altogether in character from the vibrations of the air which belong to sound. Hence the ether is not at all like air, and almost the only other thing known about it is that it has not been proved to possess any viscosity, and that the extremely tenuous matter of which the tails of comets is composed does not suffer any noticeable resistance in passing through the space which it is presumed to fill.—*Journal of Gaslighting.*

THE PROGRESS OF SCIENTIFIC KNOWLEDGE.[1]

By Lord KELVIN.

NOT the least important of the scientific events of the year is the publication, in the original German and in an English translation by Prof. De Jones, of a collection of Hertz's papers describing the researches by which he was led up to the experimental demonstration of magnetic waves. For this work the Rumford medal of the Royal Society was delivered to Prof. Hertz three years ago by my predecessor, Sir George Stokes. To fully appreciate the book now given to the world, we must carry our minds back to the early days of the Royal Society, when Newton's ideas regarding the forces which he saw to be implied in Kepler's laws of the motions of the planets and of the moon were frequent subjects of discussion at its regular meetings, and at perhaps even more important non-official conferences among its Fellows.

In 1684 the senior secretary of the Royal Society, Dr. Halley, went to Cambridge to consult Mr. Newton on the subject of the production of the elliptic motion of the planets by a central force,[2] and on December 10 of that year he announced to the Royal Society that he "had seen Mr. Newton's book, 'De Motu Corporum.'" Some time later, Halley was requested to "remind Mr. Newton of his promise to enter an account of his discoveries in the register of the Society," with the result that the great work "Philosophiæ Naturalis Principia Mathematica" was dedicated to the Royal Society, was actually presented in manuscript, and was communicated at an ordinary meeting of the Society on April 28, 1686, by Dr. Vincent. In acknowledgment, it was ordered that "a letter of thanks be written to Mr. Newton, and that the printing of his book be referred to the consideration of the council; and that in the meantime the book be put into the hands of Mr. Halley, to make a report thereof to the council." On May 19 following, the Society resolved that "Mr. Newton's 'Philosophiæ Naturalis Principia Mathematica' be printed forthwith in quarto, in a fair letter; and that a letter be written to him to signify the Society's resolution, and to desire his opinion as to the volume, cuts, etc." An exceedingly interesting letter was accordingly written to Newton by Halley, dated London, May 22, 1686, which we find printed in full in Weld's "History of the Royal Society" (vol. i., pp. 308–309). But the council knew more than the Royal Society at large of its power to do what it wished to do. Biology was much to the front then, as now, and the publication of Willughby's book, "De Historia Piscium," had exhausted the Society's finances to such an extent that the salaries even of its officers were in arrears. Accordingly, at the council meeting of June 2, it was ordered that "Mr. Newton's book be printed, and that Mr. Halley undertake the business of looking after it, and printing it at his own charge, which he engaged to do."

It seems that at that time the office of treasurer must have been in abeyance; but with such a senior secretary as Dr. Halley there was no need for a treasurer.

Halley, having accepted copies of Willughby's book, which had been offered to him in lieu of payment of arrears of salary[3] due to him, cheerfully undertook the printing of the "Principia" at his own expense, and entered instantly on the duty of editing it with admirable zeal and energy, involving, as it did, expostulations, arguments, and entreaties to Newton not to cut out large parts of the work, which he wished to suppress[4] as being too slight and popular, and as being possibly liable to provoke questions of priority. It was well said by Rigaud, in his "Essay on the first publication of the Principia," that "under the circumstances it is hardly possible to form a sufficient estimate of the immense obligation which the world owes in this respect to Halley, without whose great zeal, able management, unwearied perseverance, scientific attainments, and disinterested generosity the 'Principia' might never have been published."[5] Those who know how much worse than "law's delays" are the troubles, cares and labor involved in bringing through the press a book on any scientific subject at the present day will admire Halley's success in getting the "Principia" published within about a year after the task was committed to him by the Royal Society two hundred years ago.

When Newton's theory of universal gravitation was thus made known to the world, Descartes' *Vortices*, an invention supposed to be a considerable improvement on the older invention of crystal cycles and epi-cycles from which it was evolved, was generally accepted, and seems to have been regarded as quite satisfactory by nearly all the philosophers of the day.

The idea that the sun pulls Jupiter, and Jupiter pulls back against the sun with equal force, and that the sun, earth, moon, and planets all act on one another with mutual attractions, seemed to violate the supposed philosophic principle that matter cannot act where it is not. Descartes' doctrine died hard among the mathematicians and philosophers of continental Europe; and for the first quarter of last century belief in universal gravitation was an insularity of our countrymen.

Voltaire, during a visit which he made to England in 1727, wrote: "A Frenchman who arrives in London finds a great alteration in philosophy as in other things. He left the world full; he finds it empty. At Paris you see the universe composed of vortices of subtile matter; at London we see nothing of the kind. With you it is the pressure of the moon which causes the tides of the sea; in England it is the sea which gravitates toward the moon. . . . You will observe also that the sun, which in France has nothing to do with the business, here comes in for a quarter of it. Among you Cartesians all is done by impulsion; with the Newtonians it is done by an attraction of which we know the cause no better."[1] Indeed, the Newtonian opinions had scarcely any disciples in France until Voltaire asserted their claims on his return from England in 1728. Till then, as he himself says, there were not twenty Newtonians out of England.[2]

In the second quarter of the century sentiment and opinion in France, Germany, Switzerland, and Italy experienced a great change. "The mathematical prize questions proposed by the French Academy naturally brought the two sets of opinions into conflict." A Cartesian memoir of John Bernoulli was the one which gained the prize in 1730. It not infrequently happened that the Academy, as if desirous to show its impartiality, divided the prize between Cartesians and Newtonians. Thus in 1734, the question being the cause of the inclination of the orbits of the planets, the prize was shared between John Bernoulli, whose memoir was founded on the system of vortices, and his son Daniel, who was a Newtonian. The last act of homage of this kind to the Cartesian system was performed in 1740, when the prize on the question of the tides was distributed between Daniel Bernoulli, Euler, Maclaurin, and Cavallieri; the last of whom had tried to amend and patch up the Cartesian hypothesis on this subject.[3]

On February 4, 1744, Daniel Bernoulli wrote as follows to Euler: "Uebrigens glaube ich, dass der Aether sowohi *gravis versus solem*, vis die Luft versus terram sey, und kann Ihnen night bergen, dass ich über diese Puncte ein völliger Newtonianer bin, vnd verwundere ich mich, dass sie den Principiis Cartesianis so lang adhäriren; es möchte wohl einige Passion vielleicht mit unterlaufen. Hat Gott können eine *animam*, deren Natur uns unbegreiflich ist, erchaffen, so hat er auch können eine attractionem universalem materiæ imprimiren, wen gleich solche attractio *supra captum* ist, da hingegen die Principia Cartesiana allzeit *contra captum* etwas involviren."

Here the writer, expressing wonder that Euler had so long adhered to the Cartesian principles, declares himself a thorough-going Newtonian, not merely in respect to gravitation *versus* vortices, but in believing that matter may have been created simply with the law of universal attraction without the aid of any gravific medium or mechanism. But in this he was more Newtonian than Newton himself.

Indeed, Newton was not a Newtonian, according to Daniel Bernoulli's idea of Newtonianism, for in his letter to Bentley of date February 25, 1792,[4] he wrote: "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it." Thus Newton in giving out his great law did not abandon the idea that matter cannot act where it is not. In respect, however, of merely philosophic thought, we must feel that Daniel Bernoulli was right; we can conceive the sun attracting Jupiter, and Jupiter attracting the sun, without any intermediate medium, if they are ordered to do so. But the question remains, Are they so ordered? Nevertheless, I believe all, or nearly all, his scientific contemporaries agreed with Daniel Bernoulli in answering this question affirmatively. Very soon after the middle of the eighteenth century Father Boscovich[5] gave his brilliant doctrine (if infinitely improbable theory) that elastic rigidity of solids, the elasticity of compressible liquids and gases, the attractions of chemical affinity and cohesion, the forces of electricity and magnetism—in short, all the properties of matter except heat, which he attributed to a sulphurous fermenting essence—are to be explained by mutual attractions and repulsions, varying solely with distances, between mathematical points endowed also, each of them, with inertia. Before the end of the eighteenth century the idea of action-at-a-distance through absolute vacuum had become so firmly established, and Boscovich's theory so unqualifiedly accepted as a reality, that the idea of gravitational force or electric force or magnetic force being propagated through and by a medium seemed as wild to the naturalists and mathematicians of one hundred years ago as action-at-a-distance had seemed to Newton and his contemporaries one hundred years earlier. But a retrogression from the eighteenth century school of science set in early in the nineteenth century.

Faraday, with his curved lines of electric force, and his dielectric efficiency of air and of liquid and solid insulators, resuscitated the idea of a medium through which, and not only through which but *by* which, forces of attraction or repulsion, seemingly acting at a distance, are transmitted. The long struggle of the first half of the eighteenth century was not merely on the question of a medium to serve for gravific mechanism, but on the correctness of the Newtonian law of gravitation as a matter of fact however explained. The corres onding controversy in the nineteenth century was very short, and it soon became obvious that Faraday's idea of the transmission of electric force by a medium not only did not violate Coulomb's law of relation between force and distance, but that, if real, it must give a thorough explanation of that law.[1] Nevertheless, after Faraday's discovery[2] of the different specific inductive capacities of different insulators, twenty years passed before it was generally accepted in continental Europe. But before his death, in 1867, he had succeeded in inspiring the rising generation of the scientific world with something approaching to faith that electric force is transmitted by a medium called ether, of which, as had been believed by the whole scientific world for forty years, light and radiant heat are transverse vibrations. Faraday himself did not rest with this theory for electricity alone. The very last time I saw him at work in the Royal Institution was in an underground cellar, which he had chosen for freedom from disturbance; and he was arranging experiments to test the time of propagation of magnetic force from an induction coil through a distance of many yards to a fine steel needle polished to reflect light; but no result came from those experiments. About the same time, or soon after, certainly not long before the end of his working time, he was engaged (I believe at the shot tower near Waterloo Bridge on the Surrey side) in efforts to discover relations between gravity and magnetism, which led also to no result.

Absolutely nothing has hitherto been done for gravity either by experiment or observation toward deciding between Newton and Bernoulli as to the question of its propagation through a medium, and up to the present time we have no light, even so much as to point a way for investigation, in that direction. But for electricity and magnetism, Faraday's anticipations and Clerk Maxwell's splendidly developed theory have been established on the sure basis of experiment by Hertz's work, of which his own most interesting account is this year presented to the world in the German and English volumes to which I have referred. It is interesting to know, as Hertz explains in his introduction, and is very important in respect to the experimental demonstration of magnetic waves to which he was led, that he began his electric researches in a problem happily put before him thirteen years ago by Prof. von Helmholtz, of which the object was to find by experiment some relation between electromagnetic forces and dielectric polarization of insulators, without in the first place any idea of discovering a progressive propagation of those forces through space.

It was by sheer perseverance in philosophical experimenting that Hertz was led to discover a finite velocity of propagation of electromagnetic action, and then to pass on to electromagnetic waves in air and their reflection, and to be able to say, as he says in a short reviewing sentence at the end of his eighth paper: "Certainly it is a fascinating idea that the processes in air which we have been investigating represent to us on a million fold larger scale the same processes which go on in the neighborhood of a Fresnel mirror, or between the glass plates used for exhibiting Newton's rings."

Prof. Oliver Lodge has done well in connection with Hertz's work to call attention[3] to old experiments, and ideas taken from them, by Joseph Henry, which came more nearly to an experimental demonstration of electromagnetic waves than anything that had been done previously. Indeed, Henry, after describing experiments showing powerful enough induction due to a single spark from the prime conductor of an electric machine to magnetize steel needles at a distance of thirty feet in a cellar beneath with two floors and ceilings intervening, says that he is "disposed to adopt the hypothesis of an electrical plenum," and concludes with a short reviewing sentence: "It may be further inferred that the diffusion of motion in this case is almost comparable with that of a spark from a flint and steel in the case of light."

Prof. Oliver Lodge himself did admirable work in investigations with reference to lightning rods,[4] coming very near to experimental demonstrations of electromagnetic waves: and he drew important lessons regarding "electrical surgings" in an insulated bar of metal "induced by Maxwell's and Heavisides' electromagnetic waves," and many other corresponding phenomena manifested both in ingenious and excellent experiments devised by himself and in natural effects of lightning.

Of electrical surgings or waves in a short insulated wire, and of interference between ordinary and reflected waves, and positive electricity appearing where negative might have been expected, we hear first, it seems, in Herr von Bezold's "Researches on the Electric Discharge" (1870), which Hertz gives as the third paper of his collection, with interesting and ample recognition of its importance in relation to his own work.

In connection with the practical development of magnetic waves, you will, I am sure, be pleased if I call your attention to two papers by Prof. G. F. Fitzgerald, which I heard myself at the meeting of the British Association at Southport in 1883. One of them is entitled "On a Method of Producing Electromagnetic Disturbances of comparatively Short Wave Lengths." The paper itself is not long, and I shall read it to you in full, from the "Report of the British Association," 1883: "This is by utilizing the alternating currents produced when an accumulator is discharged through a small resistance. It is possible to produce waves of as little as two meters wave length, or even less." This was a brilliant and useful suggestion. Hertz, not knowing of it, used the method; and, making as little as possible of the "accumulator," got waves of as little as 10 cm. wave length in many of his fundamental experiments. The title alone of Fitzgerald's other paper, "On the Energy Lost by Radiation from Alternating Currents," is in itself a valuable lesson in the electro-magnetic theory of light, or the undulatory theory of magnetic disturbance. It is interesting to compare it with the title of Hertz's eleventh paper, "Electric Radiation;" but I cannot refer to this paper without express-

[1] Abstract from the presidential address before the Royal Society, Nov. 30, 1893.

[2] Whewell's "History of the Inductive Sciences," vol. ii. p. 77.

[3] It is recorded in the Minutes of Council that the arrears of salary due to Hooke and Halley were resolved to be paid by copies of Willughby's work. Halley appears to have assented to this unusual proposition, but Hooke wisely "desired six months' time to consider of the acceptance of such payment."

The publication of the "Historia Piscium," in an edition of 500 copies, cost the Society £400. It is worthy of remark, as illustrative of the small sale which scientific books met with in England at this period, that a considerable time after the publication of Willughby's work, Halley was ordered by the Council to endeavor to effect a sale of several copies with a bookseller at Amsterdam, as appears in a letter from Halley requesting Boyle, then at Rotterdam, to do all in his power to give publicity to the book. When the Society resolved on Halley's undertaking to measure a degree of the earth, it was voted that "he be given £50 or fifty 'Books of Fishes.'" (Weld's "History of the Royal Society," vol. i. p. 310.)

[4] "The third [book] I now design to suppress. Philosophy is such an impertinently litigious lady that a man had as good be engaged in lawsuits as have to do with her. I found it so formerly, and I am now no sooner come near her again but she gives me warning. The first two books without the third will not so well bear the title of 'Philosophiæ Naturalis Principia Mathematica,' and therefore I have altered it to this, 'De Motu Corporum Libri duo'; but, upon second thoughts, I retain the former title. 'Twill help the sale of the book, which I ought not to diminish now 'tis yours." *Ibid.*, p. 311.)

[5] *Ibid.*, p. 310.

[1] Whewell's "History of th Inductive Sciences," vol. ii. pp. 202–203.

[2] *Ibid.*, vol. ii. p. 201.

[3] *Ibid.*, vol. ii. pp. 198, 199.

[4] "The Correspondence of Richard Bentley, B.D.," vol. i. p. 70.

[5] "Theoria Philosophiæ Naturalis redacta ad unicam legem virium in natura existentium auctore P. Rogerio Josepho Boscovich, Societatis Jesu," 1st edition, Vienna, 1758; 2d edition, amended and extended by the author, Venice, 1763.

[1] "Electrostatics and Magnetism," Sir W. Thomson, Arts. I. (1842) and II. (1845), particularly § 25 of Art. II.

[2] 1837, "Experimental Researches," 1161–1306.

[3] "Modern Views of Electricity," pp. 369–372.

[4] "Lightning Conductors and Lightning Guards," Oliver J. Lodge, F.R.S. Whittaker & Co.

ing the admiration and delight with which I see the words "rectilinear propagation," "polarization," 'reflection," "refraction,'' appearing in it as sub-titles.

During the fifty-six years which have passed since Faraday first offended physical mathematicians with his curved lines of force, many workers and many thinkers have helped to build up the nineteenth century school of *plenum*; one ether for light, heat, electricity, magnetism; and the German and English volumes containing Hertz's electrical papers, given to the world in the last decade of the century, will be a permanent monument of the splendid consummation now realized.

But, splendid as this consummation is, we must not fold our hands and think or say there are no more worlds to conquer for electrical science. We do know something now of magnetic waves. We know that they exist in nature, and that they are in perfect accord with Maxwell's beautiful theory. But this theory teaches us nothing of the actual motions of matter constituting a magnetic wave. Some definite motion of matter perpendicular to the lines of alternating magnetic force in the waves and to the direction of propagation of the action through space, there must be; and it seems almost satisfactory as a hypothesis to suppose that it is chiefly a motion of ether with a comparatively small but not inconsiderable loading by fringes of ponderable molecules carried with it. This makes Maxwell's "electric displacement" simply a to and fro motion of ether across the line of propagation, that is to say, precisely the vibrations in the undulatory theory of light according to Fresnel. But we have as yet absolutely no guidance toward any understanding or imagining of the relation between this simple and definite alternating motion, or any other motion or displacement of the ether, and the earliest known phenomena of electricity and magnetism—the electrification of matter, and the attractions and repulsions of electrified bodies; the permanent magnetism of lodestone or steel, and the attractions and repulsions due to it; and certainly we are quite as far from the clew to explaining by ether or otherwise, the enormously greater forces of attraction and repulsion now so well known after the modern discovery of electromagnetism.

Fifty years ago it became strongly impressed on my mind that the difference of quality between vitreous and resinous electricity, conventionally called positive and negative, essentially ignored as it is in the mathematical theories of electricity and magnetism with which I was then much occupied (and in the whole science of magnetic waves as we have it now), must be studied if we are to learn anything of the nature of electricity and its place among the properties of matter. This distinction, essential and fundamental as it is in frictional electricity, electro-chemistry, thermo-electricity, pyro-electricity of crystals and piezo-electricity of crystals, had been long observed in the old known beautiful appearances of electric glow and brushes and sparks from points and corners on the conductors of ordinary electric machines and in exhaustive receivers of air pumps with electricity passed through them. It was also known probably as many as fifty years ago, in the vast difference of behavior of the positive and negative electrodes of the electric arc lamp. Faraday gave great attention to it[1] in experiments and observations regarding electric sparks, glows, and brushes, and particularly in his "dark discharge" and "dark space" in the neighborhood of the negative electrode in partial vacuum. In [1523] of his twelfth series, he says: "The results connected with the different conditions of positive and negative discharge will have a far greater influence on the philosophy of electrical science than we at present imagine." His "dark discharge" ([1544–1554]) through space around or in front of the negative electrode was a first installment of modern knowledge in that splendid field of experimental research which, fifteen years later, and up to the present time, has been so fruitfully cultivated by many of the able scientific experimenters of all countries.

The Royal Society's Transactions and Proceedings of the last years contain, in the communications of Gassiot,[2] Andrews and Tait,[3] Cromwell Varley,[4] De la Rue and Muller,[5] Spottiswoode,[6] Moultan,[7] Plucker,[8] Crookes,[9] Grove,[10] Robinson,[11] Schuster,[12] J. J. Thomson,[13] and Fleming,[14] almost a complete history of the new province of electrical science which has grown up largely in virtue of the great modern improvements in practical methods for exhausting air from glass vessels, by which we now have "vacuum tubes" and bulbs containing less than 1/190,000 of the air which would be left in them by all that could be done in the way of exhausting (supposed to be down to 1 mm. of mercury) by the best air pump of fifty years ago. A large part of the fresh discoveries in this province have been made by the authors of these communications, and their references to the discoveries of other workers very nearly complete the history of all that has been done in the way of investigating the transmission of electricity through highly rarefied air and gases since the time of Faraday.

Varley's short paper of 1871, which, strange to say, has lain almost or quite unperceived in the Proceedings during the twenty-two years since its publication, contains an admirable first installment of discovery in a new field—the molecular torrent from the "negative pole," the control of its course by a magnet, its pressure against either end of a pivoted vane of mica according as it is directed by a magnet to one end or the other, the shadow produced by its interception by a mica screen. Quite independently of Varley, and not knowing what he had done, Crookes was led to the same primary discovery, not by accident, and not merely by experimental skill and acuteness of observation. He was led to it by carefully designed investigation, starting with an examination of the causes of irregularities which had troubled[1] him in his weighing of thallium; and, going on to trials for improving Cavendish's gravitational measurement, in the course of which he discovered that the seeming attraction by heat is only found in air of greater than 1/1,000[2] of ordinary density; and that there is repulsion increasing to a maximum when the density is decreased from 1/1,000 to 36/1,000,000, and thence diminishing toward zero as the rarefaction is farther extended to density 1/20,000,000. From this discovery Crookes came to his radiometer, first without and then with electrification, powerfully aided by Sir George Stokes.[3] As he went on he brought all his work more and more into touch with the kinetic theory of gases; so much so that when he discovered the molecular torrent he immediately gave it its true explanation—molecules of residual air, or gas, or vapor projected at great velocities[4] by electric repulsion from the negative electrode. This explanation has been repeatedly and strenuously attacked by many other able investigators, but Crookes has defended[5] it, and thoroughly established it by what I believe is irrefragable evidence of experiment. Skillful investigation perseveringly continued brought out more and more of wonderful and valuable results; the non-importance of the position of the positive electrode; the projection of the torrent *perpendicularly* from the surface of the negative electrode; its convergence to a focus and divergence thenceforward when the surface is slightly convex; the slight but perceptible repulsion between two parallel torrents due, according to Crookes, to negative electrification of their constituent molecules; the change of direction of the molecular torrent by a neighboring magnet; the tremendous heating effect of the torrent from a concave electrode when glass, metal, or any ponderable substance is placed in the focus; the phosphorescence produced on a plate coated with sensitive paint by a molecular torrent skirting along it; the brilliant colors—turquoise blue, emerald, orange, ruby red—with which gray colorless objects and clear colorless crystals glow on their struck faces when lying separately or piled up in a heap in the course of a molecular torrent; "electrical evaporation" of negatively electrified liquids and solids;[6] the seemingly red hot glow, but with no heat conducted inward from the surface, of cool, solid silver kept negatively electrified in a vacuum of 1/1,000,000 of an atmosphere, and thereby caused to rapidly evaporate. This last mentioned result is almost more surprising than the phosphorescent glow excited by molecular impacts in bodies not rendered perceptibly phosphorescent by light. Both phenomena will surely be found very telling in respect to the molecular constitution of matter and the origination of thermal radiation, whether visible as light or not. In the whole train of Crookes' investigations on the radiometer, the viscosity of gases at high exhaustions, and the electric phenomena of high vacuums, ether seems to have nothing to do except the humble function of showing to our eyes something of what the atoms and molecules are doing. The same confession of ignorance must be made with reference to the subject dealt with in the important researches of Schuster and J. J. Thomson on the passage of electricity through gases. Even in Thomson's beautiful experiments showing currents produced by circuital electromagnetic induction in complete poleless circuits, the presence of molecules of residual gas or vapor seems to be *the essential.* It seems certainly true that without the molecules there could be no current, and that without the molecules electricity has no meaning. But in obedience to logic I must withdraw one expression I have used. We must not imagine that "presence of molecules is *the* essential." It is certainly *an* essential. Ether also is certainly *an* essential, and certainly has more to do than merely to telegraph to our eyes to tell us of what the molecules and atoms are about. If a first step toward understanding the relations between ether and ponderable matter is to be made, it seems to me that the most hopeful foundation for it is knowledge derived from experiment on electricity in high vacuum; and if, as I believe is true, there is good reason for hoping to see this step made, we owe a debt of gratitude to the able and persevering workers of the last forty years who have given us the knowledge we have; and we may hope for more and more from some of themselves and from others encouraged by the fruitfulness of their labors to persevere in the work.

The president then presented the medals awarded by the society as follows: The Copley medal to Sir George Gabriel Stokes, Bart., F.R.S., for his researches and discoveries in physical science; a Royal medal to Prof. A. Schuster, F.R.S., for his spectroscopic inquiries, and his researches on disruptive discharge through gases and on terrestrial magnetism; a Royal medal to Prof. H. Marshall Ward, F.R.S., for his researches into the life history of fungi and schizomycetes; and the Davy medal to Prof. J. H. Van't Hoff and Dr. J. A. Le Bel, in recognition of their introduction of the theory of asymmetric carbon, and its use in explaining the constitution of optically active carbon compounds.

[1] "Experimental Researches," Series 12 and 13, January and February, 1838.

[2] Roy. Soc. Proc., vol. 10, 1860, pp. 36, 269, 274, 432.

[3] Roy. Soc. Proc., vol. 10, 1860, p. 274; Phil. Trans.

[4] Roy. Soc. Proc., vol. 19, 1871, p. 236.

[5] Roy. Soc. Proc., vol. 23, 1875, p. 356; vol. 26, 1877, p. 519; vol. 27, 1878, p. 374; vol. 29, 1879, p. 281; vol. 35, 1883, p. 292; vol. 36, 1884, pp. 151, 206; Phil. Trans., 1878, pp. 55, 155; 1880, p. 65; 1883, 477.

[6] Roy. Soc. Proc., vol. 23. 1875, pp. 356, 455; vol. 25, 1875, pp. 73, 547; vol. 26, 1877, pp. 90, 323; vol. 27, 1878, p. 60; vol. 29, 1879, p. 21; vol. 30, 1880, p. 302; vol. 32, 1881, pp. 385, 388; vol. 33, 1882, p. 423; Phil. Trans., 1878, pp. 163, 210; 1879, 165; 1880. p. 561.

[7] Roy. Soc. Proc, vol. 29, 1879, p. 21; vol. 30, 1880, p. 302; vol. 32, 1881, pp. 385, 388; vol. 33, 1882, p. 453; Phil. Trans., 1879, p. 165; 1880, p. 561.

[8] Roy. Soc. Proc., vol. 10, 1860, p. 256.

[9] Roy. Soc. Proc., vol. 28, 1879, pp. 347, 477; Phil. Trans., 1879, p. 641; 1880, p. 135; 1881, 387.

[10] Roy. Soc. Proc., vol. 28, 1878, p. 181.

[11] Roy. Soc. Proc., vol. 12, 1862, p. 202.

[12] Roy. Soc. Proc., vol. 37, 1884, pp. 78, 317; vol. 42, 1887, p. 371; vol. 47, 1890, pp. 300, 506.

[13] Roy. Soc Proc., vol. 42, 1887, p. 343; vol. 49, 1891, p. 84.

[14] Roy. Soc. Proc., vol. 47, 1890, p. 118.

THE corner stone of an engineering college for the University of Illinois, to cost $160,000, was laid at Champaign on December 13. Prof. Thurston, of Cornell University, delivered the principal address.

[1] Tribulation, not undisturbed progress, gives life and soul, and leads to success when success can be reached, in the struggle for natural knowledge.

[2] Crookes, "On the Viscosity of Gases at High Exhaustion," § 655, Phil. Trans., February, 1881, p. 403.

[3] Phil. Trans. vol. 172 (1881), pp. 387, 435.

[4] Probably, I believe, not greater in any case than two or three kilometers per second.

[5] Address to the Institute of Telegraphic Engineers, 189.

[6] Roy. Soc. Proc., June 11, 1891.

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