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The New Physics.

SIR JOSEPH THOMSON is being entertained to dinner at Cambridge to-day on the attainment of his seventieth birthday. The event is one in which all who are concerned with scientific progress would desire to be associated in congratulatory expression, though only a limited number of friends are participating in the actual celebration. We are therefore devoting the main part of this week's issue of NATURE, as well as the whole of the special Supplement, to articles on the Cavendish Laboratory and its directors, with particular reference to Sir Joseph Thomson's work and influence. It would of course have been easy to obtain hundreds of appreciative messages for publication on this occasion, but in inviting such expressions of congratulation, we limited ourselves to some distinguished men of science abroad who have carried on research in the Cavendish Laboratory or have devoted themselves particularly to investigations along lines which started there. We are proud of the opportunity of publishing the tributes of affection and esteem received in response to our invitation, and we are sure that they represent the feelings of the whole scientific world.

When, in 1879, the late Sir William Crookes showed his beautiful experiments on the properties of what he called radiant matter, he could scarcely have foreseen the immense importance which such experiments were destined to assume before the close of the century. Nor could he then have suspected that the Cavendish Laboratory, inaugurated in 1874, would become the focus of the world's researches on pure electricity.

The influence of Cambridge upon electrical science since the foundation of the Cavendish Laboratory and the chair of experimental physics, has been unexampled and unrivalled. Glasgow through Kelvin, Leyden through Lorentz, Liverpool through Lodge, Manchester through Moseley, and Berlin through Helmholtz, Planck, and Einstein, have made great contributions to electrical fact and theory, but the galaxy of talent which has followed in the footsteps of Maxwell on the banks of the Cam must surely be generally acknowledged as the finest team yet engaged in the elucidation of a special branch of physics.

The importance of the work of Clerk Maxwell lay in the unification of two disciplines—those of light and of electricity respectively. It passed through several phases, in which the ultimate analysis of the phenomena then known was based upon more or less mechanical models with an underlying conception of essential continuity. Such continuity was a legacy of Faraday's recognition of the importance of the medium across which electric and magnetic forces are transmitted.

It lent a new significance to the conception of a luminiferous ether, and although the idea of a grainless continuum must always have presented certain philosophical difficulties, there is no doubt that it had a considerable heuristic value both in optics and in electromagnetism.

It does not detract from the great tradition of the Cavendish Laboratory that Sir J. J. Thomson's most noteworthy achievements have pointed in the opposite direction. His discovery of the electron in 1897 brought about a fundamental change in the ultimate conceptions of physical science. How radical was that change can only be realised by those whose electrical training took place in the 'eighties. Their preoccupation then was with the vectors and stresses in the electromagnetic field, with mechanical and hydrodynamical analogies, with sources and sinks, with the slip of lines of force on a conductor. Instead of all this, the new science of physics gives us electrons and positive nuclei, ionisation in gases, in liquids, and in solids, the emission of 'naked electricity' from hot wires, the collapse of electronic orbits, and the discrete structure of energy. Could a revolution be more complete; and the revolutionaries are the successors and disciples of the man who conceived the continuous electromagnetic ether and identified it with the all-pervading vehicle of light!

Nor can any one deny that this change of front has been exceptionally fruitful. The arch-revolutionary himself, who is now the revered Master of Trinity, devised the most powerful method of chemical analysis yet known, that positive-ray method which in the hands of Aston has brought about a new era in chemistry and shed a flood of light on the constitution of the atom and the true meaning of atomic weight. It has revealed the underlying unity of chemical substances, and has justified the bold guesses of Prout and Crookes, both of whom contended that all substances are derived from the same fundamental substance.

The Cavendish Laboratory, though one of the oldest of physical laboratories, is by no means the best equipped. Many of the new universities can boast of a better provision of expensive and up-to-date apparatus, and a few of them are far better designed. The more powerful electrical corporations, both in Europe and America, have magnificently equipped research laboratories. But tradition counts for much, even in pure science, and the habitual cultivation of science for its own sake imparts a breadth of outlook and of enterprise which is foreign to institutions working under a perpetual utilitarian effort.

We may confidently expect that Cambridge, under the guidance of Sir Ernest Rutherford, will regard the complete elucidation of atomic structure as its main function; and this elucidation may produce some very unexpected results. For no one can say that physical conceptions have settled down into a groove, as they seem to do after some revolutions. Quite the contrary. For the science is actually in a state of acute crisis. Never has its instrumental power been greater, but each new method of research seems to lead us to new and formidable problems. Einstein's conceptions of the universe as a four-dimensional manifold of space and time have gained ground rapidly, especially since the corroboration of his astronomical predictions by photographs taken during total solar eclipses. They have gained ground in spite of the greatest reluctance of the older physicists to give up their Newtonian principles. They have won the day in spite of several serious and fundamental modifications introduced by Einstein himself in his original arguments. These modifications have not cancelled his main thesis, and time is no longer independent of space, or space independent of time. Gravitation is a form of inertia, and produces a sort of kink in space impossible to conceive and impossible to formulate in terms of ordinary geometry. No wonder there are still some physicists of the old school who think that their cherished science has gone awry, and have given up all theorising in the expectation of better days.

Another powerful German challenge to accepted views came from Max Planck, whose conception of 'quanta' of energy has been more readily accepted than relativity, solely because we happen to live in an era which looks for discontinuous structure everywhere. If the value of a physical hypothesis is to be measured by the extent of the field covered by it and by its success in connecting phenomena previously isolated, then the quantum hypothesis must be accorded a very high rank indeed. For it provides a link between such hitherto unrelated things as specific heat, atomic volume, photoelectricity, series spectra, and the decay of radioactive substances. Even if it were not true, it would deserve to be so, and it might be adjudged *ben trovato* on account of its quickening effect upon research. Much remains to be done, however, before the new views have been linked up with the old. A multitude of questions remain to be solved. If all energy is transferred in discrete quanta, what is the nature of such a quantum? What is its form or structure while it is on its way from one material system to another? Is it a wave-train? And if so, what is it that undulates?

The physical theories prevalent in 1870 would have unhesitatingly ascribed the undulation to a fixed ether resembling in many respects a stiff jelly, but otherwise permeable to ordinary matter. Twenty years afterwards, an incompressible continuum having many analogies to water would have occurred to most physicists when asked to provide (as the late Lord Salisbury asked them in his presidential address to the British Association at Oxford in 1894) a nominative case to the verb 'to undulate.' Einstein's 'gesture of despair' in renouncing all hope of ever discovering a phenomenon which would exhibit the ether drift demanded by many ingenious experimental arrangements led to a widespread abandonment of all ether hypotheses, on the plea that a substance the existence of which could not be proved might as well not exist at all.

The generalised theory of relativity more recently formulated by Einstein does, in effect, re-introduce the ether as the bearer of gravitational and electromagnetic energies, but it is an ether which remains inaccessible to any analysis based upon the classical mechanics. The same defect—if it is a defect—attaches to Niels Bohr's beautiful model of the hydrogen atom consisting of a positive nucleus with its valency electron in one of several possible orbits. The change from one orbit to the next is attended by the emission or absorption of one energy quantum, the value of which is in exact proportion to the frequency of the radiation. This model has been completely successful in explaining the spacing of the spectrum lines of hydrogen, but no one can say why the model should be so constituted. Attempts have been made, notably by Schrödinger, to formulate a new system of mechanics, provisionally called 'undulatory mechanics,' which shall be founded upon atomic phenomena rather than upon those large-scale observations which since Galileo have provided the foundation for mechanical hypotheses.

All this, however, is yet in embryo. Practical physicists concern themselves less with theoretical foundations than with the applications of a ready-made hypothesis which appears to provide a valuable guidance through unexplored mazes; and so they try the new weapons furnished by the quantum theory on all sorts of things, the excitation of line and band spectra, catalysis, photochemistry, X-rays, heat radiation, absorption spectra, and countless other matters. Some of them even attempt to count the quanta emitted by a feeble radiator by allowing them to impinge upon a fine metallic point and observing the photoelectric flash produced. Such experiments, first initiated by Geiger, will no doubt be followed up soon. The human eye, gazing upon a sixth-magnitude star, receives several hundred quanta per second. It is just possible that its insensibility to anything below that brightness is the result of a long evolutionary adaptation. For the utility of vision must depend upon the constancy of the visual impression of a point fixed in space, and an eye provided with a much greater sensitiveness would defeat the object of vision by revealing discontinuities in the light itself, due entirely to its discontinuous structure.

There is little doubt that practice will rush ahead of theory. The conception of a magneton or elementary magnetic moment, first advanced by Weiss and Langevin, has been elaborated by Bohr on the basis of his hydrogen model, and may yet do for magnetism what the electron has done for electricity. But experimentalists appear to be content with the slenderest guidance by hypothesis. They are tackling the constitution of crystals, the refraction and dispersion of Röntgen rays, and the artificial disintegration of the atomic nucleus without pausing to ask themselves for a clear conception of what they are doing or whither their results will lead. If we may follow Jeans in his graphic exposition of conditions in the distant bowels of space, we may rest assured that the most astounding discoveries made in our laboratories will fall short of those hidden in the interiors of distant stars and nebulæ, where unknown elements heavier than uranium are generated from unknown power sources, where substances have densities amounting to tons per cubic inch, where matter dies in a burst of radiant energy, and is born again by a process as mysterious as time itself.

The younger generation of physicists may well be envied. So far from being confined in their discoveries to the 'third decimal place,' they have before them an untilled field in which the transition from the apparently unknowable to the knowable, and from the knowable to the known, is not only rapid, but is also undergoing a constant acceleration.

A Half-way House of Science.

Tierpsychologie: vom Standpunkte des Biologen. Von Prof. Dr. Friedrich Hempelmann. Pp. viii+676. (Leipzig: Akademische Verlagsgesellschaft m.b.H., 1926.) 36 gold marks.

THE rival camps of mechanists and vitalists have been struck, the contending parties have advanced to meet one another, holding out the 'right hand of fellowship,' and are jointly engaged in constructing a half-way house, a shelter for those who wander on the path between biology and physiology. Here, in this shelter, the teaching of the school of animal psychology and its new technique can be imbibed. Its fundamental study has commanded attention, since interest in the phenomena of living matter was first aroused, but its establishment as a science is of recent date, and is at present much handicapped by the incompleteness of the physiological record of structure and function. The *Sturm und Drang* period in the history of science is now passed, and a stage entered upon where the accumulation of knowledge by careful individual effort adds stone after stone to an ever-growing structure. The *Zeitgeist* seems to call for a survey of the work done on animal psychology, and with this object in view Dr. Hempelmann prepared his book. A glance at the exhaustive bibliographical section will show at once with what thoroughness and conscientiousness he has attacked the work.

The physiological aspect of the subject is necessarily of primary importance, and as an assemblage of such data alone the book would be valuable. Dr. Hempelmann points out that it is useless to compare the nervous systems of invertebrates and vertebrates structurally, for too many branches have been lopped off the evolutionary tree and lost. Functionally, it is possible to adopt the human brain as a criterion, and given that it functions as a centre for (1) the summation of stimuli, (2) associative thought and memory, (3) initiation of spontaneous behaviour, (4) maintenance of muscle tone, (5) reception of sensory stimuli, to trace these functions back to their earliest appearance, collectively or singly, in simpler animal forms.

The first section of the book is devoted to these considerations. The experimental work done on the various phyla of the animal kingdom is collected here and reviewed, and immediately, while dealing only with the Protozoa, the complexity of the problem becomes apparent. Even in the Amœba it is impossible to discriminate between 'cause and effect'; the reactions of the animal are indirect, *i.e.* external stimuli induce internal changes in the protoplasm, and these in their turn produce visible reactions. These internal protoplasmic changes are also variable owing to nutritive and respiratory mechanisms, yet continuous adjustment enables the habit and behaviour of the animal to remain constant, not only in one individual, but also throughout successive generations.

Increasing complexity of structure enables the ciliate and flagellate Protozoa to exhibit an increasing variety of reactions. There is no power of discriminating between stimuli, since they automatically exhibit the whole sequence of reactions, until a suitable one is arrived at, even after continued stimulation in the same way. This method of 'trial and error' (Lloyd Morgan) results in increased efficiency of reaction which, according to some workers, may indicate the glimmerings of very primitive memory, but is more generally interpreted as being due to greater power of automatic co-ordination.

Dr. Hempelmann urges the importance of applying Loeb's theory of tropisms only in its strictest sense. Response to stimuli, being explained as a chemical reaction, presupposes the organism to be either unicellular or else of very simple structure. Higher animals, with greater complexity of structure, cannot be said to exhibit tropisms in the true sense of the word.

Clearly, then, to the simplest animals, their external life must consist of those things in the environment which stimulate the senses. The reactions which this environment calls forth in the nervous system depend on the degree of complexity of structure. These reactions, together with the internal physiological processes which they invoke, constitute the inner life of the animal (V. Uexküll). With the development of a head, and the concentration of ganglionic masses in this region, as in worms, higher control of these reactions probably comes into existence, and with the appearance of an elaborate eye, as in the Cephalopoda, calculated movements can be made. The exact significance of perception cannot be gauged—as V. Uexküll remarks, it is impossible to determine whether the animal perceives an external world of light spaces and dots, or a cerebral world of water, land, etc.

The great specialisation of the body in the Insecta has led to a corresponding specialisation of the nervous system. All action is still instinctive, although great plasticity is displayed, which gives the appearance in some cases of controlled action. Recognition of objects, and hence memory, is shown by the homing powers and activities of bees, etc.

Associative thought, power of recognition, and memory are manifested by vertebrates to a varying degree, and above the Amphibia, purposeful action, *e.g.* as in the search for food, is met with. The warm-blooded birds and mammals appear more active and have a greater range of plastic instincts, which leads to a highly developed associative memory.

The effects of domestication are sometimes advantageous, sometimes disadvantageous to the animal. Instinctive reactions are part and parcel of the type of animal body. A greater or less power of co-ordination of sensory impressions with body movement constitutes the degree of education and stimulation. Certain reactions will be found to be an actual structural impossibility for some animals to perform, and these animals are probably judged as being 'less intelligent' than others more plastically designed. Dogs, if possessed of fore-limbs like a monkey, would probably prove more teachable, although the process might be a slower one. Apes lack application and are easily deflected from the business in hand, but they learn to do a thing suddenly, and after that generally perform it correctly. It is questionable whether this lack of application does not indicate a more marked individuality, and is, hence, a sign of greater mental development or of higher consciousness.

The question of higher consciousness is dealt with, in comparison with the psychology of man, in the second part of Hempelmann's book, and involves much repetition of what has gone before. This division of the subject into what might be termed the mechanistic and vitalistic aspects does not make for clearness or brevity, and the first special section would have been more appropriately included under the sub-headings of the second general section. In the present state of our knowledge we are not in a position to attempt to discriminate sharply between instinctive and controlled behaviour, as Dr. Hempelmann remarks. Yet from the arrangement of his book this is what he would appear to wish to do. As a result, the information is scattered, and the index does not help very materially in its co-ordination.

Such problems as the homing instinct, sense of numbers, 'thinking animals,' and so forth, are discussed. The explanation in the case of the first two probably lies in the recognition by the animal of the 'look of the thing,' and in its perception of some expression, conscious or sub-conscious, on the part of the master in the last case. Bühler's suggestion that a slight sense of time is developed in predacious animals, since they are able to judge the interval which elapses between disappearance and reappearance of their prey behind trees, etc., is an interesting one.

The subjective side of animal life can only be examined by analogy with that of man. Even the primitive sensation, pain, with its corresponding emotion, fear, cannot be accurately gauged. How much reaction is due to the stimulus, and how much is due to the inherent excitability of nervous tissue, it is impossible to say. The Arthropoda appear insensible to pain and continue their avocations though seriously maimed, and frogs exhibit ordinary 'pain' reflexes when the higher nerve centres have been destroyed. The sensation of pain is usually associated with the 'free' nerve endings in the skin of higher vertebrates, birds, and mammals. Ziegler denies its existence as a separate sensation, and there is difficulty in distinguishing between the touch and pressure centres, etc., in the skin. V. Uexküll, on the other hand, regards it as a biological necessity, having a definite place in the scheme of things, while Plate believes it is primitively developed in the Metazoa. Research shows that the physiological occurrences which accompany sensations and emotions in man occur similarly in animals. In man, these sensations are regarded as manifestations of higher consciousness, and though we must credit their similar occurrence in animals, we may be far from allowing the latter a similar possession of consciousness. Only the higher vertebrates show signs of this, and then only by analogy with man; yet it is impossible to say what processes are lacking to make its presence dubious.

Physiological occurrences, as at present understood, do not explain all the phenomena of living matter, and this realisation is at the root of vitalistic theories, which postulate some extra force or factor, entelechy, élan vital, cell intelligence, and so on. Since man is conscious of the possession of a directive will, he tends to see manifestations of the same force among the animals. As Dr. Hempelmann points out, this introduces the danger of anthropomorphism into the subject. In the words of the Chinese story: "'How delightfully the fishes are enjoying themselves!' exclaimed Tschuang-Tse, while walking on the bank of a river with Hui-Tse, who rejoined: 'You are not a fish; how do you know that the fishes are enjoying themselves?' 'You are not myself,' returned Tschuang-Tse, 'how do you know that I do not know that the fishes are enjoying themselves? I know because of my own pleasure in the water.'"

HELENE E. BARGMANN.

Alchemy and Mysticism.

The Secret Tradition in Alchemy: its Development and Records. By A. E. Waite. Pp. xxii+415. (London: Kegan Paul and Co., Ltd.; New York: Alfred A. Knopf, 1926.) 15*s.* net.

THERE are two varieties of alchemical literature, which may be roughly described as mystical and practical. The former employs the terms of alchemy in a figurative sense in the attempt to convey the ineffable, to picture the soul's progress along the Way, and to delineate the states of ecstasy which are common to the mystics of all nations and religions. The practical books, on the other hand, are exactly what they profess to be, namely, text-books of chemistry in which the principal theme is the transmutation of the metals. If this were all, there would be no particular difficulty in deciding to which of the two classes any given book should be assigned, and the task of the historian of chemistry—or of mysticism—would be correspondingly lighter. Unfortunately, however, the actual state of affairs is not so simple, for there is no sharp line of demarcation which divides the alchemical allegory from the text-book of chemical transmutation.

It is not difficult to understand how the confusion may have arisen, for there were two factors at work which would almost inevitably cause it. In the first place, the distinction which we now recognise between physics and metaphysics was not clearly defined in the middle ages, and Aristotle's postulation of a *Fifth Essence* afforded a convenient basis for occult speculations of all kinds. It is significant that the greatest of early chemists, Geber, was also a member of the mystical order of Sufis, then newly founded, and that mastery of alchemy was considered to be acquired by direct or indirect inspiration from divinity itself. Secondly, the belief that lead, tin, and the other base metals were diseased or imperfect forms of gold and silver which could be cured or perfected by elixirs soon led to an exaggerated idea of the supposed powers of the latter. Besides healing the metals, why should they not be able to cure man of his bodily infirmities, since man is composed of the same four elements combined in a not very dissimilar way? We do in fact find that, even so far back as Geber, the medicine of metals is also the medicine of man, which will cure him of all disease and restore him to that state of equilibrium which is health. From this stage to the final conclusion that there must be a spiritual alchemy which would do for the soul of man what the most noble elixir did for his body, was but a short step.

The outcome of this intricate interweaving has been that chemists have tried to read experimental facts into allegorical descriptions, and have failed, and that students of mysticism have tried to read allegory in laboratory notebooks, and have succeeded. In other words, a school of thought has arisen which claims that all alchemical books, not merely a section of them, deal with spiritual alchemy, and that to interpret them in their literal sense is to go astray. It is very difficult to understand how such a position can be seriously maintained, but we should perhaps not be unduly surprised that it has been taken up when we remember the ‘Bacon-is-Shakespeare’ thesis and the occult interpretation of the Great Pyramid.

It is, therefore, with genuine pleasure that we welcome Mr. Waite's book, in which he has set himself the problem of ascertaining as conclusively as possible the grounds upon which the extravagant claims just described are based. No one could be more fitted for the task than Mr. Waite, whose knowledge of occult literature of all kinds has probably never been surpassed, and who will certainly not be accused of undue bias toward the literal interpretation of alchemy. “The transmutation of metals *per se*,” he says, “is no concern of mine; but it has been said that great secrets of the soul are hidden under veils of *Chemia*; that they are of a kind which called for concealment in those persecuting days when the literature came into being; and that even now—when things are proclaimed on the housetops which used to be whispered in crypts—it is impermissible to speak of them openly because they are liable to abuse.”

It is clearly of great importance that the question should be settled one way or the other, for if alchemical treatises are nothing more than rhapsodies of the mystic, the historian of chemistry is wasting his time when he investigates them. Hence, although the answer Mr. Waite gives may be guessed beforehand—since, according to the mystical school, chemistry must have sprung into being fully armed at some undetermined period—he has done good service in exposing the pretensions to a searching and ruthless criticism. He first examines the alchemical literature of the Greeks, Byzantines, Syrians, Arabs, and Persians, and shows that, obscure though the language and vocabulary may be, the subject matter is definitely concerned with physical, metallic transmutation or with such technical matters as dyeing, metal-working, and the like. Marvellous medicinal properties of elixirs are mentioned less frequently, but that alchemy was undoubtedly a branch of contemporary natural science is demonstrated with an unassailable logic. Medieval Latin alchemy, in the cold light of this analysis, yields the same results.

According to Mr. Waite, it is only with Heinrich Khunrath (1560–1605) that the mystical variety of alchemy definitely began. Khunrath, he says, “is concerned solely with an *itinerarium mentis in Deum*, and because he was an alchemist he used things seen, imagined, or reported in the process of the Stone to illustrate—as he understood them—the states and stages of the soul's ascent upward. To adopt popular terms, it follows that the *Amphitheatrum Sapientiae Aeternae* is a Book of Divine Alchemy, and it is of great importance, not only because the physical aspects of the Art dissolve continually and are intended to merge in the spiritual, but most especially because it is the very first of the kind.” The well-spring and fountain-head of spiritual alchemy was Jacob Boehme, who began to unfold his revelations in 1610, by which time physical alchemy was about to merge into chemistry proper. The later developments of the fashion set by Khunrath and Boehme need not detain us; no historian of chemistry has ever been misled by them, and their study belongs rather to occultism than to the history of science.

Mr. Waite has, in short, finally and irretrievably demolished the fantastic thesis set up by Mrs. Atwood and others, and has proved beyond refutation that early and medieval alchemy was almost entirely concerned with physics or physic. Our only criticism is that he has perhaps overstated his case in referring the origin of spiritual alchemy to so late a date as that of Khunrath, for the great mystic of Islam, Al-Ghazzali (died A.D. 1111), had already applied the terms of alchemy to the life and progress of the soul, though in such a way as to cause no confusion to any one except him who set out to be confused.

E. J. HOLMYARD.

John Barleycorn.

The Barley Crop: a Record of some Recent Investigations. By Dr. Herbert Hunter. Pp. viii + 166. (London: Ernest Benn, Ltd., 1926.) 10*s.* 6*d.* net.

THIS little book serves to illustrate a thesis which frequently finds expression in this journal—that science embraces more than the mere ascertainment and enunciation of the laws of Nature: it connotes a logical system universal in its application. When in 1900 the Irish Department of Agriculture, in association with a leading firm of brewers, set out to solve the problem of what is the best barley to grow in Ireland, they attacked it scientifically. They cleared the ground of so-called practical opinions depending, as they do so often, on irrational ideas and traditions, and from the outset employed the tools upon which, as Kelvin once declared, all accurate scientific work must be founded, namely, weighing and measurement.

After upwards of twenty years of careful work, a variety of barley was found which can be proved to be best, and, as a matter of fact, it now occupies 90 per cent of the total area under the crop in that country. It is a further tribute to the value of scientific work that the variety of barley in question was ultimately made 'according to plan' by the application of the discoveries of Mendel to the problem. The author of the work under notice, Dr. Hunter, to whom the credit of the final achievement is due, has an interesting story to tell, and it is a story with a lesson for the plant-breeder and administrator with a definite economic object in view; a lesson, moreover, learned and practised in other countries which (like Ireland in pre-War days) possessed a Government ready to adopt the teachings of the scientific worker.

The finding and making of a super-barley followed what is now a well-trodden path: first, the recognition that the age-old varieties of the farmer are populations of various genetic types; the isolation of pure lines by propagation from a single plant; and, finally, the crossing of two pure lines in order to effect an exchange of desired characters. The first ten years were spent in isolating a pure line of "Danish Archer" barley as the best of existing types, giving both yield and quality, but possessing a weak straw: the next five in extracting the stiff straw from "Spratt" barley, transferring it to "Archer" and building up a stock of seed, starting again, be it noted, from a single plant. In the end, as already noted, nearly 100 per cent. of the barley area in Ireland is now sown with "Spratt Archer." Note also that when the original plans were laid in 1900, Mendel had just been re-discovered, Johannsen's pure line had not been heard of: scientific methods were the only guide.

There was much the same story to tell in noticing Mr. A. Howard's book on "Crop-Production in India," and there also the plant-breeder's work has been exploited by the State. As Mr. Howard has said, the making of a new variety is futile, unless some means of introducing it to the farmer can be definitely organised. Nor is it less important, as Dr. Hunter points out, to provide for a continuous flow of pure seed ever after.

A few words of criticism: Dr. Hunter does not indicate what the desired quality of barley, other than low nitrogen content, may be, or the part played by diastase in the malting process. It is not quite clear, either, what effect various manurial treatments of the soil may have on quality. In a future edition it might be well to bring the statistics of barley acreage and yield in Ireland down to a later date than 1922. Finally, a problem is suggested by the graphs of nitrogen content and yield. May it not be the case that high nitrogen content of the seed promotes vigorous growth and that, consequently, a super-excellent malting sample (within the line) is not necessarily a good mother for the resulting plant? We seem to recollect some work of Dr. Beaven (of barley fame) which suggests this query.

We must compliment the author not only on a successful piece of work, but also on the interesting account he has written of its progress and results. It should prove of extreme interest to the enlightened farmer, the professional plant-breeder, and even to the ordinary men, who, through the ages, have hymned John Barleycorn and his products! A. B. B.

Labour Policy in Education.

From Nursery School to University: a Labour Policy. Report of the Education Advisory Committee of the Trades Union Congress and the Labour Party. Pp. 93. (London: Trades Union Congress and the Labour Party, n.d.) Paper, 6*d.*; cloth, 1*s.*

THE little book which has just appeared with a foreword by Mr. Ramsay MacDonald follows lines now well understood in the matter of labour zeal for public education. It is the strongest side of Socialist policy, and all those who have experience, either official or private, with educational work, will testify that members of the Labour Party are foremost everywhere in their efforts to promote the education of the people.

Those who feel compelled to take a somewhat critical attitude will fasten on three points in the manifesto before us which give occasion to pause, if not actually to oppose the steps proposed. The first, and to most people the most serious, difficulty is dealt with in the last section of the pamphlet. It lies, of course, in the expenditure involved. The detailed proposals would raise the educational budget by about fifty per cent., making the total equal to the expenditure on army, navy, and air forces combined—*i.e.* just over a hundred millions a year. No one can on principle complain of that, but the proposition will assume a rather different aspect to a Chancellor of the Exchequer who has to raise the money from a country clamouring for a reduction in taxation, and in face of the strongly organised bodies of opinion which demand the maintenance of the other types of expenditure.

The question is, in short, a matter of the balance of public opinion which every government has ultimately to follow. The same consideration occurs when the writers point out that we actually spend more both on tobacco and on drink (separately) than the total they propose for education. Here, of course, the spending is in our own hands. We do as a nation prefer to spend all that money on smoke and drink rather than on books or pictures or going to concerts, and, so long as we do so, it is still less likely that we shall be willing for the government to take the money away from us in order to spend it on some purpose which some members of it think better.

There is another consideration of rather wider bearing which arises from the memorandum. The policy suggested throughout is for the State to do all that is wanted out of public funds. It is urged that all secondary schools should be free, and every one should be encouraged, if not compelled, to go to the State preparatory schools.

This encouragement has, as a matter of fact, become practical compulsion in some other countries, notably in the more socialist parts of Germany, but it would be, we are convinced, both a grave mistake and very repugnant to the free spirit of England. The practical problem for wise statesmanship in England is to combine State encouragement and financial help to education with the freedom of choice and teaching which we have always followed, and with the private generosity which has done so much in the past and is by no means wanting in the present.

The pure educationist will raise one more question when he reads of the hosts of new teachers who will be needed to carry out the vast extension of secondary schools and the further limitation of the size of classes. Both excellent things, ardently to be desired, but, in view of the supreme importance of good teachers, will it not be wise as well as necessary to go a little slowly?

F. S. M.

Our Bookshelf.

The Outline of History: a Plain History of Life and Mankind. By H. G. Wells. New edition, fully revised. Parts 1-24. (London: Cassell and Co., Ltd., 1925–1926.) 1*s.* 3*d.* net each part.

Mr. Belloc objects to "The Outline of History." By H. G. Wells. (The Forum Series.) Pp. vii + 55. (London: Watts and Co., 1926.) 1*s.* net.

On the appearance of the first number we directed attention to the new edition of Mr. H. G. Wells's "Outline of History," the serial issue of which has recently been completed. The "Outline" has been almost entirely rewritten, brought up-to-date, and provided with a fresh set of illustrations—perhaps as remarkable a collection of photographs covering all sides of human evolution and history as has ever been gathered together within the covers of one book. Apart from matters of opinion, in which Mr. Wells is characteristically individual—it is emphatically Mr. Wells's outline of history—there is little even in matters of detail which requires criticism. To have mastered so vast a body of material, and to have kept abreast of current opinion on so many technical subjects, is in itself no small intellectual feat. To take an example only, he is prepared to assign the Taungs man a place in his evolutionary scheme, although this skull was discovered only while the book was in process of writing. On certain points in dealing with the bronze and iron ages, and on ethnological questions relating to the origin and migrations of races, the views adopted by Mr. Wells are open to argument, just as his views of great personalities, such as Alexander, Julius Cæsar, or Napoleon, invite or even provoke discussion. These, however, are little more than matters of detail in relation to the broad scheme of evolutionary history which Mr. Wells has set himself to expound.

To those, be they men of science or historians, who grasp the broader issues of biological and anthropological science, Mr. Wells may seem neither revolutionary nor iconoclast; but it is evident that the "Outline" has been a stumbling-block to a certain type of orthodoxy. Criticisms of Mr. Wells and his views by Mr. Hilaire Belloc have appeared in certain Roman Catholic newspapers *pari passu* with the fortnightly issue of the "Outline." Mr. Wells, having failed to secure adequate opportunity for reply in the periodicals in question, now replies in a little volume which he has issued himself. It is scarcely necessary to say that Mr. Wells's humorous, if caustic, pen makes short work of Mr. Belloc's criticisms as well as his views on the subjects of natural selection, evolution, and the ancestry of man.

Pitman's Building Educator. Edited by Richard Greenhalgh. Complete in 30 fortnightly parts. Part 1. Pp. ii + 56. (London: Sir Isaac Pitman and Sons, Ltd., 1926.) 1*s.* 3*d.* net each part.

The aim of this publication is to cover the whole range of work embraced not only under the term 'building,' but also architecture. Modern buildings require an extraordinary range of services for which an architect takes responsibility, and the task imposed in an endeavour to cover this field, even in a work which, it is presumed, will eventually comprise some 1600 pages, is no light one. Among the editor's forty contributors appear the names of a number of leading men in the architectural and engineering professions who have each taken a subject upon which progressive articles will appear in successive issues, and the editor points out that many ordinary text-books are incomplete and that certain administrative and commercial aspects of building work have been much neglected by writers in the past, which gaps it is proposed to fill.

The issues will deal with such diverse subjects as architectural design, builder's accounts, gas fitting and building law. Opinions may differ as to whether the presentation of so many subjects in articles of only two or three pages is the best method of education, inasmuch as these subjects are obviously not equally suitable for study at one particular age, but a selection of articles is suggested for the special perusal of the aspiring architect, surveyor, builder and engineer.

The work is well illustrated by photographs and cuts and well printed, but we think that the compilation which involves back references, such as an article on superintendence continued on page 31 from page 49, open to criticism. A full index is promised on the completion of the publication, which should reduce the labour of a student wishing to traverse the ground of a particular subject with the thirty parts before him.

The Evolution of the Horse. By Prof. F. B. Loomis. (The Amherst Books: Second Series.) Pp. xvi + 233 + 26 plates. (Boston, Mass.: Marshall Jones Co., 1926.) 3 dollars.

FOR the Mammalia the family of horses has long been the standard example of an evolutionary series. There is no doubt that of all such series it is the most completely known and that it throws much light on the manner, if not on the method, of evolutionary change in mammals. Properly to understand this change, and the theoretical arguments which arise therefrom, it is necessary that the available data should be presented in considerable detail. The majority of text-books fail in this respect from lack, no doubt, of sufficient space. A summary of what has happened in some millions of years (forty-five millions according to the present author), illustrated by perhaps a dozen examples of horses chosen from successive strata to form a gradated series, cannot give a true impression of the facts. There is, moreover, a danger of such series becoming stereotyped, and for the student in consequence to come to think that it represents the whole picture and so to remain ignorant not only of the wealth of material that has been collected together but also of the fact that the horses, so far from being a straight line of evolution, and no more, had, on the contrary, many lines adapted in different ways, some successful, others the reverse.

Prof. Loomis has contributed to our knowledge of the horses, not only by original work in the field, but also by the publication of a small book which will help in some measure to correct the mistaken views adverted to above. It should be useful in the hands of the student, and is not too technical for the layman who is interested in the subject. Within the limit of some two hundred pages the family is traced from its beginning in the Eocene period up to modern times. The twenty-six plates are good, but the text figures, while adequate, are perhaps not up to the usual standard of artistic excellence of American palæontological publications. Here and there the specialist may find a statement with which he is not in complete accord, but as a general account, Prof. Loomis's book can safely be recommended.

Tabulae Biologicae. Herausgegeben von C. Oppenheimer und L. Pincussen. Band 2: *Thermochemie, Physikalische Chemie der Fermente, Elektrizität und Elektrochemie, Strahlenlehre, Spezielle Biophysik, Sekrete.* Pp. viii + 567 + 25 Tafeln. (Berlin: W. Junk, 1925.) 55 gold marks.

THE "Tabulae Biologicae" gives in compact tabular form the data which have been accumulated by many authors in all branches of the biological sciences. In addition, information is given on certain aspects of related sciences, where knowledge of the latter is a necessary preliminary to the performance and evaluation of biological experiments: for example, in this, the second, volume, the electrical section includes data on the electrical properties of metals, dissociation constants, and the *p*H values of different solutions. Data are also given on X-rays, radioactivity, and the spectral energy of lights of different wave-lengths. The kinetics and conditions of activity of the different enzymes are very fully treated.

Among the other subjects dealt with, attention may be directed to the data on the special senses and the physiology of muscle and nerve: tables of the actions of drugs on the vegetative nervous system and plates of the physiological anatomy of the central nervous system are included. The composition of the following will also be found in this volume: blood, skin, the digestive secretions, lymph, cerebrospinal fluid and transudates, milk. We note a useful table of the structure of the salivary glands in different animals. Although possessing a strong Teutonic flavour, the volume may be thoroughly recommended as an extremely useful work of reference. A fairly full table of contents acts also as an index.

Die intraindividuelle fluktuierende Variabilität: eine Untersuchung über die Abänderung des Pflanzenindividuums und die Periodizität der Lebenserscheinungen. Von Prof. Dr. E. Dennert. (Botanische Abhandlungen, Heft 9.) Pp. 149. (Jena: Gustav Fischer, 1926.) 7 gold marks.

WHEREAS most of the statistical work on fluctuations (Darwin's individual variations) has related to their occurrence in a race or species, the author has focussed his attention upon fluctuations within the individual plant. His extensive data are derived from measurements of leaves and various floral parts, as well as by counting the numbers of ray-florets in two species of Aster. The conclusion is reached that extra-individual fluctuation is a summation of the fluctuations within the various individuals and that the two phenomena are identical. The graphs in all cases betray a definite rhythm, a rise to a maximum and a subsequent fall to a minimum, but the fluctuations show slight differences in character which can be reduced to three principal types. In the final section the author concludes that the ultimate causes of this fluctuation are inherent in the protoplasm, but that its manifestation, and to some extent also its special character, are dependent on external and internal factors.

Letters to the Editor.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, nor to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Conservation of Photons.

WHATEVER view is held regarding the nature of light, it must now be admitted that the process whereby an atom loses radiant energy, and another near or distant atom receives the same energy, is characterised by a remarkable abruptness and singleness. We are reminded of the process in which a molecule loses or gains a whole atom or a whole electron but never a fraction of one or the other. When the genius of Planck brought him to the first formulation of the quantum theory, a new kind of atomicity was suggested, and thus Einstein was led to the idea of light quanta which has proved so fertile. Indeed, we now have ample evidence that radiant energy (at least in the case of high frequencies) may be regarded as travelling in discrete units, each of which passes over a definite path in accordance with mechanical laws.

Had there not seemed to be insuperable objections, one might have been tempted to adopt the hypothesis that we are dealing here with a new type of atom, an identifiable entity, uncreatable and indestructible, which acts as the carrier of radiant energy and, after absorption, persists as an essential constituent of the absorbing atom until it is later sent out again bearing a new amount of energy. If I now advance this hypothesis of a new kind of atom, I do not claim that it can yet be proved, but only that a consideration of the several objections that might be adduced shows that there is not one of them that can not be overcome.

It would seem inappropriate to speak of one of these hypothetical entities as a particle of light, a corpuscle of light, a light quantum, or a light quant, if we are to assume that it spends only a minute fraction of its existence as a carrier of radiant energy, while the rest of the time it remains as an important structural element within the atom. It would also cause confusion to call it merely a quantum, for later it will be necessary to distinguish between the number of these entities present in an atom and the so-called quantum number. I therefore take the liberty of proposing for this hypothetical new atom, which is not light but plays an essential part in every process of radiation, the name *photon*.

Let us postulate for the photon the following properties: (1) In any isolated system the total number of photons is constant. (2) All radiant energy is carried by photons, the only difference between the radiation from a wireless station and from an X-ray tube being that the former emits a vastly greater number of photons, each carrying a very much smaller amount of energy. (3) All photons are intrinsically identical. As the molecules of hydrogen differ from one another in direction and energy of translation, and in direction and amount of rotation, so two photons, as seen by a single observer, differ in direction of motion, in energy, and in polarisation. If we were moving with rapid acceleration toward a wireless station, its photons would appear to possess increasing amounts of energy, and would pass over the whole spectral scale through the visible and into the ultraviolet. At a certain instant, for example, they would be indistinguishable from the photons emitted by excited sodium atoms. (4) The energy of an isolated photon, divided by the Planck constant, gives the frequency of the photon, which is therefore by definition strictly monochromatic; although two photons coming even from similar atoms would never have precisely the same frequency. (5) All photons are alike in one property which has the dimensions of action or of angular momentum, and is invariant to a relativity transformation. (6) The condition that the frequency of a photon emitted by a certain system be equal to some physical frequency existing within that system, is not in general fulfilled, but comes nearer to fulfilment the lower the frequency is.

The serious objections to the idea of the conservation of photons are met in a consideration of the thermodynamics of radiation and of the laws of spectroscopy. According to the classical thermodynamics of radiation, the energy of a *hohlraum* at a given temperature is determined solely by the volume. If we define the number of photons in a small spectral interval by the amount of energy in that interval divided by $h\nu$, then, by Wien's displacement law, the number of photons remains constant in any reversible adiabatic process. Also, in the irreversible adiabatic process of free expansion from a given volume to a larger volume (both with perfectly reflecting walls) the number of photons remains constant, for neither the energies nor the frequencies are changed. If the original radiation, corresponding to a definite temperature, freely expands, let us say, to sixteen times the first volume, then, according to the thermodynamics of Wien and Planck, it may be brought to a new temperature equilibrium by introducing an infinitesimal black body. Calculating from their equations, we find that in this process the number of photons is doubled. If this is so, there obviously can be no conservation law for photons. However, if we analyse carefully the thermodynamics of radiation, we find that Wien and Planck have tacitly employed a postulate which is supported by no experimental facts; namely, if an infinitesimal black body is introduced into a *hohlraum*, the radiation will come to a certain temperature, and then *no further change will ensue* when a large black body of the same temperature is introduced.

Dispensing with this postulate, and adding a new variable, the number of photons, to the variables which have previously been deemed sufficient to define the state of a system, we obtain a greatly enlarged science of thermodynamics. In this new thermodynamics, which includes as true and stable equilibria such states of equilibrium as those to which Einstein has applied the terms "aussergewöhnlich" and "improprement dit" (*Ann. Phys.*, 38, 881, 1912; *Jour. de Phys.*, 3, 277, 1913), the familiar laws of radiation and of physical and chemical equilibrium become special cases, true only for an unlimited supply of photons. Even so fundamental a process as the flow of heat must involve two factors, the amount of energy and the number of photons transferred. A fuller account of this new thermodynamics will shortly be published.

Turning to spectroscopy, we find that the principle of the conservation of photons is in obvious conflict with existing notions of the radiation process. We must assume that in an elementary process of radiation one, and only one, photon is lost by the emitting atom. Suppose that an atom which is in the 4-2 state drops to the 3-3, then to the 2-2, then to the 1-1. It thus loses three photons, but the same atom dropping directly from the 4-2 state to the 1-1 loses only one photon. If, therefore, we are to admit the conservation of photons, we must say that the atom does not pass from precisely the same initial to the same final state by the two paths, but rather that either the 4-2 or the

1-1 states must be multiple. Even if the inner quantum number is given, as well as the total and the azimuthal quantum numbers, the atomic states must still be regarded as not completely specified. Indeed, numerous examples have been found (see the review by Ruark and Chenault, *Phil. Mag.*, 50, 937, 1925) of a superfine structure which is not yet accounted for.

I had hoped to be able to derive certain familiar selection principles from the conservation of photons. Here I have not as yet succeeded, and can only state that if we assume the existence of a number of atomic states with nearly the same energy but with different numbers of photons, the new theory is not in conflict with the results of spectroscopy.

The rule that one, and only one, photon is lost in each elementary radiation process, is far more rigorous than any existing selection principle, and forbids the majority of processes which are now supposed to occur. To account for the apparent existence of these processes, it is necessary to assume that atoms are frequently changing their photon number by the exchange of photons of very small energy, corresponding to thermal radiation in the extreme infra-red. The new theory therefore predicts that many atomic processes will be inhibited at very low temperatures, and for this there seems to be some experimental evidence. But the existence of numerous extraneous factors obscures the issue. In order to simplify matters, a molecular stream might be passed through the centre of a tube cooled to a very low temperature, so as to reduce to a minimum the amount of thermal radiation. The theory would predict that in such circumstances certain processes within the stream, such as fluorescence or the emission of light from activated atoms, would be profoundly changed. Experiments in this direction are now in progress.

GILBERT N. LEWIS.

Berkeley, California,
October 29.

The Synthesis and Disintegration of Atoms as Revealed by the Photography of Wilson Cloud Tracks.

IN 1923 Harkins and Ryan developed a simple method (NATURE, 112, 54; *J. Am. Chem. Soc.*, 45, 2095) for obtaining a knowledge of what occurs when an atom disintegrates. This consisted of the use of very fast α-rays, such as those of thorium or radium-C, in a modified Wilson (Shimizu) ray track apparatus, and the photography of an extremely large number of tracks. By the use of these fast rays, about 20,000 photographs in air and 21,000 in argon were obtained. Each photograph gave two views at right angles. That Wilson's apparatus had not been applied earlier for this purpose was undoubtedly due to the fact that it had previously seemed almost hopeless to obtain sufficient photographs.

In 1925 Blackett (*Proc. Roy. Soc. A.*, 107, 349), by the use of the method applied earlier by Harkins and Ryan, obtained 22,000 photographs in nitrogen. In these, eight disintegrations of nitrogen atoms to give protons were obtained, and it was found that in rare instances a fast α-particle attaches itself to the nucleus of a nitrogen atom.

The purpose of this letter is to present the results obtained from 34,000 photographs not previously reported. These contained an average of about 14 α-particle tracks each, so that about 270,000 tracks of 8·6 cm. range were obtained.

These have given two cases in which an α-particle attached itself to a nitrogen nucleus. One of the protons was emitted in a forward direction, that is, with a component of velocity in the same direction as that of the α-particle, while in the other case the proton was emitted backward.

Fig. 1 is from a photograph which was presented to the National Academy of Sciences in April 1926.

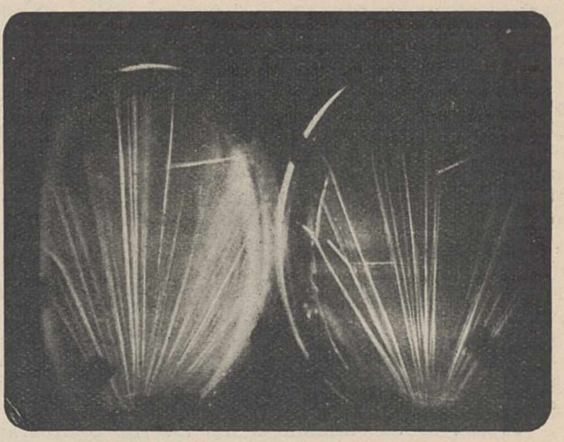

FIG. 1.—Simultaneous views at right angles of Wilson cloud tracks. In the original negative the track of the H+-particle has about one-tenth the intensity of that of the α-particle or of the oxygen of mass 17. The disintegration is shown at the left side of each of the two views, but is much more distinct in the right-hand view.

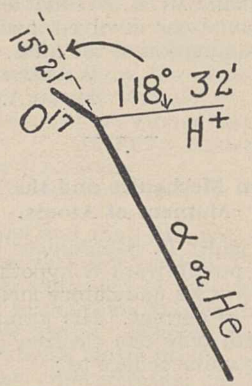

FIG. 2.—The space relations of the synthesis and disintegration exhibited in Fig. 1.

The values obtained in this case (Fig. 2) are θ, 118° 32′; ϕ, 15° 21′; remaining range of α-particle at time of collision, 6·3 cm.; velocity of α-particle at collision, $1{\cdot}86 \times 10^{9}$ cm. per sec.; initial velocity of proton, $2{\cdot}7 \times 10^{9}$ cm. per sec.; range of the proton, 19·6 cm., and velocity of heavy nucleus formed, $5{\cdot}3 \times 10^{8}$ cm. per second.

The kinetic energy after collision is 89 per cent. of that of the α-particle at time of impact, so 11 per cent. of the kinetic energy is stored up in the atom (presumably oxygen of mass 17) which is synthesised.

The hydrogen track of Fig. 1 has, on the original film, only about one-tenth the intensity of the tracks produced by the helium and the oxygen. That this is actually a hydrogen track is also indicated by the fact that the visible range in the original photograph is more than three times the range for an α-particle under the same conditions.

It may be noted that we have obtained only two atomic syntheses and disintegrations in the photography of 265,000 tracks, while with the same number of tracks Blackett obtained eight similar events, which, however, the Cambridge workers consider an abnormally high number. Since we have investigated carefully every bend in the α-ray tracks, it seems improbable that we could have overlooked more than two such events.

It is of interest that the smallest velocity of the α-particle which has thus far been sufficient to disintergrate a nitrogen atom in the work at Cambridge and in Chicago corresponds to a remaining range at 15° of about 6·2 cm. Now the range of an α-particle from polonium, which was used in practically all of the early work on α-ray tracks, is only 3·925 cm. It is evident that if polonium had been used as a source, not one of the ten disintegrations thus far found would have been obtained. It was because the early work on the scintillation method indicated such a result that Harkins and Ryan changed to a source which contained thorium C′, such as thorium C, or thorium B and C.

According to Petterson and Schmidt ("Atomzertrümmerung," p. 109) the α-rays from polonium liberate H-particles from aluminium, but the results cited above do not indicate this to be true in the case of nitrogen. However, it will be necessary to obtain more photographs, and to examine again those already obtained before a definite conclusion can be drawn.

A part of the measurements on the photograph (Fig. 1) were obtained by a new method, which corrects for errors in the lenses and mirrors. It is equivalent to the establishment in the ionisation chamber of a set of co-ordinates in space.

We wish to thank Mr. C. N. Shah for a considerable part of the calculations involved, and for much time spent in the examination of the films.

WILLIAM D. HARKINS.
HUGH A. SHADDUCK.

Quantum Mechanics and the Magnetic Moment of Atoms.

IN a recent paper E. Schrödinger (*Ann. d. Phys.*, **81**, 1926, 109) put forward a hypothesis connecting the field scalar ψ of his undulatory mechanics with the electric density of current. His formulæ in the case of a system with only one electron (expressing j in electromagnetic units) reduce to:

$$\rho = e\psi\bar{\psi}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$j = \frac{he}{4\pi imc}(\psi \text{ grad. } \bar{\psi} - \bar{\psi} \text{ grad. } \psi), \quad . \quad (2)$$

where ρ and j are respectively the electric density and density of current. The field scalar must be normalised in such a way that

$$\int \psi\bar{\psi}\, d\tau = 1, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$d\tau$ being the element of volume.

Evidence in favour of the expression for ρ is given by the fact that the calculation of the intensity of emission of the atom, based on it, is in accordance both with experiment and with the results of Heisenberg's quantum mechanics. Expression (2) for j is assumed by Schrödinger as the simplest way of satisfying the equation of continuity of electricity.

It is desirable, therefore, to find some further evidence for this expression. I propose to show that it leads to the right expression for the magnetic moment of a hydrogen-like atom. We will consider only the part of the magnetic moment which, in the old quantum theory, was supposed to be due to the orbital motion of the electron, not taking into account the magnetic moment of the spinning electron, which can be considered separately.

The wave equation for a hydrogen-like atom in a magnetic field (without considering the spinning electron) was integrated by Fock (*Zeit. f. Phys.*, **28**, 1926, 242). His expression for ψ can be written in the form

$$\psi = f(\theta, r)e^{in_1\phi}e^{2\pi i\frac{E}{h}t}, \quad . \quad . \quad . \quad (4)$$

where r, θ, ϕ are polar co-ordinates; $f(\theta, r)$ is a *real* function of r and θ; n_1 is an integer corresponding to the magnetic quantum number of the old theory.

It is easily seen from (2) and (4) that j is a vector perpendicular at each point to the plane passing through the point and the polar axis, the magnitude of which is

$$j = \frac{he}{4\pi imc}\frac{1}{R}\left(\psi\frac{\partial\bar{\psi}}{\partial\phi} - \bar{\psi}\frac{\partial\psi}{\partial\phi}\right) = -\frac{he}{2\pi mcR}n_1 f^2, \quad . \quad (5)$$

R being the distance of the point from the axis. The lines of current are circles situated in planes perpendicular to the axis and with their centres on the axis.

Let us now consider the current passing through an element $d\sigma$ of a meridian plane; its intensity is given by $jd\sigma$ and its circuit is a circle with radius R. The magnetic moment of this current is therefore $\pi R^2 jd\sigma$ (in the direction of the field).

The magnetic moment of the atom becomes, putting for j the expression (5),

$$\mu = \pi\int R^2 jd\sigma = -\frac{hen_1}{2mc}\int Rf^2 d\sigma. \quad . \quad . \quad (6)$$

The last integral can be valuated by substituting (4) in (3), and observing that $d\tau = 2\pi R d\sigma$; we find then $2\pi\int Rf^2 d\sigma = 1$. Putting this value into (6) we find at last

$$\mu = -\frac{he}{4\pi mc}n_1. \quad . \quad . \quad . \quad (7)$$

That is, the component of the magnetic moment in the direction of the field is the product of a Bohr magneton $\left(-\frac{he}{4\pi mc}\right)$ and the magnetic quantum number n_1, as it was expected.

It is noteworthy that the magnetic moment (7) arises in a certain way through the action of the field. In the absence of a magnetic field, the two states with n_1 equal in absolute value but of opposite signs, have the same energy; every linear combination of these two states is therefore a quantum state. Now it is easily seen from (4) that we can combine the two states in such a way that ψ becomes the product of $e^{2\pi i\frac{E}{h}t}$ and a real function of r, θ, ϕ (containing cos $n_1\phi$ or sin $n_1\phi$, as a factor). In this case the density of current j would vanish everywhere, and the magnetic moment would reduce to zero.

ENRICO FERMI.
Physical Institute of the University,
Rome, November 14.

Psychical Phenomena and their Interpretation.

PUISQUE une controverse courtoise a pris place dans ce journal au sujet des recherches psychiques, il me sera permis de vous adresser quelques mots au sujet de mon opinion, motivée par de longues recherches (voir mon "Traité de Métapsychique," traduit en anglais, 1925, "Thirty Years of Psychical Research").

Il importe avant tout de distinguer les faits et les théories.

Pour les faits, ils ont été, à ce qu'il me semble, établis en toute évidence, et on ne peut attribuer ni à la fraude, ni à l'erreur, ni à l'illusion, la constatation de la plupart des phénomènes. Encore, à l'extrême rigueur, peut-on soutenir que les faits d'ectoplasmie, de lumières, de télékinésie, demandent quelques confirmations nouvelles. Mais quant à la télépathie, la lucidité (second sight), la cryptesthésie, la prémonition, tous phénomènes n'impliquant pas d'action physique matérielle, il y a des témoignages si abondants et si précis qu'on ne peut se refuser à les admettre. A moins qu'on ne fasse fi de la méthode expérimentale. C'est se diminuer que de ne pas les reconnaître, suivant la forte parole d'Oliver Lodge.

L'expérience est là pour établir, en toute certitude, qu'il y a, à la connaissance, d'autres voies que *les voies sensorielles habituelles*.

Et c'est tout ce que je puis admettre, jusqu'à présent, comme irrévocablement démontré. Mais c'est beaucoup déjà. L'existence de ce sixième sens est véritablement un monde nouveau qui s'ouvre à nous.

L'intelligence est atteinte par des forces qui lui révèlent des faits que ni la vue, ni l'ouïe, ni le toucher ne peuvent lui faire connaître.

Alors, comme l'a dit mon illustre ami O. Lodge, deux hypothèses sont en présence. Ou bien ce sont des *esprits*, les âmes des défunts qui se manifestent à nous. Ou bien ce sont des *vibrations* (de nature inconnue) qui agissent sur notre organisme.

L'hypothèse des *esprits* est quelquefois très commode et s'adapte admirablement à certains faits ; mais elle soulève des objections formidables. Je ne la repousse nullement. Pourtant je ne peux guère y croire et je ne la regarde que comme une hypothèse de travail.

L'hypothèse des *vibrations* (inconnues) me paraît préférable.

Après tout pourquoi ne pas supposer que la réalité émet des vibrations ? Ne voyons-nous pas des vibrations innombrables et puissantes, comme les ondes hertziennes et les ondes magnétiques, qui ne sont décelées que par des *détecteurs* spéciaux, et qui sans ces détecteurs, passeraient inaperçues ?

A vrai dire, je me préoccupe peu de ces hypothèses. Ce qui m'intéresse passionément, c'est la constatation des faits. Or il a été démontré par Crookes, Lodge, Myers, Sidgwick, et beaucoup d'autres, que l'intelligence humaine a parfois sur la réalité (très rarement, je le veux bien), des notions que les sens normaux ne peuvent lui donner. Tous ceux qui ont méthodiquement et sans préjugés étudié cette psychologie nouvelle ont été finalement forcés de l'admettre.

CHARLES RICHET.

The Fluorescence of Superheated Mercury Vapour.

IN a letter to NATURE (April 17, 1926, p. 555) I gave a short report of experiments on the fluorescence of mercury vapour. The conclusion arrived at was that the total intensity of the visible fluorescence in saturated mercury vapour, as well as that of different bands and lines in the visible and ultra-violet part of the spectrum, is, for a given exciting light, a function of the temperature only. These results are in disagreement with those of R. W. Wood and van der Lingen (*Proc. Roy. Soc.*, **99**, 362, 1921, and *Astrophys. Jour.*, **54**, 149, 1921). According to these authors the fluorescence can only be excited in freshly formed mercury vapour.

Further experiments, with various vessels containing mercury vapour and different kinds of exciting light, have fully confirmed my previous results. The fluorescence does not depend on whether the saturated mercury vapour is old or freshly formed.

These experiments suggest the possibility of exciting the fluorescence in the unsaturated mercury vapour. For this purpose some experiments have been carried out. An evacuated and sealed quartz tube was used containing an amount of mercury just sufficient to fill the vessel with saturated vapour at about 240° C. The vessel was heated in an electric furnace closed with quartz windows. The temperature was measured by a thermometer and a thermopile fixed to the walls of the tube in two different places. The fluorescence excited by a condensed aluminium spark was observed visually and the spectrum taken with a quartz spectrograph. With rising temperature the minute mercury droplets in the vessel disappear gradually. At about 240° C. the last droplets vanish, the intensity of the fluorescence remaining unchanged. The intensity did not change either when the vessel was heated to 355° C. or kept for several hours at some constant temperatures above 300° C. The intensities of single bands and lines of the fluorescence spectrum remain likewise practically unchanged. The appearance of the spectrum is practically the same as in the case of saturated mercury vapour at about 240° C., and this proves that, above 240° C., the vapour was really unsaturated. Indeed, for the saturated mercury vapour the line 2537 Å.U. would disappear at about 260° C., and similarly the band situated on the long wave-length side of this line at about 280° C.

Previous experiments have shown that with changing vapour density (by varying the temperature of the saturated vapour) the fluorescence changes. In the present experiments the vapour density above 240° C. was constant and the fluorescence remained very nearly constant.

These results may be summed up by saying that for a given exciting light the fluorescence of the mercury vapour depends mainly on the density of the vapour. The age of the vapour is of no importance.

HENRYK NIEWODNICZAŃSKI.

Stefan Batory University,
Wilno, Poland, October 8.

Dr. Jeans and the 'Disease' of Life.

DR. JEANS can see after himself, I feel sure ; and if he accepts the statement of the writer of the note on his address in NATURE of December 4, p. 812, that in his view life is " possibly merely a disease infesting the rubbish heap in the corner " of the universe, no other interpretation can live. But as a student of life I was on the point of writing to thank Dr. Jeans not only for his masterly address, but also, most of all, for his stately and hopeful close wherein no such opinion as the above was fathered by him. Readers of NATURE can turn to the original in the Supplement to the issue of December 4 and judge for themselves. Dr. Jeans put two interpretations, in two different ways, and his final brace were: Is life of the nature of a disease which affects matter in its old age, or is it the only reality, which creates, instead of being created by ? The writer of the paragraph takes the first only and fixes it as Dr. Jeans's own view. Why ?

It is probably of no importance to anybody but myself to say that I welcomed the second, and have long taken it, for the simple, and I suggest scientific, reason, among many others, that matter as I see it cannot apprehend or understand me, and that I at least am able to apprehend it. I do not see why the apprehender should be subordinate to the apprehended, which, whatever its volume, does not apprehend anything, not even itself.

J. J. ROBINSON.

Barnham, Sussex.

MR. ROBINSON'S letter reveals a misapprehension which might be shared by others, and should at once be removed. The note in question was not an independent report of Dr. Jeans's article. It was a comment on the article, published in the same issue of Nature, and intended to be read in conjunction therewith. Its purpose was to direct attention to the Supplement and to indicate its general tendency. It was obviously impossible and undesirable to mention all Dr. Jeans's suggestions about life. Those suggestions, indeed, were entirely secondary to the main thesis, which was that "the primary physical process of the universe is the conversion of matter into radiation." The possible view of life referred to in the note was selected because it threw the main thesis into the strongest relief. The selection carried no implication whatever that the view was the one favoured by Dr. Jeans or by modern scientific thought. As Mr. Robinson says: "readers can turn to the original . . . and judge for themselves." In case Mr. Robinson imagines that there has been an insidious attempt to propagate materialism in the guise of science, let me say that personally I hold the idealistic view—so strongly, indeed, as to be able to contemplate the alternative with equanimity.

It may possibly save further misunderstanding if this opportunity is taken of correcting two misprints in the original notes. In line 3 of the first paragraph, "contributions" should be "constitution," and in line 4 of the second paragraph, "testing" should be "tasting."

The Writer of the Notes.

The Action of Silica on Electrolytes.

The importance of this subject in chemistry and pedology is my excuse for a further letter in reply to that of Prof. Mukherjee in Nature of October 9. I could wish that he had told us more as to the method by which he demonstrates 'primary adsorption,' as until it is established—and this is the point of disagreement between us—it is scarcely worth while discussing its mechanism. I have now tried so many variations of the experiment in attempting without success to obtain Prof. Mukherjee's results, that I am a little discouraged, and this the more because the magnitude of the effects described in successive communications has been greatly reduced. Thus the early statement that considerable quantities of acids could be adsorbed by silica was modified by his letter in Nature (January 31, 1925) stating that the silica used contained alkali. Again, in the issue of April 4, 1925, it was stated that 10 grams silica adsorbed 42 c.c. of decinormal hydrochloric acid, *i.e.* 250×10^{-4} gram molecules of acid per gram molecule of silica. But in the letter under reply, this 250 has been reduced to 1.

In the letter of January 31, 1925, it is stated that the *p*H value hydrochloric acid rose from 3·4 to 4·6 by passage through air-dried silica. We tried mixing pure silica with acid of this strength and found the *p*H values to be 3·45 before and 3·46 after the addition of the silica, results well within the experimental error.

It is most desirable that this question should be settled definitely. I would willingly try further direct experiments on mixing silica with acid if Prof. Mukherjee would tell me the strengths and quantities which must be taken to be sure of obtaining his results. Meanwhile, I am of the opinion that silica does not adsorb acids.

A. F. Joseph.

Wellcome Tropical Research Laboratories,
Khartoum, November 3.

"Colloid Chemistry."

Referring to the review of vol. 1 of "Colloid Chemistry: Theoretical and Applied," which appeared in Nature of October 23, p. 585, three references to electrodialysis are given in the book on p. 937, ultrafiltration dialysis is treated on p. 832, and electro-ultrafiltration on p. 834 *et seq.*, where a foot-note states that Dr. P. H. Prausnitz' paper on electro-osmosis (Graf Botha Schwerin, etc.) will appear in vol. 3. It has so many descriptions of practical applications, that it justly belongs in the volume on technology. Diffusion is, furthermore, treated in the latter part of Svedberg's paper on centrifugal and diffusion methods for the study of dispersity and hydration in sols, and in papers by Liesegang, Bradford, and Holmes; and as Liesegang says, p. 784, "dialysis is practically the same thing as diffusion in jellies, only it permits us to recognise more distinctly the processes going on inside of the membrane." It would seem, then, that dialysis has more than "casual mention in two places," and that electrodialysis is not "neglected," even though neither is given elementary discussion.

Jerome Alexander.

50 East 41st Street,
New York City, November 15.

The process of dialysis is so widely used for the purification of colloids that one would expect to find some general treatment of it in a book of this size dealing with 'theory and *methods*.' Diffusion into jellies, or without a septum, is not dialysis, nor are these methods used to any extent for the removal of non-colloidal substances. Similarly, the scattered references to electrodialysis are no substitute for a systematic treatment, nor is the promise of an article, in a future volume, on electro-osmosis, which is a different phenomenon.

P. C. L. Thorne.

The 'Bleeding' of Trees through Injury.

More than two years ago there was a short correspondence in Nature (**113**, pp. 492 and 604) on the 'bleeding' of trees in the spring through injury. The trees especially mentioned were the sycamore and birch. It may be of interest to record the fact that the common walnut (*Juglaus regia*) may be included among the 'bleeders'—at any rate, the specimen I have had under observation behaved markedly so. Casually severing a twig of this tree in mid-spring, 1925, I was a little surprised to find that sap immediately exuded. On February 1 last this tree for certain reasons had reluctantly to be felled. As the walnut is late in coming into leaf and adapted to a climate warmer than Cumberland can afford it, it was scarcely to be expected that so early in the year there would have been any exudation of sap. My surprise therefore was great to find such an outflow, on the application of the crosscut-saw to the base of the trunk, as to cause the sawdust to become quite moist. In addition, as the woodmen lopped off the branches of the felled tree, sap issued so freely from the cut surfaces that it could have been collected.

The tree still further astonished me, for throughout the spring, summer, and early autumn, sap exuded copiously from the stump, making the soil around quite soppy. Micro-organisms of one kind or another revelled in the exudation and gave rise to a sickly stench. This during a spell of warm weather became so obnoxious that the surface of the stump had to be cleaned, dried, and disinfected. The outflow still continued after the treatment, and appears only to have stopped through the setting-in of the cold weather of October.

I have never noticed such a long-sustained bleeding in the case of the stools of the sycamore or birch. These, however, usually sucker, and the sap may be utilised in this way, thus preventing any visible outflow. The walnut stump in question has shown no signs of sending up sucker-shoots.

John Parkin.

Blaithwaite, Wigton, Cumberland,
November 18.

Sir J. J. Thomson's Seventieth Birthday.

MESSAGES OF CONGRATULATION FROM ABROAD.

Prof. E. P. ADAMS,
Princeton University, N.J.

IT is pleasant to have this opportunity to express the satisfaction I feel in having had the privilege of working in the Cavendish Laboratory under Sir Joseph Thomson. Modern physics owes much to his genius; in fact, modern physics is very largely built upon the work he himself did and the ideas he furnished to the workers in the Cavendish Laboratory. Under his inspiring leadership the Cavendish Laboratory became the dominant centre for physical research, attracting students from all over the world who wished to come under his influence.

In addition to his stimulating interest in all the research carried out in the laboratory, Sir Joseph's lectures contributed much to the influence that the Cavendish Laboratory has exerted upon modern physics. His wonderful facility for making his hearers feel the reality of his point of view made his lectures full of suggestions for further work, and thus they carried out the principal object of lectures, which is to make the hearers think for themselves.

Here in Princeton Sir Joseph is remembered with real affection. For in 1896, at the time of the sesquicentennial of the founding of the College, he came over and lectured on the results of his latest work. He aroused great interest in the new discoveries, and in his belief, rather startling at the time, that electrons are constituents of the chemical atoms. It was largely through his influence that Profs. J. H. Jeans and O. W. Richardson came to Princeton from Cambridge. Prof. Richardson was mainly responsible for building up at Princeton, in the Palmer Physical Laboratory, a school of physical research. This is only one illustration of many that might be given of the way in which the influence of Sir Joseph Thomson has spread out far beyond the Cavendish Laboratory.

Prof. S. J. M. ALLEN,
University of Cincinnati.

If we say that a prophet receives more credit abroad than at home, it is equally true that in estimating the work of a great scientist the opinion of those outside his own country may be of more critical value than even that of his countrymen. This is especially true of Sir J. J. Thomson. To a reputation gained in his own country, and richly deserved, has been added the unstinted admiration of his co-workers in foreign lands, especially in the United States and Canada.

A fertile imagination, and true understanding of the nature of the problem, combined with the finest experimental skill, produced results of the greatest importance, reminding one of the immortal Faraday. His method of subjecting a moving charge to the combined action of electrostatic and magnetic fields, with the resulting determination of e/m, for the electron, and later for the positive ray, made his fame secure, and paved the way for the recent brilliant work of Aston on the isotopes. When the time comes to erect his monument (may it be far distant), perhaps the noblest symbol that could be placed thereon would be "e/m."

The American, or Canadian, who has had the great pleasure and honour of meeting 'J. J.' as director of the Cavendish Laboratory, or as Master of Trinity, carries back with him the remembrance of a great scholar and genial host, and gains through this association a wealth of knowledge and suggestions that are invaluable to him in his future work.

Prof. NIELS BOHR, For.Mem.R.S.,
University Institute of Theoretical Physics, Copenhagen.

It is with great pleasure that I accept the invitation of the editor of NATURE to take part in the universal celebration of the seventieth birthday of Sir J. J. Thomson, to whom every one interested in the problem of the constitution of the atom is so greatly indebted. Not to speak of the leading part he has taken in the discovery of the electron as a common constituent of all atoms, we owe to him an abundance of ideas which have proved fruitful in the attempts to develop a detailed theory of atomic constitution based on this fundamental discovery. At a time when even the existence of atoms was regarded by many prominent scientists with scepticism, Thomson had the courage to venture on an exploration of the inner world of the atom. Guided by his wonderful imagination and leaning on the new discoveries of the cathode rays, Röntgen rays and radioactivity, he opened up an unknown land to science. By following electrical particles and ether-waves on their way through atoms he obtained the first estimate of the number of electrons contained in an atom and of the forces by which they were bound, laying in this way the foundation of that elaborate structure which has been built up in recent years through the joint efforts of a large number of workers. We find in his famous attempt to account for the remarkable periodicity of the physical and chemical properties exhibited by the elements when arranged in the order of their atomic weights, the germ of the ideas characteristic of the modern interpretation of the periodic table. Indeed, it is difficult for scientists of the younger generation, who are working on the new land to which Thomson has opened the gates, fully to realise the magnitude of the task with which the pioneers were confronted.

M. LE DUC DE BROGLIE,
Membre de l'Académie des Sciences, Paris.

Il suffit d'un coup d'œil jeté sur l'histoire scientifique des quarante dernières années pour voir que le développement de la physique moderne, c'est-à-dire de la théorie électronique de la matière a pris naissance quand on a compris en quoi consistait le passage de l'électricité à travers les gaz.

C'est la notion d'ionisation qui a permis d'étudier les rayons X et de découvrir la radioactivité comme c'est elle également qui a mis sur la voie des représentations actuelles de l'atome, si profondes, si fécondes déjà et si pleines de promesses pour l'avenir.

Or c'est à Sir Joseph Thomson et à ses élèves qu'est due presque entièrement l'interprétation de l'ionisation des gaz, expliquée pas à pas par les physiciens de Cambridge et si objectivement mise en évidence par l'un d'eux, le professeur C. T. R. Wilson.

Aujourd'hui encore le premier livre à mettre entre les mains de celui qui veut s'initier à la physique du vingtième siècle est la "conductibilité électrique des gaz" écrit par le chef et l'animateur du Cavendish Laboratory. Il y a peu de temps, en en voyant une récente édition j'admirais combien la plus ancienne avait été merveilleusement composée pour avoir été si peu modifiée par les travaux des vingt-cinq années les plus actives de la physique électronique. Sir Joseph Thomson avait tracé un cadre si parfait des nouvelles perspectives de la science que les progrès ultérieurs de celle-ci semblent n'avoir été que le développement et l'achèvement des divers chapitres de son ouvrage. La part personnelle prise par l'ancien chef du Cavendish Laboratory, tant au point de vue théorique qu'expérimental dans toute cette partie de la physique, et le couronnement plus récemment apporté par la découverte fondamentale des spectres de masse des corps simples lui vaudront la gloire difficile d'inscrire sans crainte son nom à côté de celui de ses illustres prédécesseurs.

Le titre auquel a droit le très grand savant que l'on honore aujourd'hui est peut-être le plus beau que puisse ambitionner un homme de science et de génie: il a été le guide et le flambeau de toute une génération de physiciens qui portent aujourd'hui les noms les plus illustres de la science actuelle et son influence se fait sentir maintenant sur les élèves de ses élèves.

Toutes les nations du monde envient le Cavendish Laboratory pour son passé comme pour son présent.

Prof. D. F. COMSTOCK,

Massachusetts Institute of Technology, Boston.

On the occasion of Sir Joseph Thomson's seventieth birthday, will you allow one of his old students, who long since strayed from physics into the field of engineering research, to express his congratulations and to acknowledge gratefully a many-sided debt to him?

I went to the Cavendish for the purpose of absorbing, if possible, a little of his vision, a little of his way of looking at problems which were still nebulous; and through his kindly patience with the immaturity of a young student, I absorbed as much of this subtle influence as my capacity would allow. Whatever originality I may have shown through the intervening years is traceable in no small part either to his direct influence at that time or to his writings before or since.

I know that many, who are not primarily physicists, have benefited by this extraordinary power of Sir Joseph's to stimulate the originality of others, and because of this characteristic of his genius, his influence has spread far beyond the confines of pure physics.

I know that I am expressing the feeling of industrial research workers in general when I gratefully acknowledge our obligation to Sir Joseph not only for the new truths of Nature which his researches have uncovered, but also for the subtle stimulus which comes even from his writings and makes research work in any field happier and more successful.

Mme. CURIE,

Radium Institute, Paris.

Sur la demande de M. l'Éditeur de NATURE, il m'est agréable de m'associer à l'hommage rendu par le monde scientifique à M. le Professeur J. J. Thomson dont j'apprécie hautement l'œuvre, aussi bien pour la profondeur et l'originalité des vues théoriques que pour l'important ensemble de recherches expérimentales. Sans essayer d'envisager toute la variété des travaux de Sir J. J. Thomson, je désire rappeler que nous lui devons un grand nombre de résultats fondamentaux relatifs à l'étude des particules chargées, molécules, atomes et électrons. Parmi ces résultats, nous trouvons les premières déterminations de la charge élémentaire et du rapport de la charge à la masse pour l'électron, ainsi que la première notion de la masse électromagnétique. Non moins riches en conséquences sont les recherches de Sir J. J. Thomson sur les rayons positifs et l'interprétation si sûre qu'il en a donnée, conduisant à une nouvelle méthode d'analyse chimique qui est applicable à de très petites quantités de matière et dont on connaît le succès dans la généralisation de la notion de l'isotopie.

Le nom de Sir J. J. Thomson est souvent prononcé dans les laboratoires de physique, plus particulièrement dans ceux qui se consacrent à l'étude des particules portant une charge électrique et à celle de leur mouvement. C'est là un des problèmes qu'on aborde fréquemment dans les laboratoires destinés aux recherches sur les phénomènes radio-actifs—et c'est à ce titre que j'ai plaisir à exprimer à Sir J. J. Thomson ma gratitude pour l'importante contribution scientifique représentée par ses travaux personnels ainsi que par ceux qu'il a inspirés et dirigés.

Prof. GEORGE E. HALE, For.Mem.R.S.,

Mount Wilson Observatory, Pasadena, California.

I have always felt the exceptional stimulus of two great English laboratories, which for generations have maintained positions of leadership because of the men who have worked in them. The Royal Institution, dating from the time of Rumford and Young, is rich in traditions for the astronomer scarcely less than for the physicist. But in these days, when cosmic experiments are so frequently necessary to test the tenets of relativity or the ultimate consequences of theories of matter, we who devote ourselves to astrophysical research perhaps feel an even greater indebtedness to the Cavendish Laboratory. What a lineage of directors it holds before us—Maxwell, Rayleigh, Thomson, Rutherford! Among these we must attribute chiefly to Thomson its advance to an international position as a centre of research.

I remember with special pleasure my frequent visits to Cambridge, in peace and war, and the never-failing inspiration of my talks with Sir Joseph. It has been a great privilege to watch the growth of his ideas and to see the vacuum tube transformed under his hand from a coiled spectacle for the curious to a powerful instrument of research. He was among the first to explain the presence of charged particles in the sun, and in many other ways he has placed students of astrophysics under obligation. I am personally indebted to his writings for more than one basic suggestion, and I appreciate no less his encouragement during my efforts to extend the use of laboratory methods in astronomical observatories. I am, therefore, pleased to be permitted to join in warm congratulations to Sir Joseph and the Cavendish Laboratory on the occasion of the celebration of his seventieth birthday. I can offer no better wish than that the splendid successes of the past may continue through a long and happy future.

Prof. CHAS. T. KNIPP,

University of Illinois, Urbana.

Sir Joseph Thomson's influence reached the central of the United States during the early 'eighties. I remember distinctly, a decade later, as a pupil in a small county seat high school in north-western Ohio, that our teacher in ' natural philosophy ' directed the attention of the class to a possible new theory of the structure of matter proposed by Prof. J. J. Thomson, of the Cavendish Laboratory of physics at Cambridge, England. This reference was to his vortex theory. The story caught the attention of the class, for we all had seen the wonderful smoke rings shoot up from the old style funnel-shaped locomotive stacks. From that time on any reference in the newspapers to Prof. Thomson was eagerly sought, and almost unconsciously I became a great admirer of his ability.

A few years later while in the university, I found this same feeling of respect and admiration shared by those studying in the domains of physics and chemistry. Theories advanced by Thomson formed our topics for discussions. His discovery that the electron carries a negative charge and is a constituent of all matter, his identification of the Edison effect as due to electrons, and his later measurement of the charge and mass of the electron, were accomplishments that promptly received the commendation of American physicists and chemists. He was the leader during this the electron period. Afterwards his great contribution on the study of the properties of the elements by positive ray analysis attracted wide attention in America. Here again he was the pioneer worker and blazed the way for entrance upon the positive electricity or proton period, as it may now be called.

Nor are we in the States unmindful of Sir Joseph Thomson's brilliant conception of atomic structure, in which the various elements are represented as combinations of negative electrons and positive charges arranged as miniature solar systems—a conception that will always stand for the guidance of research on this fundamental problem.

Beginning with 1890, and continuing up to the present, the writings of no one person have had a greater influence upon the research activities in physics in American universities than those of Sir Joseph Thomson. That this statement is correct is attested by the large number of students from there that have sought out the Cavendish Laboratory for the doing of productive research. Personally, it is this high regard for Sir Joseph as a scholar, investigator, and as a gentleman, that impelled my second visit (the present one) to the Cavendish Laboratory, to engage in research and to lay claim, as it were, to his terse and illuminating criticisms and valuable suggestions.

Research in physics and chemistry in the United States owes a debt of gratitude to Sir Joseph Thomson for his marvellous record of nearly fifty years of uninterrupted original contributions. As one of many from across the Atlantic, I humbly add the foregoing as a message of appreciation of the accomplishments of an active and productive personality.

Prof. ALOIS F. KOVARIK,

Yale University, New Haven, Connecticut.

During the past as well as during the present generation, the Cavendish Laboratory has been the Mecca for physicists from all parts of the world, and we Americans owe a debt of gratitude to it for aiding and developing the spirit of research in physics in America. There is scarcely a physicist in America who has not been a pupil of Sir Joseph or else a pupil of one of his pupils. In the case of the latter, the enthusiasm for research and the high esteem for the Master came not only by reflection from those more fortunate ones who had been with the Master himself, but even in some cases with added force, for the powers inherent in his pupils were released by virtue of their contact with him. To these men Sir Joseph was made known not only as a physicist who opened the fields of electronics and of atomic structure, but also as a most congenial and sympathetic man. To them also he soon became simply ' J. J.' They seemed to learn to know him intimately even before meeting him personally on his several welcome visits to America.

Those of us who have been more fortunate and to be privileged to associate with him at one time or another, have had an increasing esteem for him not only because of the great scientific achievements which his mind made possible, but also for those things which a man likes to find in another man. Not only did he show interest in one's scientific work, but also he always showed human interest in the man himself, his friends, and the institutions with which he was connected. We all love his characteristic smile, and every one of us felt a certain pleasure within ourselves on hearing a footstep that every Cavendish man recognises as solely ' J. J.'s.'

It was my good fortune to have been a pupil of two of his pupils and to have experienced the delights from such associations, but at more recent time I had the privilege to be associated with him in the Cavendish and in Trinity—as a guest in both—and it is a pleasure to admit that my love for him, for his human and personal qualities, causes no less pleasure to me than my great admiration for his genius as a physicist.

Prof. PAUL LANGEVIN,

Collège de France, Paris.

J'ai eu la bonne fortune de commencer ma carrière scientifique par un séjour d'une année au Cavendish Laboratory, dans la période où, sous l'impulsion de Sir Joseph Thomson, immédiatement après la découverte des rayons de Röntgen et de la radioactivité, se constituait cette École de Cambridge dont l'influence a été si grande depuis trente ans sur le développement de la physique moderne.

Le plus charmant souvenir m'est resté de cette époque déjà lointaine depuis laquelle l'importance et la réputation du laboratoire n'ont cessé de grandir. Nous étions une quinzaine de jeunes gens venus de pays bien différents pour mettre en commun l'enthousiasme que leur inspirait la science nouvelle et réunis dans une commune admiration pour le jeune maître dont le rayonnement les avait attirés.

Il y avait là Brown, Child, McClelland, Henderson, Henry, Richardson, Rutherford, Shakespear, Townsend, Vincent, Wade, C. T. R. Wilson, H. A. Wilson, Zeleny. Nous avons eu récemment à déplorer la mort prématurée de McClelland; celui qui est maintenant Sir Ernest Rutherford est venu diriger un Cavendish Laboratory singulièrement agrandi; les autres sont dispersés un peu partout dans le monde. J'espère qu'un bon nombre d'entre eux se retrouveront la semaine prochaine à Cambridge pour évoquer des souvenirs et reprendre, autour du maître resté si actif malgré les années, les conversations anciennes qui s'échangeaient à l'heure du thé, dans son cabinet où nous nous réunissions après sa visite quotidienne à chacun de nous.

De cette époque datent les premiers travaux de Sir Joseph Thomson sur les rayons cathodiques et la décharge disruptive. Il commençait cette admirable série d'expériences qui devait aboutir à la découverte de l'électron négatif et à la création de la méthode si féconde des spectres de masse.

Du côté théorique comme du côté expérimental, l'œuvre si personnelle de Sir Joseph n'a fait que grandir en même temps que de plus en plus nombreux ont été ceux qui, chaque année, sont venus s'inspirer de sa parole et de son exemple pour répandre ensuite sa pensée et garder entre eux des liens précieux pour eux-mêmes et pour l'avenir de la science.

Les témoignages de reconnaissance et d'admiration qui viendront vers lui à l'occasion de son soixante-dixième anniversaire montreront à Sir Joseph Thomson combien son action a été grande et combien sont fortes les affections qu'il a su rassembler autour du laboratoire illustré par lui.

Prof. Theodore Lyman,

Jefferson Physical Laboratory, Harvard University, Cambridge, Mass.

When I entered the Cavendish Laboratory as a graduate student in the fall of 1901, I found myself in an atmosphere of a stimulating quality which I have never seen equalled in any other place. Not infrequently the influence of a great man upon his time is indirect, but Prof. Thomson not only illuminated the scientific world of the period of which I speak, but also managed to carry his students with him into his own realm of intellectual enthusiasm.

To the majority of us the prosecution of research is a laborious business; the road is long and steep, blind byways beset us, the perversity of inanimate objects foils us. But those of us who know Sir Joseph Thomson and appreciate his genius are in a position to realise that to one whose mind is attuned to Nature the path of knowledge is neither tortuous nor laborious. The experience gained at the Cavendish remains, even after the lapse of a quarter of a century, a happy memory and a source of lasting inspiration.

Prof. A. A. Michelson, For.Mem.R.S.,

Ryerson Physical Laboratory, University of Chicago.

It gives me sincere pleasure to add to the others an expression of my high appreciation of the magnificent work of Sir Joseph Thomson and the influence it has had on the whole field of electronics and radioactivity. No other discovery has so profoundly altered our notions of the ultimate structure of matter. I welcome the opportunity of expressing my most cordial congratulations and best wishes for the continuation of his activities for many decades to come.

Prof. Robert A. Millikan,

California Institute of Technology, Pasadena.

It is not merely the group of erstwhile workers in the Cavendish Laboratory who owe to Sir Joseph Thomson the chief inspiration of their scientific careers. I speak for many who have never been students there when I say that no man living has exerted so large an influence upon my own activity in physics as has Sir Joseph. It was his writing which stimulated my entrance in 1903 into the domain of photoelectric research, a domain which has been one of my chief interests ever since. It was he who inspired me so early as 1905 to try to pull electrons out of cold metals by fields alone, a research which led through all the mazes of high vacuum technique, and yielded results of importance in the direction aimed at only after ten years of effort. It was he whom I followed in making cloud experiments when I first observed, and worked with, isolated electrons. It was he who directed the thought of all of us in seeking to build atomic models based on electron configuration. It was he who made the first analysis of space charge effects, who pointed out the existence of mass as a function of electric charge and showed how to compute its value. In a word, it was he who, more than any other one individual, was responsible for formulating, and gaining general acceptance for, the theory of the electronic construction of matter—an idea that is probably to exert larger influence upon the destinies of the race than any other idea which has appeared since Galileo's time.

Prof. Vlad. Novák,

Technical High School, Brno.

Forty years ago little was known of British methods in physics and other natural sciences in the historic lands of Czechoslovakia. Our instructors, having been taught and trained in German schools, followed German methods. The German University of Prague was divided into German and Bohemian schools in sections in 1882. The natural sciences were studied in the Bohemian Philosophical Faculty of the Charles and Ferdinand University of Prague in the first ten years in buildings so primitive that one of the Austrian ministers himself described them as "a European scandal."

No wonder, then, that our students went to foreign universities! I believe the first of our scientific men who brought us some of the English research method in natural science was Prof. Boh. Brauner, our distinguished chemist, who studied at Owens College, Manchester (1880–82), and is well known to readers of NATURE.

Thirty years ago I had the opportunity to begin some research at Cambridge at the Cavendish under the guidance of Prof. J. J. Thomson. We were about eleven research students, and made quite a family party, meeting every day at tea-time in the professor's room. Prof. Thomson's discourses on these occasions, on the work going on in the laboratory, and his method of investigation, together with his clear and instructive mode of lecturing, his papers and books, made a great impression upon my whole future life. As a private lecturer at the University of Prague, where I trained young teachers in physics, and some years after, as professor of experimental physics at the Technical High School at Brno, I tried to follow my great teacher.

The laboratory work in our physical institutions at Prague and Brno, the spreading of English scientific literature throughout our libraries, our new text-books of physics, for both colleges and high schools, and especially the progress in new editions of books of practical physics, show clearly how we in Czechoslovakia have followed Sir Joseph Thomson's inspiring example.

Twenty years ago my friend Dr. Záviška, professor in the Bohemian University, worked at the Cavendish, and his scientific work and method of teaching through these years are proofs of the inspiring guidance of the great director of the Cavendish Laboratory.

I am very happy to send this modest message of our deepest thanks, and appreciation of Sir Joseph Thomson's influence upon the progress of physical science in our country, on the occasion of the celebration of his seventieth birthday.

Prof. KARL PRZIBRAM,
Radium Institute, Vienna.

While deeply indebted to the editor for giving me the opportunity of joining in the world-wide chorus of congratulation in the columns of NATURE, I should feel abashed if it were not possible for me to say that I am "a citizen of no mean city"—the city of Doppler and Loschmidt, of Stefan and Boltzmann.

It is obvious that a centre of learning that boasts of such names on its roll of honour would offer fertile ground for the new ideas so lavishly given to the world by Sir J. J. Thomson. Yet, apart from certain researches on ionic mobility and condensation, which were, so to say, imported directly from Cambridge, this Thomsonian influence on Viennese science is difficult to trace in detail, not because it is too slight, but, on the contrary, too universal; it is, to-day, all-pervading, like the air we breathe.

To the world of science in general, J. J. Thomson stands out as one of the great pioneers who, beginning in the last decades of the nineteenth century, blazed the trail for the new physics. But to the awe and veneration due to such a leading genius there is added for those who, like myself, were privileged to enjoy the genial hospitality of the Cavendish Laboratory years ago, a warmer, more personal feeling: it is the yearning for those unforgettable old days in Cambridge and for that band of inspired workers with J. J. Thomson as their centre.

Prof. A. SOMMERFELD, For.Mem.R.S.,
University of Munich.

May I be permitted to mention an activity of Sir J. J. Thomson's which has almost been lost in the shadow of his later brilliant successes, but nevertheless constitutes a characteristic feature in the record of this investigator; I refer to his "Recent Researches in Electricity and Magnetism"—the so-called third volume of Maxwell. He is dealing here with the good old mathematical physics, the actual formal treatment of special physical problems. Both in Maxwell's "Treatise," and in J. J. Thomson's continuation of it, these are found in equilibrium, so to say, with the new and specifically Maxwellian ideas; but in recent times they have been in danger of being suppressed under the weight of modern interests. It was thus that J. J. Thomson at first continued the great school of mathematical physics which arose in Cambridge under Green and Stokes. In this way he laid for himself and his students the solid foundations on which the researches on the electron could be built.

Modern physical theories are transient, the mathematical methods eternal; if they seem temporarily to be asleep, they revive again in previously unsuspected regions. This is illustrated in the theory of relativity, where the Riemann geometry suddenly sprang into physical life, and it is now illustrated in the quantum theory, which has recently come under the sway of the boundary condition problems of classical mathematical physics.

We hope that the physics of the future will not be lacking in men like J. J. Thomson, masters at once of physical problems and mathematical methods.

Prof. P. ZEEMAN, For.Mem.R.S.,
University, Amsterdam.

"*From the point of view of the physicist, a theory of matter is a policy rather than a creed; its object is to connect or to co-ordinate apparently diverse phenomena, and above all to suggest, stimulate, and direct experiment.*" —J. J. Thomson, "The Corpuscular Theory of Matter," p. 1, 1907.

The invitation to contribute a message to NATURE on the occasion of Sir J. J. Thomson's seventieth birthday, is one which I gladly accept, though I realise how difficult it will be to formulate in a few lines the feelings of admiration Sir Joseph's influence on physical science provokes in all parts of the world where science is flourishing. Whenever any one personally acquainted with Sir Joseph reads his name, they see in their mind's eye his powerful head and characteristic appearance, inspirations for an artist on various occasions.

It is a delight to inspect the photographs taken from time to time in the Cavendish Laboratory during J. J. Thomson's tenure of the directorship and representing the professor surrounded by his research students. The influence Thomson has exercised upon his generation is nobly illustrated by these pictures. It is remarkable that so many of his former pupils are now filling important posts in other learned institutions and that several of them are in the very first rank of physicists. The photographs honour the great teacher, and at the same time set in a clear light the exceptional ability of the British mind to see the root of things.

A paper of great importance, " On the Electric and Magnetic Effects produced by the Motion of Electrified Bodies," appeared in 1881. Thomson showed that the magnetic field set up by an electrified particle in motion increases the apparent mass of the particle. Indeed, he gave us the first formula for the electro-dynamic mass. The investigation is a most remarkable example of early genius, its author being at the time only twenty-five years of age. Unintentionally, Thomson was preparing himself for his grand work on the true nature of the cathode rays and the isolation of the electron.

The profound mathematical insight of Thomson we can admire in his " Applications of Dynamics to Physics and Chemistry," published 1885–1887, forerunner of Hertz's " Prinzipien der Dynamik," written at a time when the mirage of a real kinetic theory of matter was firmly believed in.

Before the last-mentioned investigations were published, Thomson was giving a great deal of his attention to experimental work in the laboratory, and apparently he did so more and more. Thomson's papers reveal the talents of the experimenter and of the mathematician, combined in a most remarkable and happy way. Unfortunately this combination, which enables their possessor to penetrate into the hidden depths of Nature, is very rare.

During a period of forty-five years, the results of Thomson's restless mental activity and originality have been given to the world in a series of investigations happily summarised in several books published as the subjects became ripe for description. They all exhibit the clearness and conciseness which are characteristic of Thomson's writings. I refer here not only to the fundamental work on the " Conduction of Electricity through Gases," but also to the delightful smaller books: "The Discharge of Electricity through Gases" ; " Electricity and Matter " ; " The Corpuscular Theory of Matter " ; and " The Electron in Chemistry."

Thomson's researches are often directed on questions relating to the deep and hidden nature of matter and electricity. He counts the number of electrons in the atoms ; he arranges with his fertile imagination the electrons in coplanar rings inside a sphere of positive electricity ; he gives us suggestions toward the explanation of the periodic law of the chemical elements ; he gives us his views on the structure of light ; and boldly explains some great difficulties of the old wave theory by his speckled wave front. A charming episode in the midst of these difficult investigations is the discovery of the isotopes of neon by Thomson's parabola method.

We hope that physics may still for many years benefit from Sir J. J. Thomson's labours.

Prof. John Zeleny,
Yale University, New Haven, Connecticut.

Science knows no national boundaries. An important discovery made in one laboratory soon becomes common knowledge and has its stimulating effect upon investigators throughout the world. The influence of any individual scientist upon research work in countries outside of his own is dependent therefore to a large extent upon the pioneer character and importance of his investigations. Greatly indebted as physical research in the United States is to Sir Joseph Thomson on such grounds alone, there are special reasons why his influence has been even greater than this measure would indicate.

In the first place, during Sir Joseph's incumbency of the Cavendish professorship at Cambridge, physical research in the United States expanded enormously and spread to many institutions where previously little or none had been done. The inspiration that produced this growth was supplied in part by the few centres in our country where research was already thriving, and this influence must not be under-estimated. But the eyes of the scientific world were upon Sir Joseph and the Cavendish Laboratory, because from this source in rapid succession new ideas were being brought to light that were making a very deep impression on scientific thought. It was inevitable that this activity should spur men on to do research work and to choose as the field of their endeavour the one that was yielding such remarkable results.

Another important channel through which Sir Joseph's influence was disseminated in the United States was through those of our men who in this period worked under his direction in the Cavendish Laboratory. Many of these men already held influential positions in our universities, and on returning imparted to an increasing circle the enthusiasm for research which they had absorbed at the fountain-head.

Mention must be made, too, of the beneficial effect upon our research men of Sir Joseph's visits to the United States and of the lectures he gave there. Large numbers were thus not only able to hear him explain his most recent theories but also to benefit greatly from an intimate discussion with him of their own problems.

Those of us who had the rare privilege of working with Sir Joseph at the Cavendish Laboratory count the years spent there as among those that we cherish most. We lived in an atmosphere sparkling with new thought. We enjoyed a free and happy comradeship. New ideas and interpretations of experimental results had to stand the test of friendly but none the less vigorous criticism. We experienced the thrill of being present when important discoveries were in the making, and day by day we were permitted to observe the working of Sir Joseph's penetrating and resourceful mind.

Over and above all is the fact that this great man is wonderfully human. One cannot forget his many interests, his kind heart and lovable nature, his humour, the cheerfulness he radiates, his hospitality, and his readiness to serve others. To know him is to have for him a reverence and affection that but grows with the years.

The Jolly Electron.

Sung to the Tune of "The Jolly Miller," at the Physical Society Club.

1\. There was a jolly electron—alternately bound and free—
Who toiled and spun from morn to night, no Snark so lithe as he;
And this the burden of his song for ever *used* to be:—
"I care for nobody, no, not I, since nobody cares for me."

Chorus.

And this the burden of his song for ever *used* to be,
"I care for nobody, no, not I, since nobody cares for me."

2\. Though Crookes at first suspected my presence on this earth,
'Twas J. J. Thomson found me—in spite of my tiny girth.
He measured first the "e by m" of my electric worth;
I love J. J. in a filial way, for he it was gave me birth!

Chorus.

But this the burden of my song, etc.

3\. 'Twas Johnstone Stoney invented my new electric name,
Then Rutherford, and Bohr, too, and Moseley brought me fame;
They guessed (within the atom) my inner and outer game,
You'll all agree what they did for me,
I'll do it for them, the same!

Chorus.

But this the burden of my song, etc.

4\. Then Wilson, known as 'C. T. R.,' his camera brought to bear,
And snapped me (and the Alphas too) by fog-tracks in the air.
We like that chap! For a camera snap is a proof beyond compare;
A regular star is C. T. R., we'd follow him anywhere!

Chorus.

But this the burden of my song, etc.

5\. So whether I rest as static charge, or rove in the ether free,
Or whether I settle in nuclear state, perched up on a proton's knee,
Or whether I spin my quantum yarns, in a spectroscopic key,
I'll love the 'Physicals' all the time, since all of 'em dote on me.'

Chorus.

But this the burden of my song for ever *used* to be,
"I care for nobody, no, not I, since nobody cares for me."

R. A. S. Paget.

The Cavendish Laboratory as a Factor in a Counter-Revolution.

Sir Napier Shaw, F.R.S.

Now that Parliament in its wisdom has once more made the University of Cambridge responsible for the teaching of its students, the wheel has come round full circle and we may fairly indulge in a retrospect about the teaching of science. The story may begin with 1851, when reforms were introduced largely at the instance of the Prince Consort, who had been elected Chancellor in 1847. There is an early pencilling in *Punch*, by John Leech, representing the election day with a placard of the opposition "Boats no Botany." It is astonishing how placidly an undergraduate can go through his course unconscious of living in a time of momentous change. The final statutes for the University of the 1851 Commission are dated 1858; but for the first time, in 1851, the class lists record the results of a Moral Sciences Tripos and a Natural Sciences Tripos, both quite new. Previously, dating back to 1824, there had been a Classical Tripos; but before 1851 it was limited to candidates who had already obtained honours in the Mathematical Tripos. The published class lists of that educational instrument go back to 1747–48 and form the connecting link between the triposes of to-day and the academic system of the Middle Ages when the University was paramount.

In my undergraduate days there were still some notable relics of that system. Within the structure of the central University building, the Library, there were a Divinity School, a Law School, and an Arts School, each arranged like a court of justice. There was a Medical School also; but already it possessed a separate building with a museum and dissecting-room.

The schools were originally arranged for the candidates for honours to 'keep' their 'acts' for degrees in the faculties of Divinity, Law, Arts, and Medicine, with moderators for judges, proctors for police, and University students for the general public. The candidate sat upon his tripos as proponent of a thesis in theology, law, medicine, or philosophy, with opponents to take sides in a public disputation. That had been the medieval plan for deciding upon the relative merits of the candidates. Since the beginning of the sixteenth century there had been Regius professors in divinity, law, and physic, who could act as moderators for their faculties. There was also a Regius professor of Greek who might have been regarded as dean of the general faculty of arts or philosophy; but the colleges had captured the right of moderation in that faculty before the end of Queen Elizabeth's reign, and moderators were nominated by the colleges in turn, according to a fixed rota.

The colleges had also assumed responsibility for the education of their students in preparation for the final act. The University had only to decide the relative merits of the candidates from different colleges. It accepted for matriculation, without examination, the candidates presented by a representative of a college in person. The colleges had their own curricula and provided their own teaching upon which they based their right to certify candidates as worthy of degrees.

The college curriculum was still in vogue in my time; it consisted of lectures on classical and mathematical subjects which were combined in one annual examination upon the results of which prizes were awarded. The subjects were in fact selected with an eye to the University examinations, which at that time and developed into a system. Physical science formed no part, except in so far as physical subjects were included in the comprehensive subject of mathematics.

It must be remembered that what was called mathematics in the third quarter of the nineteenth century was the direct successor of the arts or natural philosophy of the schoolmen. Latin was the language of the arts, and the colleges taught Latin. There was no professorship of Latin in the University until 1869. From 1663 there was a Lucasian professor of natural philosophy. Isaac Barrow was the first, Newton the second. From 1680 there was a professor of moral philosophy. Before the end of the eighteenth century there were in addition two professors of astronomy, professors of chemistry, modern history, Arabic, botany, geology, and natural and experimental philosophy, all of whom may have looked to the curriculum in arts for their students.

With the development of mathematics after Newton, 'arts' became concentrated in natural philosophy. The Act in the schools gave insufficient scope for adequate testing and a written examination was instituted, which carried off the name of the tripos and became the Mathematical Tripos; but when I was introduced to it its range was by no means restricted. It included statics, dynamics, hydrostatics, optics (geometrical and physical), spherical astronomy, lunar and planetary theories, hydrodynamics, sound, waves and tides, elasticity, heat, electricity, and magnetism. The last three were in a sense new; the University had restored the original comprehensive range of natural philosophy by adding them as additional subjects, and appointed an additional examiner for them. James Clerk Maxwell was the first.

Thus the range was very wide; but the teaching was not coterminous with the subjects named. From the nature of the examination, it became limited to the mathematical extracts from the original contributions of accepted authorities. There were, in fact, in the colleges some survivals of an experimental method in the shape of an unused skeleton or the residue of an Atwood's machine; but the philosophy and history of the sciences were disregarded. I remember an example. We had to be examined in thermodynamics. P. G. Tait had written a book about it. Tait was a first-class talker and disputant; his book has three chapters; the first two were interesting and polemic, the third was thumb-nail mathematics. We were instructed to confine our attention to the third—to 'cut the cackle' and come to the differential equations.

Thereby I am reminded that in the period from 1865 until 1875 there was an effervescence of text-books in pure and applied mathematics, the like of which can never have been seen before or since. They were mostly bound in green, and judging from the survivals in my own library, the firm of Messrs. Macmillan and Co. must have been mainly responsible for the output. It was also a period of the greatest exaltation of the private tutor, in connexion with which the names of Hopkins and Routh will be remembered. Routh had an astonishing faculty for enabling pupils to absorb the mathematical juice of the great crop of text-books, so that the amount of information appropriated in the course of three years is now almost beyond belief. To those of us who had some experience of practical physics and chemistry at school or at home it was an invaluable discipline. What it could mean to those who had not, I cannot guess.

Then came Nemesis. The University, or somebody else, realised that all this tremendous appropriation of the mathematical secretions of the workers in natural philosophy without any experiment was not exactly healthy. There was indeed a chemical laboratory, and the Lucasian professor at the time was an experimentalist, so was the Jacksonian professor; but practical astronomy in my experience was one visit to the Observatory to be shown the adjustments of the transit instrument and the mural circle—we might be asked about those in the tripos, but not about stars.

The new idea of restoring the experimental study of natural philosophy found expression by the Duke of Devonshire, Chancellor of the University, in the foundation of the Cavendish Laboratory. Michael Foster had been brought to Cambridge by Trinity College about the same time as prælector in physiology, and a laboratory was found for him somewhere.

The early days of the Cavendish Laboratory were of peculiar interest. People who have since distinguished themselves worked there at their leisure: George Chrystal, Sir Donald MacAlister, W. M. Hicks, Sir Arthur Schuster, and Sir Richard Glazebrook. There were lectures by the professor and more elementary lectures by his demonstrator, W. Garnett; but there were no practical 'classes,' though the Natural Sciences Tripos included a practical examination. So the last quarter of the nineteenth century may indeed be aptly called the age of the practical class in science. Any one who visited the University now would scarcely suppose it possible that fifty years ago there were no classes in practical work.

It may perhaps be said now that there are too many; and indeed it is scarcely an exaggeration to write that the only thing to do with any system of education that has become established is to change it. No plan can be permanently successful; at least no scheme of examination can be.

Sir Richard Glazebrook and I made a text-book and, as we thought, a very good one. It was designed to enable a class of students to inform themselves about the physical nature of things by using different aggregations of the miscellaneous collection of apparatus at the Laboratory.

I sometimes wonder how many of those experiments we had ourselves performed when we described them in such detail. But with the examination before them, our students and their successors have insisted on doing them all, and perhaps from their point of view they were right; but the result is wrong. The experiments were designed and described as a short cut to something better.

Nevertheless, the practical work in science, of which the Cavendish Laboratory is typical, furnished demonstrations not only of experimental physics but also of the University's care for teaching. The example has been followed, expanded, and improved until this term, under the new statutes, the University has recaptured from the colleges the right to teach its students the arts, as well as divinity, law, and medicine, a right which it surrendered in the spacious days of Queen Elizabeth.

News and Views.

PHOTOGRAPHS of the 'Pithecanthropus skull' recently obtained at Trinil in Java by Dr. Heberlein have now been received by Dr. Dubois of Haarlem. At the time of the discovery it was positively announced to be of the pithecanthropus type; but Prof. Elliot Smith has received a cablegram from Dr. Dubois in which he says, "Photographs received show *caput humeri stegodon.*" The second pithecanthropus skull thus turns out to be a pleistocene elephant! Much disappointment will be felt at this pronouncement, which, however, does not come entirely as a surprise. The information received in England from America soon after the first announcement made it clear that the discovery was not likely to prove so important as at first indicated. The more complete statement of the character of the find and the conditions in which it was obtained—it was not found *in situ* as at first stated, but was obtained from the inhabitants of the village, and it was also said not to be a complete skull—pointed to the need for suspending judgment on the importance of the find. The wide publicity given to the discovery serves to emphasise the dangers of over-hasty dissemination of news through the ordinary channels of the press without effective and well-informed supervision such as might be afforded through the medium of a centralised scientific news service.

THE RIGHT HON. NEVILLE CHAMBERLAIN, Minister of Health, presided at the reopening ceremony of the Wellcome Bureau of Scientific Research and the Museum of Medical Science on December 8. In the unavoidable absence abroad of the founder, Mr. Wellcome, Dr. C. M. Wenyon, Director of the Bureau, received the guests. Mr. Chamberlain, in the course of his remarks, dwelt on the great advances made by medical science in the course of the last century, largely due, in his opinion, to the improvement in the means of imparting knowledge to those interested throughout the world. He considered that the motive actuating Mr. Wellcome in founding the Bureau was a conception of the two greatest factors which help mankind to overcome the infirmities to which all, in some degree or other, are subject; these two factors are research and education. Research is carried on not only at the Bureau, but also at the Wellcome Physiological and Chemical Research Laboratories; education is subserved by the Museum of Medical Science, to which all interested are welcomed, where, moreover, teachers are permitted to give demonstrations to their students.

SIR WALTER FLETCHER, Secretary of the Medical Research Council, followed Mr. Chamberlain with an address on "Research and Citizenship." After referring to the admirable scientific work of the staff of the Bureau, he turned to the consideration of the means by which research has been in the past and is at present financed. Many brilliant investigators have been enabled to pursue their work owing to the accident of possessing private means or owing to the liberality of some friend or private institution. It is only within the last twenty-five years that the State has made grants in aid of research work, with the establishment of such institutions as the National Physical Laboratory in 1900 and the Medical Research Council (then a Committee) in 1913. Business firms have forwarded scientific work in three ways. Private fortunes made in commerce have been devoted to the endowment of research. Manufacturing firms have set up their own research laboratories; and although the investigators are not always free to publish the results of their work, yet the public are indirectly benefited by the increased efficiency of the firms in question. Finally, a few firms have adopted the plan of setting up research laboratories and leaving the workers as free to follow their own line and publish their results as men in any university laboratory. In Great Britain, the Wellcome Bureau is an example of the latter method of endowing research; abroad, the work of Langmuir and Coolidge in America and of Sørensen in Copenhagen has been made possible by similar endowments from commercial firms. The proceedings were terminated by Mr. Chamberlain declaring the Bureau and Museum open, and the guests then accompanied him and Dr. Wenyon on a tour of the halls and laboratories.

IN an address on "International Interests in Raw Materials" to the Royal Society of Arts, which is published in the Society's *Journal* for November 26, Sir Thomas Holland laid stress on the importance, for economic as well as military reasons, of making a precise estimate of the mineral resources of the British Empire. A large number of minerals are essential for the maintenance of civilisation, and many of them cannot be replaced in the functions for which they are used. Minerals are wasting assets, and their consumption is annually accelerating to such an extent that a partial famine for some important substances will confront the next generation. The United States has recently inaugurated a systematic inquiry into its stocks of essential minerals on lines of precision and thoroughness that will leave little doubt as to resources available. For the British Empire the matter is at least of equal importance. The Mineral Resources Bureau collects figures of production and movement, but that is a small part of the problem. To make a complete or even partially complete survey, private sources of information must be tapped. The specialist in the course of his professional practice must have acquired information which, in the aggregate, would be of immense value even if the results were published only as totals for each considerable section of the British Empire.

ON October 27, Dr. R. J. Tillyard, chief of the Biological Department, Cawthron Institute, Nelson, New Zealand, delivered the Trueman Wood lecture before the Royal Society of Arts, upon " The Progress of Economic Entomology (with special reference to Australia and New Zealand) " ; and this is published in the *Journal* of the Society for November 12. Dr. Tillyard points out that the main problem of economic entomology is how to intervene scientifically, in the most successful manner, in order to prevent the huge losses caused by insects to man's food supply and forests. The past thirty years have witnessed an immense development of what we may term the chemical method of attack, and the technique of the latter has been revolutionised during the last few years, particularly in America. Special reference is made to the commercial use of aeroplanes for dusting large areas of vegetation, and Dr. Tillyard expresses the hope that Great Britain will not be long in following Germany and Russia, which have already conducted promising trials with regard to the possibilities of this new line of attack. He deals very fully with biological control and emphasises the fact that three organisms are involved in the problem, namely, (1) the plant, (2) the insect attacking the plant, and (3) the parasite or predator of that insect. Occasionally we have also to consider secondary or even tertiary parasites of the parasite or predator.

THE possibilities of biological control are being explored in Australia and New Zealand, and the recent very striking success achieved by the introduction of the Chalcid parasite *Aphelinus mali* from North America into New Zealand, for controlling the woolly aphis of apple, is an example of it. The re-exportation of this same parasite into Australia is also proving highly beneficial in the latter country. The biological control of imported weeds which have so far resisted all other means of eradication is also discussed. The possibilities of introducing insect enemies of such weeds from their countries of origin into lands where the weeds have become established are worthy of the fullest exploration. Experiments of this nature, however, need to be carried out with adequate safeguards, under expert guidance, lest such insects turn their attention to cultivated plants and thereby become new evils rather than benefactors as originally intended. The work that is being carried out in Australia on prickly pear control by means of introducing Opuntia-feeding insects to prey upon that plant is particularly noteworthy. It appears that this problem is nearer solution than hitherto by means of the biological control exercised by insect enemies on this formidable pest. The importance of attempting likewise the control of blackberry, gorse, and other noxious weeds in New Zealand through the utilisation of certain insect enemies is also stressed by Dr. Tillyard, and his scheme for meeting the problem is outlined.

THE League of Nations' Committee on Intellectual Co-operation has published an important memorandum by Madame Curie, the eminent physicist, on the question of international scholarships for the advancement of the sciences and the development of laboratories. Madame Curie directs attention to the ever-increasing specialisation in the equipments and programmes of laboratories and the increasingly complex organisations of their staffs, and urges that fellowship and scholarship schemes should be so framed as to fit in with and take advantage of these conditions. To do so they must have due regard to two essential necessities : advanced workers, who have already made their names by scientific work, must be given the means of continuing their work ; all candidates anxious to devote themselves for some time to science must be given a chance of developing their talents on the sole condition that they are recommended by their tutors or have obtained satisfactory university degrees.

WITH such a scheme, a complete foundation would accordingly control scholarships of two grades: for research fellows and for probationary scholars (two or three times as many). It would, further, provide grants to laboratories amounting to perhaps a quarter of the amount of the scholarships, to enable them to meet the increase of expenditure resulting from increase in the number of workers. Each probationary scholar would be assigned by the director of the laboratory to one of the research groups in which a vacancy is expected. A research fellow would work independently on a subject chosen by himself with the approval of the director or set for him by the director. The memorandum concludes with proposals, which have been adopted by the committee, for an inquiry into national and post-graduation scholarships and the best means of organising a system of international scholarships. Lord Balfour's research sub-committee of the Imperial Conference might well devise and provide for financing some such scheme for minimising the obstacles in the way of interchange between research workers in different parts of the British Empire and so meeting, to some extent, the competition of the magnificently equipped and endowed laboratories in the United States for the best brains of the Empire.

IT is surprising that, notwithstanding the advances made in recent years in the study of phonetics and the methods of recording spoken sounds, there is still no agreement as to the form of script to be used in recording the spoken languages of India, although the question has often been discussed. With the example of America before us, it should be possible for something to be done. In the United States a script has been evolved adequate to meet all the needs of recording a group of languages of considerable phonetic difficulty. The languages of India should present far fewer stumbling-blocks. This question is again raised with special reference to the Munda group of languages by Mr. P. O. Bodding in the *Journal and Proceedings of the Asiatic Society of Bengal*, vol. 25, part 1, where he insists upon its urgency, now that changing conditions are beginning to affect the habits of the people. He quotes a number of errors which, as a matter of practice, arise through an imperfect method of recording the language, while from the scientific point of view the importance of an accurate method of recording the spoken sounds has been greatly enhanced by the extension of the comparative study of linguistics. A question of such moment as this would seem to call for action on the part of some influential body, such as the Asiatic Society of Bengal, before it is too late and these languages have been profoundly modified by contact with the outer world.

IN many physical theories—as, for example, in the atomic theory of gases or in J. J. Thomson's corpuscular theory of light—the theory of probability plays a leading part. In a paper read by Mr. G. F. O'dell on December 2 to the Institution of Electrical Engineers on certain aspects of automatic telephone working, this theory is also largely used when discussing purely technical matters. The problems considered were those arising when provision has to be made for more than one telephone call at one time. In automatic exchange working, when the caller removes his receiver, his ' preselector ' hunts for a disengaged first selector. On his dialling the first digit, the first selector steps up to the corresponding level and then hunts until it finds an idle second selector. On the receipt of the second digit, the second selector rises and searches for an idle final selector, which in turn responds to the third and fourth digits. In addition to the subscriber being actually engaged, there are three places in this chain of events at which the call may be ' lost ' or delayed. In order to discuss the theory, telephone engineers find it convenient to define a ' telephone traffic unit.' This unit is the average number of calls in progress simultaneously during a specified period. It is simply a number and has no physical dimensions. One of the problems discussed by Mr. O'dell was the best method of arranging switching plant to the best advantage. This necessitates using the theory of probability. Another problem the solution of which forms the basis of the design curves in common use in the United States is to find the grade of the service. This is measured by finding the proportion of the traffic lost when a given volume of traffic, measured in traffic units, is offered to a definite group of switches. Mr. O'dell showed that telephone engineers fully realise the help that an advanced knowledge of mathematics can be to them. He also developed an analogy between ' trunking ' problems and those which arise when discussing the performance of heat engines. It is thus shown how one branch of applied theory can help another.

THE present year marks the centenary of the founding of *Crelles Journal für die reine und angewandte Mathematik* or Crelle's Journal, the oldest organ of mathematical research in Germany surviving to the present day. Of earlier mathematical journals, only the French *Journal de l'École Polytechnique*, founded in 1794, has enjoyed a longer period of continuous publication. The earlier volumes of Crelle were enriched by papers from Gauss, Abel, Jacobi, and Steiner, while many of the more distinguished Continental mathematicians have contributed to its pages in more recent years. Among Englishmen, Cayley, with eighty papers, was the most frequent contributor. It is the intention of the editors to celebrate the centenary worthily by issuing two commemoration volumes (157 and 158) in which will be shown, in the true perspective of distance, the part played by this journal in developing the science of mathematics during the last hundred years.

PROF. J. H. PRIESTLEY, of the department of botany, University of Leeds, is to give a course of post-graduate lectures and demonstrations to students of the departments of botany and bio-chemistry of the University of Berkeley, California, during the spring of next year. Prof. Priestley will deal with the subject of developmental anatomy. His lectures will include some account of the work done in the botanical department at Leeds upon the structure, function, and distribution of the endodermis; the structural features associated with the phenomena of etiolation; and the problem of phototropism. Prof. H. H. Dixon, of Trinity College, Dublin, has similarly been invited to lecture at the University of Berkeley in the summer months of 1927.

A SERIES of violent earth-shakes disturbed the Rand on the morning of December 7, and in the Wolhuter gold mine there were two rock-bursts which caused the death of four persons. The earth-shakes seem to be due to the collapse of the surface-rocks over disused mining tunnels. In the early days of the mines, no earthquakes were noticed in the Rand, but in 1905 they began to occur and soon increased so much in frequency that in 1910 a seismograph was erected in the Johannesburg observatory. During the next fourteen years, 5427 local shocks were recorded. It was noticed that they occurred in series, and most frequently in the dry season. This led to greater care being used in timbering and re-filling abandoned tunnels, and during the last two years local shocks have decreased in number (*Volcano Letter*, October 31, 1926, issued by the Hawaiian Volcano Research Association).

THE seventeenth annual Exhibition of the Physical Society and the Optical Society to be held on Tuesday, Wednesday, and Thursday, January 4, 5, and 6, at the Imperial College of Science and Technology, Imperial Institute Road, South Kensington, will be open in the afternoon from 3 P.M. to 6 P.M. and in the evening from 7 P.M. to 10 P.M. On January 4 at 8 P.M. Prof. E. N. da C. Andrade will reproduce with contemporary apparatus a physical lecture of the early eighteenth century. On January 5 at 8 P.M. Dr. C. V. Drysdale will lecture on "Progress in Electrical Instrument Design and Construction," and on January 6 at 8 P.M. Mr. J. L. Baird will give a lecture on "Television." Some seventy firms are exhibiting apparatus, and in addition there will be a group of non-commercial exhibits, including demonstrations of famous historical experiments in physics, recent research and effective lecture experiments. Tickets, which can be obtained from the secretaries of related scientific societies or direct from Prof. A. O. Rankine, Imperial College of Science and Technology, South Kensington, S.W.7., are required for January 4 and 5, but on January 6 the Exhibition will be open to the general public without tickets.

THE issue for November 15 of *Power Plant Engineering* contains a short illustrated account of the remarkable high-voltage laboratory erected at Stanford University, California. The laboratory has been erected by the University with the aid of some of the big electrical firms to ensure the continuance of the research work of Dr. Harris J. Ryan. The main building of the laboratory is 173 feet long, 60 feet wide, and 65 feet high, and in this are installed the six specially designed transformers, each of which is rated at 350,000 volts high tension, 2300 volts low tension, and each of which weighs 22 tons. These have been constructed so that they may be used in every manner of connexion for the whole range of voltages single-phase or three-phase, up to 2,100,000 volts single-phase and 1,200,000 volts three-phase. The work of the laboratory was inaugurated on September 17, when before an assemblage of men of science a 2,100,000 volt spark was discharged between points 20 feet 1 inch apart.

WE much regret to record the death, on December 11, of Sir William Tilden, F.R.S. Sir William, who was formerly professor of chemistry and dean of the Royal College of Science, London, and emeritus professor in the Imperial College of Science and Technology, South Kensington, had reached the age of eighty-four years.

LORD D'ABERNON has accepted the chairmanship of the Industrial Fatigue Research Board, to which he has been appointed by the Medical Research Council. Mr. William Graham has relinquished the chairmanship of the Board under the pressure of other public work, but will remain a member of the Board.

IT is stated in *Science* that Prof. J. J. Abel, professor of pharmacology at Johns Hopkins University, is to receive the Willard Gibbs gold medal for 1926 awarded by the Chicago section of the American Chemical Society. Prof. Abel's work has been largely in the field of glandular extracts; he isolated epinephrin as a pure crystalline body from the suprarenals, and recently he has announced the preparation of insulin in crystalline form.

IT has now been decided to close the fund raised to signalise the long services of Prof. A. G. Perkin to science and to the University of Leeds. Prof. A. G. Perkin, it will be remembered, has recently retired from the chair of colour chemistry and dyeing. Those who desire to be associated with the purpose of the fund are asked to send their subscriptions to Prof. A. F. Barker, The University, Leeds, at an early date.

THE twenty-fifth anniversary of the historic wireless experiments conducted by Senatore Marconi between the Poldhu wireless station in Cornwall and St. John's, Newfoundland, which resulted in the transmission and reception of wireless signals between the old and the new worlds for the first time, fell on Sunday, December 12. Senatore Marconi's achievement in 1901, only six years after his earliest experiments at his father's house in Bologna, Italy, marked an epoch in the development of wireless communication and firmly laid the foundations of the long-distance wireless communication which to-day covers the whole world.

THE following lecture arrangements at the Royal Institution before Easter next year have been announced. The Christmas Course of six lectures for juveniles will be delivered by Prof. A. V. Hill, on "Nerves and Muscles: How we Feel and Move," commencing on December 28 at 3 P.M. On Tuesdays, at 5.15 P.M., beginning on January 18, there will be two lectures by Prof. R. Whytlaw-Gray on smokes as aerial disperse systems, six by Prof. Julian Huxley on problems of animal growth and development, two by Dr. G. Shearer on X-rays and the chemical molecule, and two by Prof. J. W. Cobb on some properties of coke. Thursday afternoon lectures, at the same hour, include three lectures by Sir William Bragg on acoustical problems treated by Lord Rayleigh; three by Prof. John Garstang on the progress of Hittite studies; two by Mr. J. Guild on colour measurement and standardisation; and two by Mr. Harold J. E. Peake on the beginnings and early spread of agriculture. Saturday afternoon lectures at 3 P.M. include four by Sir Ernest Rutherford on the α-rays and their application to atomic structure. The Friday evening meetings will begin on January 21, when Sir William Bragg will deliver a discourse on Tyndall's experiments on magne-crystallic action. Succeeding discourses will probably be given by Prof. E. P. Cathcart, Mr. T. L. Eckersley, Dr. Ernest Law, Sir Josiah Stamp, Prof. D'Arcy Thompson, Sir Herbert Jackson, Dr. George Macdonald, Mr. E. Hatschek, Prof. C. T. R. Wilson, Sir Ernest Rutherford, and other gentlemen.

WE have received the annual report of the Laboratory of the Joint Board of Research for Mental Disease, City and University of Birmingham, of which the late Sir Frederick Mott was honorary director. Determinations of the iodine content of the thyroid gland have been made in various conditions, and show that septic infection and tuberculosis cause considerable variation. Investigations have been pursued on basal metabolism in conditions of conscious and unconscious contraction and relaxation of muscles, on changes in the central nervous system as a result of the administration of hypnotic drugs, and on the permeability of the membranes of the brain to the bromine ion after administration of sodium bromide.

THE topographical survey of the colony of Sierra Leone is at length to be put in hand. Up to the present no satisfactory map of this part of British

Africa has been available. A year ago the financial position of the country justified the formation of a new survey department, of which the first annual report has now appeared. It has been decided to aim at a one-inch scale for the whole colony, which will entail 111 sheets. A start is to be made in the central and southern parts of the Northern Province. A school for training native surveyors has been started, but in order not to delay the work, trained surveyors have been temporarily transferred from the Gold Coast. A large scale cadastral survey of Freetown is in hand and well advanced.

Applications are invited for the following appointments, on or before the dates mentioned :—A research student at St. Mary's Hospital Institute of Pathology and Research—The Secretary of the Institute, St. Mary's Hospital, Paddington, W.2 (December 20). A research assistant in the Department of Pharmacology of the University of Sheffield, to help in an investigation on cancer, and a laboratory attendant with experience of physiological or pathological and chemical technique for the same institution—Prof. E. Mellanby, The University, Sheffield (December 22). A demonstrator in chemistry at Guy's Hospital Medical School—The Dean, Guy's Hospital Medical School, London Bridge, S.E.1 (December 29). An adviser in agricultural chemistry in the University of Manchester, under the scheme of the Ministry of Agriculture and Fisheries—The Registrar, The University, Manchester (January 20). Lecturers in organic chemistry, physical chemistry, and biochemistry at the Indian Institute of Science, Bangalore, India—The Director (January 30). A professor of physiology in the University of the Witwatersrand, Johannesburg—The Secretary, Office of the High Commissioner for the Union of South Africa, Trafalgar Square, W.C.2 (January 31). A senior lecturer in philosophy in the Transvaal University College, Pretoria—The Registrar, Transvaal University College, Pretoria (January 31). A professor of anatomy in the University College of South Wales and Monmouthshire — The Registrar, University College, Cardiff (February 26). A professor of philosophy at Armstrong College—The Registrar, Armstrong College, Newcastle-upon-Tyne (March 12).

Our Astronomical Column.

The Comet Grigg-Skjellerup.—Mr. G. Merton read a paper on this comet at the meeting of the Royal Astronomical Society on December 10. The identity of the comet found by Skjellerup in 1922 with that found by Grigg in 1902 was first suggested by Crawford and Meyer. Mr. Merton has made it a practical certainty. He gets practically the same mean motion in 1922 from the observations in that year alone (they extended over three months, so the value is trustworthy) as by combination with those of 1902. The comet is due to return to perihelion on May 10, 1927 ; Mr. Knox Shaw is now searching for it with the large reflector at Helwan. It may be expected to be found not later than February. It approaches within 17 million miles of the earth in June ; Comet Pons-Winnecke makes a still nearer approach (some 4 million miles) near the end of June.

International Longitude Determinations.—Dr. J. Jackson gives in the *Observatory* for November an interesting account of the extensive scheme of longitude determination by radio signals which has been in progress during October and November.

The observatories of Algiers, Shanghai, and San Diego (California) form the principal chain, but some fifty other observatories are co-operating. The use of travelling wire micrometers practically eliminates personal equation, and enables longitude differences to be determined without interchange of observers. Dr. Jackson obtained star observations on seventeen nights between September 27 and November 1, the transit instrument being reversed on each star to eliminate collimation. The Shortt clock has such a regular rate that its error can be interpolated for days without star observations. Thirty-four series of time signals are sent out daily from five stations ; all except Saigon were regularly received at Greenwich.

Theory of Sunspots.—An important contribution to the theory of sunspots and the sun's general circulation is made by Prof. V. Bjerknes in the *Astrophysical Journal*, September 1926, under the title " Solar Hydrodynamics." For the details of the theory reference must be made to the paper in question, but a short outline of the main points may be given as follows. On the assumption that a sunspot is a vortex decreasing in intensity from the photosphere downwards, their low temperatures are explained from general hydrodynamical and thermodynamical principles.

The results deduced are in accordance with the accepted temperatures of sunspots and the probable velocities of the gases involved in the vortex. A preliminary account of this part of Bjerknes' investigation was given in Nature, March 27, p. 463. The well-known properties of sunspots (their usual occurrence in pairs having opposite magnetic polarities, the progression of the spot zones towards the equator during the 11-year cycle, the magnetic-polarity cycle of 22 years, etc.) are explained by making the following suppositions. In each of the sun's hemispheres, northern and southern, there are two zonal vortices, having opposite rotations and surrounding the sun approximately as parallels. Wherever part of either vortex rises and cuts the photosphere, a typical bipolar pair of sunspots makes its appearance. As part of a scheme of general circulation, these two zonal vortices revolve around each other in a period of 22 years, being brought alternately near to the surface of the photosphere in latitudes about 40°, progressing equatorwards in the course of 11 years, and descending again into the interior near the sun's equator. The scheme of general circulation is one demanding a condition of what is known as stratified circulation.

Renewed investigations are required on the part of observers to determine any possible systematic movements which may be shown by sunspots, faculæ, calcium and hydrogen flocculi, and prominences. The systematic drifts suggested by the theory are apparently too slow to be observed spectroscopically (cf. *Astrophysical Journal*, 32, 80, 1910, where St. John compares the mean wave-length of K_2 and K_3 near the sun's poles and at the equator for detection of systematic movements).

Prof. Bjerknes' paper is also discussed, together with remarks bearing on the question of observed systematic motions of spots and faculæ, by 'W. M. H . G.' and 'H. W. N.' in the *Observatory* for December.

Research Items.

THE VALUE OF TRADITION.—In the course of a discussion of the value of tradition in Polynesian research, which appears in the *Journal of the Polynesian Society*, vol. 35, No. 3, Dr. P. H. Buck (Te Rangi Hiroa) gives some remarkable examples of the manner in which Maori traditions are corroborated from outside sources, legendary and other. The Maori tradition states that their ancestors made voyages between the Sandwich Islands and their own Hawaiki or Tahiti of the Society group. The traditional sailing directions from Ahuahu (Oahu, Sandwich Islands) to Aotearoa (North Island, N.Z.) give a bearing south from Maui-tahu and Maui-pae to Hawaiki and from Hawaiki to New Zealand a little to the right of the setting sun. In Hawaiian tradition in the directions for the voyage to Tahiti, the North Star is left directly astern. Further, Maui-tahu and Maui-pae are probably the twin islands of Lanai and Kahoolawe; Hawaiian tradition makes Ko-ola-i Kahiki the point of departure for Hawaiian voyagers to Tahiti, and this has been identified as a point on Kahoolawe. Even more striking is the evidence of the magic calabash. Hawaiian voyagers to Tahiti on passing the equator lost the North Star and picked up the Southern Star. On their return they picked up the Northern Star and sailed in a north-easterly direction (owing to the prevailing wind) until they judged the star was the same height as in Hawaii. They then turned and sailed due west, checking the height of the star each night, in the early days no doubt by eye, but later by the magic calabash. This calabash was fashioned into a primitive form of sextant with which the star was sighted on an elevation determined in Hawaii. An examination of one of these calabashes has shown that it is mathematically accurate, the angle being 19°, and Hawaii being in lat. 19° N.

DEFORESTATION AT PUEBLO BONITO.—After six seasons' excavations on the important site of Pueblo Bonito in New Mexico, it is now possible to arrive at some definite conclusions as to the causes which led to the gradual decline and final abandonment of this once populous centre of prehistoric Pueblo culture. The population, some twelve hundred in number, was agricultural, living on the produce of the fields, once fertile, but now barren. According to a Bulletin of the Smithsonian Institution, the expedition of the National Geographic Society, under the leadership of Mr. Neil M. Judd, archæologist of the Smithsonian Institution, in the course of its excavations has found an ancient arroyo completely filled up which has been traced for a considerable distance. Its original bed was found at a depth of 18 feet, and on it were fragments of pottery made during the third and greatest building period of the Pueblo. Further, it appears that the inhabitants used pine logs to roof the 800 rooms of their great dwelling. The nearest pine woods are at present forty miles away; but evidence was discovered that pine had at one time grown in the canyon. It may therefore be concluded that the present sterility of the soil was brought about by the destruction of the pine forests which once surrounded the site, baring the soil to wind and water and allowing the spring rains essential for the crops to drain off into the arroyo, which then became wider and deeper. The fertility of the fields then became less, and the population decreased owing to migration as the food supply diminished, until the twelve hundred inhabitants had dwindled to a few families. The increase in the number of defensive works in each successive building period suggests that marauding raids helped to drive the inhabitants from their homes. The excavations of 1926 show that there were four main periods of construction covering a period of about a thousand years. Differences in architecture and pottery indicate that two distinct populations in friendly relation inhabited the site at one time. Later arrivals of a more advanced culture made Pueblo Bonito the most famous and important settlement of the south-west, but they were the first to abandon the site.

DETERMINING THE SEX OF CARRIER PIGEONS.—I. Iwata (*Jour. Coll. Agr.*, Imp. Univ. Tokyo, vol. 73, No. 4, 1926) points out differences, which enable the sex of carrier pigeons to be distinguished at various stages of growth. In very young birds (from the time of hatching to the 24th day) the dorsal rim of the cloacal opening is more developed than the ventral in the male, while in the female the reverse is the case. In older birds, after they have left the nest to the time of pairing, the wall of the cloaca of the male exhibits six internal elevations, whereas in the female five elevations are present and the anus is seen close to the postero-median elevation. In general, too, the opening of the male cloaca is more tightly closed than that of the female. In adult birds (more than six months old) the cloacal opening of the male is usually pentagonal in outline, and at its centre is the anal opening, also usually pentagonal. The adult female cloaca presents a more or less triangular opening, the external skin at the posterior border of the cloaca projecting into the cloacal space along the short base of the triangle. By this time, however, the general appearance and behaviour of the birds is usually sufficient to indicate their sex. Another point of difference is that in birds more than 120 days old the male presents a whitish line along the median portion of the dorsal side of the beak, between the nasal protuberances, which is wanting in the female.

NEMATODES FROM JAPANESE FROGS AND TOADS.—K. Morishita records (in vol. 1, No. 1, Section 4 (Zoology), *Jour. Fac. Sci.*, Imperial Univ. Tokyo, 1926, which is a continuation of *Jour. Coll. Sci.*, Imp. Univ. Tokyo) eight species of nematodes found during the examination of about a thousand frogs and toads. These nematodes include a new species of Hedruris, the female of which adheres to the wall of the duodenum of frogs and the spiral turns of the body of the male envelop the female; a new species of Spiroxys also from the duodenum of frogs (the genus Spiroxys has hitherto been recorded from tortoises); and a new species of Spinitectus from the stomach of *Rana nigromaculata*, the four previously known indubitable species of Spinitectus having been found only in freshwater fishes of Europe and America.

GROUPING OF FORMS IN SCENERY.—In a paper in the *Geographical Journal* for November, Dr. Vaughan Cornish discusses the conditions of harmonious grouping in natural landscape and illustrates his theme from various aspects of scenery by land and sea. The reason for rearrangement, or composition, which the landscape painter adopts, arises largely from the fact that disharmonies of form occur in purely natural scenery, and the eyes find perfect satisfaction in the landscape only when there is a broad angle of outlook free from obtrusive disharmony. Dr. Cornish points out that the generally expressed opinion that purely natural surroundings are always harmonious is due to the co-operation of the senses. From descriptions of several mountain views he shows that ranges of the Alpine type provide the finest forms of mountain skyline. The roughly pyramidal peaks are of sufficient size to appear important at the greatest distance at which atmospheric conditions allow visibility. On nearer approach, the distant view of a row of pyramids gives place, as a rule, to a pyramidal complex bending upwards to a culminating peak: yet nearer, this vanishes and is replaced by a number of visually independent pyramids. The eye always finds harmony in the pyramidal form. Other features of scenery are discussed in this interesting paper.

Pleistocene Plants from North Carolina.—The study of Pleistocene plants has not made so much progress in America as in Europe, but Dr. E. W. Berry has recently made an interesting and important contribution to the subject (*U.S. Geol. Survey Prof. Paper* 140-*C*, 1926, p. 97). Beautifully preserved plant remains have been found in three (Wicomico, Chowan, Pamlico) of the five terrace plains which can be traced for long distances along the Atlantic coast of North Carolina and the adjoining States. The species recognised number 48, including two conifers, the remainder being Angiosperms; 11 are classed as extinct species new to science, several of these being regarded by the author as doubtful, but he feels confident that two or three are definitely new. The most abundant forms are the bald cypress, river birch, beech, various oaks. More than half are plants living in wet habitats, and may represent elements of the vegetation of estuary streams. None of the species can be regarded as a definitely northern form, though the terrace deposits contain boulders of considerable size which may have been brought down by river ice. On the other hand, the present distribution of some of the species suggests that the climate may have been somewhat milder than at present. The author considers that the present major floristic regions of North America were already well marked at the end of the Tertiary period, and he adds a map illustrating his views on the probable directions of post-glacial plant dispersal.

Upper Wind Variations.—A Memoir entitled "The Variance of Upper Wind and the Accumulation of Mass," by Mr. Lewis F. Richardson, Mr. Denis Proctor, and Mr. Robert C. Smith, vol. 1, No. 4, has just been issued by the Royal Meteorological Society (price 2*s*. 6*d*. to non-fellows of the Society). The memoir contains a collection of statistical facts concerning deviations of wind from its mean. The distances range from 1 km. to 100 km., deviations so large as or larger than gusts and smaller than cyclones. The effect of height on the variation is said to be very different in different circumstances, but on the average it is not much changed with height. Morning and afternoon in summer have different variances, that in the afternoon being usually much the greater, when the stations were 11 km. or 12 km. apart, but at 28·5 km. separation the distinction was not clear. The greater variance on summer afternoons is explained as an effect of the increased turbulence due to the decreased statical stability of the air. Other values relative to variance are given, and the memoir is of a high scientific order.

Recent Fundamental Work in Physics.—The address entitled "The Last Fifteen Years of Physics," which Prof. R. A. Millikan delivered at the annual meeting of the American Philosophical Society in April, is reproduced in Part 2 of the *Proceedings* of the Society for 1926. Sixteen of the twenty-one fundamental discoveries of the past thirty years fall within the last fifteen years, and there seems no sign of diminution of the rate of progress in the near future. Amongst the more recent discoveries may be mentioned that of isotopes by positive ray analysis, that of the excited atom, which Prof. Millikan thinks may be the foundation of a new era in science, the interpretation of the fine structure of spectral lines as due to changes in quantum numbers, the bridging of the gaps in the spectrum between electromagnetic and long heat waves and between ultra-violet light and X-rays, and the recent discovery that, by assuming every electron has unit angular momentum, we get an explanation of doublets to replace that previously provided—unsatisfactorily however—by an assumed change of mass of the electron with speed. His own discovery of cosmic rays of enormous power hitherto unknown—one of the most important and stimulating—gets an all too modest notice of nine lines.

Electrical Conductivity of Cyanogen Bromide.—According to the theory of 'pseudo atoms' put forward by Grimm, it is possible to regard atoms possessing the same total nuclear charge as similar. If this idea is true, the cyanogen radical is like the sodium atom, and therefore its fused salts should conduct electrically. This reasoning led G. Glockler to study the electrical conductivity of fused cyanogen bromide (m.p. 52°). Details of the preliminary experiments and an approximate value for the specific conductivity are given in the *Proceedings of the United States National Academy of Science* for August 1926.

Hydrion Concentration and Photographic Emulsions.—The effect on the sensitiveness of a gelatine emulsion of adding a little acid or alkali during its preparation has long been known in a general sense, and Messrs. S. O. Rawling and J. W. Glassett, of the British Photographic Research Association, have sought to find more exactly the effect of changing hydrion concentration during the washing and digesting stages (*Journal of the Royal Photographic Society*, November 1926). The range of *p*H values studied extends from 5 to 9. They find that with samples of gelatine having widely different photographic properties, and emulsions precipitated in the presence or absence of ammonia, the photographic sensitiveness obtained after digestion is greater as the hydrion concentration is increased. With any one emulsion, on prolonged digestion, a steady value is obtained and maintained for a time which is different for each different *p*H value employed. The grain size and appearance seem to be unaffected, and in most cases, if digestion is continued long enough, the development velocities and shape of the characteristic curve of the emulsion also are independent of the hydrion concentration during digestion.

Oxidation of Phosphorus Vapour.—In an examination of the oxidation of phosphorus vapour at low pressures, Chariton and Walta (*Zeit. für Phys.*, p. 547, October 1926) have found that, for a given vapour pressure of the phosphorus, there exists a definite critical pressure of oxygen below which no reaction takes place. They also found that this critical pressure was lowered by the addition of small quantities of argon, and at the same time the light emitted during the reaction was decreased in intensity. As a working hypothesis, the authors assume that the critical pressure of oxygen is that pressure above which each active centre is able to produce other active centres. Consequently, the argon atoms added may be considered to assist in the formation of active centres by means of 'collisions of the second kind,' and hence the reaction may proceed in the presence of less oxygen. It is found that when such centres are produced artificially, by electric discharge or by means of a glowing wire, the critical pressure of oxygen is lowered.

Atmospheric Electricity.[1]

THE director and staff of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington have of late years given much attention to atmospheric electricity, and have published several conclusions of much interest, the evidence for which is contained in the volume before us. Thus, although it includes a variety of other interesting matter — magnetic, meteorological, and instrumental—we shall confine our present remarks to the subject of the potential gradient of atmospheric electricity, a subject dealt with by Messrs Bauer, Ault, and Mauchly, especially the last mentioned.

Perhaps the most remarkable conclusion, due to Dr. Mauchly, is that re-stated on p. 402: " At least the greater part of the diurnal variation of the potential gradient over the oceans is due to a wave which progresses according to universal rather than local time. . . . All potential-gradient observations at sea tend to give values which are lower than the mean of the day if made in the forenoon of the Greenwich civil day, and values higher than the mean of the day if made in the afternoon of the Greenwich day."

The main evidence for this conclusion is a series of 24-hourly data from 59 Greenwich days, given on pp. 390-391. Details of the actual observations appear on pp. 212-265.

The natural way of checking Dr. Mauchly's conclusion would be to group the observations according to longitude. If we do so, taking 30° zones centering respectively at 0°, 30° E., etc., we find that there are only four zones—those centering at 180° E., 210° E., 240° E., and 270° E.—for which there are more than four days' observations. In an element so variable as potential gradient, uncertainties are sure to be large unless a considerable number of days is employed. Diurnal variations have thus been calculated only for the four zones stated, the 'days' available numbering respectively 9, 14, 9, and 10. Diurnal variations were also calculated for two groups each of five days from the 270° zone, and finally for the whole 42 days included in the four zones. Space allows us to mention only a few of the details, the hours referring to Greenwich civil time.

Zone.	Daily Mean.	Minimum.		Maximum.		Range.
E.	*v/m.*	h.	*v/m.*	h.	*v/m.*	*v/m.*
180°	128	2	112	20	152	40
210	106	3	93	19	136	43
240	124	6	100	15	156	56
270	133	0	113	19	165	52
270*a*	145	4	117	19	194	77
270*b*	122	23	101	12	160	59
42 days	121	3	105	19	148	43

The inequality derived from the 42 days accords with Dr. Mauchly's conclusion that potential gradient at sea tends to be below the mean in the Greenwich forenoon, and the times it gives for maximum and minimum, 19 h. and 3 h., are in general accord with his. But the 42 days results refer only to a limited part of the ocean, and the results from the different zones differ amongst themselves. In the case of the 270° zone the results from the two groups of days are widely different, and the inequality from the whole 10 days shows a prominent secondary maximum and minimum at 12 h. and 14 h. respectively.

When we look more closely into the data two unsatisfactory features appear. About 12 per cent. of the hourly values for the 59 days are interpolations; for the observations of 1921 the percentage is 21. In the majority of cases the interpolations represent arithmetic means from two observations separated by two hours, but in other cases they are of a more questionable character. For example, for 'day' 52 the earliest and latest observations available refer respectively to 18 h. on one Greenwich day and to 13 h. on the next. These appear as 13 h. and 18 h. on the same day, and data are interpolated for 14 h., 15 h., 16 h., and 17 h. as if this were the case. The probable existence of a progressive change in the 24 hours is neglected.

The second unsatisfactory feature is the total disregard of n.c. changes. Usually only 24 successive hourly values were obtained, and it is impossible to say what the n.c. change really was. So far as the run of the figures enables us to judge, it was usually very sensible, and sometimes very large. We have, for example, the following data assigned to three successive hours, 259 | 165, 184; 166 | 51, 53; 118, 139 | 266; 313, 300 | 150. In each case the entries which immediately precede and follow the vertical line represent respectively the latest and the earliest of the observations taken during the 'day.' If the hour at which the 'day' began was quite irregular, and if there were a very large number of days, uncertainties would naturally largely cancel out. But such cancellation cannot be assumed when the number of days is limited, and several of the inequalities, the results of which appear above, were apparently seriously prejudiced.

In the case of 210° E. longitude three n.c. discontinuities lead to an apparent excess of 160 *v/m* at 18 h. over 17 h., the excess in the aggregate from the whole 14 days being 182 *v/m*; and two n.c. discontinuities cause an apparent excess of 157 *v/m* at 19 h. over 18 h., the excess in the aggregate from the whole 14 days being only 92 *v/m*. Thus the maximum at 19 h. in the inequality from the 14 days may have been entirely due to the neglect of n.c. changes. As it so happens, these n.c. discontinuities in the case of the four zones considered were almost entirely confined to the (Greenwich) afternoon. Only one serious n.c. discontinuity appeared in the (Greenwich) forenoon. It led on 'day' No. 3 to an apparent fall of 98 *v/m* between 1 h. and 2 h., and was probably responsible for the hour at which the minimum occurred in the 180° zone. The effect of this discontinuity is not wholly negligible even in the case of the 42-day inequality. In spite of it, a remarkable feature stands out, namely, a close approach to constancy in the voltage from 1 h. to 6 h. G.M.T. Hourly means during the six hours vary only between 105·5 *v/m* at 3 h. and 108·7 *v/m* at 1 h. This suggests a diurnal variation, which is either very small or largely dependent on local time.

The results now obtained should not be regarded as disproving Dr. Mauchly's conclusions, but they unquestionably do suggest that further observations on somewhat different lines are required to justify any final conclusion. A term in the diurnal variation following universal time is not perhaps improbable *a priori*. The earth's surface varies notably in different zones, and there may be regions, *e.g.* the Himalayas, which exercise a dominant influence on the diurnal flow of the air-earth current on land. But even if a term depending on universal time exists, there may be even at sea terms of equal or greater importance depending on local time. The ideal arrangement to investigate the matter properly would be to have three similar ships—which need not be non-magnetic—with similar apparatus, observing simultaneously at three limited areas about the same latitude, the two extreme areas being about

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[1] Researches of the Department of Terrestrial Magnetism, vol. 5: "Ocean Magnetic and Electric Observations, 1915–1921." Washington, D.C. Published by the Carnegie Institution of Washington, 1926. Pp. vii+430, with 15 plates and 31 figures in the text.

180° apart, and the third midway. Each ship should observe in succession at each area for a month or two, as continuously as possible. In any case, an overlap of two or three hours should be secured for each 'day's' observations.

The relation of potential gradient to sunspots has been dealt with by Dr. Bauer in various recent papers which have been discussed in Nature. Dr. Mauchly refers on pp. 405-406 to the bearing of the ocean observations on this point. He says: "The mean values for each 3-month period . . . show throughout the years beginning with 1915, first an increase to 1916 or 1917, and then a gradual and consistent decrease to the end of 1921. This is so closely in accord with what has been observed at land stations, where reliable or undisturbed data of required extent are available, as to leave no doubt regarding the reality and universality of this phenomenon" (*l.c.* p. 405); and again: "in all latitudes for which there were sufficient observations . . . the mean values observed on cruise VI (mean epoch 1920·8) were from 15 to 20 per cent. lower than those observed in cruise IV. (mean epoch 1916·2)" (*l.c.* p. 406). These statements should, however, be taken in conjunction with the following two: "It should be noted that all potential gradient values shown in the graphs and tables of this report are of the order of 20 to 25 per cent. greater than those given in the author's earlier papers" (*l.c.* p. 397); and "At that time (1920), owing to various causes brought on by the War, there had been no final determinations of the instrumental constants to be used for reducing to absolute values the results of the . . . observations made aboard the vessel" (*l.c.* p. 387). Apparently all the observational data depend on reduction factors got out during expedition No. VI., and these are apparently assumed to have been unchanged since 1915. Those having experience in these matters may perhaps be pardoned a doubt whether the factors used in Dr. Mauchly's earlier papers were so much in error as he now supposes.

The general subject of sunspot influence is considered in more detail by Dr. Bauer himself, pp. 361-384. The following statement (*l.c.* p. 381) embodies his present opinions: "The general conclusion from the investigations based on land and ocean results . . . is to indicate with a high degree of probability that during the cycle of 1913–1922 the atmospheric potential gradient increased with increasing sun-spottedness by at least 20 per cent. of its mean value for the cycle between the years of minimum and maximum sun-spottedness. The same statement applies with regard to measures of the diurnal variation and of the annual variation of the potential gradient." These conclusions are based on the results obtained by the *Carnegie* at sea and on the published data of the Ebro, Eskdalemuir, and Kew observatories. The methods employed seem practically the same as in Dr. Bauer's earlier papers on the subject. No notice seems to be taken of the criticisms which have appeared in Nature, and no further vindication is supplied of the omission of the results from Potsdam, which gave a small decline in potential gradient with increased sun-spottedness. The reason assigned for disregarding Potsdam is alleged uncertainty as to the reduction factor. But uncertainty on this ground has been denied by the Potsdam authorities, and it seems scarcely likely *a priori* to have been more serious than in the case of the ocean observations which Dr. Bauer seems to have no doubts about.

As considerable use is made of the amplitudes and phase angles of Fourier coefficients by both Dr. Bauer and Dr. Mauchly, it may not be amiss to point out that the calculation of these coefficients from data in which large n.c. changes exist and have not been eliminated is unsatisfactory from the mathematical point of view. Unless the n.c. change is very small as compared with the daily range the effect of its neglect may be serious, especially in the case of the waves of least amplitude.

C. Chree.

The Formation of Lactic Acid in Muscle.

A very important advance in our knowledge of this fundamental process has been made by Prof. Meyerhof in continuation of his well-known researches on the subject. Last January he announced (in *Die Naturwissenschaften*, 14, Heft 10) that he had succeeded in extracting the enzyme responsible for the production of lactic acid from carbohydrates in muscle, and gave an account of its properties. He has now carried matters a step further (*Die Naturwissenschaften*, 14, 32) and has been able to penetrate much more deeply into the mechanism of the change. The enzyme, obtained by extracting the finely divided muscle with isotonic potassium chloride solution at - 1° to - 2° and then centrifuging, readily forms lactic acid from starch and glycogen at a rate about two-thirds of that of the spontaneous formation of the acid in minced muscle. It is separated by ultra-filtration into a heat-labile inactive residue and a stable filtrate—the previously known coenzyme—which reactivates this residue. The coenzyme can be obtained either from muscle or yeast, and its addition greatly increases the amount of lactic acid producible from an excess of glycogen. The hexoses and the disaccharides are scarcely attacked, whereas lactic acid is freely formed, and at nearly the same rate, from glycogen, starch, and various degradation products of starch.

Hexosediphosphoric acid, the monophosphoric ester obtained from it by partial hydrolysis (Neuberg), and the isomeric monophosphoric ester from yeast juice (Harden and Robison) are all attacked. This decomposition of hexosephosphoric acid into equivalent amounts of lactic acid and phosphoric acid is, however, independent of the presence of the coenzyme which is necessary for the decomposition of glycogen and starch.

The production of lactic acid, either from glycogen or glucose, is preceded by the formation of a hexosephosphoric acid, and the reason why glycogen is the more easily acted on appears to be that a reactive hexose is formed from it which is more readily esterified than glucose in its stable form.

Meyerhof, hoping to be able to accelerate the action on the hexoses, turned to yeast—that unfailing storehouse of physiological surprises—and met with astonishing success. By precipitating autolysed yeast with 50 per cent. alcohol he obtained a new activator—different from zymase, coenzyme, and insulin—which, when added to the mixture of muscle enzyme and the hexoses, increases the rate of lactic acid formation until it amounts to several times that of its production from glycogen and 10 times that of the spontaneous production of lactic acid in minced muscle. The decomposition of glycogen, on the other hand, is scarcely affected.

At the same time the esterification of the phosphoric acid is still more accelerated, and the result is that during the first 15-30 minutes there is a rapid accumulation of hexosephosphoric acid, which continues so long as both hexose and free phosphoric acid are present. The rate of production of the acid then falls rapidly, and is followed, when the supply of both hexose and phosphoric acid is exhausted, by a stage in which a slow production of lactic acid and phosphoric acid in equivalent amounts takes place. The hexosephosphoric acid formed is approximately equivalent in amount to the amount of lactic acid produced.

This state of affairs presents, as will be seen, the most complete analogy to what occurs in alcoholic fermentation, with the difference that in one case the product is lactic acid and in the other alcohol and carbon dioxide. There is the same great acceleration in the rate of the reaction, accompanied by the accumulation of hexosephosphoric acid, the same drop in the rate when the free phosphoric acid is exhausted, and the same succeeding slow rate of change. As in the case of alcoholic fermentation, fructose reacts much more rapidly than glucose. There seems to be no doubt that the same fundamental change is in progress in the two cases.

Meyerhof believes that the hexosephosphoric acid produced is converted whilst *in statu nascendi* into the final products—lactic acid or alcohol and carbon dioxide (or some precursor of these) and phosphoric acid. As, however, the rate of esterification is greater than that of decomposition, some of the phosphoric ester becomes stabilised in the less reactive form in which it can be isolated.

In the intact yeast cell and the natural muscle there is no accumulation of hexosephosphoric acid, " probably because, as the result of an accurate co-ordination, the whole of the labile ester produced is fermented, and not only about one-half of it, as in the extracts."

This explanation scarcely accounts for the approximate equivalence between the amounts of hexose decomposed and converted into the stable ester, and there is doubtless more yet to be learned on this point.

The striking similarity established by Meyerhof between the changes of carbohydrates in muscle and in the yeast cell is seen to be much closer than has been believed. The remarkable phenomena accompanying alcoholic fermentation are now duplicated in the case of lactic acid production, and it may reasonably be expected that most of the fermentative decompositions of the sugars will be found to be initiated in a similar manner.

It is a great achievement to have pushed the analysis of the production of lactic acid so far, and further results of this line of investigation will be awaited with the greatest interest.

ARTHUR HARDEN.

A Royal Collection of Scientific Instruments.

AN interesting historical document is described by Mr. Robert S. Whipple in " An Old Catalogue and what it tells us of the Scientific Instruments and Curios collected by Queen Charlotte and King George III." (Reprinted from the *Proceedings of the Optical Convention, 1926*. Part 2.) The catalogue, which has been preserved at the Kew Observatory, was prepared in 1770 or a few years later. A reproduction of the first page of the catalogue is given, the complete catalogue (319 entries), and a " Catalogue of Presents by Sundry Persons" (77 entries) being printed as an appendix to the paper.

Whilst the first catalogue is confined wholly to scientific apparatus and instruments, a large proportion being of the educational or teaching type, the ' presents ' are mainly of the ' natural history ' type, including such objects as ' a dried cat,' ' a lizzard in spirits,' ' fosils from Virginia,' ' two Batavia rats,' ' two spiders from Bardadoes,' etc. In the latter list the names of the distinguished donors are given.

Before describing the contents of the collection, Mr. Whipple gives an account of the foundation of the King's Observatory at Kew for the purpose of observing the transit of Venus in 1769. Some of the instruments included in the catalogue were used in the observations, and the original notes (preserved in the Library of King's College, London) by Dr. Demainbray recording the observations are published, it is believed for the first time. King George III. acted as an assistant during the observations. Queen Charlotte and the Princes Ernest and George of Mecklenburg-Strelitz were present, and others taking part were Col. Desaguliers, Rev. Geo. Wollaston, Stephen Rigaud, Justin and Benjamin Vulliamy, Jeremiah Sisson, and John Cuff.

Full details are given of the instruments used and of the observations made. It is interesting to read that " His Majesty the King who made his Observation with a Short's Reflecting Telescope, magnifying Diameters 170 Times, was the first who saw the Penumbra of Venus touching the Edge of the Sun's Disk." There is a note afterwards to the effect that " his Majesty thinks he saw it before he gave his Signal to Doctor Demainbray, who attended at the Regulator. And that Mr. Sisson (fearful of giving a false Alarum) waited an Instant before he caused Mr. Cuff to ring the Bell."

The collection of instruments and apparatus as specified in the Queen's Catalogue, with other objects afterwards acquired, remained as a national collection in the Kew Observatory until 1841, when the Government decided to discontinue the maintenance of the Observatory. Mr. Whipple gives in detail the circumstances leading to this decision, and the consequent distribution of the collection to various institutions and individuals.

Some of the instruments and apparatus which went to King's College, with a request that " they should be preserved together as a whole" (forming the George III. Museum), are described and illustrated. The present locations of some of these, as well as of instruments which went to other institutions, are given.

Of the larger instruments included in the Catalogue, perhaps the best known to the general public are the large eight-feet mural quadrant by Jeremiah Sisson, and the observatory clock by Benjamin Vulliamy. These have been exhibited for many years in the Science Museum at South Kensington. Of the transit instrument, which went to Armagh Observatory, only the object-glass and the mahogany stand for lifting and reversing the telescope are still in existence, the object-glass being used at present for testing planes. The lens and some parts of the five-feet " astronomical sector in dome," as well as the six-inch telescope (probably that used by the King in 1769) made by Short in 1745, are still preserved at Armagh.

Mr. Whipple gives interesting evidence of the circumstances in which the valuable old microscopes were acquired by the late Sir Frank Crisp from the George III. Museum. The King's College authorities, though aware of the great historical value of the instruments, agreed to the transference only after much pressure, and " having been given the assurance that the Crisp collection was destined for the nation." Mr. Whipple adds: " The fact that the assurance was not fulfilled illustrates the importance of entrusting instruments or other objects of historical interest to a National Museum."

Since this interesting and valuable paper was read at the Optical Convention last April, the George III. collection, which was then still at King's College, has been transferred on loan to the Science Museum.

D. B.

University and Educational Intelligence.

Cambridge.—A valuable bequest of works of art, including a collection of the finest Italian majolica, has been left to the Fitzwilliam museum along with 10,000*l.*, as an endowment for the purchase of works of art, by the late Right Hon. F. Leverton Harris, of Gonville and Caius College.

Dr. R. R. Marett has been appointed to the Frazer lectureship in anthropology.

It is proposed to abandon the examinations for the diploma in horticultural science, it having proved impracticable to acquire adequate facilities for the teaching.

Mr. E. G. D. Murray has been elected to a research fellowship at Christ's College.

Sydney.—The following appointments are announced: Mr. John Anderson (Edinburgh) to be professor of philosophy in succession to the late Prof. B. Muscio; Dr. W. S. Dawson, of the Maudsley L.C.C. Hospital, Denmark Hill, London, to be professor of psychiatry in succession to Sir John Macpherson; Mr. H. Finnemore (Guy's Hospital Medical School), to be lecturer in pharmacy in succession to Mr. S. H. Stroud; Dr. T. Iredale, of Armstrong College, Newcastle-on-Tyne, to be lecturer in chemistry, in succession to Prof. J. A. Schofield.

The University College of North Wales, Bangor, announces in its Calendar for 1926-27 the opening of new War Memorial Laboratories for all the science departments, excepting applied electricity. This development was rendered possible by the help of the Council of the North Wales Heroes' Memorial Fund, the total subscriptions to which, including an initial gift in 1917 of 20,000*l.* by Sir R. J. Thomas, amount to nearly 100,000*l.* The interesting historical summary given in the preface to the Calendar recalls the remarkable unanimity with which all classes of the community contributed to the establishment of the College forty years ago. Never before, it says, in so short a period, have so many persons, either in England or in Wales, subscribed towards a movement for the promotion of higher education. In the first twelve months more than 30,000*l.* was subscribed by nearly 8000 donors. The total sum subscribed towards the College since its establishment is 407,735*l.* One of the two main types of approved schemes of study for the B.A. (pass) degree is known as the scheme of civic studies, the general aim of which is to emphasise the more definitely humanistic side of the subjects in the arts curriculum and cultivate a more intelligent appreciation of national life.

The directors of the Commonwealth Fund of New York City, which was founded in 1918, awarded in 1925 and 1926 a number of Commonwealth Fund Fellowships to British graduates for study in selected American universities. Entries last year numbered 178, and the committee of award states that the general level of attainment was higher than that of 1925. A maximum of twenty fellowships are awarded each year, and candidates must be of British birth who are domiciled in Great Britain or Ireland, and less than thirty years of age. Personality, health, and character are taken into account as well as intellectual ability. With the exception of purely clinical work in medicine, which is not considered likely to be promoted by the type of travelling fellowship offered, there is no limitation as to subject of study. In 1927, for the first time, the committee of award will be empowered to appoint three " Extra Fellowships," which are intended for candidates from British Dominions who have studied in British universities or such as hold appointments in Colonial administrative or educational service. The annual value of a Commonwealth Fund fellowship is about 600*l.* Further information can be obtained from the secretary of the Committee, Commonwealth Fund Fellowships, 50 Russell Square, London, W.C.1.; applications on the prescribed forms must be completed and reach him by February 19.

The report of Battersea Polytechnic for 1925–26, though registering a slight increase in comparison with the preceding year's record of work done, directs attention to the fact that its valuable plant would be more fully utilised but for the raising of the fees against residents of Surrey (Wimbledon and Croydon), owing to which the technical college has suffered a continuous decline in the number of its day students since 1920–21. Judged by the list of academic honours gained in 1925–26, the Polytechnic has achieved very considerable success in the field of higher education in science. It includes three doctorates, three degrees of M.Sc., and twenty-five B.Sc. In the Chemistry Department a substantial amount of original research work was done, and led to the three doctorates and to the Meldola medal awarded by the Institute of Chemistry. Among the subjects in which there are specialist courses in this Department are paper testing and making, chemical engineering, soaps and detergents, mineral oils and waxes, catalytic hydrogenation and secondary constituents of plants. The Department of Hygiene and Public Health, which trains candidates for such posts as health visitor and sanitary inspector, has expanded largely in the past three years, as has likewise the Training College of Domestic Science, now the largest of its kind in Great Britain, which sends out teachers and organisers to all parts of the Empire and to foreign countries. For some time past all day students, in whatever department of the Polytechnic, who have satisfactorily completed the full course, have obtained employment.

" University extension " in the United States includes (1) instruction in agriculture and home economics provided with the aid of Federal subsidies through the land-grant colleges, and (2) very varied activities of universities and colleges in other fields to which the name " General University Extension " has been applied. A report on this latter group of activities by the Dean of Extension of the University of Texas has been published by the Bureau of Education as Bulletin, 1926, No. 5. The progress of extension of leading universities impels the writer of the report to the conclusion that " before many decades have passed all Americans, both old and young, will have the advantages of university training and university service at their very doors." President Birge, of the University of Wisconsin, the pioneer in America in extension work, holds that it is essential not only for individual progress on the part of students, but equally " as a means of preventing the crystallisation of social groups." Among the extension activities which have shown exceptional development in the past two years are radio courses. It has been proved that courses of instruction in foreign languages, social sciences, etc., can easily be given by this method, which combines the lecture with written reports. One university enrolled 1636 radio-course students belonging to more than half the States and to Canada, the enrolment fee being one dollar, and entitling the student to receive mimeographed study material. Some of the other branches of extension work are described as community drama, home-reading courses, graduate medical lectures, labour education, community institutes. The total number of students enrolled in extension and correspondence courses in 1923–24 was 141,000.

Contemporary Birthdays.

December 17, 1853. M. Émile Roux, For.Mem.R.S.
December 17, 1861. Mr. Edward Heron-Allen, F.R.S.
December 18, 1856. Sir J. J. Thomson, O.M., F.R.S.
December 19, 1852. Prof. A. A. Michelson, For.Mem.R.S.
December 20, 1876. Dr. Walter Sydney Adams.
December 22, 1862. Dr. Vaughan Cornish.

DIRECTOR of the Pasteur Institute, Paris, a foreign member of the Royal Society of London, and Copley medallist, Dr. Émile Roux was born at Confolens. Early in his career he worked in Pasteur's laboratory, and in the course of time became his collaborator in pathological research. In 1889 Roux delivered, on behalf of M. Pasteur, whose health did not allow attendance, the Royal Society's Croonian lecture, on the subject "Les Inoculations Préventives."

Mr. E. HERON-ALLEN is a Londoner, and was educated at Harrow. He has written many papers on the Foraminifera, including one published in the *Philosophical Transactions* (1915), entitled "Bionomics of the Foraminifera." In 1916–18 Mr. Heron-Allen was president of the Royal Microscopical Society. He has done original work in many departments of natural history, and is, in addition, an authority on Persian literature.

Prof. MICHELSON was born at Strelno, Germany. Entering and graduating at the U.S. Naval Academy, he joined the Nautical Almanac Office, Washington; afterwards he studied at the Universities of Berlin, Heidelberg, and the École Polytechnique, Paris. From 1889 until 1892 he was professor of physics at Clark University, Worcester, Massachusetts, leaving to become professor and head of the Department of Physics in the University of Chicago. A foreign member of the Royal Society of London, he was awarded the Copley medal in 1907. In that year he was also Nobel laureate in physics. Prof. Michelson was a pioneer in the construction of interferometers, and his optical inventions have rendered possible the reproduction of accurate metric standards, which are now widely used. He has received many foreign recognitions; he is Hon. Sc.D., Cambridge, and an associate of the Royal Astronomical Society, which awarded him its gold medal in 1923. Prof. Michelson was added to our roll of "Scientific Worthies" in NATURE of January 2, 1926.

Dr. W. S. ADAMS was born at Antioch. He was sent to Dartmouth College, Hanover, U.S.A., and afterwards to the University of Chicago. Appointed an assistant at Yerkes Observatory, he removed in 1901 to fill a similar post at Mount Wilson Observatory, California, becoming Director in 1923. A member of the National Academy of Sciences, Washington, he is one of its Draper medallists. The Royal Astronomical Society awarded Dr. Adams its gold medal in 1917 for his investigations in stellar spectroscopy. Last year his observations on the spectrum of the companion of Sirius enabled him to confirm Einstein's prediction of the gravitational displacement of spectral lines, and also Eddington's calculations of the very high density of white dwarf stars.

Dr. VAUGHAN CORNISH, a zealous geographer, was born at Debenham, Suffolk. He was educated at St. Paul's School and the Victoria University, Manchester. In 1900 the Royal Geographical Society allotted him the Gill Memorial Award for extended researches on sea-beaches, sand-dunes, and wave-form in water. President of Section E (Geography) at the Liverpool meeting of the British Association in 1923, he gave an address on the British Empire regarded as a maritime organisation. He is the author of "The Great Capitals: an Historical Geography" (1923).

Societies and Academies.

LONDON.

Optical Society, November 11.—J. W. T. **Walsh** and W. **Barnett**: The effect of slightly selective absorption in the paint used for photometric integrators. A sensibly non-selective internal coating for photometric integrators is very difficult to produce and still more difficult to maintain. When lamps of different colour temperatures are compared in an integrator with an internal coating which shows selective absorption in, say, the blue, the values of candle-power obtained for the lamps of lower colour temperature will be too high, and vice versa. A simple method is given for calculating the magnitude of the effect for sources having a spectrum approximating to that of a black body. In work on normal type electric lamps, to an accuracy of 1 to 2 per cent., a quite noticeable coloration of the light may be produced by the sphere (either on account of paint or window selectivity or both) without the necessity for making any correction to the measured values of candle-power.—Conrad **Beck**: An accurate method of ascertaining the position of the focal point of an optical system. The method consists essentially in placing a diaphragm with two slit apertures behind the object-glass to be tested, the directions of the slits being at right angles to one another, and finding the position where the images form a symmetrical cross. Results of measurements by this method of the zonal aberrations of apochromatic microscope object-glasses are given.

PARIS.

Academy of Sciences, November 15.—Georges **Perrier**: The regular triangulation of Morocco. Outline of geodesic work done in Morocco since 1910, with key map.—F. E. **Fournier**: Effects useful or detrimental to the velocity of ships.—Charles **Moureu**, Charles **Dufraisse**, and Marius **Badoche**: Autoxidation and antioxygen action (XX.). Catalytic actions of a new series of nitrogen compounds. General observations on the nitrogen compounds. Details of the study of the catalytic properties of thirty-seven new nitrogen compounds.—H. **Douvillé**: The marbles of Sarrancolin and of Saint-Béat in the Central Pyrenees.—Jean Baptiste **Senderens** and Jean **Aboulenc**: The etherification of the hydroaromatic alcohols. Cyclohexanol, heated with 2 per cent. sulphuric acid (concentrated or diluted) does not give cyclohexyl oxide, but a mixture of cyclohexene and its polymer. Similarly, a mixture of cyclohexanol and a fatty alcohol fails to give a mixed ether. On the other hand, the hydroaromatic alcohol and an aromatic alcohol under the same conditions gives a mixture containing benzyl ether and the mixed ether.—Camille **Sauvageau**: The development of *Colpomenia sinuosa*.—A. **Calmette**, J. **Valtis**, and M. **Lacomme**: The intra-uterine transmission of the tubercle virus from mother to infant. In the course of some grave tuberculous infections, the passage of the tubercle virus from mother to fœtus during gestation is less exceptional than has hitherto been supposed.—Bertrand **Gambier**: The deformation of surfaces and the method of Weingarten.—E. **Goursat**: Observations on the preceding communication.—**Potron**: The fundamental theorems of the theory of finite continuous groups of transformations.—R. **Wavre** and A. **Bruttin**: A continuous transformation and the existence of an invariant point.—J. **Delsarte**: Rotations in functional space.—E. F. **Collingwood**: A theorem on integral functions.—S. **Saks**: The differentiation of the area of surfaces.—G. **Vranceanu**:

Non-holonomial spaces.—Jean **Chazy**: The field of gravitation in the interior of a hollow sphere in rotation in the relativity theory.—H. **Beghin** and P. **Monfraix**: The realisation of a damped zenithal gyrostatic compass.—R. **Swyngedauw**: The slipping of belts.—J. F. **Saffy**: The influence of prolonged maintenance, at a red heat, on the resilience of some metals used for exhaust valves. Measurements of resilience of four samples of chrome-steel of different composition are given. These steels had been submitted to a temperature of 600° (in one case to 850°) for varying periods (maximum twelve weeks), before taking the measurements. An austentitic chrome-nickel steel retained a practically constant resilience, even after twelve weeks and at 850° C. This steel is markedly superior to other steels for use in valves of internal combustion engines.—R. **Mazet**: The formation of liquid jets.—Robert **Lévi**: The theory of universal and discontinuous action.—**Sauger**: Energy extensions of the Newtonian potential.—A. **Guillet**: The value of the constant of time most favourable to the direct electrical maintenance of an oscillatory movement.—L. **Caignard**: The variation of the dielectric constant of benzene with pressure. The high frequency oscillations used in the measurements had a wave-length of 6425 metres, the electric field applied to the dielectric 650 volts per cm., maximum pressure, 22 atmospheres. At constant temperature the dielectric constant is a linear function of the pressure.—G. **Gamow**: L. de Broglie's theory of waves of phases.—C. **Mihul**: The structure of the spectrum of oxygen of the second order.—D. K. **Yovanovitch** and Al. **Proca**: The slow β-rays of mesothorium-2.—Maurice **Lecat**: Azeotropism, particularly of binary systems with closely chemically related constituents. An attempt to measure the tendency of two substances to form a mixture of constant boiling point, with special reference to the chemical nature of the two constituents. Numerical data are given for twenty-eight binary mixtures forming constant boiling mixtures.—F. M. **Ostroga**: Chrome-cobalt steels. From the point of view of the influence of the temperature of heating and velocity of cooling on the constitution of these alloys, the chrome-cobalt steels show a sensibility at least equal, if not superior, to that of chrome-nickel high-speed steels.—J. **Cournot** and R. **Pages**: Studies of the viscosity of copper and its alloys.—E. **Tassilly** and R. **Savoire**: The spectrophotometric determination of nitrites and nitrates by diphenylamine sulphate. With the instrument used (Ch. Féry spectrophotometer) the useful range is from 0·1 to 5 milligrams of N_2O_3, with a possible error of 0·1 mgm.—J. A. **Le Bel**: The stereochemistry of the ethylene derivatives. The experimental results in a recent note by Dufraisse and Gillet afford a confirmation of the theoretical views of the author.—**Lespieau** and **Deluchat**: 1,7-Octadiine. This hydrocarbon, $\mathrm{HC}\vdots\mathrm{C}-(\mathrm{CH}_2)_4-\mathrm{C}\vdots\mathrm{CH}$, has been isolated, starting with dibromobutene, and its compound with silver nitrate prepared.—E. **Raguin**: The presence, to the north-east of Vanoise, of schists assimilable to those of the Tertiary Flysch.—Octave **Mengel**: An apparent analogy between the Alpino-Dinaric and Pyreneo-Iberic border. Common origin of the Alpine lakes and ancient Pyrenean lakes.—L. **Picard**: The Cenomanian of Carmel (south-east of Haïfa).—Henri **Coupin**: The rôle of fleshy pericarps.—E. **Chemin**: A new species of Colaconema on *Asparagopsis lamifera*.—René **Souèges**: The embryogeny of the Papaveraceæ. The development of the proembryo in *Papaver Rhœas*.—E. **Chemin** and R. **Legendre**: Observations on the existence of free iodine in *Falkenbergia Doubletii*. Free iodine has been found by M. Sauvageau in *F. Doubletii* collected at Guéthary and at Cherbourg. The authors have examined specimens from Brest and from Glénans, but no free iodine could be detected. Weak acids, even carbonic acid, set free iodine, which hence would appear to be present as a labile compound other than iodide. O. **Munerati**: Observations on the seed yield of beetroot in the first year.—Lucien **Daniel**: Intermittent heredity in the Jerusalem artichoke.—Adrien Davy de **Virville**: The influence of submersion on the mode of development of a moss: *Aulacomnium androgynum*.—J. **Dubar** and G. **Thieulin**: An attempt at the determination of the static refraction of the eye of the dog and cat.—Mme. Marguerite **Lwoff**: A mode of asexual reproduction in a Hydra of the family of the Tubularidæ.—Louis **Roule**: The displacements of *Orcynus thynnus* (or *Thunnus thynnus*) in the western basin of the Mediterranean.—L. **Lutz**: The soluble ferments secreted by the Hymenomycetes fungi: simple antioxygen actions.—V. **Omeliansky** and Mlle. M. **Kononoff**: A method of culture of the bacillus causing the retting of flax.—N. **Bezssonoff**: The true specific régime of experimental scurvy. Heated milk contains an appreciable proportion of vitamin C, and this is the cause of its varying effects on guinea-pigs. An alternative diet is suggested which gives more definite results.—L. **Ambard** and F. **Schmid**: The treatment of respiratory syncope by inhaling carbon dioxide. An arrest of respiration in a dog produced by a dose of chloralose, on which artificial respiration was without effect, was immediately removed by causing the animal to inhale a mixture of air and carbon dioxide. It is suggested that in many cases this method possesses advantages over artificial respiration.

VIENNA.

Academy of Sciences, October 21.—A. **Kailan**: The chemical effects of the penetrating radium radiation. The rays from 110 milligrams of a radium preparation were passed through 1 mm. of glass and allowed to act for more than 1000 hours on uric acid, on pyridin, on anilin, on nitrobenzol. Slight changes were noticed.—F. **Hölzl**: Organic acids and bases in non-aqueous solution. Conductivity measurements were made.—S. I. **Mayr**: Free albumin crystals in the endosperm of the seeds of *Loranthus europaeus*.—R. **Weiss** and W. **Knapp**: Triphenylmethanes the benzene rings of which are interconnected.—S. **Loewe** and E. **Spohr**: Detection and determination of contents of the female oestral hormone in the female organs of the vegetable kingdom. Experiments were made on mice and rats with extracts prepared from the flowers of the yellow water-lily and from willow catkins.—P. **Ludwig**: Tensile strength, cohesion, and danger of fracture. The fundamental factor in brittleness depends on the ratio of rigidity of form to tensile strength.—R. **Rotter**: The condensation of unsaturated compounds with diazomethane. Condensation of diazomethane with carbon disulphide and xyloquinone.—R. **Seka** and H. **Sedlatschek**: Complex compounds of pyromellithic acid anhydride.—G. **Klein**: Assimilation of nitrates by moulds.—N. A. **Puschin** and B. **Vaic**: Equilibrium in binary systems which contain guaiacol as one of the constituents.

October 28.—F. **Heritsch**: The tectonic 'window' [a break in a sheet-fold or overthrust layer, through which lower rocks are exposed] at Felsbach.—R. **Seka** and K. **Sekora**: Reduction products of dinaphtanthracene-diquinone.

Official Publications Received.

British and Colonial.

Aeronautical Research Committee: Reports and Memoranda. No. 1014 (Ae. 217): An Extension of the Vortex Theory of Airscrews with Applications to Airscrews of small Pitch, including Experimental Results. By C. N. H. Lock, H. Bateman and H. C. H. Townend. (A. 3. d. Airscrews, 71 and 86.—T. 1867.—T. 2112.) Pp. 40+9 plates. 1*s.* 6*d.* net. No. 1018 (Ae. 219): Experiments on the Air Forces on Rotating Cylinders. By Dr. A. Thom. (D. 1. Special Technical Questions, 121.—T. 2023.) Pp. 8+15 plates. 6*d.* net. No. 1036 (M. 45): The Constitution and Age-Hardening of some Ternary and Quaternary Alloys of Aluminium containing Nickel. By Kathleen E. Bingham. Work performed at the National Physical Laboratory for the Engineering Research Board of the Department of Scientific and Industrial Research. (B. 1. a. Metals, 54.—T. 2250.) Pp. 21. 1*s.* net. No. 1039 (Ae. 228): Stresses in a Stiff-Jointed Polygonal Frame under a System of Loads Perpendicular to the Plane of the Frame. By J. F. Baker. (B. 2. 9. Strength and Design. General 70.—T. 2220.) Pp. 9+7 plates. 9*d.* net. (London: H.M. Stationery Office.)

Department of Commercial Intelligence and Statistics, India. Agricultural Statistics of India, 1923–24. Vol. 2: Area, Classification of Area, Area under Irrigation, Area under Crops, Live-Stock, and Land Revenue Assessment in certain Indian States. Pp. viii+87. (Calcutta: Government of India Central Publication Branch.) 1 rupee; 1*s.* 9*d.*

Transactions of the Royal Society of Edinburgh. Vol. 55, Part 1, No. 4: The Development of the Vascular System in the Human Embryo prior to the Establishment of the Heart. By Dr. Donald M'Intyre. Pp. 77-113+3 plates. 5*s.* 6*d.* Vol. 55, Part 1, No. 5: Development of Cavia; Implantation. By Dr. Norman Maclaren. Pp. 115-123+3 plates. 2*s.* 6*d.* (Edinburgh: Robert Grant and Son; London: Williams and Norgate, Ltd.)

Memoirs of the Department of Agriculture in India. Chemical Series, Vol. 8, No. 6: The Determination of Available Phosphoric Acid of Calcareous Soils. Part i: Inapplicability of Dyer's Method to Highly Calcareous Soils; Part ii: Extraction of Phosphoric Acid of Calcareous Soils with Salt Solutions; Part iii: Potassium Carbonate Method for Estimation of Available Phosphoric Acid of Highly Calcareous Soils. By Surendralal Das. Pp. 67-104. (Calcutta: Government of India Central Publication Branch.) 12 annas; 1*s.* 3*d.*

Hull Museum Publications, No. 143: Notes on Yorkshire Ammonites. By Dr. L. F. Spath. Pp. 72. (Hull.) 1*s.*

Aeronautical Research Committee: Reports and Memoranda. No. 1030 (Ae. 223): Experiments with a Family of Airscrews, including Effect of Tractor and Pusher Bodies. Part 4: On the Effect of placing an Airscrew in various Positions within the Nose of a Streamline Body. By H. Bateman, H. C. H. Townend and T. A. Kirkup. (A. 3. d. Airscrews, 88.—T. 2218.) Pp. 27+11 plates. 1*s.* 3*d.* net. No. 1034 (Ae. 227): The Efficiency of an Airscrew. By H. Glauert. (A. 3. d. Airscrews, 93.—T. 2285.) Pp. 11+6 plates. 9*d.* net. No. 1040 (Ae. 229): The Accuracy of the Vortex Theory of Airscrews in the Light of recent Experimental Work and its Application to Airscrew Design. By H. Glauert and C. N. H. Lock. (A. 3. d. Airscrews, 91.—T. 2271.) Pp. 16+9 plates. 9*d.* net. (London: H.M. Stationery Office.)

Quarterly Journal of the Royal Meteorological Society. Edited by a Committee of the Council. Vol. 52, No. 220, October. Pp. 351-443. (London: Edward Stanford, Ltd.) 7*s.* 6*d.*

Canada. Department of Mines: Victoria Memorial Museum. Museum Bulletin No. 41, Biological Series No. 10: Birds of Western Canada. By P. A. Taverner. (No. 2053.) Pp. ii+380+84 plates. (Ottawa: F. A. Acland.) Paper, 75 cents; cloth, 1 dollar.

Biological Reviews and Biological Proceedings of the Cambridge Philosophical Society. Edited by H. Munro Fox. Vol. 2, No. 1, November. Pp. 90. (Cambridge: At the University Press.) 12*s.* 6*d.* net.

The National Institute of Agricultural Botany. Seventh Report and Accounts, 1925–26. Pp. 20. (Cambridge.)

Proceedings of the Prehistoric Society of East Anglia. Edited by G. Maynard. Vol. 4, Part 2 (for 1923–24). Pp. xii+133-247. 10*s.* net. Vol. 5, Part 1 (for 1925). Pp. xi+90. 10*s.* net. (Ipswich: W. E. Harrison.)

Imperial Department of Agriculture for the West Indies. Report on the Agricultural Department, St. Vincent, for the Year 1925. Pp. iv+44. (Trinidad, B.W.I.) 6*d.*

Livingstone College. Annual Report and Statement of Accounts for the Year 1925–26. Pp. 24. (Leyton, London, E.10.)

Imperial Department of Agriculture for the West Indies. Report on the Agricultural Department, St. Lucia, 1925. Pp. iv+33. (Trinidad, B.W.I.) 6*d.*

Tide Tables for the Pacific Coast of Canada for the Year 1927: including Fuca Strait, the Strait of Georgia, and the Northern Coast; with Data for Slack Water in the Navigable Passes and Narrows and Information on Currents. Issued by the Tidal and Current Survey Branch of the Hydrographic Survey, in the Department of Marine and Fisheries of the Dominion of Canada. (Twenty-seventh Year of Issue.) Pp. 72. (Ottawa: F. A. Acland.)

The Royal Technical College, Glasgow. Annual Report on the One Hundred and Thirtieth Session adopted at the Annual Meeting of Governors held on the 19th October 1926. Pp. 79. (Glasgow.)

Aeronautical Research Committee: Reports and Memoranda. No. 1028 (M. 43): Report on Study of Mechanical Properties of Silicon-Aluminium Alloys, Parts 1 and 2. By J. D. Grogan. Work performed for the Engineering Research Board of the Department of Scientific and Industrial Research. (M.C. 165; L.A. 47.) Pp. 16+6 plates. 1*s.* net. No. 1042 (A. 4): An Investigation on Wing Flutter. By R. A. Frazer. (D. 1. Special Technical Questions, 171.—T. 2232.) Pp. 22+4 plates. 1*s.* net. (London: H.M. Stationery Office.)

International Federation of University Women. Bulletin No. 8: Report of the Fourth Conference, Amsterdam, July 28 to August 2, 1926. Pp. 176. 1*s.* 6*d.* Pamphlet No. 3: What Dutch University Women do in Holland and the Colonies. Pp. 32. 8*d.* (London.)

Foreign.

Stanford University Publications: University Series. Biological Sciences, Vol. 4, No. 1: The Fossil Fishes of the Miocene of Southern California. By David Starr Jordan. Pp. 51+21 plates. 1.50 dollars. Biological Sciences, Vol. 4, No. 2: Studies on Growth. Part 1: The Point Binomial and its Derivatives, by Prof. L. G. M. Baas-Becking; Part 2: Experimental Data—Application of the Theory, by Prof. L. G. M. Baas-Becking and Leland S. Baker. Pp. 53-135. 1 dollar. Biological Sciences, Vol. 4, No. 3: Bones of the Ethmoid Region of the Fish Skull. By Prof. Erwin Chapin Starks. Pp. 137-338. 2 dollars. Geological Sciences, Vol. 1, No. 1: The Marine Shells of the West Coast of North America. By Ida Shepard Oldroyd. Pp. 247+57 plates. 4 dollars. (Stanford University, California.)

Department of the Interior: U.S. Geological Survey. Bulletin 783-E: Summary of Recent Surveys in Northern Alaska. By Philip S. Smith, J. B. Mertie, jr., and W. T. Foran. (Mineral Resources of Alaska, 1924-E.) Pp. ii+151-168. (Washington, D.C.: Government Printing Office.)

Proceedings of the National Academy of Sciences of Philadelphia. A new Genus and Species of Phosphorescent Fish, Kryptophanaron Alfredi. By Charles F. Silvester and Henry W. Fowler. Pp. 245-247. Fishes from Florida, Brazil, Bolivia, Argentina and Chile. By Henry W. Fowler. Pp. 249-285. (Philadelphia, Pa.)

Journal of the Faculty of Science, Imperial University of Tokyo. Section 1: Mathematics, Astronomy, Physics, Chemistry. Vol. 1, Part 5: Studies in Overvoltage. Part i: Hydrogen Overvoltage. By Tadashi Onoda. Pp. 223-247. 0·70 yen. Section 2: Geology, Mineralogy, Geography, Seismology. Vol. 1, Part 8: Fossil Shells from Sado. By Matajiro Yokoyama. Pp. 249-312+6 plates. 1·80 yen. Vol. 2, Part 1: The Tazima Earthquake of 1925. By Dr. Bundjirō Kotô. Pp. 75+8 plates. 2·50 yen. (Tokyo.)

U.S. Department of Agriculture: Weather Bureau. Monthly Weather Review, Supplement No. 26: An Aerological Survey of the United States. Part 2: Results of Observations by Means of Pilot Balloons. By Willis Ray Gregg. (W.B. No. 900.) Pp. v+60. (Washington, D.C.: Government Printing Office.)

Report on Norwegian Fishery and Marine Investigations. Vol. 3, No. 7: Quantitative Investigations of Plankton at Lofoten, March-April, 1922–1924. Preliminary Report, by Birgithe Ruud. Pp. 30. (Bergen: A. S. John Griegs Boktrykkeri.)

Obras completas y correspondencia científica de Florentino Ameghino. Volumen 5: Paraná y Monte Hermoso. Edición oficial ordenada por el Gobierno de la Provincia de Buenos Aires. Dirigida por Alfredo J. Torcelli. Pp. 524. (La Plata: Taller de Impresiones Oficiales.)

Catalogues.

Tech. 926: "Universal Technical" Microscope. Pp. 6. (London: James Swift and Son, Ltd.)

Stu. 926: Microscopes for Students and other Workers in the Biological Sciences. Pp. 14. (London: James Swift and Son, Ltd.)

Diary of Societies.

SATURDAY, December 18.

North of England Institute of Mining and Mechanical Engineers (at Newcastle-upon-Tyne), at 2.30.—Discussion on paper by T. S. Durham on Thin Seam Mining.—Prof. G. Hickling: The Chemical Relations of the Principal Varieties of Coal.

Royal Institution of Great Britain, at 3.—Dr. C. Rootham: Henry Purcell and his Contemporaries (2).

MONDAY, December 20.

Institution of Electrical Engineers (Tees-Side Sub-Centre) (Informal Meeting) (at Cleveland Technical Institute, Middlesbrough), at 7.—W. R. Cooper and others: Discussion on Some Electrical Problems.

Chemical Industry Club, at 8.

Royal Geographical Society (at Æolian Hall), at 8.30.—Prof. J. W. Gregory: The Fjords of the Hebrides.

TUESDAY, December 21.

Royal Statistical Society (at Royal Society of Arts), at 5.15.—H. W. Macrosty: Inflation and Deflation in the United States and the United Kingdom, 1919–23.

Royal Photographic Society of Great Britain (Kinematograph Group), at 7.

Institution of Automobile Engineers (Wolverhampton Centre) (at Engineering Club, Wolverhampton), at 7.30.—F. Randle: Radiator Design.

Institution of Engineers and Shipbuilders in Scotland (at 39 Elmbank Crescent, Glasgow), at 7.30.—T. M. Service: High Elastic Limit Steel for Shipbuilding and Marine Work.

Royal Anthropological Institute, at 8.30.—Capt. T. A. Joyce: Excavations at Lubaantun, 1926.

PUBLIC LECTURE.

SUNDAY, December 19.

Guildhouse (Eccleston Square), at 3.30.—Miss Maude Royden: The Debt of Theology to Science.

CONFERENCE.

December 30 to January 7.

Conference of Educational Associations (at University College).

Supplement to NATURE

No. 2981 DECEMBER 18, 1926

Retrospect.

By Sir J. J. THOMSON, O.M., F.R.S.

THE editor has asked me for some personal recollections of the Cavendish Laboratory, and I feel it would be churlish to refuse, though it is difficult to condense the happenings of more than forty years into a reasonable space, and still more difficult to avoid being egotistical. My connexion with the laboratory did not begin until I took my degree in January 1880. Before coming up to Cambridge I had, at the Owens College, under the inspiration of Balfour Stewart, become keenly interested in experimental physics and had done a small piece of research which was published just fifty years ago in the *Proceedings of the Royal Society*. The work for the Mathematical Tripos was, however, too heavy to leave time for experiments in the laboratory. As soon as the Tripos was over I started work at the laboratory, where Lord Rayleigh had just been made professor, and at his suggestion I began using a method, which he designed, an investigation on the ratio of the electrostatic to the electromagnetic unit of electricity. I made also theoretical investigations on applications of generalised dynamics to physics and chemistry, on vortex motion, and on the effects produced by the motion of electrical charges. There was great activity at the laboratory during Lord Rayleigh's time; Glazebrook and Shaw organised courses of instruction in practical physics; and Mrs. Sidgwick, Schuster, Glazebrook, Shaw, Poynting, E. B. Sargant, McConnel, J. A. Fleming, Hart, Miss Harland, and J. H. Middleton were working at various researches.

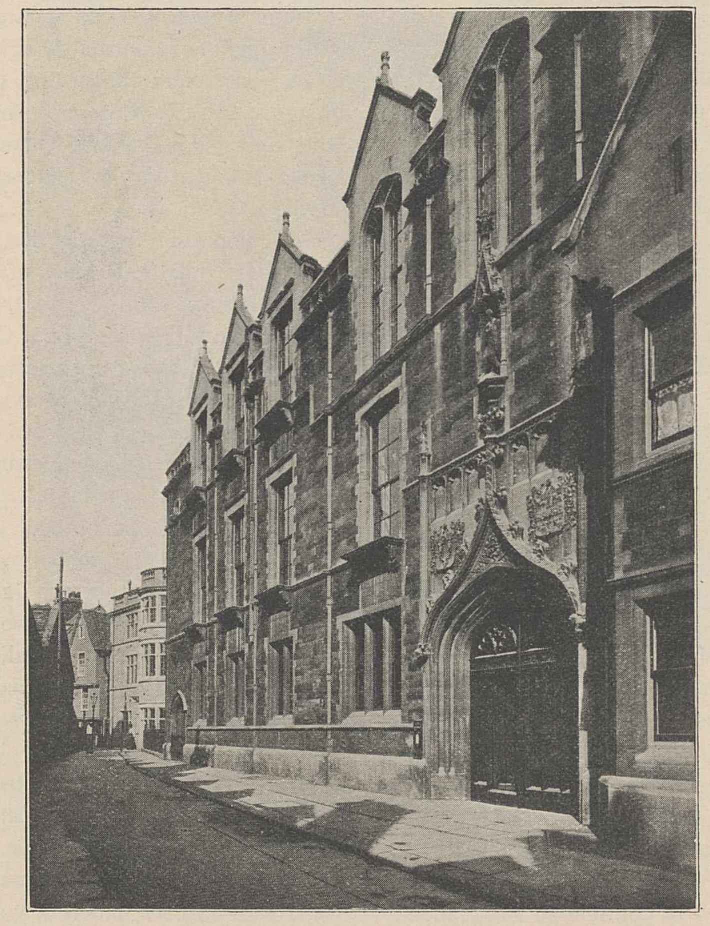

FIG. 1.—General view of part of the Cavendish Laboratory from Free School Lane. The new Rayleigh Wing is seen in the distance.

At the end of 1884, Lord Rayleigh resigned the professorship, and to my great surprise, and I think to that of the rest of the University, I was appointed to succeed him. I heard at the time that one well-known college tutor had expressed the opinion that things had come to a pretty pass in the University when mere boys were made professors. My want of experience was made less harmful than it might have been by the kindness of Glazebrook and Shaw, who continued to take charge of the teaching in practical physics. Immediately after my election to the professorship I began, in collaboration with my old friend Richard Threlfall, one of the most skilful experimenters I have ever met, some experiments on the passage of electricity through gases, and since then I think there has never been a time at which I have not had some experiments in hand on this subject. I had come to the conclusion that whenever a gas conducts electricity, some of its molecules must split up and that it is these particles which carry the electricity. My idea at that time was that the molecule was split up into two atoms, one of which was positively, the other negatively, electrified, and my first experiments were intended to test this idea. It was not until the beginning of 1897 that I discovered that the decomposition of the molecules was of quite a different type from ordinary atomic dissociation. I found then that one of the bodies into which the molecules split up, the one carrying the negative charge, is something totally different from an atom, and has a mass less than a thousandth part of the smallest atom known. I was fortunate enough soon after becoming professor to be able to persuade H. F. Newall, now professor of astrophysics in the University, to come and help in the work of the laboratory, and for some time we worked in collaboration on various researches.

In 1887 a great scientific discovery was made which appealed especially to every one connected with the laboratory: I allude to the detection by Hertz of electrical waves. The existence of these waves is a necessary consequence of Maxwell's electromagnetic theory, but though numerous attempts to detect them had been made, some of them at the Cavendish Laboratory, they had all failed owing to the want of a sufficiently delicate instrument to detect their presence. Hertz, by observing the variations in intensity of the minute sparks which pass between conductors near together when electrical waves pass over them, was able to demonstrate the existence of these waves. The younger school of mathematical physicists at Cambridge, who had been brought up on Maxwell's "Electricity and Magnetism," received Hertz's discovery with the welcome which might be expected from those who found their most cherished convictions confirmed by a series of most beautiful experiments; it established instead of revolutionising our ideas. I well remember the enthusiasm of the undergraduates when I repeated Hertz's experiments in my lectures on electricity and magnetism in 1888. The enthusiasm spread to all the workers in the laboratory, and soon experiments on electric waves were going on all over the building.

Among the Cambridge men doing researches in the laboratory up to 1895—the year of the admission of research students—were, in addition to those already mentioned, Cassie; W. H. Bragg for a short time; Wilberforce, who was a demonstrator until he became professor of physics at Liverpool; Chree, who became superintendent of Kew Observatory; H. L. Callendar, now professor at the Imperial College of Science, who was elected to a fellowship at Trinity College for natural science after having taken first classes in both classics and mathematics, and began at the Cavendish Laboratory those researches on the effect of temperature on the resistance of metals which have revolutionised thermometry; T. C. Fitzpatrick, now president of Queen's College, who for more than twenty years had the charge of the classes to medical students, and in 1904 presented to the laboratory apparatus for producing liquid air—this gave us the means of producing high vacua, and without it many of the most important researches would have been impossible; G. F. C. Searle, to whose devotion and enthusiasm the laboratory owes more than I can express, and whose fertility and ingenuity in designing instructive and ingenious experiments for demonstration in practical physics remains quite unabated; W. C. D. Whetham, later a tutor of Trinity College, who worked at problems on the properties of solution; Sidney Skinner, now principal of the South-Western Polytechnic, who was for many years a demonstrator; J. W. Capstick, until he became junior bursar at Trinity College; J. B. Peace; Alexander Anderson; C. E. Ashford; Miss Freund, and Miss Klaassen. Among our guests from other universities were Sir Oliver Lodge, who worked for one long vacation in collaboration with Glazebrook on the determination of v; A. M. Worthington, who made an important investigation on the tensile strength of liquids; while H. F. Reid, of Johns Hopkins, Baltimore, Olearski, from Lemberg, and Natanson, from Cracow, were the precursors of the many distinguished teachers from foreign universities who have honoured us by working in the laboratory.

The year 1895 was an epoch in the history of the laboratory, as then the University decided to admit as research students graduates of other universities whether at home or abroad, and in certain cases students who are not graduates, provided they satisfy the authorities that they are qualified to do research. In October 1895, the first two research students—Rutherford, from New Zealand, and Townsend from Trinity College, Dublin—arrived within a few hours of each other. Did a new system ever have such a good beginning? Rutherford began by working at wireless telegraphy, using a detector which he had invented before leaving New Zealand: it was so successful that the laboratory held for a time the record for long-distance telegraphy, communications being established between the laboratory and the observatory nearly two miles away. Townsend made a very important investigation on the magnetic properties of iron when in chemical combination.

It was fortunate that the new regulations, which brought to the laboratory a large influx of students bent upon research, came just at the time when the discovery of the Röntgen rays gave us a very powerful method of investigating the mechanism of the conduction of electricity through gases. A great many researches had already been made in the laboratory on this subject, and our studies had shown the fundamental importance of certain investigations which were almost impracticable with the means then at our command. The Röntgen rays gave us a means more amenable to purposes of investigation than those hitherto available and made many investigations possible which had before been hopelessly difficult. The investigation of the electrical properties of gases under the action of Röntgen rays soon engaged the attention of many workers in the laboratory. Rutherford, Townsend, McClelland, Langevin, who had come to us from Paris, were all hard at work. C. T. R. Wilson began those researches on condensation which, *ohne Rast ohne Hast*, he has pursued ever since, obtaining results of continually increasing importance for the development of modern physics.

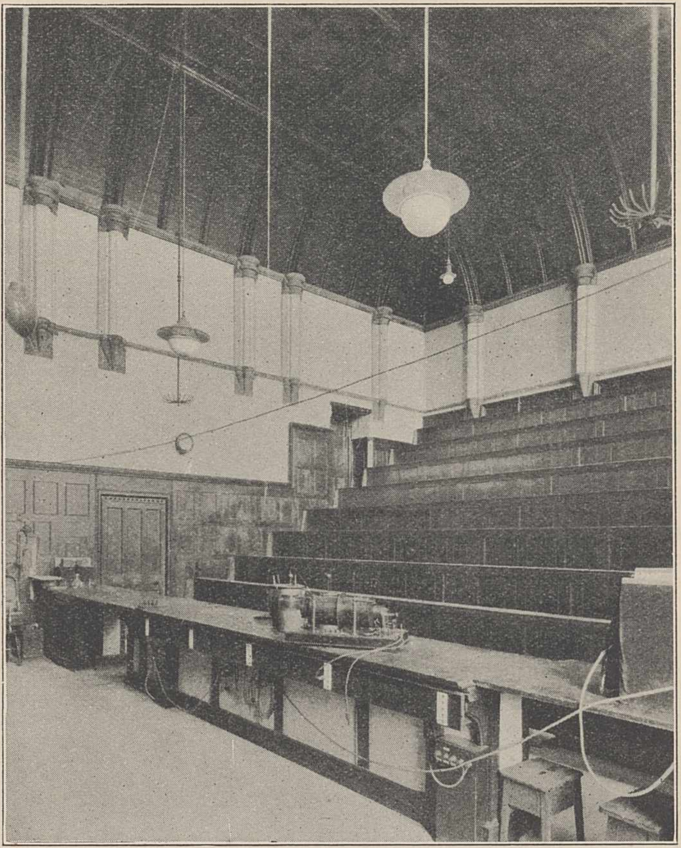

Fig. 2.—Lecture room of the Cavendish Laboratory, designed by Clerk Maxwell. Reproduced from a photograph kindly supplied by Mr. Rollo Appleyard, International Standard Electric Corporation.

It is not, I think, claiming too much to say that these researches played a great part in establishing the theory now universally accepted that the electricity is carried through gases by particles charged with electricity. The properties of these particles were studied in great detail at the laboratory, and they seemed to indicate that they were at least as large as the atoms of the gas in which they were produced. Some experiments which I made at the beginning of 1897 led, however, to the discovery of particles of quite a different order of magnitude. The cathode rays, which proceed from the cathode when an electrical discharge passes through a gas at low pressure, were discovered by Hittorf, and many experiments had been made on them by Goldstein, Varley, and Crookes.

Two widely different views were, however, held as to the nature of these rays. The majority of the German physicists maintained that they were analogous to electric waves, and urged in support that they could pass through thin sheets of metal. The English physicists held that they were charged particles, and brought forward the argument that when the rays entered an insulated vessel they communicated to it a charge of negative electricity. I had for a long time been convinced that these rays were charged particles, but it was some time before I had any suspicion that they were anything but charged atoms. My first doubts as to this being the case arose when I measured the deflexions of the rays by a magnet, for these were far greater than I could account for on any hypothesis that seemed at all reasonable if the particles had a mass approaching even that of the hydrogen atom—the smallest mass then known. I made a series of measurements of the velocity of these particles and the ratio of their mass to their charge. These all showed that the ratio of mass to charge for the cathode particles was less than one-thousandth part of the same ratio for the atom of hydrogen and its electrolytic charge. Thus, unless the electric charge on the cathode ray were at least a thousand-fold that on the electrolytic ion, the mass of the cathode ray particles must be less than that of an atom of hydrogen. Methods were devised by which the charge on these particles could be measured, and it was found to be the same as the electrolytic charge. I could see no escape from the conclusion that in the cathode rays there were particles far more minute than any hitherto recognised. I think the first announcement of this result was made at a Friday evening meeting of the Royal Institution on April 30, 1897.

Later investigations have shown that these particles—the electrons as they are now called—are always of the same kind and form a part of every kind of matter. The number of electrons in the atoms of the different elements was determined by applying a theory of the scattering of these rays by electrons which I had worked out from the valuable measurements by Barkla on the amount of radiation scattered by different gases, and it was found that this number was proportional to the atomic weight of the element and was of the same order as that number. This led me to construct a theory of the structure of the atom which had special reference to the chemical properties of the element, for it was shown that such a structure would give a variation of the chemical properties with the atomic weight quite similar to that expressed by Mendeléef's law. Another excursion I made into chemistry was to use the magnetic and electric deflexions of the positive rays to determine the chemical composition of the gas in which they were generated. It proved a useful method, and Dr. Aston has modified and improved it, so that it is now one of extraordinary delicacy and accuracy.

The number of those engaged in research grew very rapidly, and for many years before the War there were always thirty or more researches going on in the laboratory. This put a great strain upon the apparatus and upon the workshop. We have been very fortunate in our chief assistants, and the laboratory owes much to Mr. Sinclair, Mr. Bartlett, and Mr. Pye, and especially to Mr. Lincoln, who has been chief assistant since 1899. Personally, I am greatly indebted to the skill and care of Mr. E. Everett, who has been my private assistant for nearly forty years, and has given me most valuable and able assistance in my investigations.

It is impossible here to mention individually the research workers between 1895 and the beginning of the War: their number is too great. Suffice it to say that three of them have gained the Nobel prize, twenty-two have been elected fellows of the Royal Society, and more than fifty have become university professors of physics. When the War began the usual work of the laboratory stopped. The research workers went either to the front or to laboratories formed for developing and testing methods likely to be of use to the fighting services. Our own workshop was employed in making gauges, and all the research at the laboratory was war-work. Soon after the end of the War my duties as master of Trinity obliged me to resign the professorship. The University was fortunate enough to induce Sir Ernest Rutherford to take the post. At the same time, with great kindness and consideration, they gave me a position which enabled me to retain my connexion with the laboratory which for more than forty years had given me unrivalled opportunities for doing the work I liked best, which has brought me many cherished friends and has created rich memories of kind acts, good fellowship, and goodwill.

James Clerk Maxwell.

By Sir Joseph Larmor, F.R.S.

THE course of evolution of the career of a man of genius is always an interesting, and should be an instructive, study. In the case of Clerk Maxwell the materials are ample, thanks to the care of his two biographers, one of them a classical scholar with Greek predilections, the other his intimate and sympathetic scientific assistant. Their account leaves an impression of the absence of any formal education outside his home life. His father was his chief early friend; his real intellectual initiation was identical with his amusements, mainly concerned with dynamical contrivances such as spinning tops, and extending into explorations in practical hydrodynamics by feats of swimming in the bath. Perhaps the Scottish atmosphere of practical engineering, fanned into interests of pure science by men such as Robison, Kelvin, Rankine, explained the origin of this mental bent; but it does not account for the literary grace and charm of his writing.

Another main formative influence was the Scottish Calvinist theology, in its more humane and devotional aspect, supported by intimate conversance with the phrasing and poetry of the English Bible, which has so often been the well-spring of distinction in literary expression. This Puritan trend, while it treasured the historical formularies of the Scottish Church, was in him far from sectional; it ramified into enjoyment of literature, such as the poetry of Milton, and also the descriptive and evolutional poetry of Tennyson, whose influence was still dominant in the Cambridge of his undergraduate time.

Another phase of Maxwell's thought was made public in the biography, in some occasional papers of metaphysical and religious import, which had been read to a private Cambridge society. At the time of publication, as one remembers, the interest of them was regarded as mainly personal, being outside the analytic severity of the British psychology of that period. But they were discovered by Prof. Höffding of Copenhagen, and have come back to us, in the English translation of his works, as among the competent pronouncements on the philosophy of Nature.

It is Maxwell's main achievement that he unified physical science, by connecting light and radiation with electricity so as to form one interlocked systematic whole. This advance made the Cavendish Laboratory, from its foundation in 1872, the focus of electrical pure science which it has remained ever since. It must have been remarked by students of his writings how abruptly he left off the development of this electrodynamic and optical theme in the last six years of his life, after the publication of the treatise on "Electricity and Magnetism" early in 1873. For example, no question ever occurred as to the dynamical riddle, then prominent in physical speculation, which was presented by the Fresnel laws of refraction of optical waves: though when Helmholtz, and Lorentz, and J. J. Thomson entered into the study of his theory, they at once recognised independently that the form, at any rate, of these laws was immediately involved, while FitzGerald, going deeper, had already connected the electric theory intimately with the entire *corpus* of the optical theories of MacCullagh.

One reason may have been Maxwell's preoccupation with fundamental objections of fact, operating against his views. His dynamical mentor, Lord Kelvin, to whom he largely owed the impulse to the elucidation through kinematic models of deep-seated physical phenomena, had here failed him, and to the end of his days held to the view that the foundations of the electric theory of light were unintelligible, by which he seems to have meant not sufficiently disentangled dynamically, his own fundamental developments on latent momenta notwithstanding. The wide discrepancy between the index of refraction of a substance and the square root of its dielectric inductive constant was such a threshold difficulty of fact, and loomed important in those days when the influence of dispersion was not very fully recognised. The first fundamental service of the illustrious Boltzmann to the theory of his master in research was the measurement of the dielectric constants of gases, resulting in confirmation of Maxwell's law for such simple forms of matter: while the experimental work of Hopkinson on organic compound substances also brought out significant correlations.

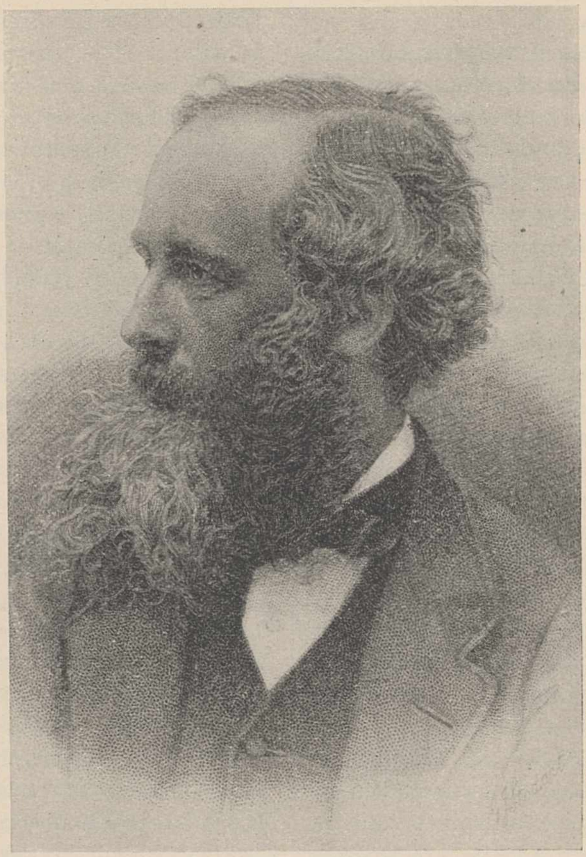

Fig. 3.—James Clerk Maxwell, Director 1871–79.

Curiously enough, it is now recognised universally, on the initiative of the late Lord Rayleigh, that Maxwell himself had been the pioneer in clear-cut dynamical atomic views of dispersion. He had formulated a fully illustrative scheme, in the guise of a rather detailed examination question in the Cambridge Mathematical Tripos of 1869. This must have implied that the floating dynamical instincts of that time were adequate, at any rate in the view of himself and his co-examiners, for a rapid apprehension and verification of the ideas involved; while the collections of Maxwell's tripos problems, that were treasured and utilised in Cambridge teaching, must have made them not unfamiliar to expert optical students.

There is a cognate question, why in his expositions Maxwell made so slight use of moving electric charges as the originators of the electrodynamic fields which, after the example of Faraday, were his main concern. For the atomic constitution of electricity had been fully established by Faraday's own laws of electrolysis, as Helmholtz afterwards emphasised. The idea of electrons acting on one another through a law of attraction at a distance had been long before placed by W. Weber at the foundation of the reasoned exposition of his fundamental electric measurements. Maxwell himself hovers around the phrase 'atoms of electricity.' He even sets in special prominence the remark of Gauss that what was needed was, above all, some notion of how influence was propagated in time between the sources of the electric manifestations. Gauss confesses that he had not been able to find it; while Maxwell implies that it now lies exposed in his equations of the electrodynamic field. Yet he did not pursue this path. Hertz found no difficulty, in 1887, in determining, on Maxwell's principles, the radiation from a vibrating source such as an oscillating electron. Indeed FitzGerald had already, five years earlier, incidentally determined the nature and amount of the radiation from a rapidly alternating current circuit, that is, from a magnetic vibrator; and on the same occasion he pointed out that a spark from an electric accumulator, discharged through a small resistance, ought to produce electric waves as short as ten metres or less. He thus was well qualified to expound Hertz's epoch-making detection of the Maxwellian waves in free space, in an address to the British Association soon after it was announced.

In other respects also Maxwell had largely confined himself to the medium of electric propagation as a whole, as modified by its content of material atoms and ions in bulk. He had measured, incidentally, with the assistance of Hockin ("Treatise" § 798; more fully in the Memoir[1] of 1864), the transparency of metallic foils such as gold-leaf, and found that it implied effective electric conductance far smaller than the steady value as determined on a bridge. The natural reason for this discrepancy he indicated: also the reason why electrolytes are not opaque; but there he left it, for indeed the relevant experimental knowledge was as yet a blank, while other subjects were pressing on him. We may perhaps add to this the distractions involved in the creation of the Cavendish Laboratory, and the laborious editing and expansion of the Cavendish electrical manuscripts.

In the same context ("Treatise," § 792; more fully in the Memoir of 1864, § 107) he propounds very briefly his law of radiation pressure, now fundamental in physics and astronomy; he tries to estimate the magnitude of the pressure for sunlight, coming to grief in detail over the arithmetic. He perhaps reached the law more by physical instinct than by demonstration. If we imagine a limited train of electric waves reflected back at a perfect conductor, a current sheet is induced on it, and the Amperean force on that sheet constitutes a pressure of the train on the conductor. But the train was originally isolated in space, and will again be isolated with only its direction reversed; thus there is no other source for the pressure exerted by it, on the electrons of the current, but a change, here a reversal, of momentum associated somehow with the radiation. This is the idea developed directly in experiment by Poynting, that a train of radiation is a carrier of momentum. Maxwell associated the pressure with his quadratic stress, in origin purely formal, in the electrodynamic field. Close scrutiny, first in time by Lorentz, reveals, however, an outstanding motional part of the mechanical force which is not absorbed into that stress-tensor, significant though it was; and Poincaré and Abraham remarked that it was just such as could arise from a distribution of momentum in the field, thus without vitiating the stress-representation. The complete forcive on the material content of any region is then expressed as the resultant of this formal electromagnetic stress over its boundary, when there is such stress there, together with the rate of communication of this aethereal momentum throughout its volume: if there is no material content these will balance.

It is, then, the transfer of this postulated momentum of radiation that constitutes radiation pressure on material bodies. The transfer is effected, as above, through Amperean force on the current of electrons. Reversing the argument, radiation pressure, as experimentally confirmed by Lebedew, Nichols and Hull, Poynting, implies momentum in radiation; and its existence to the requisite amount implies a reality, of some kind scarcely yet fully explored, in the Maxwellian quadratic field-stress. An intimate knowledge of the structure of the electron which sustains the stress ought to involve both its own dynamical nature and the structure of the field in which it subsists.

It is not necessary to follow up Maxwell's share in the practical settlement of the scheme of electric units, the essential preliminary to the present electrotechnic age; in that Kelvin was the leader. But we may note his early memoir in determination of the nature of subjective colour, in which he verified with precision the surmise of Young, following on Newton, while Maxwell himself was followed by Helmholtz and by Rayleigh, that all the gorgeous play of colour that is a main glory of our natural world arises from combinations of only three independent elements.

It has been already remarked that Maxwell added little in later life to the essentials of the electric theory. The first edition of the "Treatise" had obviously been thrown hurriedly into the press, in a series of rather disjointed fragments which taxed the ingenuity of dynamical interpretation of his British disciples for years; some additions for the early chapters of a second edition were mainly developments of the usual standard knowledge. But he could still be drawn on the subject. Compare his answer in keen and humorous verse to Tait ("Life," p. 684 (1877), already however in "Treatise" (1873), § 577), who proposed apparently to repeat with a spinning excited dielectric disc Rowland's then recent experiment of the magnetic convection effect of a charged rotating disc, as was afterwards carried through by Röntgen. He wanted Tait to spin instead a copper coil with galvanometer in circuit, suddenly to arrest its motion, and note if

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[1] The abstract of this memoir (*Roy. Soc. Proc.* **13**, pp. 531-536), accidentally omitted in the "Scientific Papers," is an interesting general exposition of the author's ideas, largely in line with modern points of view

there was any result; for that would give "the electric current's true direction," or alternatively let it "be your boast to prove . . . that there is no Electric Fluid." This test for sensible inertia of the electrons, as we would now say, has been carried through to success in America only the other day.

Plenty of electric discussion was, however, going on, especially in the years just after Maxwell's death. It was regarded as a great improvement when Heaviside and Hertz, nearly simultaneously, got rid of his vector potentials as being mere mathematical figments, though with him of heuristic dynamical origin; yet the two resulting circuital relations, as Kelvin called them, had already been formulated long before by Maxwell himself, as a concession to a demand for the essential outcome of his theory, in concise form freed from tentative dynamical implications. But these circuital equations are concerned only with the smoothed-out electrodynamic fields. Ultimate dynamical theory, going back to the sources of the field, has not yet been able to do without their vector potentials. Nowadays the circle has indeed gone round full tilt; we have been familiarised with the point of view that the electric and magnetic fields, so tangible in the world of engineering, are in theory only two partial aspects of one six-vector, itself the (Hamiltonian) gradient of a fourfold vector potential that alone is fundamental in Nature, as presented to our minds.

Maxwell's other main contribution to science, equally monumental, lay in the domain of the molecular kinetic theory of gases; it provided the more severe mathematical occupation of his later years. He had taken over the subject in his early days from Joule and Clausius and their predecessors, in the form of a rough *aperçu* of the phenomena of a crowd of independent moving molecules: he converted it into an exact theory, thereby creating the science of statistical dynamics which dominates modern molecular physics. The root principle of that science is Maxwell's law of distribution of velocity, or of other quality, in a multitude of molecules which has attained a steady state. As results there came to him exact dynamical theories of friction and diffusion and conduction of heat in gases, and analytical developments for rarefied gas arising out of the phenomena revealed by the Crookes radiometer. Here also Boltzmann was in readiness to follow up this train of research. From their memoirs in general statistical dynamics, the law of equipartition of energy among the various modes of molecular freedom stood out as a cardinal result. The problem of how the inadmissible consequences of this law are to be evaded has opened up new regions of physics, practical as well as speculative, which at present tend to dominate the whole field.

A characteristic illustration of his genius was the early enforcement of the averaging character of the processes of thermodynamics, by appeal to the possible achievement of ideal minute intelligences, named Maxwell's demons by Kelvin, who could by merely guiding or sifting interference upset the fundamental principles of that science; this arresting quip carried the new doctrine of the statistical character of natural law for molecular structures into regions of thought where abstract dynamical argument could scarcely have penetrated. His lucid expositions in formal thermodynamics need only be mentioned; in them he appeared mainly as the simplifier of the fundamental advances achieved in the American work of Willard Gibbs, the foundation of modern physical chemistry.

Maxwell spent his summers on his small estate of Glenlair in Galloway, among his own people, living as a Scottish laird. Doubtless it was there, among the solitudes of the hills, that illumination mainly came. In the autumn he usually attended the British Association, where, as one used to hear, his gaiety and humour were looked forward to as enhancing the value of the annual scientific discussions, then at their prime. The writer recalls that, returning to Cambridge as an undergraduate one October, a man of the type of a country farmer came into his compartment of the train at Dalbeattie, remained silent for a time, then remarked with emphasis, as something that concerned the world to know, to this effect: Clerk Maxwell has been taken away, mortally stricken; he will never come home again. He died in 1879 at only forty-eight years of age.

Lord Rayleigh.

By Sir Arthur Schuster, F.R.S.

MAXWELL'S health began to fail in the early part of 1879. Troubled by his wife's illness, which weighed heavily upon him, his accustomed good spirits had left him; but there were no signs that he himself was suffering from a mortal disease until his return from the summer holidays, when we were shocked to hear that he had only a few weeks to live. His death was a calamity which might have been fatal to the continued prosperity of the Cavendish Laboratory had Lord Rayleigh not consented to accept the professorship. His hesitation and the pressure put upon him by those who had the interest of the laboratory at heart, are set out in the 'Life' of the father written by the son.

After his election, Rayleigh lost no time in making plans for the conduct of his new duties. I am sorry not to have preserved a letter I received from him asking whether I had any suggestions to make, as I had gained some experience in its working during the two years I had spent in the laboratory. Others received similar requests, but I doubt whether the new director of the laboratory received any useful hints on essential matters.

On his own initiative Rayleigh adopted a definite and novel policy, its essential point being the fostering of a spirit of community among the advanced students. To make a beginning, he desired to identify the laboratory as a whole with some research in which a combination of workers was necessary. The question of electrical units had, at that time, gained practical importance and the subject seemed suitable for a combined attack. There was the additional reason that Maxwell had been connected with the original determination of the Ohm fathered by the British Association, and that the principal piece of apparatus, the rotating coil, was preserved in the laboratory.

FIG. 4.—LORD RAYLEIGH, Director 1879–84.

Different methods of procedure having led to values which differed more than was desirable, Rayleigh decided to repeat the former work, using the same coil, but paying more attention to what others might have considered minor details. The method by which the rotational velocity of the coil was kept constant was simple and effective, and the same applies to the very ingenious method devised to compare the frequency of the tuning-fork, to which the rotation was referred, with the rate of a standard pendulum clock. When sufficient experience had been gained with the original coil, a new one was constructed giving a result which was decidedly more accurate than any obtained up to that date. Rayleigh was disappointed in his original desire, no volunteers besides myself offering to help, until Mrs. Sidgwick joined us, and the later history of the research is well known.

I may, perhaps, here refer to a personal recollection which has always remained in my mind. During the early stages of the investigation of the Ohm, Lord Rayleigh made the remark to me that this was the first piece of work he had undertaken that required great accuracy of measurement, and he added that he was not at all sure that he would really like it. I replied that I felt sure that the satisfaction of getting another decimal place in a physical constant was a pleasure which would grow on him more and more. Later events, I think, justified my remark.

Persevering in his desire to form a kind of research community within the laboratory, Rayleigh introduced an innovation that may seem trivial, but has been effective in more than one laboratory since. A tea-interval was introduced, during which the different workers could meet and join in informal discussion on scientific problems. Tea was served in a room in which other experiments were kept going, and those who had the privilege of attending these informal scientific meetings will more especially remember the water jets breaking into drops, and the effects of electricity on the appearance presented by them. There were also the spinning colour discs in which we could compare our colour senses.

A good example of Rayleigh's method of work was shown by the little appliance made with card-board, sealing-wax, simple glass lenses, and a prism or two, by which he could determine the relative amount of red and green required by different persons to produce the yellow of a sodium flame. By comparing the results of different observers it was found that, while most persons agreed fairly well in their estimate, there are anomalies which run in families. Lord Balfour and two of his brothers, for example, required a considerably larger proportion of green to get the yellow sensation. With a more elaborate apparatus designed by Rayleigh, I examined, on my return to Manchester, seventy-two persons and found among them four possessing the same peculiarity and one with an anomaly in the opposite direction. Among the four was a woman with two of her three sons. It is a curious coincidence that the colour sense of Clerk Maxwell was, and that of J. J. Thomson is, affected in the same way. An examination of the present director's sight is obviously indicated.

Though not belonging to the Cambridge period, Rayleigh's great work on the weighing of gases should be mentioned, because it was really planned at Cambridge. He frequently referred to the desirability of keeping a research going as a 'stand by,' that is, an investigation which presumably could be continued for a long time and dropped or taken up again as more urgent demands had to be satisfied or a slack time occurred. His choice fell on the weighing of gases, because he was always impressed by the probability of a unity of matter and the likelihood of the correctness in some form or other of Prout's law. Quotations from his address as president of Section A of the British Association giving his views on the subject will be found in the biography written by his son.

During Rayleigh's tenure of office at Cambridge a systematic and very successful instruction in laboratory work was introduced by Glazebrook and Shaw. For this and research purposes a substantial annual income was required, and Rayleigh raised a fund of 1500*l.*, to which he himself contributed one-third.

When he gave up the directorship of the Cavendish Laboratory, it was in a highly efficient state both as a teaching and research institution.

Sir J. J. Thomson, O.M., F.R.S.

By Sir Oliver Lodge, F.R.S.

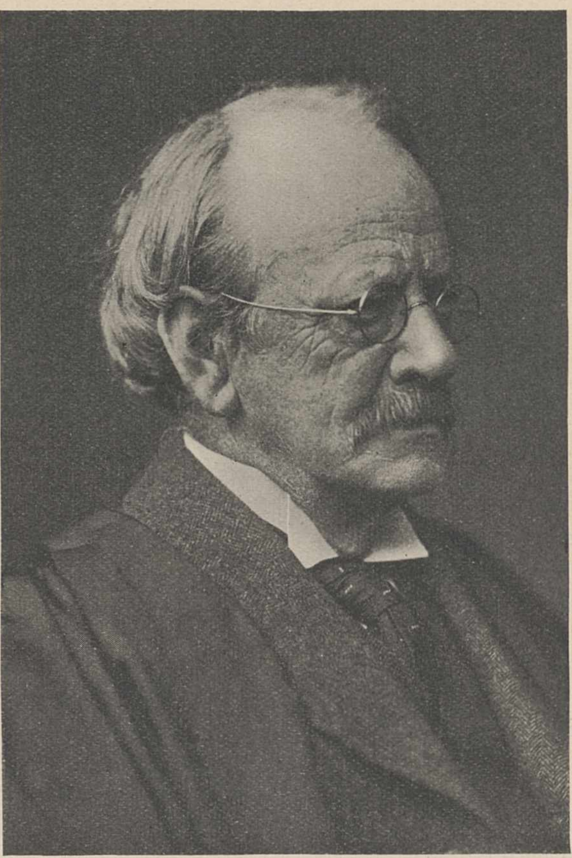
HOW much less the world would know if the Cavendish Laboratory had never existed; and how diminished would be the glory even of that laboratory if Sir J. J. Thomson had not been one of its directors! We used to think of him as one of the younger physicists; but now that his seventieth birthday is being celebrated that notion must be given up, even by his seniors. But, whether young or old, we have all venerated him for his brilliant achievements.

The discovery of the electron and the foundation of the electrical theory of matter cannot, any more than other fundamental discoveries, be attributed to any single man: these great advances are the outcome of the work of at least a generation. To them Helmholtz, Crookes, Johnstone Stoney, Sir Joseph Larmor, and in the electrolytic stage even Faraday, have all contributed; and, doubtless, in mentioning some names I am omitting others. But researches into the phenomena connected with the discharge of electricity through gases have been Sir J. J. Thomson's special field; and, as Clerk Maxwell hinted would happen, that branch of inquiry has thrown great light upon the nature of electricity itself. Very few before our time can have supposed that electricity was discontinuous. Maxwell's equations and Cavendish's experiments either postulate or appear to demonstrate continuity and incompressibility. That electricity was a fluid, comparable in any respect to a gas, seemed like a popular superstition. The discovery of a discontinuity in Nature must always have notable consequences; and though we may be willing to grant that ultimately every atomic character will be resolved into a deeper-seated and more fundamental continuity, yet for a long time it will be the business of science to absorb and work out the consequences of every discontinuity that is revealed.

John Dalton was the earliest to emphasise the chemical discontinuity of matter. J. J. Thomson is likewise the first to emphasise effectually the atomic character of electricity. His work is a happy combination of experimental and mathematical ability. He arranges ingenious experiments to display and dissect the phenomena; and at the same time most skilfully applies dynamics to analyse those phenomena in an illuminating and metrical manner. In his hands the magnetic deflexion of cathode rays observed by Crookes, coupled with certain other experiments by C. T. R. Wilson and J. S. Townsend, sufficed to determine the mass and the speed of the electric particles; and when electric deflexion of cathode rays was combined with magnetic deflexion, the determination could be made in a particularly neat and convincing manner. So that when Sir J. J. Thomson gave an account of his researches throughout the years 1897 and 1898, before Section A of the British Association meeting at Dover in 1899 (in the presence of a number of Continental physicists, many of whom had come over that day from Boulogne), the whole world rose to the conviction that a new era had dawned in electrical science. A foundation-stone was then laid for the innumerable researches which have gone on during the present century in every laboratory and library of the world.

The facts are so well known now that it is needless to elaborate them: within thirty years they have become almost ancient history. Naturally the renowned discoveries of Becquerel, Röntgen, and Madame Curie fell into line. The discovery of Zeeman, verifying that the radiating particle was not an atom but an electron, worked out initially by the genius of H. A. Lorentz, fell in also with a classical theory of radiation conforming to ideas promulgated by Hertz and Larmor and FitzGerald; though it is true that certain difficulties and puzzles afterwards arose, in which all physicists, including J. J. Thomson himself, are now deeply involved.

We used to think that Clerk Maxwell had explained to us the nature of light; and so no doubt to a great extent he had; but our theory of the emission and absorption of light has been singularly complicated by the action and reaction between electrons and radiation, to which O. W. Richardson, Barkla, and other experimenters, including Compton, have contributed so much information. Especially have both complication and illumination been enhanced by the discovery of that new discontinuity which now penetrates our treatment of every interaction between ether and matter—the outcome of singularly successful speculative reasoning by Max Planck at the beginning of the present century.



Photo] [*J. Palmer Clarke.*

Fig. 5.—Sir J. J. Thomson, O.M., F.R.S., Director 1884–1919.

The accuracy with which both the charge and the mass of the electron are now known (largely through the ingenious measurements of Prof. Millikan), and the researches of spectroscopists all over the world, from Kayser and Runge, Schuster and Hicks, Balmer and Rydberg, down to Prof. A. Fowler—illuminated as they are by the discoveries of Rutherford and the brilliant constructions of Bohr—have raised an enduring monument, a sort of cathedral, glorifying the electrical theory of matter. To the completion of that splendid structure, the present generation, and probably many future generations, are contributing and will contribute. All this must be a great satisfaction to the Master of Trinity, who is still so happily engaged in his own researches that he has perhaps scarcely time to bethink himself of the beauty of the edifice which has been raised on his foundation-stone.

Of course J. J. Thomson has done many other things. At a very early stage he secured the Adams Prize for a theory of the behaviour of vortex rings, which may still one day be usefully drawn upon when the hydrodynamical structure of the ether, already begun long ago by Larmor, is seriously undertaken. Again, the electrical theory of matter would be nowhere if we did not know that an electric charge possessed inertia. The resolution of matter into energy, insisted on by the theory of relativity, has as one of its props that early paper of J. J. Thomson's in 1881 (forty-five years ago!), in which he calculated the extra mass conferred on a body by an electric charge; a suggestive idea to which both he and Heaviside contributed the further illuminating fact that that inertia would be increased by locomotion; a prediction which, when verified by Kaufmann, made an electrical or ether-field theory of matter inevitable.

Thomson did not limit himself to metrical determinations about negative corpuscles and cathode rays. He attacked also the positive or matter rays; and thereby developed a fundamental technique of positive-ray analysis, which speedily detected a variant of neon, and then in the hands of Aston defined atomic weights with altogether unexpected precision. This method, brilliantly and pertinaciously applied, has fully upheld the isotopic conception of Soddy (preceded as that was by the unorthodox speculation of Crookes at the Birmingham meeting of the British Association in 1896), and has thereby not only brought to our ken a host of new substances with similar chemical but different physical properties, but also has established in modified form the hypothesis of Prout, that atomic weights are really whole numbers, of which hydrogen is very nearly, though not quite, the common unit.

Readers who wish for a more detailed reminder of the steps which have led to these great advances may refer back to NATURE, vol. 91, p. 1, where Prof. Righi of Bologna contributes an article appreciating, from the Continental point of view, the work of Sir J. J. Thomson up to the date 1913.

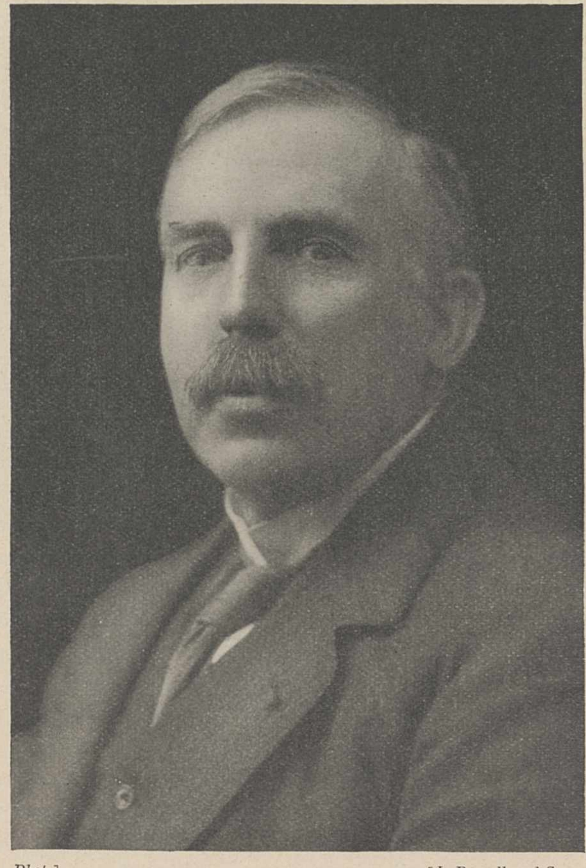
Students and disciples all over the world could contribute far more details. His own son, the professor of physics at Aberdeen, is one of the brilliant products of the Cavendish Laboratory; and many prominent physicists, such as the present Lord Rayleigh, could testify with intimate knowledge of the work of that laboratory during Thomson's régime. They know the doubts and hesitations which had to be set at rest before the absolute uniformity of electronic charges could be confidently asserted. They know the persistent help given by Mr. Everett, his laboratory assistant for nearly forty years. They are acquainted with the incipient stages of many discoveries. But an older physicist esteems it a privilege to write this brief appreciation of the achievements of one who has worked with unexampled power in the borderland between chemistry and physics, who has introduced into that great science of chemistry revolutionary conceptions the end of which none of us can see, and who is still happily flourishing and active.

Sir Ernest Rutherford, O.M., P.R.S.

By Prof. NIELS BOHR, For.Mem.R.S., University, Copenhagen.

FOLLOWING the kind invitation of the editor to write a few words in appreciation of the work and influence of the present director of the Cavendish Laboratory, I presume that the readers of NATURE will not need any detailed exposition of his achievements. As, however, I am one of those who have had the good fortune to come into close personal and scientific contact with Sir Ernest Rutherford, it is a great pleasure to me to try to describe briefly how we, who are proud to count ourselves among his pupils, regard him.

My own acquaintance dates from the period when Rutherford, after years of ardent and successful collaboration with Sir J. J. Thomson in the Cavendish Laboratory, had left Cambridge, and—after his stay at McGill, where his work on radioactive substances had established his fame—in Manchester had founded a school for investigations in radioactivity. This centre attracted young scientists from all parts of the world. In the spring of 1912, on my first visit to Manchester, the whole laboratory was stirred by one of the great discoveries which in so full a measure have been the fruits of Rutherford's endeavours. Rutherford himself and his pupils were eagerly occupied with tracing out the consequences of his new view of the nuclear structure of the atom. It would give only a poor impression of our trust in his judgment for me to say that nobody in his laboratory felt the slightest doubt about the correctness and fundamental importance of this view, although naturally it was much contested at that time. I remember being told by Hevesy soon after my arrival the story circulating in the laboratory of how Rutherford, shortly before his discovery, in a conversation with Moseley expressed the opinion that after all the troublesome investigations of the preceding years—during which he had such faithful assistance from Geiger—one would have had quite a good notion of the behaviour of an α-ray, were it not for the return of a minute number of these rays from a material surface exposed to an α-ray bombardment. This effect, though to all appearances insignificant, was disturbing to Rutherford, as he felt it difficult to reconcile it with the general ideas of atomic structure



Photo] [*J. Russell and Sons.*

FIG. 6.—SIR ERNEST RUTHERFORD, O.M., P.R.S., Director 1919– .

then favoured by physicists. Indeed, it was not the first, nor has it been the last time, that Rutherford's critical judgment and intuitive power have called forth a revolution in science by inducing him to throw himself with his unique energy into the study of a phenomenon, the importance of which would probably escape other investigators on account of the smallness and apparently spurious character of the effect. This confidence in his judgment, and our admiration for his powerful personality, was the basis of the inspiration felt by all in his laboratory, and made us all try our best to deserve the kind and untiring interest he took in the work of every one. However modest the result might be, an approving word from him was the greatest encouragement for which any of us could wish.

When the War broke out, the little community in his laboratory was dispersed. Having, however, then taken up a lecturing post in Manchester, I had the opportunity in the succeeding years of witnessing the undaunted spirit and never-failing cheerfulness of Rutherford even in the most difficult times. Although the study of the more practical physical problems arising in connexion with the defence of his country took up practically all his time and energy in those years, he could still towards the end of the War find leisure to prepare for, and finally accomplish, perhaps his greatest scientific achievement, the transmutation of an element through the disintegration of the atomic nucleus by impact with α-rays; an achievement which may be said indeed to open up a new epoch in physical and chemical science.

Just at this time Rutherford was, on Thomson's retirement, offered the directorship of the Cavendish Laboratory as his unrivalled successor. I remember on a visit to Manchester during the Armistice hearing Rutherford speak with great pleasure and emotion about the prospect of his going to Cambridge, but expressing at the same time a fear that the many duties connected with this central position in the world of British physics would not leave him those opportunities for scientific research which he had understood so well how to utilise in Manchester. As everybody knows, the sequel has shown that this fear was unfounded. The powers of Rutherford have never manifested themselves more strikingly than in his leadership of the Cavendish Laboratory, the glorious traditions of which he has upheld in every way. Surrounded by a crowd of enthusiastic young men working under his guidance and inspiration, and followed by great expectations of scientists all over the world, he is in the middle of a vigorous campaign to deprive the atoms of their secrets by all the means which stand at the disposal of modern science.

The Cavendish Laboratory: 1876–1900.

By Sir Richard Glazebrook, K.C.B., F.R.S.

It is fifty years since I first entered the Cavendish Laboratory. Eight years previously physics, in the form of questions on heat, electricity, and magnetism, had become a part of the Mathematical Tripos, and in 1869 a syndicate had reported in favour of founding a special professorship to take charge of these subjects, providing him with a laboratory, a demonstrator, and apparatus to make his teaching practical. Two years later, in 1870, the Duke of Devonshire offered to provide the material part of the scheme "so soon as the University shall have in other respects completed its arrangements for teaching Experimental Physics."

Clerk Maxwell became professor on March 8, 1871, and the laboratory was handed over to the University by its donor, the Chancellor, on June 16, 1874, confident, as he said, "that within its walls researches will be carried out which will advance to no small extent the fame of our ancient University." A prophetic statement; for within those walls have worked Maxwell, Rayleigh, Thomson, and Rutherford, men whose names will ever be landmarks in the history of British physics.

To complete the history of the building, a large ground-floor room for the practical instruction of the M.B. students was added in 1893, while in 1906 Lord Rayleigh offered to devote the greater part of the Nobel prize awarded to him to defraying much of the cost of a new wing; this was completed in 1908. In more recent years the erection of the new engineering laboratory has set free buildings and space adjoining the Cavendish, and a further important extension has been added to the laboratory.

To go back, however, some fifty years. In those days students came slowly. W. M. Hicks was the first. Wm. Garnett had become demonstrator on the opening of the laboratory. J. E. H. Gordon was the first to submit for publication the results of a research conducted at the Cavendish in a paper on the magnetic rotation of water, read before the Royal Society in 1875. When I became a student along with W. N. Shaw and Poynting in 1876, Chrystal and Saunder were at work on the verification of Ohm's law; Schuster joined a few months later. W. D. Niven had an investigation in progress, and soon afterwards Donald MacAlister carried out a modification of the well-known Cavendish experiment on the inverse square law of electrical attractions. Poynting began his classical research on the determination of the constant of gravitation during this same period.

Experimental physics was a novel subject at Cambridge. In the Mathematical Tripos of 1876, Lord Rayleigh was an examiner and had set a question on the Wheatstone bridge method of measuring resistance, with an easy rider. I had read carefully much of Maxwell's "Treatise on Electricity and Magnetism," published in 1873, but, alas, had paid no attention to Chapter xi. on "The Measurement of Electrical Resistance." My coach had originally marked it with a large O—omit. It is true he had corrected this later, but I had no idea of what was meant by Wheatstone bridge. My annoyance was great when, on my first visit to the Cavendish, Maxwell himself explained it and I realised the simplicity of the question I had passed over.

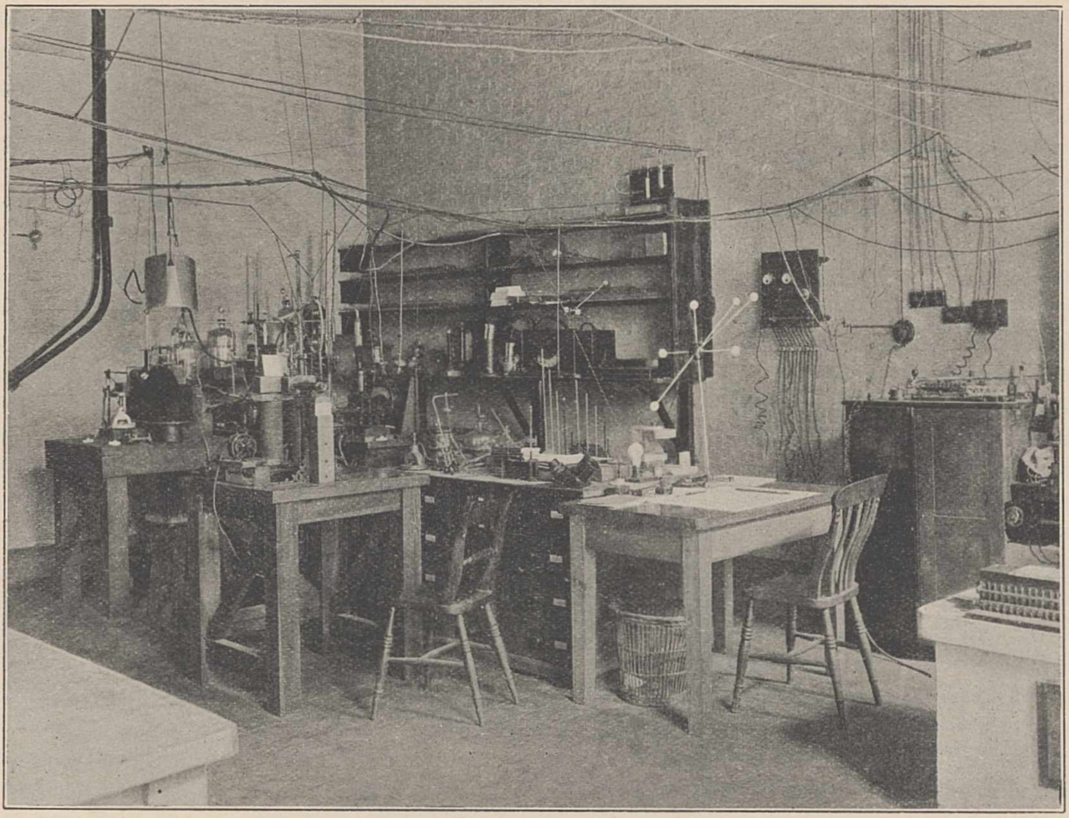


FIG. 7.—General view of Sir Ernest Rutherford's research room. On the extreme left is the electromagnet and microscope used in experiments on the artificial disintegration of some of the elements by bombardment with α-rays. On the extreme right is part of the pillar used by Lord Rayleigh in his determination of the value of the ohm and the ampere. This room was occupied for more than twenty years by Sir J. J. Thomson, for his experimental researches.

In those days there were no organised classes in practical physics. Maxwell set us on to gain acquaintance with some instrument, to adjust it to get it to work and make some measurements. Among my earlier experiences I was told off to measure at regular intervals the E.M.F. of a battery of tray Daniell cells used by Chrystal and Saunder. The instrument with which I had to do this was the large pattern of Thomson's absolute attracted disc electrometer, a horrid monster which few students of to-day have ever seen even in a text-book, and fewer still have used.

However, I soon became absorbed in optical work. Stokes, in his well-known report on optical theories, had described a method of determining the form of the wave surface in a crystal and had applied the method to Iceland spar. He encouraged me to attempt the same for a biaxial crystal and lent me the goniometer he had used. Schuster obtained for me from Hilger a suitable piece of aragonite and the work went on for some time in a dark room at the top of the Cavendish; eventuating in a paper published in the *Philosophical Transactions* for 1878, in which it was shown that Fresnel's wave surface was consistent with experimental results to a very high order of accuracy.

During the progress of the work Maxwell was a frequent visitor, interested in its progress, and ready to help. In the case of a serious difficulty he would remark that others had been asking him questions and he must take time. On one occasion I remember him explaining how thick the crust was in which their questions had encased his brain, but the next day he turned up, accompanied as usual by his dog, with the remark, "If you will alter your arrangements in such and such a way you will find your difficulties disappear," and of course they did.

Those were happy days; there have been many since at the laboratory, but the study of a new subject was full of interest, and the gradual realisation of what experimental investigations with the view of the improvement of material knowledge involved, added much to the pleasure of life and fitted one more and more for the teaching work to come in the future.

In 1879 came a change; Maxwell's health gave way during the Easter term of that year, and after a painful illness he died on November 5. The professorship was continued—it was to terminate originally with the tenure of office of the first professor unless the University determined otherwise—and Lord Rayleigh was appointed on December 12, 1879. He had accepted the post for a limited period—five years—and began work early in 1880. He was anxious from the first to organise regular teaching in practical physics, and appointed Shaw and myself as demonstrators soon after his election. The notice of the work for the Michaelmas term, 1880, after announcing lectures by the professor, by Dr. Schuster on radiation, and by Shaw and myself, continues: "The laboratory will be open daily . . . for those who have had the necessary training. Courses of demonstrations will be given during the Lent term on heat and advanced electricity, and magnetism, and during the Easter term on light, electricity, and sound."

The scheme was something of an innovation in one important detail. Shaw and I were college lecturers, receiving nearly all our remuneration from our colleges; we were among the first to give college lectures in a university building. Hitherto there had been a marked distinction between the lectures of a professor and college teaching; the growth of the natural sciences brought a change. Various colleges still had their own classes and laboratories; at Trinity, Coutts Trotter lectured on physics in college. In physiology and comparative anatomy, however, Foster as Trinity prælector, Frank Balfour and Sedgwick as college lecturers, worked in university buildings. It was clearly out of the question for each college to have its own staff and laboratory in all the new subjects, and so the plan of giving university recognition to college lecturers was gradually evolved, a plan which this year the new statutes have made universal; the formal lectures in all subjects are now given by university officers.

The task we had undertaken was a novel one for us both. Shaw had worked at Freiburg with Warburg and at Berlin under Helmholtz, but at neither place was there organised class teaching in practical physics. My experience had been gained solely as a research student under Maxwell. There were no text-books properly so called. We drew largely on Kohlrausch's "Physical Measurements" and Pickering's "Elements of Physical Manipulation." Pickering at the Massachusetts Institute of Technology had organised a method of instruction which we followed in great measure. There were, it is true, physical laboratories under Wm. Thomson (Lord Kelvin) at Glasgow, Carey Foster at University College, London, Adams at King's College, and Clifton at Oxford. But the time given to practical physics was short, the means of instruction scanty, and there was comparatively little endeavour to teach the principles of physics to a student by means of experiments which he performed himself.

Gradually the system developed; note-books descriptive of each experiment were written, and these in due time became Glazebrook and Shaw's "Practical Physics." The students were taught to describe in their own note-books their work, and enter up their observations; in the advanced class they were expected to read up the theory referred to and work to a high degree of accuracy. The material on which we drew for students was of a very high order. In my class on advanced electricity in 1881, I find the names among others of J. M. Dodds, S. L. Hart, W. B. Allcock, Edward Hopkinson, A. R. Forsyth, R. S. Heath, J. Ryan, and R. Threlfall.

Above all we had the wise counsel and cordial help of the professor: the course was planned and the syllabus arranged after careful counsel taken with him. We could go to him for advice in case of difficulty, otherwise we were free to teach as seemed best and to develop the work in the directions which seemed most desirable. In due time we had efficient helpers, Newall, Wilberforce, Searle, Skinner, Threlfall, Fitzpatrick, Callendar, Whetham, and McConnel, to name only a few, though some of these belong to a later period than that I am now describing.

It was, however, not merely the organisation of teaching which gave interest to the work. The laboratory began to be, to an extent as never before, a centre of new knowledge. A glance at Lord Rayleigh's collected works will show the importance of his output during the five years' tenure of the chair. The experiments of Rowland and Joule had led definitely to the result that the resistance coils constructed by Fleeming Jenkin and Maxwell in 1863 to represent the ohm (10^9 C.G.S. units of resistance) were in error. The Standards Committee of the British Association was reconstituted at Swansea in 1880, and Lord

Rayleigh at once began to take an active part in its work. His first series of experiments with the rotating coil of the original committee, carried out with Schuster, confirmed the fact that there was an error in their determination. Further experiments with a new rotating coil in 1882, and by the method of Lorenz, with the assistance of Mrs. Sidgwick, in 1883, led to definite values from which there has been little variation up to the present. Meanwhile he had arranged that I, as secretary of the B.A. Standards Committee, should undertake to test and issue certificates for resistance coils, and this work went on at the laboratory for the next fifteen years, being ultimately transferred to the National Physical Laboratory.

Throughout this period, and indeed until the end of his life, Sir William Thomson was a frequent visitor to the laboratory, enthusiastic always, keenly interested in the experiments in progress, and full of helpful encouragement for us all.

Work on the electrochemical equivalent of silver, and the E.M.F. of a Clark cell, followed quickly. At a later date Lord Rayleigh took an active part in designing the standard ampere balance for the Board of Trade. He acted as president of the International Electrical Conference held in London in 1908 at which the definitions of the International Ohm, Ampere, and Volt were agreed to by delegates representing Great Britain, certain of her colonies, and twenty-two other countries. The results then reached find their main support in his work[1] during the years 1881–1885.

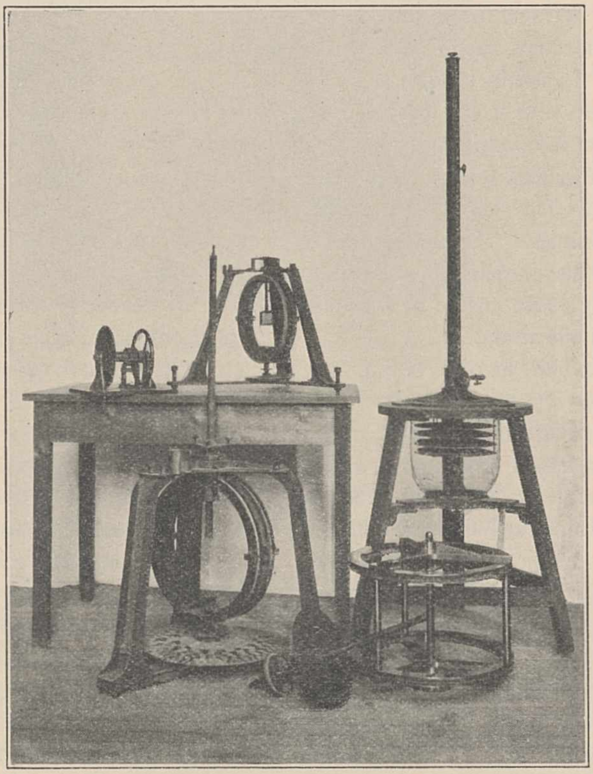


FIG. 8.—Some classical apparatus. The coil on the table is the original spinning coil of Maxwell and Fleeming Jenkin used for the determination of the ohm (1864); it is now on loan at the Science Museum, South Kensington. The coil below is the spinning coil of the late Lord Rayleigh, with stroboscopic attachment, used for the determination of the ohm (1882). The coils on the right were made by Chrystal and used by Glazebrook in his determination of the ohm. On the right is Maxwell's apparatus for measuring the viscosity of air at different pressures. A phonic wheel of Rayleigh's stands to the left on the table.

Some of the apparatus used in these and in the earlier experiments of Maxwell is shown in Fig. 8, which is reproduced in "A History of the Cavendish Laboratory, 1871–1910," published by Messrs. Longmans, Green and Co., to which volume I have referred for much in this article. At about the same period the original resistance coils of the B.A. had been brought to the laboratory, and Fleming commenced his long series of electrical researches by a careful intercomparison of these. The Darwins—George and Horace—were at work on the lunar disturbance of gravity. Schuster, in addition to assisting in the electrical experiments, continued his investigations into spectroscopy and radiation. J. J. Thomson's paper — the first dated from the Cavendish Laboratory—"On some Electromagnetic Experiments with open Circuits," appeared in the *Philosophical Magazine* for July 1881. In the previous April his paper on the electric and magnetic effects produced by the motion of electrified bodies had been published; in it he demonstrated that a moving charge will act as though it possessed mass of amount $\frac{2}{3}e^2/a$, where e is the strength of the charge supposed to be treated as a small sphere of radius a. Other papers rapidly followed, while in 1883 a determination of the number of electrostatic units in the electromagnetic unit of electricity was printed in the *Philosophical Transactions of the Royal Society*, and a paper on a theory of the electric discharge in gases in the June number of the *Philosophical Magazine.*

Shortly before his death, Maxwell had called Shaw back from Berlin to undertake work for the Meteorological Council, and thus began, with a paper on the measurement of temperature by water vapour pressure (1883), followed by his report on hygrometric methods (*Phil. Trans.*, 1888), that long series of investigations which has gained him recognition as the leader of modern meteorologists.

My own work on the form of the wave surface of

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[1] It may be of interest to compare the values for the ohm (109 C.G.S. units of resistance) expressed as the length of a column of mercury found at the Cavendish Laboratory in 1882 and 1883 with the most recent values.

1882 Rayleigh	106·26 cm.
1882 Glazebrook	106·25
1883 Rayleigh and Sidgwick	106·24
1913 F. E. Smith	$106{\cdot}24_5$
1920 Grüneisen and Giebe	$106{\cdot}24_6$

biaxial and uniaxial crystals was continued in 1886 by McConnel in his paper on quartz. I was diverted from optical work by the duties which came to me on my appointment in 1882 as secretary of the Electrical Standards Committee. Chrystal, when at the laboratory, had constructed a ballistic galvanometer and wound a pair of large coils for a determination of the ohm by a modification of Kirchhoff's method. With the aid of two of the senior students, Dodds and Sargant, these were set up and a series of determinations made, with the result already given, practically identical with that obtained by Lord Rayleigh. These same coils were used by him for his Lorenz experiments, and the close concordance of the results is of importance as an error in the calculated value of the mean radius of the coils would affect them in opposite directions.

At the end of 1884 Lord Rayleigh resigned and J. J. Thomson became professor. The work of teaching continued on much the same lines. The new professor realised no less fully than his predecessor the value of practical physics not only as an engine of research but also as an instrument of education.

Thomson, assisted by Threlfall, at once began that series of experiments on the discharge of electricity through gases which has occupied him until now. Newall returned to Cambridge and shortly afterwards became demonstrator in succession to Shaw, who in 1887 was made tutor at Emmanuel College. About this time Newall, Threlfall, Cassie, Wilberforce, Fitzpatrick, Callendar, and Chree were at work on various problems, while Olearski and Natanson were the first of the long series of research students attracted to Cambridge by the growing fame of the professor.

An addition of some importance was made to the laboratory teaching in 1888, when practical classes for medical students were commenced. For some time previously a viva voce with instruments had been part of the first M.B. examination, and the introduction of more definite teaching suited for the examination became desirable; Fitzpatrick took charge of this. Meanwhile the other classes continued to increase, and as the teaching of practical physics spread to the schools it was necessary to devise fresh experiments of a somewhat more advanced character. Searle became demonstrator in 1891 and threw himself with great zeal into this work.

In 1895 a change of far-reaching importance was made in the University regulations. Up to that date research students from other universities had no recognised position in Cambridge; some few there had been, admitted to the laboratories by the courtesy of individual professors. From this time onwards graduates of other universities, and in some cases students who were not graduates, could enter the University for purposes of research if they satisfied the proper authorities of their competence. They could obtain a degree at the end of two years if they submitted to the Board of Studies a thesis which in the opinion of the Board was "of distinction as a record of original research." In the October term after these regulations were passed, Rutherford, Townsend, and McClelland came as the first students; their number is now legion.

Rutherford's earliest work was on a detector of wireless waves. Hertz's experimental verification in 1887 of Maxwell's theory of electromagnetic waves had been received with great interest when first published. We knew the theory to be true—it was Maxwell's—and to have it verified by direct experiment had been no small satisfaction to Maxwell's pupils. I well remember some time in 1896 being taken by Rutherford into his room to see his magnetic detector at work receiving a message from, I think, the observatory.

Meanwhile Thomson had continued his work on the passage of electricity through gases, and the discovery of the Röntgen rays in 1895 gave him and the band of students he had collected a powerful method of investigating the subject. In 1897 came the discovery of the electron. Perrin in 1895, and Thomson two years later, had proved that the cathode rays of Crookes consisted of a stream of particles each carrying a negative charge. Thomson in 1897 measured the ratio of the mass of each particle to its charge and also the velocity with which the particles travel. The quantity, m/e, was found to be much smaller than would be the case if m represented the mass of the hydrogen atom and e its charge. Moreover, its value was independent of the nature of the gas in the tube, and he concluded that we have in the cathode rays a condition "in which all matter that is matter derived from different sources such as hydrogen, oxygen, etc., is of one and the same kind; this matter being the substance from which all the chemical elements are built up."

In the same year, 1897, Kaufmann and Wiechert determined independently the value of the ratio m/e, arriving at similar results. Schuster in 1884 had suggested that if the cathode rays consisted of charged particles it was possible to determine the ratio e/m by a method practically identical with that employed later by Kaufmann, and in 1890 published results of experiments which led to limits for the value of e/m for the cathode rays.

In the following year Thomson showed that the charge e carried by one of his corpuscles or electrons was the same as that of an atom of hydrogen, and from this it followed that the mass of the electron is about 1/1800 of that of an atom of hydrogen. The existence of the electron, the fundamental unit of negative electricity, became an accepted fact, and the long series of modern discoveries connected with the names of Thomson, Rutherford, Bohr, and their pupils was the result.

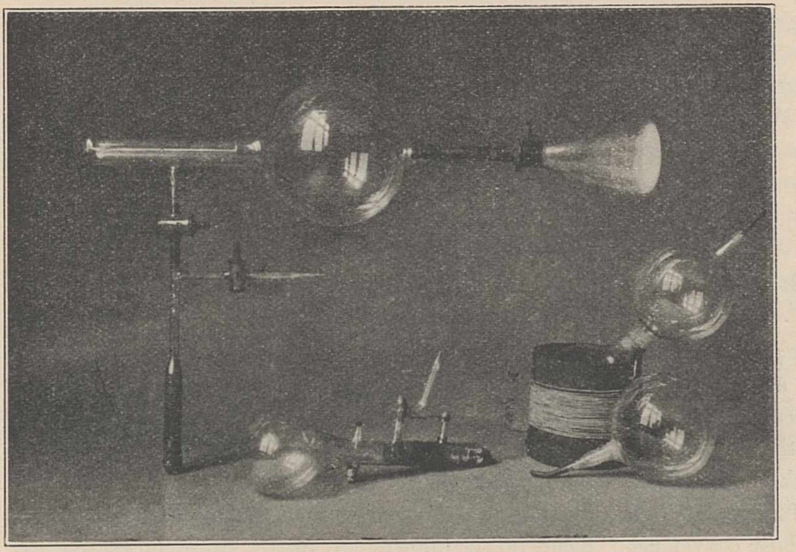

Fig. 9.—Apparatus used by Sir J. J. Thomson in his investigations. The upper tube is an early form of positive ray tube for visual examination of the parabolas. The lower, left hand, tube is one used to show the electrical deflexion of the cathode rays in 1897. By measuring the deflexion of a pencil of rays in an electric as well as in a magnetic field, the velocity and the ratio e/m for the electron were determined. On the right are examples of electrode-less discharge bulbs.

Thomson and his pupils continued this work. McClelland examined the Lenard rays which appear on the opposite side of a thin sheet of metal on which the cathode rays impinge, and showed that they were identical in properties with cathode rays. Stokes had suggested that the X-rays of Röntgen consisted of pulses set up by the sudden stoppage of particles carried in the cathode rays. Thomson investigated the nature of the effects produced by such a sudden stoppage, proving that it would give rise to a thin pulse of very intense electric and magnetic force travelling out from the particle with the velocity of light. He later applied this theory to explain the secondary radiation produced when X-rays fall on matter. Langevin, the first of the foreign students to enter the laboratory under the new regulations, investigated the character of this secondary radiation and published his results in his thesis for the degree of Doctor of Science in Paris.

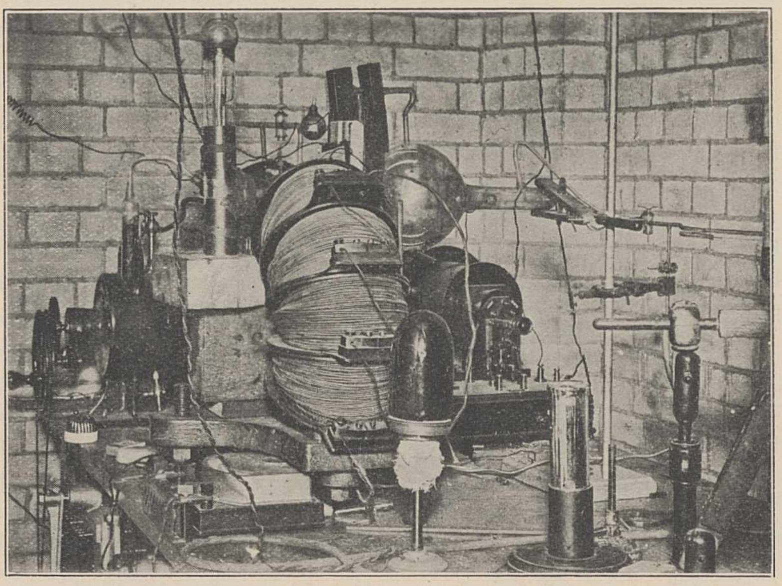

Fig. 10.—Apparatus for the analysis of positive rays by Sir J. J. Thomson's 'parabola' method. This was the first made in which the photographic plate could be lowered into position by a vacuum-tight winch. It was in this room, with a slightly modified apparatus, that the duplicate parabolas of neon were first photographed, in 1912.

New knowledge grew rapidly. Rutherford, Townsend, H. A. Wilson, Zeleny, Strutt, and others contributed. C. T. R. Wilson's paper on the condensation nuclei produced in gases by the action of various rays, published in the *Phil. Trans.* for 1899, proved of far-reaching importance. As time went on the influence of the Cavendish researches spread to other universities. Rutherford carried it across the Atlantic to Montreal, Townsend to Oxford, McClelland returned to Dublin, Langevin to Paris, while others came to take their place.

My days at Cambridge closed with my appointment as Principal of University College, Liverpool, toward the end of 1898. Formal connexion with the laboratory had ceased some two years earlier when I had undertaken administrative work for my College. The last experimental investigation I helped to carry out was undertaken along with Lodge, who spent a summer vacation in Cambridge, as an essay to be submitted for a prize offered by Elihu Thomson. The investigation included a determination of v by means of the measurement of the frequency of the oscillations in a circuit of known capacity and inductance, and was published in the Stokes Memorial volume issued in 1899. Up to that time I had been closely connected with the Cavendish for nearly twenty-five years and have written of things I knew. I must conclude now and leave to others more intimately acquainted than myself with the progress of the last quarter of a century to tell in detail what that progress has been. Thomson continued his work until 1918, through the troubled period of the War, when the researches of the Cavendish assisted to the full in the solution of many problems. In that year Dr. Butler, the revered Master of Trinity, died, and Thomson was chosen by the Crown as his successor. He resigned the Cavendish professorship with its charge of the laboratory, and the University created for him a professorship of physics which will cease with his tenure of the post. Rutherford followed as professor, and under his genial direction he and his 'boys' from day to day increase the sum of natural knowledge by their amazing results and add in no small degree to the fame and influence of the Cavendish Laboratory, thus verifying the forecast of its founder more than fifty years ago.

Research Work in the Cavendish Laboratory in 1900–1918.

By Prof. J. A. Crowther.

The progress of science and the life of a laboratory have little connexion with the artificialities of chronology, and the beginning of the century does not coincide with any natural division in the work of the Cavendish Laboratory. If, for the sake of clearness, we must make a division in what was fundamentally a natural and continuous growth, our most convenient date will be the year 1903, and our obvious landmark the publication of the first edition of Thomson's monumental book, "Conduction of Electricity through Gases." Here we find for the first time a complete and logical account of the solid achievements of the previous eight or ten years of remarkable activity. Here also we find the foreshadowings of the ideas which were to direct and regulate a large part of the work of the succeeding years.

In the main the problems attacked in the years before 1903 were those of the mechanism of the conduction of electricity through gases. Some of these, it is true, were carried over into the succeeding period. Some, perhaps, have not yet been fully solved, but by 1903 the foundations of the subject had been well and truly laid. It had been proved, beyond doubt, that the conductivity imparted to gases by various agents, such as X-rays, the rays of radium, and ultra-violet light was due to the formation of charged particles or gaseous ions in the gas, and many of the properties of these ions had been determined. Rutherford had determined their mobility; Townsend had determined their rate of diffusion through various gases; C. T. R. Wilson had shown that they could serve as nuclei for the condensation of water in a supersaturated space. Thomson had discussed their motion, and shown that in all probability they consisted of single molecules of the gas in which they were formed.

In a closely allied subject Thomson had demonstrated experimentally the existence of the electron, or negative corpuscle as we then called it, and had determined its few simple properties. The measurement of the electronic charge actually falls within the present century, though preliminary experiments had been made somewhat earlier. Determinations were made independently by Thomson and by H. A. Wilson, by somewhat different methods, and were published in 1903. During the same period Zeleny made new measurements of ionic mobilities, which when combined with further measurements on the diffusion of ions by Townsend established conclusively the identity of the charge on a gaseous ion with that on a monovalent ion in solution. These experiments mark the culmination of the first epoch.

When I commenced research work in 1905 the second epoch of activity was already well under way. With the exception of C. T. R. Wilson, who in the advanced laboratory was engaged in imparting to a new generation of students those experimental arts of which he is a master, the first group of workers had all dispersed. Their work, as collected in the pages of "Conduction through Gases," formed our text-book, and was the foundation on which we hoped to build.

It was a very happy, though crowded, community to which I had the honour of being admitted. The space available for research had not been extended since the laboratory was built, and what had been adequate for ten or twelve, the average number of research workers before 1903, was becoming a tight fit now that the numbers had risen to an average of twenty-five. A competent observer records that there was more physics to the square centimetre in the Cavendish than anywhere else in the world, and the resources of the laboratory in the way of apparatus were sometimes severely tested, though I cannot recall that they ever broke down. The oft-repeated legend that we worked entirely with sealing wax and string, though flattering to our technique, has little foundation in fact. The apparatus for the new experiments which we wished to make did not exist. It had to be invented and made on the spot. An adequate workshop, staffed with extraordinarily competent and helpful mechanics, with F. Lincoln as chief, carried us through the worst of our difficulties; while Everett, the professor's private assistant for so many years, was always willing to lend a hand in the worst intricacies of glass-blowing. For the rest there was the spirit of cheery fellowship and good comradeship which extended from the professor to the rawest recruit, and bound us all into a genuine community.

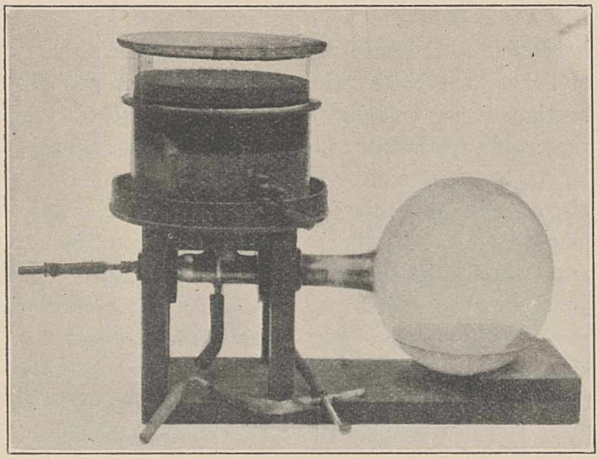

Fig. 11.—Prof. C. T. R. Wilson's original expansion apparatus for making visible the tracks of ionising particles. It was made in the Cavendish Laboratory in 1911 and is the apparatus with which his photographs of the tracks have been obtained.

It is clearly impossible to attempt to give any adequate account of the activities of so many workers, extending for so many years, within the bounds of a single article. A mere list of publications would fill more space than I have at my disposal. I can only outline the general trend of discovery in the laboratory as I saw it, and many excellent pieces of research such as those of Burton on colloids, of Wellisch on ionic mobilities, of Vegard on osmosis, and of C. T. R. Wilson on atmospheric electricity, must pass unnoticed. This is the more unfortunate in that it may tend to accentuate the erroneous impression that the research work of the Cavendish has been entirely specialised and one-sided. This has never been true. Workers with ideas in any branch of physics have always received a warm welcome, and the Cavendish has rarely been without them. The majority of the research workers, however, came to the laboratory as young students, to learn their job, and it was natural and inevitable that their work should be inspired and directed by the genius of Thomson, and should be correlated to the subjects which he had most in mind.

The dominant problem from 1903 onwards was that of the structure of matter, to which the way had been opened by the discovery of the universality of the electron. Thomson's main theoretical contributions to the subject appeared in 1903, 1904, and 1906, and dealt with possible arrangements of electrons to form atomic structures, and the properties—electrical, magnetic, and optical—which might be expected to result from these arrangements. The substance of these papers reappeared, in a somewhat more popular form, in "Electricity and Matter" and the "Corpuscular Theory of Matter." To bring these ideas to the touchstone of experiment was, perhaps, the main preoccupation of the laboratory until the year 1912.

A study of electrical conductors seemed to offer a hopeful line of approach. If the current through a metallic conductor is carried by electrons, which share in the thermal agitation of the molecules, some of them should escape from the surface if the temperature is sufficiently raised. Preliminary experiments on the subject were made by O. W. Richardson so early as 1901, and some of the theoretical consequences were discussed by Thomson in the following year. The subject was hotly pursued by O. W. Richardson in the Cavendish until 1906, and afterwards at Princeton, and has developed into the important branch of physics known as thermionics. Thomson, Horton, and others also made contributions to the subject. The preliminary work necessary to disentangle the genuine effect from the many spurious phenomena which accompany it was tedious and harassing, but important and valuable results were eventually obtained.

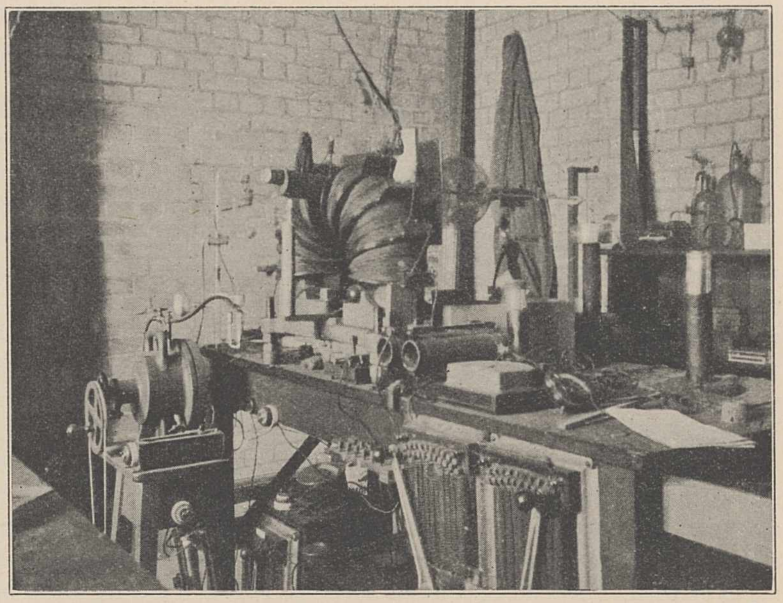

Fig. 12.—The first mass-spectrograph set up by Aston in 1919. This instrument was capable of comparing the masses of atoms and molecules with an accuracy of 1 in 1000. By its means, in five years, more than fifty elements were resolved into isotopes or shown to be simple.

The question of the radioactivity of ordinary materials, which was being investigated in the laboratory about this time by N. R. Campbell and A. Wood, also had its bearings on the main problem. Difficulties were met with in eliminating the effects of penetrating radiations from outside sources and the possibility of small traces of radium in the materials employed, and the results were indeterminate, except in the case of some of the alkali metals, which were found to emit appreciable quantities of β-radiation. Experiments made by Kleeman on the relative ionisation produced by different ionising radiations in gases, by Laby on the total number of ions produced by the complete absorption of the α-rays from uranium in different gases, and by Kaye on the output of X-radiation from targets of various metals, while important in themselves, also had a more or less direct bearing on the main problem of the atom.

In 1906 Thomson returned to the attack and gave the theory of two important methods of estimating the number of electrons in the atom of a given element. One method suggested was to measure the diffusion of a parallel beam of β-particles in their passage through thin sheets of matter. Assuming that the deflexions are due to collisions with the electrons in the atoms through which they pass, a determination of the scattering of the particles should provide an accurate estimate of the number of scattering centres. A second method consisted in measuring the intensity of the scattered X-radiation from a given radiator when subjected to a beam of primary X-radiation. Here again, assuming the correctness of the classical theory of the phenomenon which had already been given by Thomson, it should be possible to deduce the number of scattering centres from the intensity of the radiation scattered. The experimental working out of both these methods fell to my lot, and kept me occupied from 1906 to 1911. The results showed that the number of electrons per atom was quite small, approximately of the order of the atomic weight, and that the main mass of the atom must be associated, not with its electrons, but with its positive charge.

In the meantime Thomson had himself been investigating the positive charge on the atom by various modifications of the method which had proved so successful with the electron. The results of these experiments on positive rays were published in various papers from 1908 to 1914. The importance of the new method lies in the fact that for the first time atoms could be dealt with as individuals, and not merely in aggregations of many millions. The results of these experiments, together with those of Aston (1919–1925) which led to the discovery of the existence of isotopes and the whole number law for the masses of the atoms, are too fresh in the memory to need further description.

In the meantime new ideas were afloat. The phenomena of secondary corpuscular radiation excited by X-rays, as studied by Innes, and of photo-emission (Hughes and others) were becoming more and more impossible to reconcile with the classical wave theory of light. The theory that the wave front was discontinuous was put forward by Thomson so early as 1907, and experiments to test the matter were made by N. R. Campbell, and by G. I. Taylor in 1909. Laue's discovery in 1912 of the diffraction of X-rays by crystals placed a new weapon in the hands of physicists, and by 1913 W. L. Bragg, working in the Cavendish, had already employed it to measure the distance apart of the atoms in a rock-salt crystal, and to deduce an absolute value for the wave-lengths of various X-radiations. A little earlier (1911–1912), C. T. R. Wilson, by an ingenious modification of his early experiments on the deposition of water vapour on charged ions, had evolved a method of making visible the actual tracks of ionising particles in gases, a method the potency of which has not yet been fully exploited, although it is now in use in many laboratories in all parts of the world. Whiddington, by his experiments on the excitation of characteristic X-radiation by cathode rays (1912), was already approaching the quantum theory of X-ray phenomena. At the commencement of 1914 the stage seemed fully set for a new outburst of activity which should rival even the early days of the laboratory under Thomson.

Alas, however, the world stage was set for quite a different drama, and its research workers found problems for solution far different from those which they had been expecting to attempt. When the laboratory began to fill up again in 1919, it was to learn that its professor had found the double duty of directing at one and the same time a great laboratory and a great college too much for one man's strength and time, and the Cavendish professorship was resigned. This is not the end of the story. Sir J. J. Thomson still continues his theoretical and experimental researches with unabated vigour and success, and under its new chief, Sir Ernest Rutherford, the Cavendish goes on to new problems and new triumphs. But these changes may conveniently mark the end of the present article, which has already exceeded its allotted span, without doing anything like justice to the many old companions and friends by whose side I have worked in the Cavendish laboratory, and whose faces look down upon me from the framed groups upon my study walls. Having worked in the Cavendish they will understand the impossibility of the task I have undertaken, and being Cavendish men they will forgive.

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