1836) that the science was actually created, and this within the space of one week after hearing of Oersted's experiment in deflecting the needle. Ampere first received the news of Oersted's experiment on September 11, 1820, and on the 18th of the same month he announced to the Academy the fundamental principles of the science of electro−dynamics— seven days of rapid progress perhaps unequalled in the history of science.

Ampere's distinguished countryman, Arago, a few months later, gave the finishing touches to Oersted's and Ampere's discoveries, by demonstrating conclusively that electricity not only influenced a magnet, but actually produced magnetism under proper circumstances —a complemental fact most essential in practical mechanics

Some four years after Arago's discovery, Sturgeon made the first "electro−magnet" by winding a soft iron core with wire through which a current of electricity was passed. This study of electro−magnets was taken up by Professor Joseph Henry, of Albany, New York, who succeeded in making magnets of enormous lifting power by winding the iron core with several coils of wire. One of these magnets, excited by a single galvanic cell of less than half a square foot of surface, and containing only half a pint of dilute acids, sustained a weight of six hundred and fifty pounds.

Thus by Oersted's great discovery of the intimate relationship of magnetism and electricity, with further elaborations and discoveries by Ampere, Volta, and Henry, and with the invention of Daniell's cell, the way was laid for putting electricity to practical use. Soon followed the invention and perfection of the electro−magnetic telegraph and a host of other but little less important devices.

FARADAY AND ELECTRO−MAGNETIC INDUCTION

With these great discoveries and inventions at hand, electricity became no longer a toy or a "plaything for philosophers," but of enormous and growing importance commercially. Still, electricity generated by chemical action, even in a very perfect cell, was both feeble and expensive, and, withal, only applicable in a comparatively limited field. Another important scientific discovery was necessary before such things as electric traction and electric lighting on a large scale were to become possible; but that discovery was soon made by Sir Michael Faraday.

Faraday, the son of a blacksmith and a bookbinder by trade, had interested Sir Humphry Davy by his admirable notes on four of Davy's lectures, which he had been able to attend. Although advised by the great scientist to "stick to his bookbinding" rather than enter the field of science, Faraday became, at twenty−two years of age, Davy's assistant in the Royal Institution. There, for several years, he devoted all his spare hours to scientific investigations and experiments, perfecting himself in scientific technique.

A few years later he became interested, like all the scientists of the time, in Arago's experiment of rotating a copper disk underneath a suspended compass− needle. When this disk was rotated rapidly, the needle was deflected, or even rotated about its axis, in a manner quite inexplicable. Faraday at once conceived the idea that the cause of this rotation was due to electricity, induced in the revolving disk—not only conceived it, but put his belief in writing. For several years, however, he was unable to demonstrate the truth of his assumption, although he made repeated experiments to prove it. But in 1831 he began a series of experiments that established forever the fact of electro−magnetic induction.

In his famous paper, read before the Royal Society in 1831, Faraday describes the method by which he first demonstrated electro−magnetic induction, and then explained the phenomenon of Arago's revolving disk.

"About twenty−six feet of copper wire, one−twentieth of an inch in diameter, were wound round a cylinder of wood as a helix," he said, "the different spires of which were prevented from touching by a thin interposed twine. This helix was covered with calico, and then a second wire applied in the same manner. In this way twelve helices were "superposed, each containing an average length of wire of twenty−seven feet, and all in the same direction. The first, third, fifth, seventh, ninth, and eleventh of these helices were connected at their extremities end to end so as to form one helix; the others were connected in a similar manner; and thus two principal helices were produced, closely interposed, having the same direction, not touching anywhere, and each containing one hundred and fifty−five feet in length of wire.

One of these helices was connected with a galvanometer, the other with a voltaic battery of ten pairs of plates four inches square, with double coppers and well charged; yet not the slightest sensible deflection of the galvanometer needle could be observed.

"A similar compound helix, consisting of six lengths of copper and six of soft iron wire, was constructed. The resulting iron helix contained two hundred and eight feet; but whether the current from the trough was passed through the copper or the iron helix, no effect upon the other could be perceived at the galvanometer.

"In these and many similar experiments no difference in action of any kind appeared between iron and other metals.

"Two hundred and three feet of copper wire in one length were passed round a large block of wood; other two hundred and three feet of similar wire were interposed as a spiral between the turns of the first, and metallic contact everywhere prevented by twine. One of these helices was connected with a galvanometer and the other with a battery of a hundred pairs of plates four inches square, with double coppers and well charged. When the contact was made, there was a sudden and very slight effect at the galvanometer, and there was also a similar slight effect when the contact with the battery was broken. But whilst the voltaic current was continuing to pass through the one helix, no galvanometrical appearances of any effect like induction upon the other helix could be perceived, although the active power of the battery was proved to be great by its heating the whole of its own helix, and by the brilliancy of the discharge when made through charcoal.

"Repetition of the experiments with a battery of one hundred and twenty pairs of plates produced no other effects; but it was ascertained, both at this and at the former time, that the slight deflection of the needle occurring at the moment of completing the connection was always in one direction, and that the equally slight deflection produced when the contact was broken was in the other direction; and, also, that these effects occurred when the first helices were used.

"The results which I had by this time obtained with magnets led me to believe that the battery current through one wire did, in reality, induce a similar current through the other wire, but that it continued for an instant only, and partook more of the nature of the electrical wave passed through from the shock of a common Leyden jar than of that from a voltaic battery, and, therefore, might magnetize a steel needle although it scarcely affected the galvanometer.

"This expectation was confirmed; for on substituting a small hollow helix, formed round a glass tube, for the galvanometer, introducing a steel needle, making contact as before between the battery and the inducing wire, and then removing the needle before the battery contact was broken, it was found magnetized.

"When the battery contact was first made, then an unmagnetized needle introduced, and lastly the battery contact broken, the needle was found magnetized to an equal degree apparently with the first; but the poles were of the contrary kinds."[3]

To Faraday these experiments explained the phenomenon of Arago's rotating disk, the disk inducing the current from the magnet, and, in reacting, deflecting the needle. To prove this, he constructed a disk that revolved between the poles of an electro−magnet, connecting the axis and the edge of the disk with a galvanometer. ". . . A disk of copper, twelve inches in diameter, fixed upon a brass axis," he says, "was mounted in frames so as to be revolved either vertically or horizontally, its edge being at the same time introduced more or less between the magnetic poles. The edge of the plate was well amalgamated for the purpose of obtaining good but movable contact; a part round the axis was also prepared in a similar manner.

"Conductors or collectors of copper and lead were constructed so as to come in contact with the edge of the copper disk, or with other forms of plates hereafter to be described. These conductors we're about four inches long, one−third of an inch wide, and one−fifth of an inch thick; one end of each was slightly grooved, to allow of more exact adaptation to the somewhat convex edge of the plates, and then amalgamated. Copper wires, one−sixteenth of an inch in thickness, attached in the ordinary manner by convolutions to the other ends of these conductors, passed away to the galvanometer.

"All these arrangements being made, the copper disk was adjusted, the small magnetic poles being about one−half an inch apart, and the edge of the plate inserted about half their width between them. One of the galvanometer wires was passed twice or thrice loosely round the brass axis of the plate, and the other attached to a conductor, which itself was retained by the hand in contact with the amalgamated edge of the disk at the part immediately between the magnetic poles. Under these circumstances all was quiescent, and the galvanometer exhibited no effect. But the instant the plate moved the galvanometer was influenced, and by revolving the plate quickly the needle could be deflected ninety degrees or more."[4]

This rotating disk was really a dynamo electric machine in miniature, the first ever constructed, but whose direct descendants are the ordinary dynamos. Modern dynamos range in power from little machines operating machinery requiring only fractions of a horsepower to great dynamos operating street−car lines and lighting cities; but all are built on the same principle as Faraday's rotating disk. By this discovery the use of electricity as a practical and economical motive power became possible.

STORAGE BATTERIES

When the discoveries of Faraday of electro−magnetic induction had made possible the means of easily generating electricity, the next natural step was to find a means of storing it or accumulating it. This, however, proved no easy matter, and as yet a practical storage or secondary battery that is neither too cumbersome, too fragile, nor too weak in its action has not been invented. If a satisfactory storage battery could be made, it is obvious that its revolutionary effects could scarcely be overestimated. In the single field of aeronautics, it would probably solve the question of aerial navigation. Little wonder, then, that inventors have sought so eagerly for the invention of satisfactory storage batteries. As early as 1803 Ritter had attempted to make such a secondary battery. In 1843 Grove also attempted it. But it was not until 1859, when Gaston Planche produced his invention, that anything like a reasonably satisfactory storage battery was made. Planche discovered that sheets of lead immersed in dilute sulphuric acid were very satisfactory for the production of polarization effects. He constructed a battery of sheets of lead immersed in sulphuric acid, and, after charging these for several hours from the cells of an ordinary Bunsen battery, was able to get currents of great strength and considerable duration. This battery, however, from its construction of lead, was necessarily heavy and cumbersome. Faure improved it somewhat by coating the lead plates with red−lead, thus increasing the capacity of the cell. Faure's invention gave a fresh impetus to inventors, and shortly after the market was filled with storage batteries of various kinds, most of them modifications of Planche's or Faure's. The ardor of enthusiastic inventors soon flagged, however, for all these storage batteries proved of little practical account in the end, as compared with other known methods of generating power.

Three methods of generating electricity are in general use: static or frictional electricity is generated by "plate" or "static" machines; galvanic, generated by batteries based on Volta's discovery; and induced, or faradic, generated either by chemical or mechanical action. There is still another kind, thermo−electricity, that may be generated in a most simple manner. In 1821 Seebecle, of Berlin, discovered that when a circuit was formed of two wires of different metals, if there be a difference in temperature at the juncture of these two metals an electrical current will be established. In this way heat may be transmitted directly into the energy of the current without the interposition of the steam−engine. Batteries constructed in this way are of low resistance, however, although by arranging several of them in "series," currents of considerable strength can be generated. As yet, however, they are of little practical importance.

About the middle of the century Clerk−Maxwell advanced the idea that light waves were really electro−magnetic waves. If this were true and light proved to be simply one form of electrical energy, then the same would be true of radiant heat. Maxwell advanced this theory, but failed to substantiate it by experimental confirmation. But Dr. Heinrich Hertz, a few years later, by a series of experiments, demonstrated the correctness of Maxwell's surmises. What are now called "Hertzian waves" are waves apparently identical with light waves, but of much lower pitch or period. In his experiments Hertz showed that, under proper conditions, electric sparks between polished balls were attended by ether waves of the same nature as those of light, but of a pitch of several millions of vibrations per second. These waves could be dealt with as if they were light waves—reflected, refracted, and polarized. These are the waves that are utilized in wireless telegraphy.

ROENTGEN RAYS, OR X−RAYS

In December of 1895 word came out of Germany of a scientific discovery that startled the world. It came first as a rumor, little credited; then as a pronounced report; at last as a demonstration. It told of a new manifestation of energy, in virtue of which the interior of opaque objects is made visible to human eyes. One had only to look into a tube containing a screen of a certain composition, and directed towards a peculiar electrical apparatus, to acquire clairvoyant vision more wonderful than the discredited second−sight of the medium. Coins within a purse, nails driven into wood, spectacles within a leather case, became clearly visible when subjected to the influence of this magic tube; and when a human hand was held before the tube, its bones stood revealed in weird simplicity, as if the living, palpitating flesh about them were but the shadowy substance of a ghost.

Not only could the human eye see these astounding revelations, but the impartial evidence of inanimate chemicals could be brought forward to prove that the mind harbored no illusion. The photographic film recorded the things that the eye might see, and ghostly pictures galore soon gave a quietus to the doubts of the most sceptical. Within a month of the announcement of Professor Roentgen's experiments comment upon the "X−ray" and the "new photography" had become a part of the current gossip of all Christendom.

It is hardly necessary to say that such a revolutionary thing as the discovery of a process whereby opaque objects became transparent, or translucent, was not achieved at a single bound with no intermediate discoveries. In 1859 the German physicist Julius Plucker (1801−1868) noticed that when there was an electrical discharge through an exhausted tube at a low pressure, on the surrounding walls of the tube near the negative pole, or cathode, appeared a greenish phosphorescence. This discovery was soon being investigated by a number of other scientists, among others Hittorf, Goldstein, and Professor (now Sir William) Crookes. The explanations given of this phenomenon by Professor Crookes concern us here more particularly, inasmuch as his views did not accord exactly with those held by the other two scientists, and as his researches were more directly concerned in the discovery of the Roentgen rays. He held that the heat and phosphorescence produced in a low−pressure tube were caused by streams of particles, projected from the cathode with great velocity, striking the sides of the glass tube. The composition of the glass seemed to enter into this phosphorescence also, for while lead glass produced blue phosphorescence, soda glass produced a yellowish green. The composition of the glass seemed to be changed by a long−continued pelting of these particles, the phosphorescence after a time losing its initial brilliancy, caused by the glass becoming "tired," as Professor Crookes said. Thus when some opaque substance, such as iron, is placed between the cathode and the sides of the glass tube so that it casts a shadow in a certain spot on the glass for some little time, it is found on removing the opaque substance or changing its position that the area of glass at first covered by the shadow now responded to the rays in a different manner from the surrounding glass.

The peculiar ray's, now known as the cathode rays, not only cast a shadow, but are deflected by a magnet, so that the position of the phosphorescence on the sides of the tube may be altered by the proximity of a powerful magnet. From this it would seem that the rays are composed of particles charged with negative electricity, and Professor J. J. Thomson has modified the experiment of Perrin to show that negative electricity is actually associated with the rays. There is reason for believing, therefore, that the cathode rays are rapidly moving charges of negative electricity. It is possible, also, to determine the velocity at which these particles are moving by measuring the deflection produced by the magnetic field.

From the fact that opaque substances cast a shadow in these rays it was thought at first that all solids were absolutely opaque to them. Hertz, however, discovered that a small amount of phosphorescence occurred on the glass even when such opaque substances as gold−leaf or aluminium foil were interposed between the cathode and the sides of the tube. Shortly afterwards Lenard discovered that the cathode rays can be made to pass from the inside of a discharge tube to the outside air. For convenience these rays outside the tube have since been known as "Lenard rays."

In the closing days of December, 1895, Professor Wilhelm Konrad Roentgen, of Wurzburg, announced that he had made the discovery of the remarkable effect arising from the cathode rays to which reference was made above. He found that if a plate covered with a phosphorescent substance is placed near a discharge tube exhausted so highly that the cathode rays produced a green phosphorescence, this plate is made to glow in a peculiar manner. The rays producing this glow were not the cathode rays, although apparently arising from them, and are what have since been called the Roentgen rays, or X−rays.

Roentgen found that a shadow is thrown upon the screen by substances held between it and the exhausted tube, the character of the shadow depending upon the density of the substance. Thus metals are almost completely opaque to the rays; such substances as bone much less so, and ordinary flesh hardly so at all. If a coin were held in the hand that had been interposed between the tube and the screen the picture formed showed the coin as a black shadow; and the bones of the hand, while casting a distinct shadow, showed distinctly lighter; while the soft tissues produced scarcely any shadow at all. The value of such a discovery was obvious from the first; and was still further enhanced by the discovery made shortly that, photographic plates are affected by the rays, thus making it possible to make permanent photographic records of pictures through what we know as opaque substances.

What adds materially to the practical value of Roentgen's discovery is the fact that the apparatus for producing the X−rays is now so simple and relatively inexpensive that it is within the reach even of amateur scientists. It consists essentially of an induction coil attached either to cells or a street−current plug for generating the electricity, a focus tube, and a phosphorescence screen. These focus tubes are made in various shapes, but perhaps the most popular are in the form of a glass globe, not unlike an ordinary small−sized water−bottle, this tube being closed and exhausted, and having the two poles (anode and cathode) sealed into the glass walls, but protruding at either end for attachment to the conducting wires from the induction coil. This tube may be mounted on a stand at a height convenient for manipulation. The phosphorescence screen is usually a plate covered with some platino−cyanide and mounted in the end of a box of convenient size, the opposite end of which is so shaped that it fits the contour of the face, shutting out the light and allowing the eyes of the observer to focalize on the screen at the end. For making observations the operator has simply to turn on the current of electricity and apply the screen to his eyes, pointing it towards the glowing tube, when the shadow of any substance interposed between the tube and the screen will appear upon the phosphorescence plate.

The wonderful shadow pictures produced on the phosphorescence screen, or the photographic plate, would seem to come from some peculiar form of light, but the exact nature of these rays is still an open question. Whether the Roentgen rays are really a form of light—that is, a form of "electro−magnetic disturbance propagated through ether," is not fully determined. Numerous experiments have been undertaken to determine this, but as yet no proof has been found that the rays are a form of light, although there appears to be nothing in their properties inconsistent with their being so. For the moment most investigators are content to admit that the term X−ray virtually begs the question as to the intimate nature of the form of energy involved.

VIII. THE CONSERVATION OF ENERGY

As we have seen, it was in 1831 that Faraday opened up the field of magneto−electricity. Reversing the experiments of his predecessors, who had found that electric currents may generate magnetism, he showed that magnets have power under certain circumstances to generate electricity; he proved, indeed, the interconvertibility of electricity and magnetism. Then he showed that all bodies are more or less subject to the influence of magnetism, and that even light may be affected by magnetism as to its phenomena of polarization. He satisfied himself completely of the true identity of all the various forms of electricity, and of the convertibility of electricity and chemical action. Thus he linked together light, chemical affinity, magnetism, and electricity. And, moreover, he knew full well that no one of these can be produced in indefinite supply from another. "Nowhere," he says, "is there a pure creation or production of power without a corresponding exhaustion of something to supply it."

When Faraday wrote those words in 1840 he was treading on the very heels of a greater generalization than any which he actually formulated; nay, he had it fairly within his reach. He saw a great truth without fully realizing its import; it was left for others, approaching the same truth along another path, to point out its full significance.

The great generalization which Faraday so narrowly missed is the truth which since then has become familiar as the doctrine of the conservation of energy—the law that in transforming energy from one condition to another we can never secure more than an equivalent quantity; that, in short, "to create or annihilate energy is as impossible as to create or annihilate matter; and that all the phenomena of the material universe consist in transformations of energy alone." Some philosophers think this the greatest generalization ever conceived by the mind of man. Be that as it may, it is surely one of the great intellectual landmarks of the nineteenth century. It stands apart, so stupendous and so far−reaching in its implications that the generation which first saw the law developed could little appreciate it; only now, through the vista of half a century, do we begin to see it in its true proportions.

A vast generalization such as this is never a mushroom growth, nor does it usually spring full grown from the mind of any single man. Always a number of minds are very near a truth before any one mind fully grasps it. Pre−eminently true is this of the doctrine of the conservation of energy. Not Faraday alone, but half a dozen different men had an inkling of it before it gained full expression; indeed, every man who advocated the undulatory theory of light and heat was verging towards the goal. The doctrine of Young and Fresnel was as a highway leading surely on to the wide plain of conservation. The phenomena of electro− magnetism furnished another such highway. But there was yet another road which led just as surely and even more readily to the same goal. This was the road furnished by the phenomena of heat, and the men who travelled it were destined to outstrip their fellow−workers; though, as we have seen, wayfarers on other roads were within hailing distance when the leaders passed the mark.

In order to do even approximate justice to the men who entered into the great achievement, we must recall that just at the close of the eighteenth century Count Rumford and Humphry Davy independently showed that labor may be transformed into heat; and correctly interpreted this fact as meaning the transformation of molar into molecular motion. We can hardly doubt that each of these men of genius realized—vaguely, at any rate—that there must be a close correspondence between the amount of the molar and the molecular motions; hence that each of them was in sight of the law of the mechanical equivalent of heat. But neither of them quite grasped or explicitly stated what each must vaguely have seen; and for just a quarter of a century no one else even came abreast their line of thought, let alone passing it.

But then, in 1824, a French philosopher, Sadi Carnot, caught step with the great Englishmen, and took a long leap ahead by explicitly stating his belief that a definite quantity of work could be transformed into a definite quantity of heat, no more, no less. Carnot did not, indeed, reach the clear view of his predecessors as to the nature of heat, for he still thought it a form of "imponderable" fluid; but he reasoned none the less clearly as to its mutual convertibility with mechanical work. But important as his conclusions seem now that we look back upon them with clearer vision, they made no impression whatever upon his contemporaries. Carnot's work in this line was an isolated phenomenon of historical interest, but it did not enter into the scheme of the completed narrative in any such way as did the work of Rumford and Davy.

The man who really took up the broken thread where Rumford and Davy had dropped it, and wove it into a completed texture, came upon the scene in 1840. His home was in Manchester, England; his occupation that of a manufacturer. He was a friend and pupil of the great Dr. Dalton. His name was James Prescott Joule. When posterity has done its final juggling with the names of the nineteenth century, it is not unlikely that the name of this Manchester philosopher will be a household word, like the names of Aristotle, Copernicus, and Newton.

For Joule's work it was, done in the fifth decade of the century, which demonstrated beyond all cavil that there is a precise and absolute equivalence between mechanical work and heat; that whatever the form of manifestation of molar motion, it can generate a definite and measurable amount of heat, and no more. Joule found, for example, that at the sea−level in Manchester a pound weight falling through seven hundred and seventy−two feet could generate enough heat to raise the temperature of a pound of water one degree Fahrenheit. There was nothing haphazard, nothing accidental, about this; it bore the stamp of unalterable law. And Joule himself saw, what others in time were made to see, that this truth is merely a particular case within a more general law. If heat cannot be in any sense created, but only made manifest as a transformation of another kind of motion, then must not the same thing be true of all those other forms of "force"—light, electricity, magnetism—which had been shown to be so closely associated, so mutually convertible, with heat? All analogy seemed to urge the truth of this inference; all experiment tended to confirm it. The law of the mechanical equivalent of heat then became the main corner−stone of the greater law of the conservation of energy.

But while this citation is fresh in mind, we must turn our attention with all haste to a country across the Channel—to Denmark, in short—and learn that even as Joule experimented with the transformation of heat, a philosopher of Copenhagen, Colding by name, had hit upon the same idea, and carried it far towards a demonstration. And then, without pausing, we must shift yet again, this time to Germany, and consider the work of three other men, who independently were on the track of the same truth, and two of whom, it must be admitted, reached it earlier than either Joule or Colding, if neither brought it to quite so clear a demonstration. The names of these three Germans are Mohr, Mayer, and Helmholtz. Their share in establishing the great doctrine of conservation must now claim our attention.

As to Karl Friedrich Mohr, it may be said that his statement of the doctrine preceded that of any of his fellows, yet that otherwise it was perhaps least important. In 1837 this thoughtful German had grasped the main truth, and given it expression in an article published in the Zeitschrift fur Physik, etc. But the article attracted no attention whatever, even from Mohr's own countrymen. Still, Mohr's title to rank as one who independently conceived the great truth, and perhaps conceived it before any other man in the world saw it as clearly, even though he did not demonstrate its validity, is not to be disputed.

It was just five years later, in 1842, that Dr. Julius Robert Mayer, practising physician in the little German town of Heilbronn, published a paper in Liebig's Annalen on "The Forces of Inorganic Nature," in which not merely the mechanical theory of heat, but the entire doctrine of the conservation of energy, is explicitly if briefly stated. Two years earlier Dr. Mayer, while surgeon to a Dutch India vessel cruising in the tropics, had observed that the venous blood of a patient seemed redder than venous blood usually is observed to be in temperate climates. He pondered over this seemingly insignificant fact, and at last reached the conclusion that the cause must be the lesser amount of oxidation required to keep up the body temperature in the tropics. Led by this reflection to consider the body as a machine dependent on outside forces for its capacity to act, he passed on into a novel realm of thought, which brought him at last to independent discovery of the mechanical theory of heat, and to the first full and comprehensive appreciation of the great law of conservation. Blood−letting, the modern physician holds, was a practice of very doubtful benefit, as a rule, to the subject; but once, at least, it led to marvellous results. No straw is go small that

it may not point the receptive mind of genius to new and wonderful truths.

MAYER'S PAPER OF 1842

The paper in which Mayer first gave expression to his revolutionary ideas bore the title of "The Forces of Inorganic Nature," and was published in 1842. It is one of the gems of scientific literature, and fortunately it is not too long to be quoted in its entirety. Seldom if ever was a great revolutionary doctrine expounded in briefer compass:

"What are we to understand by 'forces'? and how are different forces related to each other? The term force conveys for the most part the idea of something unknown, unsearchable, and hypothetical; while the term matter, on the other hand, implies the possession, by the object in question, of such definite properties as weight and extension. An attempt, therefore, to render the idea of force equally exact with that of matter is one which should be welcomed by all those who desire to have their views of nature clear and unencumbered by hypothesis.

"Forces are causes; and accordingly we may make full application in relation to them of the principle causa aequat effectum. If the cause c has the effect e, then c = e; if, in its turn, e is the cause of a second effect of f, we have e = f, and so on: c = e = f ... = c. In a series of causes and effects, a term or a part of a term can never, as is apparent from the nature of an equation, become equal to nothing. This first property of all causes we call their indestructibility.

"If the given cause c has produced an effect e equal to itself, it has in that very act ceased to be—c has become e. If, after the production of e, c still remained in the whole or in part, there must be still further effects corresponding to this remaining cause: the total effect of c would thus be > e, which would be contrary to the supposition c = e. Accordingly, since c becomes e, and e becomes f, etc., we must regard these various magnitudes as different forms under which one and the same object makes its appearance. This capability of assuming various forms is the second essential property of all causes. Taking both properties together, we may say, causes an INDESTRUCTIBLE quantitatively, and quantitatively CONVERTIBLE objects.

"There occur in nature two causes which apparently never pass one into the other," said Mayer. "The first class consists of such causes as possess the properties of weight and impenetrability. These are kinds of matter. The other class is composed of causes which are wanting in the properties just mentioned— namely, forces, called also imponderables, from the negative property that has been indicated. Forces are therefore INDESTRUCTIBLE, CONVERTIBLE, IMPONDERABLE OBJECTS.

"As an example of causes and effects, take matter: explosive gas, H + O, and water, HO, are related to each other as cause and effect; therefore H + O = HO. But if H + O becomes HO, heat, cal., makes its appearance as well as water; this heat must likewise have a cause, x, and we have therefore H + O + X = HO + cal. It might be asked, however, whether H + O is really = HO, and x = cal., and not perhaps H + O = cal., and x = HO, whence the above equation could equally be deduced; and so in many other cases. The phlogistic chemists recognized the equation between cal. and x, or phlogiston as they called it, and in so doing made a great step in advance; but they involved themselves again in a system of mistakes by putting x in place of O. In this way they obtained H = HO + x.

"Chemistry teaches us that matter, as a cause, has matter for its effect; but we may say with equal justification that to force as a cause corresponds force as effect. Since c = e, and e = c, it is natural to call one term of an equation a force, and the other an effect of force, or phenomenon, and to attach different notions to the expression force and phenomenon. In brief, then, if the cause is matter, the effect is matter; if the cause is a force, the effect is also a force.

"The cause that brings about the raising of a weight is a force. The effect of the raised weight is, therefore, also a force; or, expressed in a more general form, SEPARATION IN SPACE OF PONDERABLE OBJECTS IS A FORCE; and since this force causes the fall of bodies, we call it FALLING FORCE. Falling force and fall, or, still more generally, falling force and motion, are forces related to each other as cause and effect—forces convertible into each other—two different forms of one and the same object. For example, a weight resting on the ground is not a force: it is neither the cause of motion nor of the lifting of another weight. It becomes so, however, in proportion as it is raised above the ground. The cause—that is, the distance between a weight and the earth, and the effect, or the quantity of motion produced, bear to each other, as shown by mechanics, a constant relation.

'Gravity being regarded as the cause of the falling of bodies, a gravitating force is spoken of; and thus the ideas of PROPERTY and of FORCE are confounded with each other. Precisely that which is the essential attribute of every force—that is, the UNION of indestructibility with convertibility—is wanting in every property: between a property and a force, between gravity and motion, it is therefore impossible to establish the equation required for a rightly conceived causal relation. If gravity be called a force, a cause is supposed which produces effects without itself diminishing, and incorrect conceptions of the causal connections of things are thereby fostered. In order that a body may fall, it is just as necessary that it be lifted up as that it should be heavy or possess gravity. The fall of bodies, therefore, ought not to be ascribed to their gravity alone. The problem of mechanics is to develop the equations which subsist between falling force and motion, motion and falling force, and between different motions. Here is a case in point: The magnitude of the falling force v is directly proportional (the earth's radius being assumed—oo) to the magnitude of the mass m, and the height d, to which it is raised—that is, v = md. If the height d = l, to which the mass m is raised, is transformed into the final velocity c = l of this mass, we have also v = mc; but from the known relations existing between d and c, it results that, for other values of d or of c, the measure of the force v is mc squared; accordingly v = md = mcsquared. The law of the conservation of vis viva is thus found to be based on the general law of the indestructibility of causes.

"In many cases we see motion cease without having caused another motion or the lifting of a weight. But a force once in existence cannot be annihilated—it can only change its form. And the question therefore arises, what other forms is force, which we have become acquainted with as falling force and motion, capable of assuming? Experience alone can lead us to a conclusion on this point. That we may experiment to advantage, we must select implements which, besides causing a real cessation of motion, are as little as possible altered by the objects to be examined. For example, if we rub together two metal plates, we see motion disappear, and heat, on the other hand, make its appearance, and there remains to be determined only whether MOTION is the cause of heat. In order to reach a decision on this point, we must discuss the question whether, in the numberless cases in which the expenditure of motion is accompanied by the appearance of heat, the motion has not some other effect than the production of heat, and the heat some other cause than the motion.

"A serious attempt to ascertain the effects of ceasing motion has never been made. Without wishing to exclude a priori the hypothesis which it may be possible to establish, therefore, we observe only that, as a rule, this effect cannot be supposed to be an alteration in the state of aggregation of the moved (that is, rubbing, etc.) bodies. If we assume that a certain quantity of motion v is expended in the conversion of a rubbing substance m into n, we must then have m + v − n, and n = m + v; and when n is reconverted into m, v must appear again in some form or other.

By the friction of two metallic plates continued for a very long time, we can gradually cause the cessation of an immense quantity of movement; but would it ever occur to us to look for even the smallest trace of the force which has disappeared in the metallic dust that we could collect, and to try to regain it thence? We repeat, the motion cannot have been annihilated; and contrary, or positive and negative, motions cannot be regarded as = o any more than contrary motions can come out of nothing, or a weight can raise itself.

"Without the recognition of a causal relation between motion and heat, it is just as difficult to explain the production of heat as it is to give any account of the motion that disappears. The heat cannot be derived from the diminution of the volume of the rubbing substances. It is well known that two pieces of ice may be melted by rubbing them together in vacuo; but let any one try to convert ice into water by pressure, however enormous. The author has found that water undergoes a rise of temperature when shaken violently. The water so heated (from twelve to thirteen degrees centigrade) has a greater bulk after being shaken than it had before. Whence now comes this quantity of heat, which by repeated shaking may be called into existence in the same apparatus as often as we please? The vibratory hypothesis of heat is an approach towards the doctrine of heat being the effect of motion, but it does not favor the admission of this causal relation in its full generality. It rather lays the chief stress on restless oscillations.

"If it be considered as now established that in many cases no other effect of motion can be traced except heat, and that no other cause than motion can be found for the heat that is produced, we prefer the assumption that heat proceeds from motion to the assumption of a cause without effect and of an effect without a cause. Just as the chemist, instead of allowing oxygen and hydrogen to disappear without further investigation, and water to be produced in some inexplicable manner, establishes a connection between oxygen and hydrogen on the one hand, and water on the other.

"We may conceive the natural connection existing between falling force, motion, and heat as follows: We know that heat makes its appearance when the separate particles of a body approach nearer to each other; condensation produces heat. And what applies to the smallest particles of matter, and the smallest intervals between them, must also apply to large masses and to measurable distances. The falling of a weight is a diminution of the bulk of the earth, and must therefore without doubt be related to the quantity of heat thereby developed; this quantity of heat must be proportional to the greatness of the weight and its distance from the ground. From this point of view we are easily led to the equations between falling force, motion, and heat that have already been discussed.

"But just as little as the connection between falling force and motion authorizes the conclusion that the essence of falling force is motion, can such a conclusion be adopted in the case of heat. We are, on the contrary, rather inclined to infer that, before it can become heat, motion must cease to exist as motion, whether simple, or vibratory, as in the case of light and radiant heat, etc.

"If falling force and motion are equivalent to heat, heat must also naturally be equivalent to motion and falling force. Just as heat appears as an EFFECT of the diminution of bulk and of the cessation of motion, so also does heat disappear as a CAUSE when its effects are produced in the shape of motion, expansion, or raising of weight.

"In water−mills the continual diminution in bulk which the earth undergoes, owing to the fall of the water, gives rise to motion, which afterwards disappears again, calling forth unceasingly a great quantity of heat; and, inversely, the steam−engine serves to decompose heat again into motion or the raising of weights. A locomotive with its train may be compared to a distilling apparatus; the heat applied under the boiler passes off as motion, and this is deposited again as heat at the axles of the wheels."

Mayer then closes his paper with the following deduction: "The solution of the equations subsisting between falling force and motion requires that the space fallen through in a given time—e. g., the first second— should be experimentally determined. In like manner, the solution of the equations subsisting between falling force and motion on the one hand and heat on the other requires an answer to the question, How great is the quantity of heat which corresponds to a given quantity of motion or falling force? For instance, we must ascertain how high a given weight requires to be raised above the ground in order that its falling force maybe equivalent to the raising of the temperature of an equal weight of water from 0 degrees to 1 degrees centigrade. The attempt to show that such an equation is the expression of a physical truth may be regarded as the substance of the foregoing remarks.

"By applying the principles that have been set forth to the relations subsisting between the temperature and the volume of gases, we find that the sinking of a mercury column by which a gas is compressed is equivalent to the quantity of heat set free by the compression; and hence it follows, the ratio between the capacity for heat of air under constant pressure and its capacity under constant volume being taken as = 1.421, that the warming of a given weight of water from 0 degrees to 1 degrees centigrade corresponds to the fall of an equal weight from the height of about three hundred and sixty−five metres. If we compare with this result the working of our best steam−engines, we see how small a part only of the heat applied under the boiler is really transformed into motion or the raising of weights; and this may serve as justification for the attempts at the profitable production of motion by some other method than the expenditure of the chemical difference between carbon and oxygen—more particularly by the transformation into motion of electricity obtained by chemical means."[1]

MAYER AND HELMHOLTZ

Here, then, was this obscure German physician, leading the humdrum life of a village practitioner, yet seeing such visions as no human being in the world had ever seen before.

The great principle he had discovered became the dominating thought of his life, and filled all his leisure hours. He applied it far and wide, amid all the phenomena of the inorganic and organic worlds. It taught him that both vegetables and animals are machines, bound by the same laws that hold sway over inorganic matter, transforming energy, but creating nothing. Then his mind reached out into space and met a universe made up of questions. Each star that blinked down at him as he rode in answer to a night−call seemed an interrogation−point asking, How do I exist? Why have I not long since burned out if your theory of conservation be true? No one had hitherto even tried to answer that question; few had so much as realized that it demanded an answer. But the Heilbronn physician understood the question and found an answer. His meteoric hypothesis, published in 1848, gave for the first time a tenable explanation of the persistent light and heat of our sun and the myriad other suns—an explanation to which we shall recur in another connection.

All this time our isolated philosopher, his brain aflame with the glow of creative thought, was quite unaware that any one else in the world was working along the same lines. And the outside world was equally heedless of the work of the Heilbronn physician. There was no friend to inspire enthusiasm and give courage, no kindred spirit to react on this masterful but lonely mind. And this is the more remarkable because there are few other cases where a master−originator in science has come upon the scene except as the pupil or friend of some other master−originator. Of the men we have noticed in the present connection, Young was the friend and confrere of Davy; Davy, the protege of Rumford; Faraday, the pupil of Davy; Fresnel, the co−worker with Arago; Colding, the confrere of Oersted; Joule, the pupil of Dalton. But Mayer is an isolated phenomenon—one of the lone mountain−peak intellects of the century. That estimate may be exaggerated which has called him the Galileo of the nineteenth century, but surely no lukewarm praise can do him justice.

Yet for a long time his work attracted no attention whatever. In 1847, when another German physician, Hermann von Helmholtz, one of the most massive and towering intellects of any age, had been independently led to comprehension of the doctrine of the conservation of energy and published his treatise on the subject, he had hardly heard of his countryman Mayer. When he did hear of him, however, he hastened to renounce all claim to the doctrine of conservation, though the world at large gives him credit of independent even though subsequent discovery.

JOULE'S PAPER OF 1843

Meantime, in England, Joule was going on from one experimental demonstration to another, oblivious of his German competitors and almost as little noticed by his own countrymen. He read his first paper before the chemical section of the British Association for the Advancement of Science in 1843, and no one heeded it in the least. It is well worth our while, however, to consider it at length. It bears the title, "On the Calorific Effects of Magneto−Electricity, and the Mechanical Value of Heat." The full text, as published in the Report of the British Association, is as follows:

"Although it has been long known that fine platinum wire can be ignited by magneto−electricity, it still remained a matter of doubt whether heat was evolved by the COILS in which the magneto−electricity was generated; and it seemed indeed not unreasonable to suppose that COLD was produced there in order to make up for the heat evolved by the other part of the circuit. The author therefore has endeavored to clear up this uncertainty by experiment. His apparatus consisted of a small compound electro−magnet, immersed in water, revolving between the poles of a powerful stationary magnet. The magneto−electricity developed in the coils of the revolving electro−magnet was measured by an accurate galvanometer; and the temperature of the water was taken before and after each experiment by a very delicate thermometer. The influence of the temperature of the surrounding atmospheric air was guarded against by covering the revolving tube with flannel, etc., and by the adoption of a system of interpolation. By an extensive series of experiments with the above apparatus the author succeeded in proving that heat is evolved by the coils of the magneto−electrical machine, as well as by any other part of the circuit, in proportion to the resistance to conduction of the wire and the square of the current; the magneto having, under comparable circumstances, the same calorific power as the voltaic electricity.

"Professor Jacobi, of St. Petersburg, bad shown that the motion of an electro−magnetic machine generates magneto−electricity in opposition to the voltaic current of the battery. The author had observed the same phenomenon on arranging his apparatus as an electro−magnetic machine; but had found that no additional heat was evolved on account of the conflict of forces in the coil of the electro−magnet, and that the heat evolved by the coil remained, as before, proportional to the square of the current. Again, by turning the machine contrary to the direction of the attractive forces, so as to increase the intensity of the voltaic current by the assistance of the magneto−electricity, he found that the evolution of heat was still proportional to the square of the current. The author discovered, therefore, that the heat evolved by the voltaic current is invariably proportional to the square of the current, however the intensity of the current may be varied by magnetic induction. But Dr. Faraday has shown that the chemical effects of the current are simply as its quantity. Therefore he concluded that in the electro−magnetic engine a part of the heat due to the chemical actions of the battery is lost by the circuit, and converted into mechanical power; and that when the electro−magnetic engine is turned CONTRARY to the direction of the attractive forces, a greater quantity of heat is evolved by the circuit than is due to the chemical reactions of the battery, the over−plus quantity being produced by the conversion of the mechanical force exerted in turning the machine. By a dynamometrical apparatus attached to his machine, the author has ascertained that, in all the above cases, a quantity of heat, capable of increasing the temperature of a pound of water by one degree of Fahrenheit's scale, is equal to the mechanical force capable of raising a weight of about eight hundred and thirty pounds to the height of one foot."[2]

JOULE OR MAYER?

Two years later Joule wished to read another paper, but the chairman hinted that time was limited, and asked him to confine himself to a brief verbal synopsis of the results of his experiments. Had the chairman but known it, he was curtailing a paper vastly more important than all the other papers of the meeting put together. However, the synopsis was given, and one man was there to hear it who had the genius to appreciate its importance. This was William Thomson, the present Lord Kelvin, now known to all the world as among the greatest of natural philosophers, but then only a novitiate in science. He came to Joule's aid, started rolling the ball of controversy, and subsequently associated himself with the Manchester experimenter in pursuing his investigations.

But meantime the acknowledged leaders of British science viewed the new doctrine askance. Faraday, Brewster, Herschel—those were the great names in physics at that day, and no one of them could quite accept the new views regarding energy. For several years no older physicist, speaking with recognized authority, came forward in support of the doctrine of conservation. This culminating thought of the first half of the nineteenth century came silently into the world, unheralded and unopposed. The fifth decade of the century had seen it elaborated and substantially demonstrated in at least three different countries, yet even the leaders of thought did not so much as know of its existence. In 1853 Whewell, the historian of the inductive sciences, published a second edition of his history, and, as Huxley has pointed out, he did not so much as refer to the revolutionizing thought which even then was a full decade old.

By this time, however, the battle was brewing. The rising generation saw the importance of a law which their elders could not appreciate, and soon it was noised abroad that there were more than one claimant to the honor of discovery. Chiefly through the efforts of Professor Tyndall, the work of Mayer became known to the British public, and a most regrettable controversy ensued between the partisans of Mayer and those of Joule—a bitter controversy, in which Davy's contention that science knows no country was not always regarded, and which left its scars upon the hearts and minds of the great men whose personal interests were involved.

And so to this day the question who is the chief discoverer of the law of the conservation of energy is not susceptible of a categorical answer that would satisfy all philosophers. It is generally held that the first choice lies between Joule and Mayer. Professor Tyndall has expressed the belief that in future each of these men will be equally remembered in connection with this work. But history gives us no warrant for such a hope. Posterity in the long run demands always that its heroes shall stand alone. Who remembers now that Robert Hooke contested with Newton the discovery of the doctrine of universal gravitation? The judgment of posterity is unjust, but it is inexorable. And so we can little doubt that a century from now one name will be mentioned as that of the originator of the great doctrine of the conservation of energy. The man whose name is thus remembered will perhaps be spoken of as the Galileo, the Newton, of the nineteenth century; but whether the name thus dignified by the final verdict of history will be that of Colding, Mohr, Mayer, Helmholtz, or Joule, is not as, yet decided.

LORD KELVIN AND THE DISSIPATION OF ENERGY

The gradual permeation of the field by the great doctrine of conservation simply repeated the history of the introduction of every novel and revolutionary thought. Necessarily the elder generation, to whom all forms of energy were imponderable fluids, must pass away before the new conception could claim the field. Even the word energy, though Young had introduced it in 1807, did not come into general use till some time after the middle of the century. To the generality of philosophers (the word physicist was even less in favor at this time) the various forms of energy were still subtile fluids, and never was idea relinquished with greater unwillingness than this. The experiments of Young and Fresnel had convinced a large number of philosophers that light is a vibration and not a substance; but so great an authority as Biot clung to the old emission idea to the end of his life, in 1862, and held a following.

Meantime, however, the company of brilliant young men who had just served their apprenticeship when the doctrine of conservation came upon the scene had grown into authoritative positions, and were battling actively for the new ideas. Confirmatory evidence that energy is a molecular motion and not an "imponderable" form of matter accumulated day by day. The experiments of two Frenchmen, Hippolyte L. Fizeau and Leon Foucault, served finally to convince the last lingering sceptics that light is an undulation; and by implication brought heat into the same category, since James David Forbes, the Scotch physicist, had shown in 1837 that radiant heat conforms to the same laws of polarization and double refraction that govern light. But, for that matter, the experiments that had established the mechanical equivalent of heat hardly left room for doubt as to the immateriality of this "imponderable." Doubters had indeed, expressed scepticism as to the validity of Joule's experiments, but the further researches, experimental and mathematical, of such workers as Thomson (Lord Kelvin), Rankine, and Tyndall in Great Britain, of Helmholtz and Clausius in Germany, and of Regnault in France, dealing with various manifestations of heat, placed the evidence beyond the reach of criticism.

Out of these studies, just at the middle of the century, to which the experiments of Mayer and Joule had led, grew the new science of thermo−dynamics. Out of them also grew in the mind of one of the investigators a new generalization, only second in importance to the doctrine of conservation itself. Professor William Thomson (Lord Kelvin) in his studies in thermodynamics was early impressed with the fact that whereas all the molar motion developed through labor or gravity could be converted into heat, the process is not fully reversible. Heat can, indeed, be converted into molar motion or work, but in the process a certain amount of the heat is radiated into space and lost. The same thing happens whenever any other form of energy is converted into molar motion. Indeed, every transmutation of energy, of whatever character, seems complicated by a tendency to develop heat, part of which is lost. This observation led Professor Thomson to his doctrine of the dissipation of energy, which he formulated before the Royal Society of Edinburgh in 1852, and published also in the Philosophical Magazine the same year, the title borne being, "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy."

From the principle here expressed Professor Thomson drew the startling conclusion that, "since any restoration of this mechanical energy without more than an equivalent dissipation is impossible," the universe, as known to us, must be in the condition of a machine gradually running down; and in particular that the world we live on has been within a finite time unfit for human habitation, and must again become so within a finite future. This thought seems such a commonplace to−day that it is difficult to realize how startling it appeared half a century ago. A generation trained, as ours has been, in the doctrines of the conservation and dissipation of energy as the very alphabet of physical science can but ill appreciate the mental attitude of a generation which for the most part had not even thought it problematical whether the sun could continue to give out heat and light forever. But those advance thinkers who had grasped the import of the doctrine of conservation could at once appreciate the force of Thomson's doctrine of dissipation, and realize the complementary character of the two conceptions.

Here and there a thinker like Rankine did, indeed, attempt to fancy conditions under which the energy lost through dissipation might be restored to availability, but no such effort has met with success, and in time Professor Thomson's generalization and his conclusions as to the consequences of the law involved came to be universally accepted.

The introduction of the new views regarding the nature of energy followed, as I have said, the course of every other growth of new ideas. Young and imaginative men could accept the new point of view; older philosophers, their minds channelled by preconceptions, could not get into the new groove. So strikingly true is this in the particular case now before us that it is worth while to note the ages at the time of the revolutionary experiments of the men whose work has been mentioned as entering into the scheme of evolution of the idea that energy is merely a manifestation of matter in motion. Such a list will tell the story better than a volume of commentary.

Observe, then, that Davy made his epochal experiment of melting ice by friction when he was a youth of twenty. Young was no older when he made his first communication to the Royal Society, and was in his twenty−seventh year when he first actively espoused the undulatory theory. Fresnel was twenty−six when he made his first important discoveries in the same field; and Arago, who at once became his champion, was then but two years his senior, though for a decade he had been so famous that one involuntarily thinks of him as belonging to an elder generation.

Forbes was under thirty when he discovered the polarization of heat, which pointed the way to Mohr, then thirty−one, to the mechanical equivalent. Joule was twenty−two in 1840, when his great work was begun; and Mayer, whose discoveries date from the same year, was then twenty−six, which was also the age of Helmholtz when he published his independent discovery of the same law. William Thomson was a youth just past his majority when he came to the aid of Joule before the British Society, and but seven years older when he formulated his own doctrine of the dissipation of energy. And Clausius and Rankine, who are usually mentioned with Thomson as the great developers of thermo−dynamics, were both far advanced with their novel studies before they were thirty. With such a list in mind, we may well agree with the father of inductive science that "the man who is young in years may be old in hours."

Yet we must not forget that the shield has a reverse side. For was not the greatest of observing astronomers, Herschel, past thirty−five before he ever saw a telescope, and past fifty before he discovered the heat rays of the spectrum? And had not Faraday reached middle life before he turned his attention especially to electricity? Clearly, then, to make this phrase complete, Bacon should have added that "the man who is old in years may be young in imagination." Here, however, even more appropriate than in the other case —more's the pity—would have been the application of his qualifying clause: "but that happeneth rarely."

THE FINAL UNIFICATION

There are only a few great generalizations as yet thought out in any single field of science. Naturally, then, after a great generalization has found definitive expression, there is a period of lull before another forward move. In the case of the doctrines of energy, the lull has lasted half a century. Throughout this period, it is true, a multitude of workers have been delving in the field, and to the casual observer it might seem as if their activity had been boundless, while the practical applications of their ideas—as exemplified, for example, in the telephone, phonograph, electric light, and so on —have been little less than revolutionary. Yet the most competent of living authorities, Lord Kelvin, could assert in 1895 that in fifty years he had learned nothing new regarding the nature of energy.

This, however, must not be interpreted as meaning that the world has stood still during these two generations. It means rather that the rank and file have been moving forward along the road the leaders had already travelled. Only a few men in the world had the range of thought regarding the new doctrine of energy that Lord Kelvin had at the middle of the century. The few leaders then saw clearly enough that if one form of energy is in reality merely an undulation or vibration among the particles of "ponderable" matter or of ether, all other manifestations of energy must be of the same nature. But the rank and file were not even within sight of this truth for a long time after they had partly grasped the meaning of the doctrine of conservation. When, late in the fifties, that marvellous young Scotchman, James Clerk−Maxwell, formulating in other words an idea of Faraday's, expressed his belief that electricity and magnetism are but manifestations of various conditions of stress and motion in the ethereal medium (electricity a displacement of strain, magnetism a whirl in the ether), the idea met with no immediate popularity. And even less cordial was the reception given the same thinker's theory, put forward in 1863, that the ethereal undulations producing the phenomenon we call light differ in no respect except in their wave−length from the pulsations of electro−magnetism.

At about the same time Helmholtz formulated a somewhat similar electro−magnetic theory of light; but even the weight of this combined authority could not give the doctrine vogue until very recently, when the experiments of Heinrich Hertz, the pupil of Helmholtz, have shown that a condition of electrical strain may be developed into a wave system by recurrent interruptions of the electric state in the generator, and that such waves travel through the ether with the rapidity of light. Since then the electro−magnetic theory of light has been enthusiastically referred to as the greatest generalization of the century; but the sober thinker must see that it is really only what Hertz himself called it—one pier beneath the great arch of conservation. It is an interesting detail of the architecture, but the part cannot equal the size of the whole.

More than that, this particular pier is as yet by no means a very firm one. It has, indeed, been demonstrated that waves of electro−magnetism pass through space with the speed of light, but as yet no one has developed electric waves even remotely approximating the shortness of the visual rays. The most that can positively be asserted, therefore, is that all the known forms of radiant energy−heat, light, electro−magnetism— travel through space at the same rate of speed, and consist of traverse vibrations—"lateral quivers," as Fresnel said of light—known to differ in length, and not positively known to differ otherwise. It has, indeed, been suggested that the newest form of radiant energy, the famous X−ray of Professor Roentgen's discovery, is a longitudinal vibration, but this is a mere surmise. Be that as it may, there is no one now to question that all forms of radiant energy, whatever their exact affinities, consist essentially of undulatory motions of one uniform medium.

A full century of experiment, calculation, and controversy has thus sufficed to correlate the "imponderable fluids" of our forebears, and reduce them all to manifestations of motion among particles of matter. At first glimpse that seems an enormous change of view. And yet, when closely considered, that change in thought is not so radical as the change in phrase might seem to imply. For the nineteenth−century physicist, in displacing the "imponderable fluids" of many kinds—one each for light, heat, electricity, magnetism—has been obliged to substitute for them one all−pervading fluid, whose various quivers, waves, ripples, whirls or strains produce the manifestations which in popular parlance are termed forms of force. This all−pervading fluid the physicist terms the ether, and he thinks of it as having no weight. In effect, then, the physicist has dispossessed the many imponderables in favor of a single imponderable—though the word imponderable has been banished from his vocabulary. In this view the ether—which, considered as a recognized scientific verity, is essentially a nineteenth− century discovery—is about the most interesting thing in the universe. Something more as to its properties, real or assumed, we shall have occasion to examine as we turn to the obverse side of physics, which demands our attention in the next chapter.

IX. THE ETHER AND PONDERABLE MATTER

"Whatever difficulties we may have in forming a consistent idea of the constitution of the ether, there can be no doubt that the interplanetary and interstellar spaces are not empty, but are occupied by a material substance or body which is certainly the largest and probably the most uniform body of which we have any knowledge."

Such was the verdict pronounced some thirty years ago by James Clerk−Maxwell, one of the very greatest of nineteenth−century physicists, regarding the existence of an all−pervading plenum in the universe, in which every particle of tangible matter is immersed. And this verdict may be said to express the attitude of the entire philosophical world of our day. Without exception, the authoritative physicists of our time accept this plenum as a verity, and reason about it with something of the same confidence they manifest in speaking of "ponderable" matter or of, energy. It is true there are those among them who are disposed to deny that this all−pervading plenum merits the name of matter. But that it is a something, and a vastly important something at that, all are agreed. Without it, they allege, we should know nothing of light, of radiant heat, of electricity or magnetism; without it there would probably be no such thing as gravitation; nay, they even hint that without this strange something, ether, there would be no such thing as matter in the universe. If these contentions of the modern physicist are justified, then this intangible ether is incomparably the most important as well as the "largest and most uniform substance or body" in the universe. Its discovery may well be looked upon as one of the most important feats of the nineteenth century.

For a discovery of that century it surely is, in the sense that all the known evidences of its existence were gathered in that epoch. True dreamers of all ages have, for metaphysical reasons, imagined the existence of intangible fluids in space—they had, indeed, peopled space several times over with different kinds of ethers, as Maxwell remarks—but such vague dreamings no more constituted the discovery of the modern ether than the dream of some pre−Columbian visionary that land might lie beyond the unknown waters constituted the discovery of America. In justice it must be admitted that Huyghens, the seventeenth−century originator of the undulatory theory of light, caught a glimpse of the true ether; but his contemporaries and some eight generations of his successors were utterly deaf to his claims; so he bears practically the same relation to the nineteenth−century discoverers of ether that the Norseman bears to Columbus.

The true Columbus of the ether was Thomas Young. His discovery was consummated in the early days of the nineteenth century, when he brought forward the first, conclusive proofs of the undulatory theory of light. To say that light consists of undulations is to postulate something that undulates; and this something could not be air, for air exists only in infinitesimal quantity, if at all, in the interstellar spaces, through which light freely penetrates. But if not air, what then? Why, clearly, something more intangible than air; something supersensible, evading all direct efforts to detect it, yet existing everywhere in seemingly vacant space, and also interpenetrating the substance of all transparent liquids and solids, if not, indeed, of all tangible substances. This intangible something Young rechristened the Luminiferous Ether.

In the early days of his discovery Young thought of the undulations which produce light and radiant heat as being longitudinal—a forward and backward pulsation, corresponding to the pulsations of sound—and as such pulsations can be transmitted by a fluid medium with the properties of ordinary fluids, he was justified in thinking of the ether as being like a fluid in its properties, except for its extreme intangibility. But about 1818 the experiments of Fresnel and Arago with polarization of light made it seem very doubtful whether the theory of longitudinal vibrations is sufficient, and it was suggested by Young, and independently conceived and demonstrated by Fresnel, that the luminiferous undulations are not longitudinal, but transverse; and all the more recent experiments have tended to confirm this view. But it happens that ordinary fluids— gases and liquids—cannot transmit lateral vibrations; only rigid bodies are capable of such a vibration. So it became necessary to assume that the luminiferous ether is a body possessing elastic rigidity—a familiar property of tangible solids, but one quite unknown among fluids.

The idea of transverse vibrations carried with it another puzzle. Why does not the ether, when set aquiver with the vibration which gives us the sensation we call light, have produced in its substance subordinate quivers, setting out at right angles from the path of the original quiver? Such perpendicular vibrations seem not to exist, else we might see around a corner; how explain their absence? The physicist could think of but one way: they must assume that the ether is incompressible. It must fill all space—at any rate, all space with which human knowledge deals—perfectly full.

These properties of the ether, incompressibility and elastic rigidity, are quite conceivable by themselves; but difficulties of thought appear when we reflect upon another quality which the ether clearly must possess—namely, frictionlessness. By hypothesis this rigid, incompressible body pervades all space, imbedding every particle of tangible matter; yet it seems not to retard the movements of this matter in the slightest degree. This is undoubtedly the most difficult to comprehend of the alleged properties of the ether. The physicist explains it as due to the perfect elasticity of the ether, in virtue of which it closes in behind a moving particle with a push exactly counterbalancing the stress required to penetrate it in front.

To a person unaccustomed to think of seemingly solid matter as really composed of particles relatively wide apart, it is hard to understand the claim that ether penetrates the substance of solids—of glass, for example—and, to use Young's expression, which we have previously quoted, moves among them as freely as the wind moves through a grove of trees. This thought, however, presents few difficulties to the mind accustomed to philosophical speculation. But the question early arose in the mind of Fresnel whether the ether is not considerably affected by contact with the particles of solids. Some of his experiments led him to believe that a portion of the ether which penetrates among the molecules of tangible matter is held captive, so to speak, and made to move along with these particles. He spoke of such portions of the ether as "bound" ether, in contradistinction to the great mass of "free" ether. Half a century after Fresnel's death, when the ether hypothesis had become an accepted tenet of science, experiments were undertaken by Fizeau in France, and by Clerk−Maxwell in England, to ascertain whether any portion of ether is really thus bound to particles of matter; but the results of the experiments were negative, and the question is still undetermined.

While the undulatory theory of light was still fighting its way, another kind of evidence favoring the existence of an ether was put forward by Michael Faraday, who, in the course of his experiments in electrical and magnetic induction, was led more and more to perceive definite lines or channels of force in the medium subject to electro−magnetic influence. Faraday's mind, like that of Newton and many other philosophers, rejected the idea of action at a distance, and he felt convinced that the phenomena of magnetism and of electric induction told strongly for the existence of an invisible plenum everywhere in space, which might very probably be the same plenum that carries the undulations of light and radiant heat.

Then, about the middle of the century, came that final revolution of thought regarding the nature of energy which we have already outlined in the preceding chapter, and with that the case for ether was considered to be fully established. The idea that energy is merely a "mode of motion" (to adopt Tyndall's familiar phrase), combined with the universal rejection of the notion of action at a distance, made the acceptance of a plenum throughout space a necessity of thought—so, at any rate, it has seemed to most physicists of recent decades. The proof that all known forms of radiant energy move through space at the same rate of speed is regarded as practically a demonstration that but one plenum—one ether—is concerned in their transmission. It has, indeed, been tentatively suggested, by Professor J. Oliver Lodge, that there may be two ethers, representing the two opposite kinds of electricity, but even the author of this hypothesis would hardly claim for it a high degree of probability.

The most recent speculations regarding the properties of the ether have departed but little from the early ideas of Young and Fresnel. It is assumed on all sides that the ether is a continuous, incompressible body, possessing rigidity and elasticity. Lord Kelvin has even calculated the probable density of this ether, and its coefficient of rigidity. As might be supposed, it is all but infinitely tenuous as compared with any tangible solid, and its rigidity is but infinitesimal as compared with that of steel. In a word, it combines properties of tangible matter in a way not known in any tangible substance. Therefore we cannot possibly conceive its true condition correctly. The nearest approximation, according to Lord Kelvin, is furnished by a mould of transparent jelly. It is a crude, inaccurate analogy, of course, the density and resistance of jelly in particular being utterly different from those of the ether; but the quivers that run through the jelly when it is shaken, and the elastic tension under which it is placed when its mass is twisted about, furnish some analogy to the quivers and strains in the ether, which are held to constitute radiant energy, magnetism, and electricity.

The great physicists of the day being at one regarding the existence of this all−pervading ether, it would be a manifest presumption for any one standing without the pale to challenge so firmly rooted a belief. And, indeed, in any event, there seems little ground on which to base such a challenge. Yet it may not be altogether amiss to reflect that the physicist of to−day is no more certain of his ether than was his predecessor of the eighteenth century of the existence of certain alleged substances which he called phlogiston, caloric, corpuscles of light, and magnetic and electric fluids. It would be but the repetition of history should it chance that before the close of another century the ether should have taken its place along with these discarded creations of the scientific imagination of earlier generations. The philosopher of to−day feels very sure that an ether exists; but when he says there is "no doubt" of its existence he speaks incautiously, and steps beyond the bounds of demonstration. He does not KNOW that action cannot take place at a distance; he does not KNOW that empty space itself may not perform the functions which he ascribes to his space−filling ether.

Meantime, however, the ether, be it substance or be it only dream−stuff, is serving an admirable purpose in furnishing a fulcrum for modern physics. Not alone to the student of energy has it proved invaluable, but to the student of matter itself as well. Out of its hypothetical mistiness has been reared the most tenable theory of the constitution of ponderable matter which has yet been suggested—or, at any rate, the one that will stand as the definitive nineteenth−century guess at this "riddle of the ages." I mean, of course, the vortex theory of atoms—that profound and fascinating doctrine which suggests that matter, in all its multiform phases, is neither more nor less than ether in motion.

The author of this wonderful conception is Lord Kelvin. The idea was born in his mind of a happy union of mathematical calculations with concrete experiments. The mathematical calculations were largely the work of Hermann von Helmholtz, who, about the year 1858, had undertaken to solve some unique problems in vortex motions. Helmholtz found that a vortex whirl, once established in a frictionless medium, must go on, theoretically, unchanged forever. In a limited medium such a whirl may be V−shaped, with its ends at the surface of the medium. We may imitate such a vortex by drawing the bowl of a spoon quickly through a cup of water. But in a limitless medium the vortex whirl must always be a closed ring, which may take the simple form of a hoop or circle, or which may be indefinitely contorted, looped, or, so to speak, knotted. Whether simple or contorted, this endless chain of whirling matter (the particles revolving about the axis of the loop as the particles of a string revolve when the string is rolled between the fingers) must, in a frictionless medium, retain its form and whirl on with undiminished speed forever.

While these theoretical calculations of Helmholtz were fresh in his mind, Lord Kelvin (then Sir William Thomson) was shown by Professor P. G. Tait, of Edinburgh, an apparatus constructed for the purpose of creating vortex rings in air. The apparatus, which any one may duplicate, consisted simply of a box with a hole bored in one side, and a piece of canvas stretched across the opposite side in lieu of boards. Fumes of chloride of ammonia are generated within the box, merely to render the air visible. By tapping with the band on the canvas side of the box, vortex rings of the clouded air are driven out, precisely similar in appearance to those smoke−rings which some expert tobacco− smokers can produce by tapping on their cheeks, or to those larger ones which we sometimes see blown out from the funnel of a locomotive.

The advantage of Professor Tait's apparatus is its manageableness and the certainty with which the desired result can be produced. Before Lord Kelvin's interested observation it threw out rings of various sizes, which moved straight across the room at varying rates of speed, according to the initial impulse, and which behaved very strangely when coming in contact with one another. If, for example, a rapidly moving ring overtook another moving in the same path, the one in advance seemed to pause, and to spread out its periphery like an elastic band, while the pursuer seemed to contract, till it actually slid through the orifice of the other, after which each ring resumed its original size, and continued its course as if nothing had happened. When, on the other hand, two rings moving in slightly different directions came near each other, they seemed to have an attraction for each other; yet if they impinged, they bounded away, quivering like elastic solids. If an effort were made to grasp or to cut one of these rings, the subtle thing shrank from the contact, and slipped away as if it were alive.

And all the while the body which thus conducted itself consisted simply of a whirl in the air, made visible, but not otherwise influenced, by smoky fumes. Presently the friction of the surrounding air wore the ring away, and it faded into the general atmosphere— often, however, not until it had persisted for many seconds, and passed clear across a large room. Clearly, if there were no friction, the ring's inertia must make it a permanent structure. Only the frictionless medium was lacking to fulfil all the conditions of Helmholtz's indestructible vortices. And at once

Lord Kelvin bethought him of the frictionless medium which physicists had now begun to accept—the all−pervading ether. What if vortex rings were started in this ether, must they not have the properties which the vortex rings in air had exhibited—inertia, attraction, elasticity? And are not these the properties of ordinary tangible matter? Is it not probable, then, that what we call matter consists merely of aggregations of infinitesimal vortex rings in the ether?

Thus the vortex theory of atoms took form in Lord Kelvin's mind, and its expression gave the world what many philosophers of our time regard as the most plausible conception of the constitution of matter hitherto formulated. It is only a theory, to be sure; its author would be the last person to claim finality for it. "It is only a dream," Lord Kelvin said to me, in referring to it not long ago. But it has a basis in mathematical calculation and in analogical experiment such as no other theory of matter can lay claim to, and it has a unifying or monistic tendency that makes it, for the philosophical mind, little less than fascinating. True or false, it is the definitive theory of matter of the twentieth century.

Quite aside from the question of the exact constitution of the ultimate particles of matter, questions as to the distribution of such particles, their mutual relations, properties, and actions, came in for a full share of attention during the nineteenth century, though the foundations for the modern speculations were furnished in a previous epoch. The most popular eighteenth− century speculation as to the ultimate constitution of matter was that of the learned Italian priest, Roger Joseph Boscovich, published in 1758, in his Theoria Philosophiae Naturalis. "In this theory," according to an early commentator, "the whole mass of which the bodies of the universe are composed is supposed to consist of an exceedingly great yet finite number of simple, indivisible, inextended atoms. These atoms are endued by the Creator with REPULSIVE and ATTRACTIVE forces, which vary according to the distance. At very small distances the particles of matter repel each other; and this repulsive force increases beyond all limits as the distances are diminished, and will consequently forever prevent actual contact. When the particles of matter are removed to sensible distances, the repulsive is exchanged for an attractive force, which decreases in inverse ratio with the squares of the distances, and extends beyond the spheres of the most remote comets."

This conception of the atom as a mere centre of force was hardly such as could satisfy any mind other than the metaphysical. No one made a conspicuous attempt to improve upon the idea, however, till just at the close of the century, when Humphry Davy was led, in the course of his studies of heat, to speculate as to the changes that occur in the intimate substance of matter under altered conditions of temperature. Davy, as we have seen, regarded heat as a manifestation of motion among the particles of matter. As all bodies with which we come in contact have some temperature, Davy inferred that the intimate particles of every substance must be perpetually in a state of vibration. Such vibrations, he believed, produced the "repulsive force" which (in common with Boscovich) he admitted as holding the particles of matter at a distance from one another. To heat a substance means merely to increase the rate of vibration of its particles; thus also, plainly, increasing the repulsive forces and expanding the bulk of the mass as a whole. If the degree of heat applied be sufficient, the repulsive force may become strong enough quite to overcome the attractive force, and the particles will separate and tend to fly away from one another, the solid then becoming a gas.

Not much attention was paid to these very suggestive ideas of Davy, because they were founded on the idea that heat is merely a motion, which the scientific world then repudiated; but half a century later, when the new theories of energy had made their way, there came a revival of practically the same ideas of the particles of matter (molecules they were now called) which Davy had advocated. Then it was that Clausius in Germany and Clerk−Maxwell in England took up the investigation of what came to be known as the kinetic theory of gases—the now familiar conception that all the phenomena of gases are due to the helter− skelter flight of the showers of widely separated molecules of which they are composed. The specific idea that the pressure or "spring" of gases is due to such molecular impacts was due to Daniel Bournelli, who advanced it early in the eighteenth century. The idea, then little noticed, had been revived about a century later by William Herapath, and again with some success by J. J. Waterston, of Bombay, about 1846; but it gained no distinct footing until taken in hand by Clausius in 1857 and by Clerk−Maxwell in 1859.

The considerations that led Clerk−Maxwell to take up the computations may be stated in his own words, as formulated in a paper "On the Motions and Collisions of Perfectly Elastic Spheres."

"So many of the properties of matter, especially when in the gaseous form," he says, "can be deduced from the hypothesis that their minute parts are in rapid motion, the velocity increasing with the temperature, that the precise nature of this motion becomes a subject of rational curiosity. Daniel Bournelli, Herapath, Joule, Kronig, Clausius, etc., have shown that the relations between pressure, temperature, and density in a perfect gas can be explained by supposing the particles to move with uniform velocities in straight lines, striking against the sides of the containing vessel and thus producing pressure. It is not necessary to suppose each particle to travel to any great distance in the same straight line; for the effect in producing pressure will be the same if the particles strike against each other; so that the straight line described may be very short. M. Clausius has determined the mean length of path in terms of the average of the particles, and the distance between the centres of two particles when the collision takes place. We have at present no means of ascertaining either of these distances; but certain phenomena, such as the internal friction of gases, the conduction of heat through a gas, and the diffusion of one gas through another, seem to indicate the possibility of determining accurately the mean length of path which a particle describes between two successive collisions. In order to lay the foundation of such investigations on strict mechanical principles, I shall demonstrate the laws of motion of an indefinite number of small, hard, and perfectly elastic spheres acting on one another only during impact. If the properties of such a system of bodies are found to correspond to those of gases, an important physical analogy will be established, which may lead to more accurate knowledge of the properties of matter. If experiments on gases are inconsistent with the hypothesis of these propositions, then our theory, though consistent with itself, is proved to be incapable of explaining the phenomena of gases. In either case it is necessary to follow out these consequences of the hypothesis.

"Instead of saying that the particles are hard, spherical, and elastic, we may, if we please, say the particles are centres of force, of which the action is insensible except at a certain very small distance, when it suddenly appears as a repulsive force of very great intensity. It is evident that either assumption will lead to the same results. For the sake of avoiding the repetition of a long phrase about these repulsive bodies, I shall proceed upon the assumption of perfectly elastic spherical bodies. If we suppose those aggregate molecules which move together to have a bounding surface which is not spherical, then the rotatory motion of the system will close up a certain proportion of the whole vis viva, as has been shown by Clausius, and in this way we may account for the value of the specific heat being greater than on the more simple hypothesis."[1]

The elaborate investigations of Clerk−Maxwell served not merely to substantiate the doctrine, but threw a flood of light upon the entire subject of molecular dynamics. Soon the physicists came to feel as certain of the existence of these showers of flying molecules making up a gas as if they could actually see and watch their individual actions. Through study of the viscosity of gases—that is to say, of the degree of frictional opposition they show to an object moving through them or to another current of gas—an idea was gained, with the aid of mathematics, of the rate of speed at which the particles of the gas are moving, and the number of collisions which each particle must experience in a given time, and of the length of the average free path traversed by the molecule between collisions, These measurements were confirmed by study of the rate of diffusion at which different gases mix together, and also by the rate of diffusion of heat through a gas, both these phenomena being chiefly due to the helter−skelter flight of the molecules.

It is sufficiently astonishing to be told that such measurements as these have been made at all, but the astonishment grows when one hears the results. It appears from Clerk−Maxwell's calculations that the mean free path, or distance traversed by the molecules between collisions in ordinary air, is about one−half−millionth of an inch; while the speed of the molecules is such that each one experiences about eight billions of collisions per second! It would be hard, perhaps, to cite an illustration showing the refinements of modern physics better than this; unless, indeed, one other result that followed directly from these calculations be considered such—the feat, namely, of measuring the size of the molecules themselves. Clausius was the first to point out how this might be done from a knowledge of the length of free path; and the calculations were made by Loschmidt in Germany and by Lord Kelvin in England, independently.

The work is purely mathematical, of course, but the results are regarded as unassailable; indeed, Lord Kelvin speaks of them as being absolutely demonstrative within certain limits of accuracy. This does not mean, however, that they show the exact dimensions of the molecule; it means an estimate of the limits of size within which the actual size of the molecule may lie. These limits, Lord Kelvin estimates, are about the one− ten−millionth of a centimetre for the maximum, and the one−one−hundred−millionth of a centimetre for the minimum. Such figures convey no particular meaning to our blunt senses, but Lord Kelvin has given a tangible illustration that aids the imagination to at least a vague comprehension of the unthinkable smallness of the molecule. He estimates that if a ball, say of water or glass, about "as large as a football, were to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion, the magnified structure would be more coarse−grained than a heap of shot, but probably less coarse−grained than a heap of footballs."

Several other methods have been employed to estimate the size of molecules. One of these is based upon the phenomena of contact electricity; another upon the wave−theory of light; and another upon capillary attraction, as shown in the tense film of a soap−bubble! No one of these methods gives results more definite than that due to the kinetic theory of gases, just outlined; but the important thing is that the results obtained by these different methods (all of them due to Lord Kelvin) agree with one another in fixing the dimensions of the molecule at somewhere about the limits already mentioned. We may feel very sure indeed, therefore, that the molecules of matter are not the unextended, formless points which Boscovich and his followers of the eighteenth century thought them. But all this, it must be borne in mind, refers to the molecule, not to the ultimate particle of matter, about which we shall have more to say in another connection. Curiously enough, we shall find that the latest theories as to the final term of the series are not so very far afield from the dreamings of the eighteenth−century philosophers; the electron of J. J. Thompson shows many points of resemblance to the formless centre of Boscovich.

Whatever the exact form of the molecule, its outline is subject to incessant variation; for nothing in molecular science is regarded as more firmly established than that the molecule, under all ordinary circumstances, is in a state of intense but variable vibration. The entire energy of a molecule of gas, for example, is not measured by its momentum, but by this plus its energy of vibration and rotation, due to the collisions already referred to. Clausius has even estimated the relative importance of these two quantities, showing that the translational motion of a molecule of gas accounts for only three−fifths of its kinetic energy. The total energy of the molecule (which we call "heat") includes also another factor—namely, potential energy, or energy of position, due to the work that has been done on expanding, in overcoming external pressure, and internal attraction between the molecules themselves. This potential energy (which will be recovered when the gas contracts) is the "latent heat" of Black, which so long puzzled the philosophers. It is latent in the same sense that the energy of a ball thrown into the air is latent at the moment when the ball poises at its greatest height before beginning to fall.

It thus appears that a variety of motions, real and potential, enter into the production of the condition we term heat. It is, however, chiefly the translational motion which is measurable as temperature; and this, too, which most obviously determines the physical state of the substance that the molecules collectively compose—whether, that is to say, it shall appear to our blunt perceptions as a gas, a liquid, or a solid. In the gaseous state, as we have seen, the translational motion of the molecules is relatively enormous, the molecules being widely separated. It does not follow, as we formerly supposed, that this is evidence of a repulsive power acting between the molecules. The physicists of to−day, headed by Lord Kelvin, decline to recognize any such power. They hold that the molecules of a gas fly in straight lines by virtue of their inertia, quite independently of one another, except at times of collision, from which they rebound by virtue of their elasticity; or on an approach to collision, in which latter case, coming within the range of mutual attraction, two molecules may circle about each other, as a comet circles about the sun, then rush apart again, as the comet rushes from the sun.

It is obvious that the length of the mean free path of the molecules of a gas may be increased indefinitely by decreasing the number of the molecules themselves in a circumscribed space. It has been shown by Professors Tait and Dewar that a vacuum may be produced artificially of such a degree of rarefaction that the mean free path of the remaining molecules is measurable in inches. The calculation is based on experiments made with the radiometer of Professor Crookes, an instrument which in itself is held to demonstrate the truth of the kinetic theory of gases. Such an attenuated gas as this is considered by Professor Crookes as constituting a fourth state of matter, which he terms ultra− gaseous.

If, on the other hand, a gas is subjected to pressure, its molecules are crowded closer together, and the length of their mean free path is thus lessened. Ultimately, the pressure being sufficient, the molecules are practically in continuous contact. Meantime the enormously increased number of collisions has set the molecules more and more actively vibrating, and the temperature of the gas has increased, as, indeed, necessarily results in accordance with the law of the conservation of energy. No amount of pressure, therefore, can suffice by itself to reduce the gas to a liquid state. It is believed that even at the centre of the sun, where the pressure is almost inconceivably great, all matter is to be regarded as really gaseous, though the molecules must be so packed together that the consistency is probably more like that of a solid.

If, however, coincidently with the application of pressure, opportunity be given for the excess of heat to be dissipated to a colder surrounding medium, the molecules, giving off their excess of energy, become relatively quiescent, and at a certain stage the gas becomes a liquid. The exact point at which this transformation occurs, however, differs enormously for different substances. In the case of water, for example, it is a temperature more than four hundred degrees above zero, centigrade; while for atmospheric air it is one hundred and ninety−four degrees centigrade below zero, or more than a hundred and fifty degrees below the point at which mercury freezes.

Be it high or low, the temperature above which any substance is always a gas, regardless of pressure, is called the critical temperature, or absolute boiling− point, of that substance. It does not follow, however, that below this point the substance is necessarily a liquid. This is a matter that will be determined by external conditions of pressure. Even far below the critical temperature the molecules have an enormous degree of activity, and tend to fly asunder, maintaining what appears to be a gaseous, but what technically is called a vaporous, condition—the distinction being that pressure alone suffices to reduce the vapor to the liquid state. Thus water may change from the gaseous to the liquid state at four hundred degrees above zero, but under conditions of ordinary atmospheric pressure it does not do so until the temperature is lowered three hundred degrees further. Below four hundred degrees, however, it is technically a vapor, not a gas; but the sole difference, it will be understood, is in the degree of molecular activity.

It thus appeared that the prevalence of water in a vaporous and liquid rather than in a "permanently" gaseous condition here on the globe is a mere incident of telluric evolution. Equally incidental is the fact that the air we breathe is "permanently" gaseous and not liquid or solid, as it might be were the earth's surface temperature to be lowered to a degree which, in the larger view, may be regarded as trifling. Between the atmospheric temperature in tropical and in arctic regions there is often a variation of more than one hundred degrees; were the temperature reduced another hundred, the point would be reached at which oxygen gas becomes a vapor, and under increased pressure would be a liquid. Thirty−seven degrees more would bring us to the critical temperature of nitrogen.

Nor is this a mere theoretical assumption; it is a determination of experimental science, quite independent of theory. The physicist in the laboratory has produced artificial conditions of temperature enabling him to change the state of the most persistent gases. Some fifty years since, when the kinetic theory was in its infancy, Faraday liquefied carbonic−acid gas, among others, and the experiments thus inaugurated have been extended by numerous more recent investigators, notably by Cailletet in Switzerland, by Pictet in France, and by Dr. Thomas. Andrews and Professor James Dewar in England. In the course of these experiments not only has air been liquefied, but hydrogen also, the most subtle of gases; and it has been made more and more apparent that gas and liquid are, as Andrews long ago asserted, "only distant stages of a long series of continuous physical changes." Of course, if the temperature be lowered still further, the liquid becomes a solid; and this change also has been effected in the case of some of the most "permanent" gases, including air.

The degree of cold—that is, of absence of heat— thus produced is enormous, relatively to anything of which we have experience in nature here at the earth now, yet the molecules of solidified air, for example, are not absolutely quiescent. In other words, they still have a temperature, though so very low. But it is clearly conceivable that a stage might be reached at which the molecules became absolutely quiescent, as regards either translational or vibratory motion. Such a heatless condition has been approached, but as yet not quite attained, in laboratory experiments. It is called the absolute zero of temperature, and is estimated to be equivalent to two hundred and seventy− three degrees Centigrade below the freezing−point of water, or ordinary zero.

A temperature (or absence of temperature) closely approximating this is believed to obtain in the ethereal ocean of interplanetary and interstellar space, which transmits, but is thought not to absorb, radiant energy. We here on the earth's surface are protected from exposure to this cold, which would deprive every organic thing of life almost instantaneously, solely by the thin blanket of atmosphere with which the globe is coated. It would seem as if this atmosphere, exposed to such a temperature at its surface, must there be incessantly liquefied, and thus fall back like rain to be dissolved into gas again while it still is many miles above the earth's surface. This may be the reason why its scurrying molecules have not long ago wandered off into space and left the world without protection.

But whether or not such liquefaction of the air now occurs in our outer atmosphere, there can be no question as to what must occur in its entire depth were we permanently shut off from the heating influence of the sun, as the astronomers threaten that we may be in a future age. Each molecule, not alone of the atmosphere, but of the entire earth's substance, is kept aquiver by the energy which it receives, or has received, directly or indirectly, from the sun. Left to itself, each molecule would wear out its energy and fritter it off into the space about it, ultimately running completely down, as surely as any human−made machine whose power is not from time to time restored. If, then, it shall come to pass in some future age that the sun's rays fail us, the temperature of the globe must gradually sink towards the absolute zero. That is to say, the molecules of gas which now fly about at such inconceivable speed must drop helpless to the earth; liquids must in turn become solids; and solids themselves, their molecular quivers utterly stilled, may perhaps take on properties the nature of which we cannot surmise.

Yet even then, according to the current hypothesis, the heatless molecule will still be a thing instinct with life. Its vortex whirl will still go on, uninfluenced by the dying−out of those subordinate quivers that produced the transitory effect which we call temperature. For those transitory thrills, though determining the physical state of matter as measured by our crude organs of sense, were no more than non−essential incidents; but the vortex whirl is the essence of matter itself. Some estimates as to the exact character of this intramolecular motion, together with recent theories as to the actual structure of the molecule, will claim our attention in a later volume. We shall also have occasion in another connection to make fuller inquiry as to the phenomena of low temperature.

APPENDIX

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