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Inventors and Patents.

THE relations which exist between an employer and his employee as regards patents for inventions are well known and, on the whole, are just and stand the test of time. On our part, we are always ready, and indeed anxious, to uphold the rights of inventors ; none the less so when the inventor happens also to be an employee. That there have been acts of injustice towards the employee is notorious, and that an inventor occasionally suffers at the hands of his employer is beyond dispute. It is well, therefore, to be reminded from time to time of the employee's views, if only to seek opportunity for the removal of hardship under which the employee-inventor may labour. To his grievances a short article by Mr. P. Freedman is devoted in the July-August number of the *Scientific Worker*, where, by a series of selected examples, there is advanced "a rough but true picture of the present trials of the needy inventor who is an employee of a private firm."

The details, however, of these examples, whereby the conclusions which have been drawn from them may be checked, are wholly absent. But without impugning in any way the accuracy of the examples, considerable experience suggests that the addition which inventors make to the stock of public knowledge is often neither so great in amount nor so important in extent as inventors would have us believe. Many a brilliant idea proves to be almost valueless to the community unless means for presenting it in practical form are devised by those whose everyday business is the immediate satisfaction of the public wants. Employers, as is said, must live, and it is to their interest to adopt the latest and most efficient devices irrespective of the quarters in which they arise. The subtle and elusive quality of inventiveness is such as to require all the efforts of employers to stimulate its exhibition by those in their employ. Harsh and inequitable treatment of employees conduces to the satisfaction of neither party. In short, mutuality in aim with due regard to the dictates of justice are, in the long run, found to pay.

In the same article the suggestion is put forward for a patents committee to be set up to report upon novel ideas, the members of the committee being thoroughly able technical men and men of high reputation. The good opinion of this body would enable the inventor "to obtain financial backing for his idea and free him from rank robbery." It would also help, it is said, in bringing the inventor into touch with those who might assist him in furthering his aims. The author of the article deliberately abstains from elaborating the scheme in detail, but in this matter urges energetic action

as the policy of the National Union of Scientific Workers.

The idea of a patents committee of this character, it may be remarked, is a favourite one with reformers, but however much there is to be said in its favour, its formation, functions, and operations would be such as to render the idea all but impossible in practice. A very near approach to the constitution and working of such a committee was to be witnessed during the War. Many consultative bodies, in the exceptional circumstances of the time, were established by the Ministry of Munitions for estimating the value of inventions. As a result, a vast accumulation of information upon the practical working of those consultative bodies was obtained; information which, if made available to the public, would indicate how little the expectation of reformers in this direction could be realised. Sound contribution to the discussion of the relation between employer and employed as regards inventions and their mutuality of interest is ever welcome, and in the proper quarters should always receive careful attention. It is open to question, however, whether the claims of the employee will be materially enhanced by the advocacy which appears in the *Scientific Worker*, where some basic misconception occurs in respect of the employee's legal position, and where the implication lies that all employers are to be judged by reference to the action of those who abuse their position.

In a second article devoted to patents in the same issue of the *Scientific Worker* Dr. N. R. Campbell urges the entire abolition of the patent system without any definite substitute. He considers that the system gives industrialists a wholly false view of the place of science in industry and, in consequence, diminishes the number of scientific workers whom they employ. "So long," says Dr. Campbell, "as we associate scientific work with patents, the delusion that is responsible for the backward state of scientific industry in this country will continue," and, if patents were abolished, manufacturers would have to rely upon the excellence of their products and the efficiency of their processes and not on the establishment of monopolies. There must be dismissed once for all, Dr. Campbell continues, the wild idea that, by some modification of patent law or machinery, there can be wrested from the greedy capitalist some enormous profit that he makes by exploitation of the inventor.

Dr. Campbell refers also to the necessity, in the case of a really important invention, of spending many thousands of pounds upon defending a patent in the courts. This necessity is and has been a crying evil which seems to be almost inseparable from the existing patent law. It is indeed remarkable that the talent of the legal expert united with the genius of the manufacturer have not yet succeeded in evolving a scheme whereby, at a relatively small cost, the scope of an invention may be accurately defined and the validity of its protecting patent readily determined. The difficulties in the production of such a scheme, which without losing sight of the interests of the public shall yet reserve to the inventor all the rights to which he is entitled, are undoubtedly great; but surely some means are discoverable whereby the present outlay for obtaining confirmation of an important patent and the settlement of the allegation of infringement could be much reduced. The National Union of Scientific Workers would indeed be doing true yeoman's service if, in all its bearings, the Union would consider this matter and assist in bringing about a much-needed reform.

The Social Influence of the Internal Combustion Engine.

The Internal Combustion Engine. By Harry R. Ricardo. Vol. 2: *High-speed Engines.* Pp. vii + 373. (London, Glasgow and Bombay: Blackie and Son, Ltd., 1923.) 30*s.* net.

IT does not seem long ago, though actually fourteen years have passed, since we overheard a well-known man of letters gently curb the impulsive pen of a younger writer who wished to record an impression that the increasing use of the internal combustion engine must prove a vital factor in our coming civilisation. Events have moved so rapidly that it now seems surprising that this impression could have appeared to be an overstatement. But fourteen years ago the man-in-the-street was quite unaware of what sort of thing an "internal combustion engine" might be, if indeed such a thing existed. Moreover, until that time no engineering writer had even dared to put so unfamiliar a title on the cover of his book. Gas engines, oil engines, and petrol engines were of course known, but it was scarcely suspected that apparatus of that sort was likely to have any primary effect on world history.

Who would have then divined that the introduction by Daimler, fourteen years still earlier, of the high-speed petrol engine was of such potentiality that it would become a debatable point whether our "sure shield," the British Navy, should be allowed to have its Singapore base strengthened, in advance of the provision of an adequate home defence force of those aircraft the very existence of which is one outcome of the pioneer work of Daimler? This is, it is true, merely a military parallel, but equally striking ones could be drawn from civilian activities. The development of internal-combustion-engine road transport is one of the

chief characteristics of the age we live in. Even in England it is sufficiently striking, but in the United States one person in every ten, man, woman, and child, has an automobile; an average of one to every alternate household. Even so the continued output of the Ford factory is measured in thousands of cars per day. America may be a land of wide spaces, but if this rate should continue it is not difficult to foresee a further field for the activities of "control" societies, this time aiming at control of the Ford "birth rate."

The growth of road transport was not due to the stimulus of the War: it was in full steady growth before 1914. But in the case of aviation, the future of the aero-engine as a prime mover is, and must be, vitally affected by the stimulus which grew from the War and still continues. During the War itself the best scientific and engineering talent was encouraged by every possible facility, and by lavish outpourings of money, to produce yet newer and newer developments of the internal combustion engine—whether for aviation, tanks, seagoing craft, or road transport; but chiefly for aviation. Aviation offered then a prospect of a way out from what seemed an endless deadlock; people had begun to fear that in the great struggle, there had unconsciously been invented a new, and very unpleasant, way of life. Since then a relatively impoverished world has sought to find less costly means of defence than the old; and the public, led doubtless by the results of certain American experiments, has begun to look towards the relatively cheap defence by aircraft as affording a loophole for escape from financial burdens which might threaten to become overwhelming. As a mere business proposition, therefore, it "pays" to encourage aviation, and the surest path of progress in this sense lies in the development of improved aero-engines which shall be of unprecedented power, of extreme lightness, and yet be able to operate with equal facility at any altitude and at any temperature.

One of the pioneers in this necessary development of the internal combustion engine is Mr. Ricardo. We reviewed some little time back the first volume of his book (NATURE, January 13, p. 43). That volume dealt mainly with the older slow-speed engine. The second volume is concerned with the high-speed engine and with its utilisation for certain specialised purposes. It is a fine record of scientific research work; carried out in no small measure by Mr. Ricardo himself, or by those with whom he has been associated, directly or indirectly, through the medium of the Aeronautical Research Committee.

We are well acquainted with most of the books on this subject which have appeared during say the last twenty years, and it is striking to reflect on the change in the point of view shown by the writers at the beginning of that period and at the end. If Mr. Ricardo be taken as typical of the modern writers, and to do so is to pay them compliment, it will be seen from even a cursory survey of the present volume that nothing, however traditional in the art, is taken for granted. Each problem is stated in scientific language, and critically and dispassionately examined: very often the results are unexpected, but whether strange or not, this critical review of them has the immense merit that a chain of possible causation is constructed to which new links can be added, by those who have scientific imagination and insight, leading to new lines of development. Each piece of analysis in fact is made to carry within it the germ of the next step forward.

The mere bulk and weight of Mr. Ricardo's two volumes is forbidding, and might with advantage have been lessened. That, however, is probably more a matter for the publishers than the author. The author has done his part of the work well, though the book would certainly have gained by the freer use of the blue pencil. We have found very few mistakes, though the puzzling letter-press associated with figure 33 on p. 121 does not seem to us to make the carburation procedure represented intelligible to the average reader.

Mr. Ricardo is evidently not satisfied with the present position as to fuel supplies. As is well known, much of his own research work has been devoted to fuel questions—his investigations for the Asiatic Petroleum Company he has fortunately been in a position to make public: much to the credit of that firm. We quote from the present volume: "The mobile internal combustion engine is now no longer a luxury; it has become one of the prime necessities of peaceful civilisation and the prime necessity in time of war; therefore, the assurance of its fuel supply should be considered a matter of national importance. It is perfectly well known that alcohol is an excellent fuel, and there is little doubt that sufficient supplies could be produced within the tropical regions of the British Empire, yet little or nothing is being done to encourage its development." It must be remembered, however, that although plants well suited for the production of alcohol are easily grown in, say, tropical British Africa, it is likely to be a costly matter with present facilities to collect and deal with the material on the spot; hence it is reasonable that a very strong case should be put up by the engine users before steps are taken to embark on large schemes for power alcohol production.

Lest it should seem that the high-speed engine is being considered too exclusively and the older engine ignored, Mr. Ricardo puts his view on record: "That the internal combustion engine has found its ultimate sphere in the light mobile high-speed type is now evidenced by the fact that, whereas in the years

immediately before the War the annual output in horse power of both the light and heavy type in this country was about equal, to-day the aggregate annual power output of the light high-speed type is at least ten times that of all other types, and in numbers probably nearer twenty times."

We welcome this book, and we congratulate the author upon its production and upon his distinguished share in the campaign towards yet further developments. Those who take their stand with the outposts in this campaign and endeavour thence to discern what yet lies in the lap of time will share with Mr. Ricardo his enthusiasm for one of the most stimulating of adventures in the world of applied science.

H. E. W.

The Secret of Life.

The Mechanism of Life in Relation to Modern Physical Theory. By Prof. James Johnstone. Pp. xii+248. (London: Edward Arnold and Co., 1921.) 15*s*. net.

THE professor of oceanography in the University of Liverpool is well known as an eminent biologist with strong philosophical leanings and an unusual knowledge of physico-chemical science. So the title of this book and the name of its author lead one to expect something of more than ordinary interest. It may be said at once that this expectation is fully justified, for Prof. Johnstone's book is uncommonly stimulating and represents a real and determined effort towards scientific synthesis.

In the first eight chapters, the subjects of which are the nature of animal life, the sensori-motor system, the principles of energy, the sources of energy, on vital production, brain and nerve, the special nervous mechanisms, and the analysis of behaviour, the author gives the reader an excellent and readable outline, well illustrated with diagrams, of some of the fundamental aspects of physiology and the theory of energy (including the second law of thermodynamics). It is fairly obvious that these chapters are written for the purpose of preparing the uninstructed reader to understand what is to follow, for it is in the last three chapters, on the mechanistic conception of life, the meaning of perception, and the nature of life, that we come to the kernel of the matter.

In the first of these chapters the author describes the mechanical system of Descartes. Having disposed of Descartes, he then proceeds to demolish Jacques Loeb, in other words, he finds the modern physico-chemical "mechanisms of life" equally unsatisfying, equally mechanical. But the last paragraph of this chapter, like the concluding sentence of one of those serial instalments of "blood and passion" that appear in certain magazines, shrewdly whets our appetite: "Anyhow, our mechanism of the organism has come again to a crisis. First of all it was a mechanical explanation of life, and that being insufficient, biology resorted to a physico-chemical explanation, which was also insufficient, since physics and chemistry are again becoming mechanical. Looking about for the new conception that biology has now again to borrow from physics, we have little difficulty in finding it, and it would appear as if it were really something new. The concept is given to us in the physical notion of statistical mechanics and to this we shall return presently."

This sounds exciting, though it is not quite evident at first sight why statistical mechanics should be any better than mechanics. However, the secret comes out in the last chapter, which treats of "The Nature of Life." Here the author deals in a very interesting way with the laws of thermodynamics, his discussion being based on the statistical methods of Boltzmann and Smoluchowsky. It is pointed out that the universe "becomes a cyclic order, such that the most probable phases are those in which entropy tends towards its maximum value, and the least probable ones are those in which the entropy tends towards its minimum value. As such it is a permanent universe, self sufficient, without beginning and without end."

Proceeding from this basis, the author arrives at the following result. In inorganic processes and tendencies available energy runs down and entropy increases; whereas in "vital" processes and tendencies available energy accumulates and entropy decreases. Summing up, he states that "In living processes the increase of entropy is retarded. This is our 'vital concept'." His exact meaning will be rendered clearer by the following quotation. Discussing the photo-synthetic action of the green leaf, he says: "Starch accumulates in the green leaf exposed to sunlight, but the *whole* system is the green leaf + the CO_2 and H_2O + the 'degrading' sunlight. In the system thus defined entropy increases very slowly. The system is one in which there are *coupled* energy transformations, (1) the degrading sunlight; and (2) the photosynthetic process. If there were no coupling, the solar energy would degrade, with a maximum entropy increase; if there is a coupling the entropy increase becomes minimal. The coupling is always the mark of life activity."

Suppose we illuminate some oxygen at room temperature with the right sort of ultraviolet light. Some ozone is formed. In this *inorganic* system we have two coupled energy transformations, (1) Oxygen $\longrightarrow$ Ozone, with increase of free energy and diminution of entropy; (2) "degrading" ultraviolet light, with diminution of free energy and increase of entropy.

Suppose again that we shake a solution of oxygen in water with zinc filings. Some hydrogen peroxide and some zinc hydroxide are formed. Here again we have an inorganic system and two coupled energy transformations, (1) Oxygen + Water ⟶ Hydrogen Peroxide, with increase of free energy and diminution of entropy; (2) Zinc + Oxygen + Water ⟶ Zinc Hydroxide with decrease of free energy and increase of entropy.

Hundreds of such examples might be given. For example, by a suitable coupling of voltaic cells we can realise the pair of coupled transformations, (1) $H_2 + I_2 \longrightarrow 2HI$ *aq.*, with increase of entropy; (2) $2HCl$ *aq.* $\longrightarrow H_2 + Cl_2$, with decrease of entropy. Thus, a coupled transformation involving, when taken by itself, a decrease of entropy, is no prerogative of the living cell or organism. The latter is not a bit from an "improbable" part of the universe, which is retarding or reversing the operation of the second law of thermodynamics in our particular part of the universe. A living cell or organism does not, as it were, act spontaneously. If we could photograph Mr. Home in the act of "spontaneous levitation," we could wager quite safely on the existence of a "coupled degradation," even if we could not see it. The continued activity and existence of a living organism depend on its utilisation of an environment which is not in perfect thermodynamic equilibrium. The totality of the actions involves a decrease of free energy (increase of entropy), while a part will in general involve a "storing of availability," *i.e.* an increase of free energy and a decrease of entropy. But this is a general characteristic of most complex physico-chemical actions and reactions, including also the physico-chemical actions and reactions of the living organism and its environment. These facts are, of course, well known. The late Prof. Benjamin Moore often pointed out that the living cell acted as an "energy transformer." What he really meant was that it acted as a transformer of "energy potential," running some energy up to a higher "potential," and some down to a lower "potential," like an electrical transformer. If such coupled transformations never occurred in what we call the inanimate world, then we might find here a real prerogative and characteristic of vital activity. But the existence of such coupled "up-and-down" transformations in the inorganic world is the commonest of occurrences. The inorganic world in its various transactions does not, in fact, only "go down hill." The progress of the rake is zigzag, and not wholly a piece of undiluted villainy.

In trying to gain an understanding of the totality of the actions of a living organism, it appears to the reviewer that we may have to seek it in the intimate actions or "behaviour" of particular individual entities, rather than in the average statistical behaviour of "crowds." A piece of radioactive material decays according to the mathematical laws of continuous change, but behind this apparent continuity there lies a series of discontinuous changes or "mutations." The apparently continuous activity manifested in an ordinary chemical reaction, which can also be represented by the mathematics of continuity, is due in reality to a hidden series of "critical" states and "critical" transformations. Everywhere the "evolutionary changes" of individuals appear to be of a discontinuous, critical, or mutational type. Behind or below the determinism of our statistical laws of physico-chemical change there lies a deeper determinism based on the transformations of particular individuals at particular moments. Modern physico-chemical science has already obtained a large measure of success in analysing this apparent "spontaneity" and in discovering the intimate laws of action of individuals. The City Actuary is being replaced by the Harley Street physician. Meanwhile, the philosopher with his *élan* of impatience (and ignorance) hurls defiance at the harmless corpse of the older determinism.

Prof. Johnstone's book contains much more, however, than his attempt to find a characteristic or criterion of vital activity in statistical mechanics. It deals with such subjects as perception, behaviour, mind, memory, freewill, habit, etc., and attacks the doctrine of determinism as applied to the deliberative actions of animals. Thus the author says: "In most animals there is some indetermination and spontaneity of behaviour, and the more highly organised is the central nervous system, the greater seems to be the degree of indetermination that is exhibited." In much of this discussion he reveals himself as a follower of Bergson.

Finally, Prof. Johnstone, the philosopher (as distinct from the psychologist and biologist) allows himself the luxury of what he calls a "metaphysical discussion," which, however, he relegates to an appendix. We need not follow him into those "faery lands forlorn." Philosophers (*i.e.* the professional sort) live by taking in each other's washing, and it is no part of good manners to interfere with these detergent ceremonies.

The general impression which one gains from this book is that the author is dissatisfied with the present-day physico-chemical description of biological sequences. But it does not appear that he has anything better to offer. We have seen that his thermodynamical (or statistical mechanical) discussion provides nothing new. He brings in the modern physical theory of relativity and seems to find some comfort in the reflection that

the electrons, atoms and molecules, when going about their lawful occasions are, after all, only successive space-time coincidences. But so, also, are the biological sequences!

Nevertheless, Prof. Johnstone's book is the work of an honest, mature and determined thinker, who possesses a good knowledge of physics, chemistry, and biology. As such it is worthy of very serious consideration and thought, and constitutes a most interesting contribution to scientific literature.

F. G. DONNAN.

The Geological Description of Britain.

(1) *Memoirs of the Geological Survey: England and Wales.* Explanation of Sheet 96: *The Geology of Liverpool, with Wirral and part of the Flintshire Coalfield.* By C. B. Wedd, B. Smith, W. C. Simmons, and D. A. Wray. Pp. vi+183. 4*s.* net.

(2) *Memoirs of the Geological Survey: England and Wales.* Explanation of Sheet 169: *The Geology of the Country around Coventry, including an Account of the Carboniferous Rocks of the Warwickshire Coalfield.* By T. Eastwood, Dr. W. Gibson, T. C. Cantrill, and T. H. Whitehead. With contributions by Dr. H. H. Thomas and the late C. H. Cunnington. Pp. viii+149+8 Plates. 5*s.* net. Also Sheet 169, 1 inch to 1 mile, colour-printed, Drift edition, 2*s.*

(3) *Memoirs of the Geological Survey: Scotland. The Geology of Corrour and the Moor of Rannoch* (Explanation of Sheet 54). By L. W. Hinxman, R. G. Carruthers, and M. Macgregor. With contributions by the late Dr. C. T. Clough, and Petrological Notes by Dr. H. H. Thomas and H. H. Read. Pp. iv+96. 4*s.* net. Also Sheet 54, 1 inch to 1 mile, colour-printed, Drift edition, 3*s.*

(Southampton: Ordnance Survey Office; London: E. Stanford, Ltd., 1923.)

UNDER the direction of Dr. J. S. Flett, the Geological Survey of Great Britain, with its happily increased emoluments and staff, remains one of the most progressive scientific institutions in the British Isles. The first two memoirs here noticed are based on the revision of mapping done in earlier days, and they form an effective answer to those who hold that geological observations once recorded are incapable of improvement in the light of later knowledge.

(1) The account of the Liverpool district is appropriately published in time for the visit of the British Association. Details derived from mining developments have been utilised, and twenty-four shaft-sections in the Flintshire coalfield are represented in a plate. The account of the recent improvements in water-supplies (pp. 127-147) records the great success of the Vyrnwy reservoir, which was completed in 1892, only 7·2 per cent. of the water used by Liverpool being now drawn from wells in the Bunter beds that underlie the city. The case of Holywell in Flintshire down to the days of the War, when the water was carted to the upper part of the town from the holy well of St. Winifred, and then dispensed in buckets, is quaintly described. This supply was seriously reduced in 1917, by being tapped by mining operations, and at present a reservoir is being utilised to receive water pumped from neighbouring shafts.

The glacial deposits of the district now receive concise description, based upon studies by Mellard Reade, G. H. Morton, and others, who have made Liverpool famous as a centre of geological observation. Important modifications have, however, been made in older views as to the mode of deposition of the drifts, and it is well to have the evidence of the striation of the rock-floor by ice from the Irish Sea conclusively put forward (p. 96). The glacial striæ occur mainly near the coast; they are directed to the south-east; and 58 per cent. of the boulders from a clay-pit in Stanley Road (p. 95), examined by Morton and Goodchild, showed striations on their surfaces. The list of erratics includes rocks from the county of Antrim, Ayrshire, Ailsa Craig, and the Isle of Man. The evidence for the existence of a great Irish Sea glacier is here complete.

(2) The memoir on the "Country around Coventry" is in reality a description of the area of the accompanying Sheet 169 of the colour-printed one-inch map, and covers the very interesting district north and north-east of the city. The whole of the Warwickshire coalfield, which extends into Sheet 155, has, however, been included in the memoir. Educationally, the map is a fine one from the contrast in structure of its eastern and western areas, the Cambrian shales and quartzite coming in west of the great fault, and underlying the Middle Coal-measures, while the drift-covered Triassic country to the east includes the remarkable inlier of ancient quartz-diorite, formerly styled granite, that is quarried at Lane's Hill.

It is suggested on p. 20 that this and the similar rock of Mount Sorrel, which formed part of the land-surface in Triassic times, may be of Devonian age, like the granites of the Lake District and of southern Scotland. We note among the geographical features the growth of Coventry in consequence of the mining activity north of it (p. 1), and on the map the grand old line of Watling Street, with the main route of the London, Midland, and Scottish Railway, keeping similarly to the Triassic lands.

(3) The third memoir dealt with in this notice leads us to a very different country. The Moor of Rannoch

has now been traversed by the railway to Fort William; but its essential wildness remains, and has become known to thousands who otherwise could have realised little of the grimness of the central highlands. We are here on the watershed between the North Sea and the western inlets, and its rugged characters seem typified in the trench-like hollow in which Loch Ericht lies (p. 9). The contoured geological map, with its audacious mass of scarlet where the early Devonian granite forms the moorland, should be studied side by side with the hill-shaded sheet of 1876, on which Mr. R. McFadden gave us what is surely one of the finest examples of hachuring in the world.

The question of recumbent folds among the metamorphosed stratified series has been raised by Mr. E. B. Bailey, and the views of the official geologists in this difficult region show healthy differences that will stimulate yet further work. By any one who has emerged on the moorland from the deep cleft of Glencoe, the courage of those who have investigated the district yard by yard must be gratefully acknowledged and admired. The most striking feature of the description of the glacial deposits is the evidence that boulders of the Rannoch granite have been abundantly lifted by the land-ice to heights of 1000 feet above the level of the moorland mass. G. A. J. C.

Medical Science in the War.

History of the Great War: Based on Official Documents. Medical Services: Diseases of the War. Vol. 2: *Including the Medical Aspects of Aviation and Gas Warfare, and Gas Poisoning in Tanks and Mines.* Edited by Maj.-Gen. Sir W. G. Macpherson, Maj.-Gen. Sir W. B. Herringham, Col. T. R. Elliott, and Lt.-Col. A. Balfour. Pp. viii+621+7 plates+6 maps. (London: H.M. Stationery Office, 1923.) 25*s.* net.

AS the details of the War fade away into the past, our perspective of the ordeal emerges more and more clearly, and when viewed from a distance of five or more years, the magnitude of our effort begins to make itself apparent. Time, if it has not yet healed our wounds, has at least enabled a considered diagnosis and history to be made. How wonderful that history was, how resourceful our resistance, how well-earned our victory, can be gathered by reading this truly fascinating account of the work of the Medical Services during the War. Never before in war has the air played so big a part; its physical properties have loomed large in problems of aviation, while its importance in respiration has made physiology one of the most indispensable of sciences in connexion with aviation, gas warfare, and mining operations.

Application of the results of scientific research led to the solution of most of our difficulties. By careful tests men could be selected who were physiologically suitable for flying, while those unfit could be eliminated; "flying strain" could be detected and treated; by the use of liquid oxygen aviators could reach heights otherwise unattainable, and still retain their efficiency. Many lives were saved, and considerable advantage gained in consequence.

After that portentous experiment of April 22, 1915, when the aspect of warfare was changed by the use of asphyxiating gas by the Germans, stupendous efforts were made to devise protection against this form of attack. For a while, gas offensive and anti-gas protection strove, on either side of that awful strip of neutral land, each to outdo the other; the ultimate victory was with the defence.

That the British box respirator was easily the best in the field cannot be denied by any one who knows all the facts, for it was satisfactory both from the chemical and physiological point of view, and hence this form of respirator was greatly in demand not only for the use of our own troops, but also for those of several of our allies. Its evolution from less perfect predecessors is fully explained in the tenth chapter of the book. Chapter ix. contains a full account of several gas attacks made upon our troops; in one case at least the reviewer can testify to the complete accuracy of this official account, and has no doubt that all the other accounts are equally accurate, since they were written up in the field by exceptionally able Army chemical advisers, on the basis of verified reports by the units concerned.

Gas warfare reached a crescendo in July 1917 when mustard gas was first employed, and the number of casualties suddenly jumped up, and even with the most stringent precautions remained high until the end of the War. This was due, not to inefficiency of the respirator, but to difficulties of detection of the gas, and to damage done to the general body surface by the substance. In spite of all our precautions the total reported gas casualties were 180,983, not counting some who died on the spot, or were taken prisoner; something more than 6000 of these died, while about 19,000 had been classed for pensions during the year 1919–1920. This forms about 2 per cent. of the total post-War disabilities, which is only a small number; very few of these men have since died from indisputable effects of gassing. The medical treatment of gas-poisoning may therefore be said to be fairly satisfactory, and is fully discussed, together with the pathology of gas-poisoning, in the official account.

Much has been written elsewhere about gas warfare; it has been described as a cheap, effective, and humane means of attack, and also as the most costly, most

ineffective, and most brutal weapon yet devised. That it has come to stay is certain; that it cannot be ignored is incontestable; that it may even be the means of ultimately extinguishing the civilisation which has engendered it seems not impossible. No one could read this considered account without being impressed by its fundamental significance.

There is one gas against which the respirators are not effective; this is carbon monoxide. It was not, and could not easily be used as a means of attack, but was encountered in ill-ventilated tanks and in mines after a blow had occurred. This danger was met, as similar danger is met in coal mines, by the use of some form of oxygen respirator. Mine rescue work, and the treatment of carbon monoxide poisoning, form the concluding chapters of this valuable and interesting document.

The Foundations of Future Psychology.

The Nature of "Intelligence" and the Principles of Cognition. By Prof. C. Spearman. Pp. viii+358. (London: Macmillan and Co., Ltd., 1923.) 15*s.* net.

"IN these principles, then, we must venture to hope that the so long missing genuinely scientific foundation for psychology has at last been supplied, so that it can henceforward take its due place along with the other solidly founded sciences, even physics itself. In particular, these principles (together with commentaries upon them) appear to furnish both the proper framework for all general text-books and also the guiding inspiration for all experimental labours."

This is the author's very confident conclusion. The source of cognition, he holds, is experience. This he defines as "that which is immediately lived, undergone, enjoyed, or the like"—a definition which would appear to include digestion and the hardening of one's arteries.

The first intelligent operation is the apprehension of experience. This is said to include sentience, affection, cognition, conation, and the *ego*. The inclusion of the *ego* is firm but apologetic, "pending some much more plausible alternative explanation being proffered." It is hinted (but not argued) that the fundamental connectedness of these items is also apprehended at this primary level.

The second principle—the "eduction of relations"—states that "the presenting of any two or more characters tends to evoke immediately a knowing of relation between them." The proof of this "tendency towards evocation" appears to be that these relations may be discovered. These relations include all the categories—time, space, causality, and the rest. All are neatly ticked off.

The third principle—the eduction of correlates—is that "the presenting of any character together with a relation tends to evoke immediately a knowing of the correlative character." This principle is very thoroughly elaborated and illustrated.

These principles and their manifestations are called "noegenetic" because they are "noetic" (self-evident) and generate further knowing. They are "the principles of intelligence" and fundamental for cognition.

The book, we are told, is "solely psychological and by predilection practical." The author, in consequence, believes himself justified in adopting the methods of a drumhead court-martial on the frequent occasions when he tackles metaphysical points. Since the essence of his argument, however, is noetic self-evidence, it is difficult to understand what he means by unadulterated psychology. Certainly he makes a most resolute attempt to illustrate and corroborate his results from laboratory evidence; and this is the most valuable, as it is also the most distinctive, feature of his discussion. He is far too clear-headed, however, to mistake corroborative for fundamental evidence.

Take, for example, one of his favourite topics—the initial status of sense-experience. Neither his choice of this topic nor the greater part of his treatment appears to be predominantly psychological. He begins with the argument commonly known as physiological scepticism, and ignores the vicious circle it contains. Satisfied with this, he appears to rely on self-evidence until quite late in the work, when he brings corroborative experiments to bear upon his implied assumptions concerning this "tremendous problem of objectivity." It is true that he assigns to these experiments much greater value than is due; but his fallacy is logical, not experimental. To pass other points, the brunt of his discussion here concerns subjectivity in the sense of "actually constituting your state of consciousness as when you say 'My consciousness was that sensation.'" Since many philosophers hold that no one can ever truthfully say any such thing, it is plain that this "experimental" question is a flagrant *petitio principii*.

The same remarks seem apposite when the author deals expressly with "transcendence." It is clear to him that somehow we come to know what is not a state of ourselves, and he alleges that we do this by educing correlates. We apprehend the *ego*, grasp the relation of otherness, and educe a not-self (p. 107). This looks simple. Self and otherness, together, will give you, of course, "other selves," or "other than any self," or "anything other than yourself." You can therefore "educe" or "draw out from the very nature of the item (yourself) presented" your parents or, if you will, the rest of the universe. In short, anything can be done by these methods, and it is not at all clear why the author did not choose to "educe" "non-experience"

from "experience" or "infinite objectivity" from "finite subjectivity."

These principles are called "qualitative," but there are also five "quantitative" ones, and three further "anoegenetic" principles of reproduction, disparition, and variation of clearness. I have space only for a few remarks on these heads.

The first three of the five quantitative principles run as follows: (1) Every mind tends to keep its total simultaneous cognitive output constant in quantity however varying in quality; (2) the occurrence of any cognitive event produces a tendency for it to occur afterwards; (3) the occurrence of any cognitive event produces a tendency opposed to its occurring afterwards.

Of these (1) looks as if it meant that every sleepy mind tended to have the same cognitive output as it has when alert. This, however, is not what is meant. Our author seems to mean instead (p. 131) that the occurrence of any one noegenetic process tends to diminish the others. The second noegenetic process, however, presupposes and includes the first.

(2) and (3) are flatly contradictory, so that it is difficult to know what to do with them. They are called respectively Retentivity and Fatigue. It may be worth remarking, then, that Fatigue, in ordinary language, does not contradict (2). When you are tired you are likely to stop, but *afterwards* you may begin again.

I do not mean these criticisms to be verbal, but I should be glad if they were. For the author's courage and resource I have nothing but admiration, and his vigour is always refreshing. JOHN LAIRD.

Our Bookshelf.

Memoirs of the Geological Survey. Special Reports on the Mineral Resources of Great Britain. Vol. 1: *Tungsten and Manganese Ores.* Third edition. By Henry Dewey and H. G. Dines; with Contributions by C. N. Bromehead, T. Eastwood, G. V. Wilson, and R. W. Pocock. Pp. iv+83+3 plates. (Southampton: Ordnance Survey Office; London: E. Stanford, Ltd., 1923.) 2*s.* net.

THE latest edition of the report on the British ores of tungsten and manganese merely brings up-to-date the information contained in the previous editions, but it cannot be said that it has brought out any new facts of importance. Perhaps it only serves to emphasise the industrial unimportance of the British sources of supply. Temporarily the War directed attention to the domestic sources and caused these to be actively worked, but under post-War conditions, the home deposits have again been found to be unable to compete with the richer deposits that exist abroad. This is well exemplified by the ores of tungsten, the British output of which touched nearly 400 tons per annum during the War, while the output to-day is probably less than a quarter of this quantity; in the same way the price, which during the War reached 55*s.* per unit, is to-day only about 12*s.* Furthermore, the total output from Great Britain is only about 2½ per cent. of the world's production.

In the case of manganese ores the figures show the same tendency, though not to so marked an extent; this is due in part to the fact that the great bulk of the British ores of manganese are of low grade compared to the imported ores. The chief centre of our home supplies is in North-West Wales, in Carnarvonshire and Merionethshire, both of which districts are well described in the present report. These ores appear to average less than 30 per cent. of metallic manganese, while imported ores contain at least 50 per cent. Even so, however, the tonnage of domestic ores is barely 1 per cent. of the world's production, and only about 2 per cent. of our imports. Economically, therefore, the British production of both these ores is negligible, and a careful study of the report before us affords no ground for hope that it will ever become a factor to be reckoned with in the world's markets for either mineral.

Cements, Limes and Plasters: their Materials, Manufacture and Properties. By E. C. Eckel. Second edition, revised and partly rewritten. Pp. xxxi+655. (New York: J. Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1922.) 32*s.* 6*d.* net.

ECKEL'S treatise on cements, although dealing almost exclusively with American practice, is one of the most thorough works on this subject, and the new and revised edition will be accepted as a standard authority. The scanty references to English practice are not always accurate, and the casual reader might suppose that the industry in Great Britain was insignificant, but the information in regard to the United States and Canada is very full. A more detailed account of the fixed mechanical kilns which are now assuming so much importance would have been welcome, as they are now becoming serious rivals of the rotary kiln. It has been found possible to include a short account of the high-alumina cements recently introduced, although there is no systematic consideration of their properties. The section on slag cements is misleading. Only the older pozzolanic cements, consisting of mere mixtures of granulated blast-furnace slag and lime, are considered, and the much more valuable "Iron-Portland" or "Blast-Furnace Portland" cements, made by mixing a suitable granulated slag with clinker and grinding together, are not even mentioned. The chemistry and physics of cement do not receive attention, the treatment being purely empirical, but within its limits the book gives an excellent survey of an important American industry.

The Causes and Prevention of Corrosion. By A. A. Pollitt. Pp. 230. (London: Ernest Benn, Ltd., 1923.) 25*s.* net.

THE literature of corrosion is extensive, but far from satisfactory in its scientific aspects. There is a large collection of facts, but a singular lack of co-ordinating principles. Each experimenter has his own hypothesis, which fits a small group of observations, but usually breaks down when applied to other, nearly related facts. The writer of the present work has prepared a useful survey of the subject, although confining

himself almost exclusively to work published in English, and without any evidence of first-hand observation. Concrete examples, so important in such a subject as this, are lacking, and the reader is thus little able to judge of the relative merits of the rival hypotheses, which are, however, fairly and accurately described. The corrosion of steel boilers and of brass condenser tubes is treated more fully, the section on the latter subject being reprinted from a pamphlet issued by the Corrosion Committee of the Institute of Metals. The most valuable part of the book deals with the prevention or diminution of corrosion, especially of boilers and condensers. Here the author is evidently at home, and the chapters on the softening and de-aeration of water, and on the protection of boilers by electrolytic methods, are fully illustrated and contain much detail. This portion of the book might well have been issued alone, a procedure which would have lessened its rather high cost. The printing is good, and the illustrations of plant are very clear.

The Bakitara or Banyoro: the First Part of the Report of the Mackie Ethnological Expedition to Central Africa. By the Rev. Canon J. Roscoe. Pp. xvi+370+42 plates. (Cambridge: At the University Press, 1923.) 25*s*. net.

ANTHROPOLOGICAL science owes a debt of gratitude to all who were concerned in the initiation and organisation of the Mackie Ethnological Expedition to Central Africa; but most of all to Mr. Roscoe, by whom the actual work of investigation was carried out. This first instalment of his report is an invaluable contribution to our knowledge, and will prove an almost inexhaustible mine of information for the student of primitive custom and belief. The dominant people of the country of Kitara are the Bahuma, Negro-Hamites, possibly of Galla strain, though this is uncertain. Coming from the north-east, they invaded the country in the lake region immediately west of Uganda, part of which they now occupy, and subdued the Bahera, the agricultural negro aborigines. Among much which is striking in their culture, the most remarkable feature is the manner in which their whole social and religious organisation centres around their herds. The entire routine of the kingly office is ordered solely to promote by sympathetic influence the well-being of the cattle. The elaborate milk ritual, which Mr. Roscoe has studied carefully in minute detail, inevitably invites comparison with the dairy cult of the Todas of Southern India.

L'Homme fossile de La Quina. Par Dr. H. Martin. (Archives de Morphologie générale et expérimentale. Fasc. 15: Anatomie.) Pp. 260. (Paris: Gaston Doin, 1923.) 25 francs.

IN this volume Dr. Martin describes the results of the investigations which he has carried out on the Mousterian site of La Quina (Charente) since 1905. His discoveries included a large number of mammalian remains and of typical implements as well as objects of bone, which at the time of discovery constituted the first evidence of the use of bone in the Mousterian age. Much of this material has formed the subject of communications to French scientific societies, and the general conclusions are well known; but anthropologists will welcome this careful and detailed study of the evidence as a whole. The author, by inference, does much to throw light upon the habits of Mousterian man, and it is noteworthy that he is inclined to regard a certain condition of the equine teeth as evidence for domestication. His most important contribution to anthropological science, however, was the discovery in 1911 of the human skeletal remains now known as the La Quina man, and in 1915 of the cranium of a child aged eight, both falling within the Neanderthal group. Dr. Martin, on the ground of inferiority to type in certain respects, is disposed to regard the former as female.

Practical Chemistry. By E. J. Holmyard. (Bell's Natural Science Series.) Pp. xvi+267. (London: G. Bell and Sons, Ltd., 1923.) 4*s*. net.

MR. HOLMYARD in the preface to his book has something to say on the heuristic system, about which so much was said a few years ago. While we may admire it at a safe distance, he remarks, "We are at least upon safe ground when we believe that a little sound knowledge acquired by the method of direct teaching is distinctly more valuable than much hazy and inaccurate knowledge gained by the so-called 'method of research'—which is, of course, not the method of research at all, but a sort of game of make-believe." He has written a sound and useful book on the lines he advocates. It covers the ground of the School and Higher Certificate Examinations, and is sufficient for University Scholarships, but is wisely not written for any examination. Gravimetric and volumetric analysis, physical chemistry and organic chemistry are included, but the author has rightly, we think, omitted qualitative analysis. The course described is one of the best we have seen, and the book should become popular in schools. It is evidently the work of an experienced teacher.

Ink. By C. Ainsworth Mitchell. (Pitman's Common Commodities and Industries.) Pp. ix+128. (London: Sir Isaac Pitman and Sons, Ltd., 1923.) 3*s*. net.

MR. MITCHELL has dealt with the origin of inks, the way in which they are made, and their characteristics in a most interesting and useful way. The use of carbon inks, he shows, dates back to very remote periods in Egypt and China. The earliest mention of iron-gall ink is said to be in the work of Theophilus the Monk, dating to about the eleventh century A.D. Before the beginning of the seventeenth century, ink was made in the household, but in 1609 it was manufactured in Paris, later in Dresden, and much later by Stephens in England. Mr. Mitchell deals with all kinds of ink, including printing ink.

Your Broadcast Receiver and How to Work It: Hints and Tips for the Radio Listener. By P. W. Harris. Second Impression. Pp. 68. (London: The Wireless Press, Ltd., 1923.) 6*d*. net.

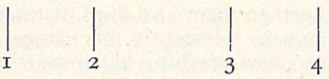
THIS book can be recommended to owners of broadcasting receiving sets. A judicious amount of elementary practical theory is given which will enable them to get the best results from their apparatus.

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.*]

On the Regularities of the Spectral Lines of Iron and the Atomic Magnetic Field.

SINCE our short account of the method of observing the Stark effect with a stabilised arc was written (see NATURE, March 31, p. 431), we have made experiments on about twenty different metals. With elements having a simple spectrum, as silver, copper, zinc, and others, the separation of the lines into different series is facilitated from the similarity in the winged appearance of the lines in the strong heterogeneous electric field near the anode, though the broadening is generally asymmetric and there is some difference among the polarised components. The examination of many thousands of iron lines is not yet completed, but choosing lines between λλ 2400 to 3000 Å showing the simplest type of the effect, in which they are enhanced and slightly shifted towards shorter wave-lengths, we have found that a few lines can be arranged in regular triplets, quartets, and sextets. These mostly belong to spark lines. In addition to these regularities, we can arrange the enhanced lines into a large number of quadruplets as shown below :



The frequency difference, $\Delta\nu(1,2)$, is equal to $\Delta\nu(3,4)$ to a fraction of the wave-number per cm. The relations between $\Delta\nu(1,2)$ and $\Delta\nu(2,3)$ are various, but the values of $\Delta\nu(2,3)$ and $\Delta\nu(1,4)$, and especially those of $\Delta\nu(2,4)$ and $\Delta\nu(1,3)$, are common to many of the quadruplets.

The remarkable numerical relation between $\Delta\nu(1,2)$'s is that they come out in groups as given in the subjoined table :

Group.	No. of Quadruplets.	Mean $\Delta\nu(1,2)$, $\Delta\nu(3,4)$.	Range of $\Delta\nu(1,2)$.
a	14	59·6	55 to 68
b	15	111·7	106 to 121
c	46	182·4	171 to 197
d	34	245·3	230 to 262
e	6	362·9	354 to 372
f	47	422·4	414 to 435
g	15	484·9	477 to 495

Values outside the ranges above cited do not appear.

Counting from group (*a*), the mean $\Delta\nu(1,2)$'s, excepting the second, are almost exactly in the ratio

$$1 : 2 : 3 : 4 : 6 : 7 : 8.$$

It is singular that 5 does not enter in the above ratio; the absence of this number will probably underlie the principle of choice. Perhaps the above ratio has some bearing on the quantum theory, and is connected with the inner quantum number ("innere Quantenzahl"). If we interpret the existence of regular separations as due to the action of an atomic magnetic field, the above relation seems to be one aspect of Runge's rule in the Zeeman effect. Taking 363 as the standard separation, the above ratio can be written as representing 1/6, 1/3, 1/2, 2/3, 1, 7/6, 4/3. The intervals of quadruplets in group (*g*), for which $\Delta\nu(1,2)=485$, frequently occurs and is closely related with the separations of numerous quadruplets, so that it seems to have some important signification. The same number occurs in two regular triplets.

In forming these quadruplets, there is no criterion but that of taking the interval $\Delta\nu(1,2)=\Delta\nu(3,4)$, with corresponding symmetry in the intensity of lines. Analysing the distribution of lines, it is found that the same line can be looked upon as belonging to more than one quadruplet. Most of them are perhaps not real quadruplets, but belong to portions of more complex separations, the true nature of which is difficult at present to unravel. Dealing with many hundreds of lines, chance coincidences may frequently occur, yet the probability of the existence of regularities can scarcely be doubted. Eventually, we shall be able to arrange the iron lines in spectral series by utilising the Stark effect, if such really exist.

If we assume that the separations are due to the Zeeman effect of the atomic magnetic field, they will probably amount to aliquot parts of a normal triplet, if they follow Runge's rule. This is not usually obeyed in iron lines by applying an external field, but if we roughly assume that the triplets ($\Delta\nu(1,2)=485$) are normal, the field must amount to 10^7 gauss, which will approximately give the order of magnitude of magnetic force acting on the light-emitting electrons. As the above value of $\Delta\nu$ corresponds to the widest separation observed, the field will be generally smaller; by choosing $\Delta\nu=354$, which is found in one of the triplets and a number of quadruplets, the atomic field is found to be 6.6×10^6 gauss, coinciding with the value found by Weiss from experiments on magnetisation. This gives strong support to the magneton theory, and though the problem of atomic field is still in a hypothetical stage, the close agreement of the results obtained from measurements made on different phenomena is worthy of further consideration.

In Bohr's equation for calculating the frequency of light, the change of electric energy is taken into account only when an electron passes from one orbit to another during the emission of light. If we assume that, in the interior of an iron atom, a strong magnetic field as given above is prevalent, we must also examine the change of magnetic energy during the emission. This adds a further complication to the discussion, especially when the orbits are not coplanar. The question is, where does the magnetic field come from; does the seat lie in the nucleus or in the orbital motions of electrons? The intricate nature of the spectral lines in ferromagnetic metals may ultimately be traced to the existence of an inner atomic field.

The list of lines and different separations will be published shortly in the *Japanese Journal of Physics*, vol. 2.

H. NAGAOKA.
Y. SUGIURA.
Institute of Physical and Chemical Research,
Hongo, Tokyo, July 20.

Embryology and Use-Inheritance.

HAVING read with great interest in the supplement to NATURE of August 18 the Huxley lecture of my friend Sir Arthur Keith, and the comments upon it in "Current Topics and Events" of the same issue, I should like as an embryologist to make some remarks on the subject. Sir Arthur, in his fascinating style, describes the manner in which during development indifferent embryonic cells are marshalled so as to build up structures of functional and adaptational use. He arrives, however, at the surprising conclusion that "functional adaptation . . . is a property

resident in the embryonic tissues; the effects of usage in the parent can have no influence on the machinery."

Sir Arthur, therefore, if I understand him aright, comes out as a predestinarian orthogeneticist. The experiences of the animal have no influence in shaping the structure of its offspring. In this attitude he outdoes that ultra-mendelian Prof. Morgan, of New York, who, when confronted with the problem of the ultimate causes of his "mutations," admitted that no other source could be found for them except the influence of the environment.

What reasons does Sir Arthur adduce for what I may term his despairing conclusion? In the last analysis they reduce themselves to two, namely: (1) functional adaptations—such as the shape of the crowns of the molar teeth—and the separation of the peronæus tertius muscle from the extensor muscle of the little toe, come into existence in the embryo before there is any possibility of the performances of the functions to which they are adapted; and (2) Sir Arthur can conceive of no mechanism by which the habits of the parent can influence the embryonic machinery.

Now when Lamarckism is dismissed on grounds such as these, it would have been just as well if Sir Arthur had made himself acquainted with the form in which the Lamarckian theory is held by modern biologists. May I briefly refresh his memory? Modern Lamarckism may be stated as follows:

(1) An animal exposed to a new environment modifies its habits, so as to adapt them to new needs.

(2) New habits, persistently indulged in, entail modifications of adult structure.

(3) The offspring of animals which have adopted the new habit, if they remain in the same environment as their parents, tend to assume the new habits more quickly and on slighter stimulus than did their parents, and to develop the corresponding structures at an earlier period of their lives.

(4) Ultimately, when the new habits have persisted for a long time, *the corresponding structures make their appearance in development before the performance of the functions to which they are adapted.*

It is obvious, therefore, that all Sir Arthur Keith's arguments against "use-inheritance" are irrelevant to the question at issue. Sir Arthur is a brilliant mammalian embryologist. Were he a comparative embryologist he would be acquainted with cases which would stagger even him in his opposition to Lamarckism. I will give one. All Macruran crustacea (lobsters, prawns, shrimps, etc.), when seeking retreat, move backwards and strive to thrust the abdomen into a dark crevice. The hermit crabs have adopted the habit of inserting the abdomen into the curved passage of an empty gastropod shell, and in consequence the abdomen has become curved. The young hermit crab, however, in its last free-swimming stage has an abdomen as symmetrical as that of a shrimp; but when it sinks to the bottom, *before it has found an appropriate shell,* the abdomen has already become curved. Does Sir Arthur ask us seriously to believe that this curvature has been produced by some mystical "adaptational" mechanism among "embryonic cells," and has had no relation to parental habits? The paragraph in "Current Topics and Events" rightly states that the crux of the whole discussion is the proof of the actual existence of use-inheritance. Many of us believe that by means of well-thought-out and patiently executed experiments this proof has already been given. Those who refuse their assent may be divided into two classes, namely: (*a*) those who are unacquainted with the full details of the experiments; (*b*) those who are acquainted with these details and strive to escape from their inevitable consequences by attributing fraud to the experimenter. It is obvious from his sympathetic references to Kammerer that Sir Arthur Keith belongs to the first of these categories. May I recommend to him a more prolonged and extensive study of Kammerer's papers?

The paragraph in "Current Topics and Events" goes on to state that every failure to demonstrate use-inheritance strengthens the Darwinian position, which is adopted by the best and most philosophical workers in biology to-day. This is a statement which I frankly fail to understand. Darwin was until the close of his life a convinced believer in the existence of use-inheritance, although he did not regard it as the sole factor in evolution. Who are at present the best and most philosophical workers in biology is, of course, a matter of opinion: I should think that Darwin, if still with us, would put in this category those who had the widest acquaintance with facts. If this criterion be granted, then I may remark that the best palæontologists and the best systematic zoologists whom I know are strongly inclined to adopt the Lamarckian point of view.

Far be it from me to say a single word in disparagement of that great biologist Huxley, whom Sir Arthur Keith claims, and I have no doubt rightly, as a predeterminist. From Huxley I received my first attraction to the study of biology, and it has fallen to my lot to succeed him in his chair. I am convinced that if Huxley were still alive, and had learned from Sir Arthur Keith's brilliant exposition the wonderful facts of the indifference of embryonic cells, and their capacity at need to form any kind of tissue, he would find it difficult to persist in his conception of the "germ-plasm" as a machine-like mosaic of molecules.

Sir Arthur compares the embryonic cells to an army of workmen capable of various tasks whose energies are co-ordinated to a common end—not by a director but by hormones or chemical messengers which they send to each other. I must frankly confess that it baffles all my powers to conceive how, from an unorganised mob of undifferentiated cells, an organised structure could arise solely by their mutual influence. Certainly the amount of constructive work accomplished in these circumstances by a crowd of British workmen would be a minus quantity. Surely the influence which organises and marshals these cells must be one external to themselves. There must, in the developing embryo, be some part which takes the lead and emits the primary hormones which control the action of the rest. This I pointed out in my address to Section D of the British Association in 1916. May I illustrate this by an example taken from a recent paper by Ruud and Spemann with which Sir Arthur is possibly not acquainted? If a small portion of the developing nerve-plate of *Triton alpestris* be grafted into the ectoderm of a gastrula of *Triton tæniatus* in a region where normally the neural plate is not found, it will organise the ectoderm cells around it into a neural plate, in the midst of which it will be found, distinguishable from the cells of the host by its different colour.

Let me in conclusion suggest to Sir Arthur Keith that these primary hormones or "formative stimuli," which initiate development and give it its course, are the physical correlates and bearers of the *memories* of the race, stored in the egg-cell which has in turn received them from the tissues of the parent generation.

E. W. MACBRIDE.

As I read over the homily which my friend Prof. MacBride has addressed to readers of NATURE in general and to myself in particular—one with which we are all becoming familiar—I was reminded of an

experience suffered by Huxley when he lectured at the Royal Institution on the cerebellum. At the end of the lecture, a devout hearer approached to inform him that she had understood and enjoyed the lecture —with the exception of one point—was the cerebellum inside or outside the skull? After I have filled 24 columns of your valuable space to prove that Huxley was altogether right when he denied that use-inheritance played any part in the evolution of man—or of any other animal—Prof. MacBride, after reading these columns, turns round and practically asks me if I have heard of Kammerer!

If Prof. MacBride will be so good as read my Huxley lecture again, he will see that I neither affirm nor deny the doctrine of use-inheritance What I have denied, in as clear terms as are in my vocabulary, is that Lamarckism—whether of the original 1809 vintage or of the brand bottled in 1923 by Prof. MacBride—has had no part in the evolution of man. To give my reasons for this conclusion would compel me to inflict on the readers of NATURE a repetition of my Huxley Lecture. Here I must content myself by saying that Lamarckism gives no explanation of man's developmental history, none of his anatomy; it leaves the ancestral forms of man, such as we know of from the discovery of their fossil remains, unexplained; it cannot explain the characters which differentiate one racial type of modern man from another. In brief, the tenets which Prof. MacBride clings to with such fidelity cannot serve the purposes of even a working hypothesis for the modern anthropologist.

Prof. MacBride is good enough to suggest that I should be staggered did I know of certain facts with which comparative embryologists are familiar. Well, I do sometimes make little excursions into the realms of invertebrate embryology and frankly confess I am staggered by the fact that men who are familiar with the developmental histories of invertebrate animals can have any belief of Lamarckism as a factor in evolution. ARTHUR KEITH.

Solar Activity and Atmospheric Electricity.

DR. BAUER'S courteous attempt (NATURE, August 11, p. 203) to reconcile our views respecting the connexion he believes in between sun-spots and atmospheric electricity calls for a reply. I should first explain that we differ as regards even the connexion between sun-spots and terrestrial magnetism. Apparently we both accept the relation

$$R = a + bS \quad . \quad . \quad . \quad . \quad (1)$$

between R, the range of the regular diurnal variation for the year, and S, the sun-spot number. Here a represents the range for no sun-spots and $100b$ the increase in range for a sun-spot frequency of 100. The value of $100b/a$ varies with the magnetic element and with the station, but is usually in the neighbourhood of 0·8. The further relation mentioned by Dr. Bauer, p. 204, "an increase of 100 in the sun-spot number would correspond to a decrease in the intensity of magnetisation of the earth of about 0·1 per cent.," is not a result I consider proved. If it were true, there should be a decided 11-year period in the secular change. Claims to have established such a period have been made, but seem to me to have broken down. Quite recently failure to detect the phenomenon at Paris, one of the most satisfactory stations, has been announced by M. A. Angot (*Ann. de l'Institut de Physique du Globe*, Paris, 1923, p. 288). But if Dr. Bauer and I are not exactly at one on this point, we are at least agreed that the influence of sun-spots on the absolute values of the magnetic elements is exceedingly small, if not zero.

Coming now to the potential gradient of atmospheric electricity, Dr. Bauer claims to have established a *substantial* spot influence both on the amplitude of the diurnal variation and on the mean value for the year. In the Physical Society paper to which he refers (Proc. Phys. Soc., London, vol. 35, p. 129), I attempted to check the alleged sun-spot influence both for the diurnal range and the absolute value by means of formula (1). In the case of the absolute value, R represented the mean value of the potential gradient for the year. In addition to results from the Ebro Observatory, on which Dr. Bauer had mainly relied, I employed data from two periods of years at Kew, determining a and b in all cases by least squares. Except in one case the value found for $100b/a$ was positive, but it was much below 0·8, and the values found for the correlation coefficients were too small to warrant the conclusion that a true sun-spot influence had been made out.

In his recent letter Dr. Bauer does not impugn the accuracy of my mathematical work. What he does is to employ instead of (1) a formula of the type

$$R = a' + b'S + c'T \quad . \quad . \quad . \quad . \quad (2)$$

where S is now the difference of the sun-spot number from its mean value, and T the time in years counted from the middle of the period. We may, I think, treat it as a mathematical certainty that the observational results must be expressible *exactly* by a formula of the type

$$R - a' - b'S = f(T).$$

What Dr. Bauer has found is that for one particular period of years $f(T) = c'T$ gives a good result at certain stations, notably Ebro and Eskdalemuir, which he considers good, and a less good result at other stations, Potsdam and Kew, which he considers inferior. He would no doubt get a still better result if he put

$$f(T) = c'T + d'T^2.$$

But is the goodness of fit in such a case any evidence of the real existence of a sun-spot influence? There might, for example, be an excellent fit with $b' = 0$.

There may admittedly be special conditions in which something is to be said for a formula of type (2). As I showed some years ago, the absolute value of potential gradient at Kew, and presumably elsewhere, is affected by the visibility (purity) of the atmosphere, potential falling as the visibility rises. If the purity of the atmosphere at a station improved at a uniform rate, potential gradient would naturally fall, and it might be a proper course to apply a corrective term $c'T$, with c' negative as found by Dr. Bauer at the Ebro, Eskdalemuir, and Kew. Again, if a station went on applying an invariable factor for the reduction to an infinite plane, while the factor was really altering owing to continuous deterioration of the insulation or other instrumental cause, a corrective term $c'T$ with c' negative might be justifiable if the rate of deterioration was constant.

The reasons assigned by Dr. Bauer, p. 203, for considering Kew an inferior station are the large size of c_2/c_1, the ratio of the amplitude of the 12-hour to the 24-hour Fourier wave, and the high mean value of potential. Now I can imagine another critic holding—and with equal reason—that a low value of c_2/c_1 and a low mean value of potential gradient are both symptoms of inferiority either in the site or in the apparatus. He might even suggest that the mean values at the Ebro, $86v/m$ in 1921 and $76v/m$ in 1922, are outstandingly low.

If a high mean potential gradient is a sign of

inferiority, the good character of Eskdalemuir seems difficult to explain, as the value there makes a much closer approach to the Kew than to the Ebro value, the latter being notably below what Dr. Bauer puts forward as the normal. Again, if a high value of c_2/c_1 is a sign of inferiority, is it not strange that c_2/c_1 is highest at Kew in summer when the potential gradient is lowest? Ebro and Eskdalemuir have a variable number of monthly quiet days, while at Kew with rare exceptions the number is uniform. Weather conditions usually reduce the number of quiet days used at Eskdalemuir below the Kew number 10. Thus *a priori* we should have expected Kew to be the station least affected by accidental irregularities. According to Dr. Bauer the sign of c' (his t) "may depend upon whether the sun-spot cycle . . . is below or above average development." Apparently he expects a revolutionary change from a steady fall to a steady rise and conversely! It is obvious that if a steady fall did go on at the Ebro at the rate obtained by Dr. Bauer we should before long have the potential gradient negative.

The fact that Dr. Bauer finds negative values for c' at all three stations, Ebro, Eskdalemuir and Kew, may possess some physical significance unrelated to sun-spots. In my Physical Society paper I referred to volcanic dust as a possible natural agency influencing potential gradient over wide areas. Even the agency of man may influence a considerable area. Thus I had myself regarded the value for 1921 at Kew as exceptionally low, and attributed this at least in part to the abnormal purity of the English atmosphere brought about by the coal strike. At all events the mean value for 1922, unlike that at the Ebro, shows a substantial rise.

In view of Dr. Bauer's concluding remarks it may not be amiss to point out that the earth's atmosphere is generally believed to contain an equal and opposite charge to the earth's surface. Thus the total charge on the earth as a planet would seem to be *nil* whether a sun-spot influence exists or not.

C. CHREE.

August 17.

Colour Vision and Colour Vision Theories.

IN his letter published in NATURE of August 25, Dr. Edridge-Green seems to admit the accuracy of the deductions from the trichromatic theory which I made in the issue of August 4. But, in making these, I used no other postulate than that of the fact of normal trichromasy. In the sense in which the word is used, trichromasy is now a qualitatively and quantitatively proved fact, although at the time of its first assertion it was in considerable part hypothetical. Strict logical development (which may be mathematical when necessary, since mathematics is merely symbolised logic from this point of view) leads directly to the explanation of certain phenomena which Dr. Edridge-Green had thought to be unexplainable on the basis of trichromasy. If the logical developments are sound, the conclusions are inevitable. But he brings forward three other facts which he still considers to be inexplicable on the theory.

First; a man, stated to be completely "red-blind," can recognise red as easily as a normal-sighted person. From the trichromatic point of view one might say, Why not? No doubt the term "red-blind" might preferably be avoided, seeing that it is a relic of the "hard-atom" stage of the theory; but the theory does not give the result that a dichromat of that type cannot distinguish red light from other lights. The notion that it must do so is a survival of ideas held under the restrictions of the early applications of the theory.

Second: 50 per cent. of the dangerously colour blind get through the wool test. Again, Why not? The theory would only use the fact, if granted, to aid in further elaboration of the details of the visual peculiarities.

Third: the theory is said to fail to explain the class of colour vision which Dr. Edridge-Green denotes as trichromatic, in which yellow is not recognised, the region of the spectrum occupied by yellow hues being called red-green. I cannot occupy space here in showing how this is directly predictable as a possibility on the trichromatic basis. I have discussed it, and other such cases, in my book on colour vision. Dr. Edridge-Green says that, in this case, the intersection of the dichromatic curves should be shifted towards the red on the trichromatic theory, and they are not so shifted. The statement is mistaken. There is no such compulsion on the theory.

The statements in Dr. Edridge-Green's last two sentences are in complete agreement with the theory. He says also that the theory is burdened with self-inconsistent subsidiary hypotheses. Actually the theory is based, and based alone, on two postulates; the qualitative postulate of trichromasy, and the quantitative postulate of the intensity law. All further development is straightforward, any definite constructive presumption being used in illustration only, and being clearly stated by Helmholtz to be quite inessential. In fact, he left the theory totally unburdened with fixed presumptions regarding structure and function. The fixation was to come later, probably by way of many supplemental theories consonant with it. All, including the views of Dr. Edridge-Green, may possibly help.

I would appeal to Dr. Edridge-Green not to pit his views against the trichromatic theory, but rather to consider wherein they may supplement it. Multi-chromasy higher than triple is without evidence. If he accepts Dr. Houston's work as the mathematical expression of his views, he thereby makes them trichromatic in the usual sense of the term. His views may supplement the theory on the side of functional physiology or psychology; they cannot refute it on the formal side. W. PEDDIE.

August 25.

The Phosphate Deposit of Ocean Island.

ON p. 787 of NATURE of June 9, which has just reached me, a notice appears, under the heading of "Mineral Fertilizers," of my paper on "The Phosphate Deposit of Ocean Island" (Quart. Journ. Geol. Soc., vol. lxxix., p. 1, 1923.)

As this notice misinterprets certain of the statements made in the paper, I beg the courtesy of your space for the necessary corrections.

(1) One of the points emphasised in the paper is the gradual and uniform change which occurs in the composition of the deposit as one passes from perimeter to centre. This change is so regular that it can be expressed by a simple formula.

There is no normal 88 per cent. and no "level . . . where the phosphate sinks from its normal 88 to 79 per cent.," the change being gradual and without break from 79 to 92 per cent.

(2) The deposit cannot be truly described as having "a depth of fully fifty feet." As stated in the paper, it is sometimes as much as 80 feet thick, but usually less than 50 feet.

(3) The excess lime shown by analysis (*i.e.* the lime over and above that required for the phosphoric, carbonic, fluoric, and sulphuric acid radicals) varies

directly with the percentage of organic matter and inversely with the percentage of tricalcium phosphate. It is therefore more rational to assume that this lime is combined with the organic matter than to state (as has been done) that it is present as a compound of the type $x(Ca_3P_2O_8).y(CaO).zH_2O$.

Dahllite, $4Ca_3P_2O_8.2CaCO_3.H_2O$, is not mentioned in my paper. The idea of its occurrence is not rejected by me, however, as its presence is not in conflict with the chemical analyses, no excess lime (as defined above) being required for it.

That the calcium fluoride shown in the analyses (about 3 per cent.) is present combined as apatite to any considerable extent is unlikely, from the behaviour of the phosphate to reagents; the purer varieties of Ocean Island phosphate being, for example, almost completely soluble in cold, dilute hydrochloric acid.

LAUNCELOT OWEN.

Monteria,
Republic of Colombia,
South America, July 21.

I HOPE that no one who consults Mr. Owen's interesting paper will have been much troubled by misinterpretations on my part. The words "normal 88 per cent." are based on the analysis on p. 13 of the paper, which is said to be "representative" and gives 87·5 per cent. I hope, again, that no reader of my note would suppose that a level exists in the rock at which the phosphate-content drops suddenly to 79 per cent. I should have written "has sunk" for "sinks."

In suggesting on p. 13 of the paper, and in his letter, that lime is associated with the organic matter in the phosphate, Mr. Owen raises a question of wide importance. The special adsorptive influence of organic colloidal gels in soils is now well known, and Mr. Owen doubtless sets a good example in not presuming the presence of dahllite or any other mineral unless it can be recognised by specific characters in the mass. The rather delicate fibrous crystallisation of dahllite may be looked for. We must remember that A. Lacroix and other mineralogists recognise definite species of mineral "calcium carbo-phosphates." E. Blackwelder, on the other hand (*Amer. Journ. Sci.*, ser. 4, vol. 42, p. 294, 1916), regards the less definite collophane as the common product of the reaction between phosphoric acid and lime salts, especially calcium carbonate, in the presence of ammonia. Collophane, as Rogers shows, can associate fluorine with its colloidal substance, and may thus suggest the presence of apatite. In his researches on the chemistry of phosphatised reefs, Mr. Owen is opening up a very interesting petrological field.

THE WRITER OF THE NOTE.

The Metric Campaign.

IN reviewing Drury's "World Metric Standardisation" (NATURE, August 18, p. 234), the statement is made that "far less opposition has been raised to the adoption of the litre and gram than to the metre, which is very much more closely related to industrial processes than the units of mass and measure."

Perhaps the following will serve to indicate to metric campaigners why those who are directly interested in industrial processes are in such an impenetrable fog over the question.

A few days ago, in a retail tool shop in a provincial town, I was shown a narrow steel measure, in four folding sections, the total length being one foot, which was divided into 305 minor, and 30½ major and numbered divisions, the first two engraved thus :— 1|METER|2, so that to the purchasers of such an instrument 30½ metres are represented as equivalent to 12 *inches, instead of* 100 *feet*! The stock included the carpenter's ordinary foot-rule, divided along one edge into inches and sixteenths of an inch, and the other into millimetres and 30½ (centi)metres. All the shopkeeper could say was that the scales were as supplied by the best makers, and must therefore be accepted as correct—the word *meter* had no other meaning than that the makers used it instead of saying the scale was French!

Four years ago, in a western London suburb, I had exactly the same experience, but if my memory serves me the makers were different. The shopkeeper informed me that in his two shops (one nearer the West End) he had already sold many hundreds of these scales.

From time to time the London and provincial Press report meetings at which there have been discussions on the great advantages of the metric system, but there the matter ends—apparently it is nobody's business, not even of the Board of Trade or the Board of Education, to take action which would ensure the circulation of correctly marked scales. All the wrongly engraved ones ought to be recalled, to have *centi-* engraved above *meter*.

HY. HARRIES.

August 20.

Direction of β-rays Produced by Polarised X-rays.

IN an abstract (NATURE, July 7, p. 26) of a paper read recently before the Royal Society, Mr. C. T. R. Wilson discusses some results on β-ray ionisation tracks which he has obtained by his cloud method. Among other things he notes (1) "Partial polarisation of the primary beams is indicated by the direction of ejection of a number of the β-particles being in one plane—that containing the direction of the cathode rays in the X-ray tube," and (2) "Of the ordinary long-range tracks, the majority have a large forward component comparable with the lateral component."

During the past year the present writer, using a beam of scattered X-rays about 90 per cent. polarised (Wilson's primary beam was probably about 10 per cent. polarised), has obtained stereoscopic photographs of β-ray ionisation tracks by the cloud method. These photographs show that most of the β-particles are ejected in a direction nearly parallel to that of the electric force of the polarised beam of X-rays. There is, however, a variation on either side of this direction.

The photographs also support Wilson's conclusion that a large majority of the β-particles have a velocity component in the direction of propagation of the X-rays.

F. W. BUBB.

Washington University, Saint Louis,
July 30.

Proposed International Survey of the Sky.

I AM informed by the director of the Office National Météorologique de France that, with the approval of Sir Napier Shaw, president of the International Commission for the Study of Clouds, the dates for taking the photographs of clouds have been postponed by one week. Photographs will be taken at the three specified hours from September 24 to October 1 inclusive. Volunteers are much needed to help in the work, and I shall be glad to send full instructions to those who will send me their names.

C. J. P. CAVE.

Stoner Hill, Petersfield, Hants,
August 27.

Gaseous Combustion at High Pressures.[1]

By Prof. W. A. BONE, F.R.S.

INTRODUCTION.

IN the course of the researches upon gaseous combustion which for many years past have been carried out in my laboratories, it became necessary to study the subject under much higher pressures than those heretofore employed. As this aspect of the work has recently assumed greater importance from the point of view of the mechanism of combustion than was at one time foreseen, an outline of it may be of interest. Before, however, explaining what our new observations have been, something should be said about the apparatus and methods employed for such work. For they must obviously differ from those used for experiments at atmospheric pressure, where the conditions are much less severe.

In the first place, the experiments must be carried out in specially designed bombs of forged steel capable of withstanding the sudden development of very high explosion pressures. Thus, in our recent experiments, the initial pressure at which the combustible mixtures were fired ranged up to 100 atmospheres; and the resulting pressures, which were developed in a small fraction of a second, were anything up to ten times as great. Hence the method of measuring and recording the pressures must be capable of following accurately, and with the least possible lag, a rise of pressure of from (say) 100 to 1000 atmospheres occurring within $\frac{1}{200}$th of a second. For this purpose we have employed a recording manometer of the form designed by Sir J. E. Petavel, which is a most efficient appliance for high-pressure explosion work.[2]

The photographic pressure-time records obtained in our experiments show (1) the rate at which the potential energy of the explosive mixture fired is transferred into kinetic (*i.e.* pressure or temperature) energy of the products; (2) the ratio of the maximum pressure attained on explosion to the initial pressure at which the mixture was fired—usually denoted as P_m/P_i; and (3) the rate of the subsequent cooling. From a study of these and other features of the records we are able to draw conclusions as to certain fundamental aspects of the combustion process itself.

SOME FEATURES OF THE COMBUSTION OF HYDROGEN AND OF CARBON MONOXIDE IN AIR.

As an example of the potentiality of high-pressure explosion research to reveal and elucidate new factors in gaseous combustion, I propose to deal mainly with the cases of hydrogen and carbon monoxide. For although at first they may seem to be of the simplest type, yet they present features of extraordinary interest and complexity which for many years past chemists have vainly tried to explain. Even engineers, who study internal combustion problems in their own way, without troubling themselves overmuch with the mechanism of the chemical changes involved, are seeking light upon what is termed the "suppression of heat" in such explosions. Indeed our present ignorance about these matters shows how far we are from really understanding the elements of gaseous combustion, and the need there is of much further fundamental research thereon.

From a chemical point of view there has always been something enigmatical about the very different behaviours of the two simplest combustible gases, hydrogen and carbon monoxide, when burning in air. For although their volumetric heats of combustion (assuming the initial and final temperatures being both about 15° C.) and the proportion by volume in which each of them combines with oxygen are the same, namely:

$$\left.\begin{array}{l} 2H_2 + O_2 = 2H_2O \ . \ . \ . \ 68{\cdot}4 \\ 2CO + O_2 = 2CO_2 \ . \ . \ . \ 68{\cdot}0 \end{array}\right\} \text{K.C.U. per gram-molecule,}$$

yet in many respects their modes of combustion in air present a striking contrast.

Thus, for example, (1) the appearance of a flame of hydrogen in air is very different from the lambent blue flame of carbon monoxide burning at the same orifice and under the same pressure; (2) hydrogen-air mixtures have lower ignition temperatures, and, under similar physical conditions, propagate flame much faster than the corresponding carbon monoxide-air mixtures; (3) the presence of even a minute quantity of steam greatly assists, if it is not absolutely essential to, the oxidation of carbon monoxide in flames, even when detonation is set up—thus a flame of the dry gas is easily extinguished on being introduced into a jar of air that has been previously dried over strong sulphuric acid; (4) a flame of carbon monoxide burning in air loses by radiation nearly 2·4 times as much energy as a hydrogen flame of the same size; also (5) the two radiations have their own characteristic wave-lengths—namely, 2·8 μ from a carbon monoxide-air flame and 4·4 μ from a hydrogen-air flame—which have been attributed to vibrational conditions in incipiently formed CO_2 and HO_2 molecules respectively, or, as I prefer to say, to the formation *at the moment of combustion* of intensely vibrating carbon monoxide-oxygen and hydrogen-oxygen complexes, which ultimately give rise to carbon dioxide and steam molecules respectively.

To summarise: carbon monoxide burns in air more slowly and with a more highly radiating flame than does hydrogen; also apparently the presence of some steam or other hydrogen-containing substance is necessary for its combustion. Precisely how steam accelerates or determines the combustion of carbon monoxide (and only a minute quantity suffices) has up to now never been completely explained; but chemists are generally agreed that carbon monoxide molecules are particularly inert towards oxygen molecules in flames. Indeed I think there are grounds for believing that in ordinary flames carbon monoxide cannot react with undissociated oxygen molecules, but that it requires the presence of either: O atoms or "activated steam": OH_2 molecules.

[1] From a discourse delivered at the Royal Institution on Friday, May 11.
[2] A full description of the bomb and accessory appliances will be found in Phil. Trans. Roy. Soc., A 215 (1915), pp. 275-318.

HIGH-PRESSURE EXPERIMENTS.

Bearing the foregoing considerations in mind, let us now see what new light has been shed on the problem as the result of high-pressure combustion research. Here it should be pointed out that, inasmuch as the chief difference between the condition of high- and low-pressure experiments lies in the absolute concentration of the interacting molecules, it may be expected that factors the operation of which chiefly depends on such concentration will become more dominant as the pressure arises. Indeed, the value of high-pressure work lies in the fact that it tends to show up and accentuate the operation of factors the influence of which may be either masked or overlooked at ordinary pressures.

One of the first things disclosed by our experiments was the absence of any direct relation between the rate at which the potential energy of an explosive mixture is transferred on explosion to its products as sensible heat (pressure) and the magnitude of the chemical affinity between its combining constituents. Thus, for example, the time required for the attainment of maximum pressure on exploding at 50 atmospheres a methane-air mixture ($CH_4+O_2+4N_2$), in which the combustible gas and oxygen are present in equimolecular proportions (*i.e.* corresponding to the primary chemical interaction in the flame), was many times longer than that required in the case of the corresponding hydrogen-air mixture ($2H_2+O_2+4N_2$), notwithstanding the fact that the affinity of methane is at least twenty, and possibly as many as thirty, times as great as that of hydrogen for oxygen in flames. In other words, the avidity with which a combustible gas seizes upon oxygen in flame combustion is not necessarily the factor which mainly determines the rate at which the potential energy of the mixture is transferred into kinetic energy of its products.

Later experiments have chiefly dealt with the explosion usually at an initial pressure of 50 atmospheres of what may be termed isothermic mixtures of either carbon monoxide or hydrogen with sufficient oxygen for complete combustion *plus* some variable diluent developing as nearly as may be the same amount of energy on combustion. I will now endeavour to explain their significance.

THE CONTRAST BETWEEN CARBON MONOXIDE-AIR AND HYDROGEN-AIR PRESSURE CURVES.

We may appropriately begin with a consideration of two typical pressure-time records (Fig. 1) obtained when normal carbon monoxide-air and hydrogen-air mixtures ($2CO+O_2+4N_2$ and $2H_2+O_2+4N_2$) were respectively fired in the bomb at an initial pressure of 50 atmospheres.

Now, although these two mixtures developed as nearly as may be the same total amount of energy on explosion, there was a striking contrast between the character of the pressure-time curves obtained. For whereas in the typical hydrogen-air curve the pressure rose with extreme rapidity (actually in 0·005 second) to its maximum (about 400 atmospheres), and almost immediately thereafter began to fall and assume the character of a simple cooling curve, in the corresponding carbon monoxide curve the pressure rose much more slowly and only attained a maximum (about 410 atmospheres) after 0·18 second, after which it was maintained almost at its maximum for a considerable time interval. The comparative slowness with which pressure energy is developed in such a carbon monoxide-air explosion, together with a considerable exothermic effect after the maximum pressure had been reached, were indeed very remarkable and significant features of our experiments. At first we were inclined to attribute them to the supposed "slow-burning" property of carbon monoxide as compared with the "quick-burning" of hydrogen; but further experiments revealed the operation of another totally unexpected factor—namely, the presence of nitrogen, which, as we discovered later, is not inert but acts as an "energy-absorber" in the combustion of carbon monoxide at such pressures.

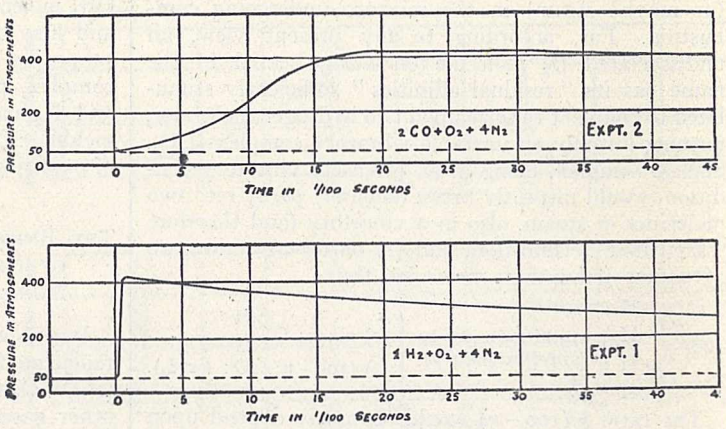


FIG. 1.—Pressure-time records for the explosion of carbon monoxide-air and hydrogen-air mixtures.

EFFECT OF ADDITION OF HYDROGEN UPON THE CARBON MONOXIDE-AIR CURVE AND UPON A CARBON MONOXIDE FLAME BURNING IN AIR.

It was next discovered that the replacement, even in very small proportions, of carbon monoxide by its equivalent of hydrogen in the aforesaid normal carbon monoxide-air mixture had a disproportionately large influence in accelerating the rise of pressure on explosion. This remarkable result, which is of considerable theoretical import, was dealt with at length in a paper published two years ago by the late W. A. Haward and myself in the Proceedings of the Royal Society.[3] Indeed at first sight it seemed as if the hydrogen had imposed its own character upon the whole course of the carbon monoxide combustion, even when the combustible part of the mixture exploded contained only one part of hydrogen to twenty-three parts of carbon monoxide by volume.

In this connexion it may be mentioned that the addition of a moderate amount of hydrogen to carbon monoxide burning in air at ordinary pressure has a considerable effect upon the character and spectrum of

[3] Proc. Roy. Soc., A. 100 (1921), pp. 67-84; see also a further paper in the current (August) number of the Journal of the Chemical Society.

the flame, a circumstance which seems to be of some significance in relation to the mechanism of carbon monoxide combustion. In conjunction with Prof. A. Fowler, of the Imperial College, South Kensington, we are now investigating it more closely with the view of finding out its meaning. But the facts known warrant us in concluding that the addition of a comparatively small proportion of hydrogen has a peculiar influence upon the combustion of carbon monoxide, whether at high pressures (as in our bomb experiment) or in flame combustion at ordinary pressures.

The Mechanism of the Combustion of Carbon Monoxide.

To explain the peculiar influence of hydrogen or steam upon the combustion of carbon monoxide, I think it must be supposed that oxygen and carbon monoxide molecules are mutually inert in flames, and that before the carbon monoxide can be oxidised the O_2 molecules must be resolved either into O atoms or into "activated" steam. This precedent condition can be brought about by the presence of hydrogen (or maybe steam) in the mixture undergoing combustion. For, according to my present view, an undissociated O_2 molecule on being heated in the flame has its "residual affinities" sufficiently stimulated to enable it to seize upon two hydrogen molecules, forming initially an unstable vibratory complex H_4O_2. Such a complex, being in an intensely vibratory condition, would instantly break down (1) *partly* into two molecules of steam, also in a vibratory (and therefore "activated") condition, and (2) partly also into two : O atoms and two H_2 molecules, thus :

$$100\left(\begin{array}{l}O\\ \|\\ O\end{array}\begin{array}{l}H\\ H\\ H\\ H\end{array}\right) \text{ would give rise to (simultaneously) } \begin{cases}(1)\ n\times 2 : O\begin{array}{l}H\\ H\end{array}\\ (2)\ (100-n)\ (2O : +2H_2).\end{cases}$$

The ratio $n/(100-n)$ would obviously depend upon both temperature and environment. The higher the temperature and the less hydrogen in the environment the less the magnitude of n. But in all conditions the hydrogen in a combustible mixture containing also carbon monoxide functions as a resolver of O_2 molecules simultaneously into (1) "activated" steam and (2) : O atoms. Thus it is suggested that the primary function of hydrogen as a promoter of the combustion of carbon monoxide is to resolve the O_2 molecules (inert towards carbon monoxide) into : O atoms and "activated" OH_2 (reactive towards carbon monoxide), itself being continuously regenerated in the process, as is shown in Fig. 2.

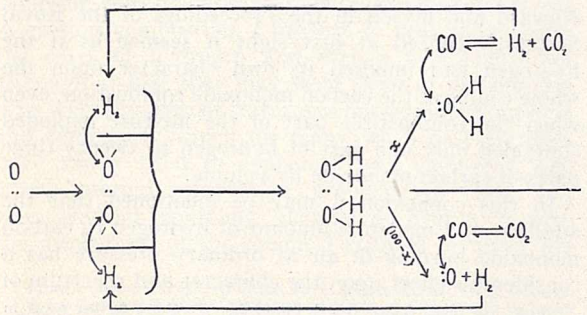


Fig. 2.

It may be observed that this view is similar to the one advanced forty years ago by Prof. H. B. Dixon to explain his discovery of the mutual inertness of dry carbon monoxide and oxygen in flames, but modified in one particular so as to make it more applicable to the further facts now known. He supposed that carbon monoxide is oxidised by OH_2 (but not by O_2) molecules in flames, the resulting hydrogen being immediately burnt to steam, which was thus continuously regenerated, as follows :

$$\left\{\begin{array}{ll}(a) & CO+OH_2=CO_2+H_2\\ (b) & 2H_2+O_2=2H_2O\end{array}\right\}.$$

If, however, only such interactions (and no others) occur, it is difficult to understand why the colour and spectrum of a flame of pure (moist) carbon monoxide are so unlike those of hydrogen burning in air. The characteristic spectrum of a carbon monoxide flame, which extends far into the ultra-violet, would surely seem to be due to the formation in it of some CO_2 molecules in a more highly vibratory state than would be likely to arise merely by interactions of CO and OH_2 molecules. The difficulty in question is obviated, and also other facts would be better explained, by supposing (as I do) that an unstable vibratory H_4O_2 complex, primarily formed by the interaction of O_2 and H_2 molecules, decomposes in each of two ways yielding : OH_2 and : O atoms, both of which are capable of oxidising carbon monoxide.

The Energy-absorbing Function and Activation of Nitrogen in the Combustion of Carbon Monoxide.

It next occurred to us to try the effects of progressively replacing the nitrogen of a normal carbon monoxide ($2CO+O_2+4N_2$) mixture by molecular equivalents of other gases, *e.g.* oxygen, carbon monoxide, or argon. The first two of these gases are diatomic, and would have much the same densities and heat capacities as the nitrogen which they replaced ; and although they might be expected to exert some "chemical mass" influence upon the combustion, yet in all other respects they would act as "diluents." In argon we had an absolutely inert monatomic gas of higher density, but smaller volumetric heat capacity, than nitrogen, and incapable of any internal vibrational energy. It would therefore presumably be incapable of exerting any effect upon the explosion other than that of merely sharing, by molecular collisions, in the increased kinetic energy acquired by the system as the result of the combustion.

It may be observed that while the said replacement of the nitrogen by the other gases would not affect in any way the total energy liberated on explosion, yet the experiments showed that it affected somewhat the proportion of the energy recorded by the gauge as pressure (temperature) at the instant of maximum pressure, and still more so the rapidity with which the said pressure energy was developed. The most important experimental results from this point of view are summarised in the following table, and illustrated by the set of pressure-time curves reproduced in Fig. 3. Here it may be pointed out that the most essential

data which must be established in such experiments are the following:

P_i = the initial pressure in atmospheres at which each mixture is fired.

P_m = the maximum pressure in atmospheres recorded in the explosion.

t_m = the time in seconds required for the attainment of the maximum pressure after ignition.

Σ = the thermal equivalent in K.C.U. of the energy liberated during the explosion.

Also the percentage amount by which P_m falls during (say) 0·5 sec. after t_m.

Mixture exploded.	Σ.	P_i.	t_m.	P_m.	P_m/P_i.	Per cent. Fall in Pressure in 0·5 sec. after t_m.
$2CO+O_2+4N_2$	10·2	50	0·190	409	8·18	11·6
$2CO+O_2+4O_2$	10·0	50	0·005	460	9·20	33·33
$2CO+O_2+4CO$	10·4	50	0·010	450	9·00	34·3
$2CO+O_2+4Ar$	10·2	50	0·025	510	10·20	26·4

These and other similar results led very decidedly to the conclusion that the nitrogen present in the normal carbon monoxide-air mixture had been exerting a specific influence on the whole course of events, which was manifested in a three-fold effect upon the pressure curves—namely, (1) a marked retardation of the rate of attainment of maximum pressure, (2) a lowering of the maximum pressure, and (3) a considerable retardation of the subsequent cooling. For whenever such nitrogen was wholly replaced by its molecular equivalent of any one of the other three gases, the development of pressure became nearly as rapid as in the explosion of a normal hydrogen-air mixture under like conditions. Moreover, comparative analyses of the pressure-time records obtained during the experiments in question have shown that, when nitrogen was present, much less kinetic (pressure) energy was absorbed up to the attainment of maximum pressure than was subsequently liberated during the cooling period. This remarkable circumstance shows that a considerable part of the radiation emitted by the burning carbon monoxide (which otherwise would have been absorbed by the walls of the explosion vessel) was intercepted by the nitrogen present. Part of the nitrogen so irradiated would then, in favourable circumstances, be oxidised to nitric oxide, thereby absorbing part of the kinetic energy developed by the explosion and consequently reducing the maximum pressure attained. Finally, the radiant energy so absorbed by the nitrogen, *plus* part of the kinetic energy (if any) absorbed in forming nitric oxide during the combustion, was liberated as kinetic energy during the cooling period, so delaying the cooling. Thus it was manifest that under our experimental conditions nitrogen has the power of absorbing part of the radiant energy developed by the combustion of carbon monoxide, and of slowly giving it out again in a kinetic form during the subsequent cooling period. In other words, nitrogen is not inert, but acts as an "energy absorbing" spring in such explosions. Indeed the results set forth in the foregoing table can scarcely be explained on any other supposition.

Another important conclusion arising out of these experiments is that when nitrogen so absorbs radiant energy developed during a carbon monoxide-air ($2CO+O_2+4N_2$) explosion under such conditions, it becomes chemically "activated," and capable of combining much more readily with oxygen than does nitrogen which has merely been raised to a correspondingly high temperature in a similar hydrogen-air ($2H_2+O_2+4N_2$) explosion. Indeed, when the bomb was rinsed out with distilled water after one of our hydrogen-air explosions at an initial pressure of 50 atmospheres, no more than a faint trace of nitric acid could be detected on applying the diphenylamine test to the washings; whereas, in the case of the corresponding carbon monoxide-air explosions, a similar test always showed a considerable formation of nitric acid.

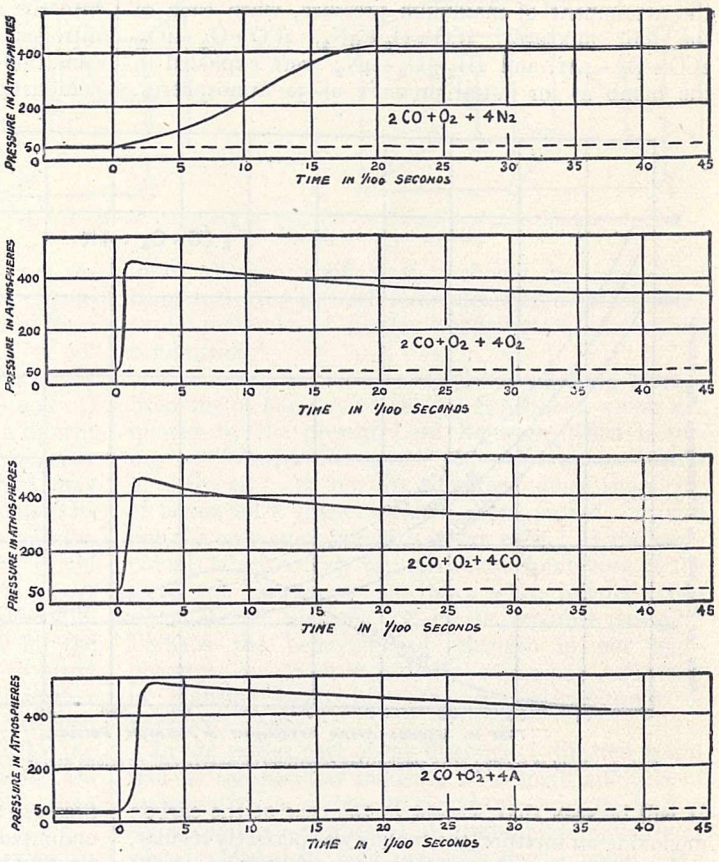


FIG. 3.—Pressure-time records for the combustion of carbon monoxide using different diluents.

It would seem as though the nitrogen molecule is able to absorb the particular quality of radiation emitted as the result of the interactions of CO and : O during a carbon monoxide-air explosion, which is different from that emitted during a hydrogen-air explosion. In other words, it seems as though there is

some constitutional correspondence between CO and N_2 molecules (the densities of which are identical) whereby the vibrational energy (radiation) emitted when the one burns is of such a quality as can be readily absorbed by the other, the two thus acting in resonance. The radiant energy so absorbed during the explosion presumably would not affect the maximum pressure attained, except in so far as the conditions permitted of any secondary oxidation of the "activated" nitrogen to nitric oxide during the actual combustion period; but radiant energy so absorbed would be liberated in a kinetic form during the subsequent cooling period, as the "activated" nitrogen slowly reverted to the ordinary form. Analyses of the pressure-time records obtained have entirely confirmed this supposition.

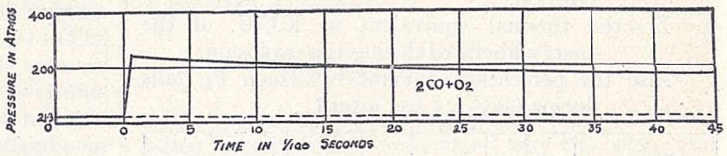


FIG. 5.—Pressure-time record of carbon monoxide-oxygen explosion.

The following graphs (Fig. 4) illustrate the strength of the evidence obtained up to this point as to the activation of the nitrogen during a carbon monoxide-air explosion at high pressures. They show the rates of cooling (expressed as pressure fall in atmospheres per second) of the gaseous systems immediately after the attainment of maximum pressure, when each of the four mixtures, $2CO+O_2+4N_2$, $2CO+O_2+4O_2$, $2CO+O_2+4Ar$, and $2H_2+O_2+4N_2$, were exploded in the bomb at an initial pressure of 50 atmospheres.

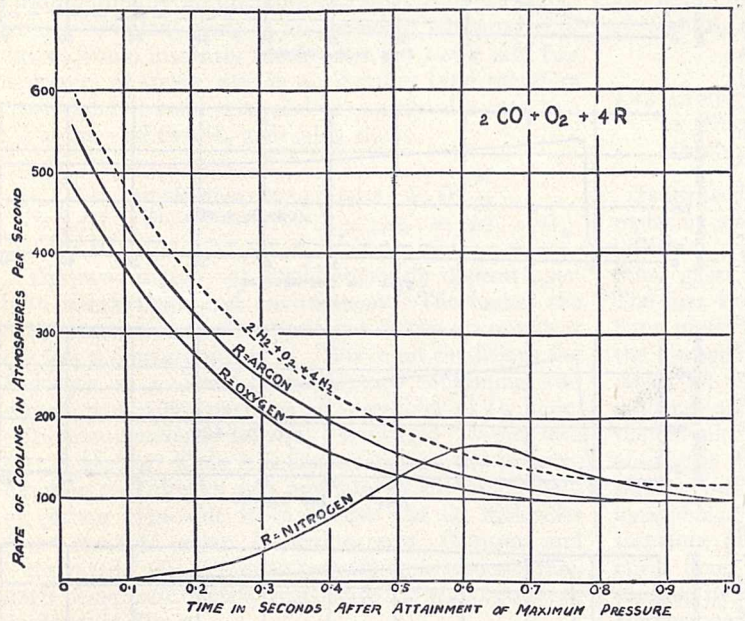


FIG. 4.—Rate of cooling after combustion of carbon monoxide using various diluents.

It will be seen that, except in the case of the carbon monoxide-air mixture, the cooling was perfectly regular, and presented no abnormal features whatever. In the case of the $2CO+O_2+4N_2$ mixture, however, there was no cooling at all during the 0·1 sec. after the attainment of the maximum pressure; and it was not until the lapse of the 0·6 sec. thereafter that anything like a normal rate of cooling was established. Attention is specially directed to the striking contrast between the perfect normality of the first 0·6 sec. of the cooling period in the case of the hydrogen-air ($2H_2+O_2+4N_2$) mixture, and its complete abnormality in the case of the corresponding carbon monoxide-air mixture. This circumstance, combined with the perfect normality of the cooling in the case of the $2CO+O_2+4Ar$ mixture, can scarcely be explained except on the assumption that the nitrogen functions differently in a hydrogen-air explosion, where it acts as an inert diluent only, from what it does in a carbon monoxide-air explosion, where in addition to its ordinary diluent action it has a peculiar energy-"absorbing" effect, whereby it becomes chemically "activated." On such an assumption the meaning of the $2CO+O_2+4N_2$ cooling curve is that the radiant energy which had been absorbed by the N_2 molecule during the previous combustion period was being slowly evolved in a kinetic form far into the subsequent cooling period, the "activated" nitrogen not having entirely reverted to its normal condition until at least 0·6 sec. after the end of the combustion period.

EXPERIMENTS WITH SOME ISOTHERMIC MIXTURES.

Much confirmatory evidence of the radiant energy-absorbing function and consequent "activation" of nitrogen in the combustion of carbon monoxide at high initial pressures has been obtained as the result of experiments in which mixtures of carbon monoxide and oxygen in their combining proportions, diluted with successive molecular proportions (2, 4, or 6) of the four diluents, argon, carbon monoxide, oxygen, or nitrogen, were fired at such initial pressures as would always result in the liberation of the same total energy (about 10 K.C.U.) during the subsequent explosion. For details of these experiments the reader is referred to the memoir recently published in conjunction with my co-workers (D. M. Newitt and D. T. A. Townend) in the Proceedings of the Royal Society, A. 103, pp. 205-232. There is, however, a significant feature about the pressure-time records (Fig. 5) obtained when an undiluted $2CO+O_2$ mixture was fired in our bomb at an initial pressure of 21·4 atmospheres, to which reference should here be made, because of its bearing on the theory of CO-combustion.

It will be seen that the maximum pressure (245 atmospheres) was developed in 0·005 sec., after which the cooling period immediately set in; the pressure fall during the next 0·5 sec. being 66 atmospheres, or about 27 per cent. of the maximum. It is evident that an exceedingly high temperature was momentarily attained in this experiment; indeed, assuming that the

"chemical contraction" involved in the passage from $2CO+O_2$ (3 vols.) to $2CO_2$ (2 vols.) was substantially completed at the instant of maximum pressure, the temperature at that instant would have been of the order of 5000° C. In any case the experiment finally disposes of the supposition that carbon monoxide is inherently a "slow-burning" gas. Moreover, the whole character of the pressure-time curve seems inconsistent with the idea, which has sometimes been put forward, that the maximum pressure attained on explosion is materially affected by the dissociation of carbon dioxide; indeed, there was no sign of any "after burning" or heat evolution after the maximum pressure had been attained.

Concluding Remarks.

The energy of a gaseous system such as we have considered is of course comprised partly of translational motions of its molecules as a whole, and partly of motions of some kind internal to these molecules. The former causes pressure (temperature), but the latter (which according to circumstances may be partly rotational and partly vibrational) produces no external physical effect other than radiation, which originates in high-frequency vibrations within the molecule.

Now in each of our experiments a definite amount of energy (thermally equivalent to about 10 K.C.U.) was liberated by the union of carbon monoxide and oxygen in the bomb. Presumably the greater part of this would appear as increased kinetic energy of the products as a whole (*i.e.* as pressure). The lesser part of the energy liberated in such explosions would manifest itself as "radiation" of wave-lengths characteristic of burning carbon monoxide; *i.e.* of incipiently forming carbon dioxide molecules in a highly vibrating state such as would result from CO and : O collisions. Now when nitrogen is present as a diluent it is able to intercept part of the "radiation" whereby it acquires energy of a vibrational kind, which may be intense enough even to dissociate the two atoms of its molecule, or in any case to "activate" it chemically. Such an absorption of radiant energy presumably would not affect the ratios P_m/P_i, except in so far as any part of the nitrogen primarily "activated" successfully competed with the burning carbon monoxide for the available oxygen, and thus became oxidised to nitric oxide during the combustion period. Indeed further experiments (now proceeding) have shown that such nitrogen "activation" is materially influenced by the initial pressure at which the explosive mixtures are fired; but this is an aspect of the matter which time does not permit me to develop.

There is of course nothing new in the idea of an "active" form of nitrogen, for ten years ago the Hon. R. J. Strutt (now Lord Rayleigh) discoursed upon it, and showed how ordinary nitrogen is chemically "activated" when subjected at low pressures to a Leyden jar discharge, whereby it glows and acquires the power of combining with various substances towards which it is normally inert. Such "active" nitrogen was found to be strongly endothermic, and Strutt favoured the view that it consists of dissociated nitrogen atoms, and recombination to form ordinary nitrogen caused the characteristic afterglow.[4]

Another view of the "activation" of nitrogen has been suggested which does not necessitate the complete dissociation of nitrogen molecules. According to Langmuir's statical representation of atomic constitution, there is a great similarity between the configuration of carbon monoxide and nitrogen molecules in the ordinary state, a circumstance to which he has directed special attention. He considers that both molecules are capable of existing in two forms, in one of which (the ordinary and more inert form) the two positive nuclei are both symmetrically located within one and the same outermost shell of eight electrons, whereas in the "active" form they are situated each within one of two separate shells, which have four electrons in common. According to this view not only would the "activation" of each gas be brought about by a reversible transformation from the one configuration into the other, as is shown in Fig. 6, but also an inert nitrogen configuration should be capable of being activated through resonance with a carbon monoxide molecule of similar configuration undergoing combustion.[5]

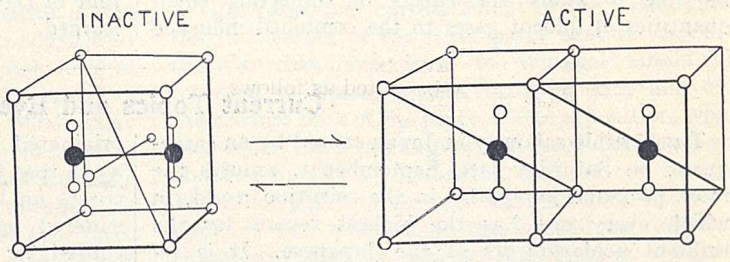


FIG. 6.—Models illustrating the reversible transformation of active nitrogen or carbon monoxide into the inactive form.

It is interesting now to recall the following passage from one of Faraday's letters to Schönbein, which was quoted by the present Lord Rayleigh, when lecturing on "Active Nitrogen" ten years ago. "What of nitrogen? Is not its apparent quiet simplicity of action all a sham? Not a sham, indeed, but still not the only state in which it can exist. If the compounds which a body can form show something of the state and powers it may have when isolated, then what should nitrogen be in its separate state?" Perhaps the behaviour of nitrogen in our high-pressure carbon monoxide-air explosions will help in realising more fully the deep significance of Faraday's words.

In the earlier part of my discourse I directed attention to the peculiar influence of a small addition of hydrogen to a carbon monoxide-air mixture undergoing combustion whether at atmospheric pressure or when exploded at high pressures in the bomb. We have also found that a similar small addition of hydrogen to a normal carbon monoxide-air mixture

[4] Proc. Roy. Inst., vol. xx. part 3 (1914), pp. 656-61.

[5] It may be noted that support is given to the idea of a similarity between the electronic configurations of nitrogen and carbon monoxide, and their capability of acting in resonance during explosions at high pressures, by a paper appearing in the Proceedings of the Physico-Mathematical Society of Japan for April last (*vide* NATURE, June 23, p. 859). For if the electronic configurations of the two gases are similar, their band spectra should be in close agreement; and this is shown to be the case. The author, however, remarks that the specific heat ratios of the two gases are incompatible with Langmuir's assumption of the two nuclei being in the same cube; and he suggests an alternative model, in which two cubes are joined at an edge.

undergoing combustion at high pressures is prejudicial to the "activation" of nitrogen. This is a point of considerable theoretical as well as practical interest; and it harmonises with the views which I have put forward respecting the mechanism of carbon monoxide combustion.

Leaving the many theoretical issues raised by our experiments to be settled by a further appeal to facts as the investigation proceeds, I perhaps may be permitted to indicate in conclusion one or two directions in which, if followed up with adequate means and resources, the work might lead to results of further interest.

In the first place, we have already in some of our experiments attained extraordinarily high temperatures; and we could go even higher were it not for the fact that we are approaching the safety limits of the bomb. If funds were forthcoming for the construction of a new bomb, with the necessary accessories, to enable us to work at still higher initial pressures than we have hitherto employed, we should be able to study the effects of subjecting small quantities of diluent gases to the combined influence of exceedingly high temperatures and intensive radiation. This is an aspect of the research which we are hoping it may be possible for us to pursue in the interests of science.

It is also obvious that our results may have considerable bearing upon the problem of nitrogen fixation. For, having proved that nitrogen can be activated by the combustion of carbon monoxide at high pressures, especially when hydrogen is so far as possible excluded from the system, we have in blast-furnace gas an almost unlimited supply of just the right kind of raw material from which nitric acid could undoubtedly be easily produced under the conditions indicated by our experiment. In view of the national importance of nitrogen fixation we hope these possibilities will be thoroughly explored at home, and not left entirely to foreign organisations, which will probably not be slow to seize upon them. High-pressure combustion work is opening up new possibilities of extending our knowledge, and however much chemical research may have taught us concerning flame and combustion since the time of Davy, there remains a vast amount still to be learned.

Current Topics and Events.

THE terrible calamity in Japan caused by an earthquake on Saturday last, September 1, arouses the most profound sympathy in the scientific world, in which every one has the highest regard for the brilliant achievements of the Japanese. It is reported that the cities of Yokohama and Tokyo, including the Imperial University buildings, have practically been destroyed and that as many as 300,000 persons have lost their lives. The catastrophe is therefore one of the greatest ever recorded, and Japan will need all the help which other nations can give in order to recover from it. The chief shocks occurred about noon on Saturday and were recorded at 4h. 11m. 18s. on Saturday morning on Mr. J. J. Shaw's seismograph at West Bromwich, Birmingham, as well as at other seismological stations throughout the world. The earthquake was preceded by a typhoon; and it will be remembered that the Messina–Reggio earthquake at the end of 1908, when 77,000 lives were lost, was similarly preceded by torrential rain. It is reported that the Osaka Observatory places the seismic centre in the Izu Peninsula. When, in 1906, an earthquake wrecked a great part of the city of San Francisco, the terrible fire which broke out immediately afterwards completed the destruction, and this appears to have been the course of events at Tokyo and Yokohama. As is usual, high sea-waves, often incorrectly called "tidal waves," have flooded low-lying land and thereby added to the destruction and casualties. Most Japanese earthquakes originate in the great trough of the ocean floor, nearly 5½ miles deep, known as the Tuscarora Deep, between the Kurile Islands and the coast of Japan. This was the place of origin in 1896 when the coast of Japan was devastated by three great waves, the largest about 50 feet in height, which caused the destruction of 20,000 lives in a few minutes. The Messina–Reggio earthquake similarly originated beneath the sea and a destructive sea-wave rose to a height of 25 feet and swept over the coasts on both sides of Messina Strait. Japan has suffered grievously from earthquakes and effects caused by them, but the catastrophe of Saturday last seems to have been the worst that it has experienced, and the Japanese people will need great fortitude in order to face the future with the confidence in which they have met other trials in the past.

SIR ARTHUR EVANS has published in the *Times* of August 28 and 29 an account of his past season's excavations at Knossos, which have produced some remarkable results. In particular, a wonderful series of frescoes was recovered from a town house, belonging to the beginning of the Late Minoan age, which was found at a depth of about five metres in an unexcavated strip of ground running up almost to the western border of the Palace. The frescoes had been torn from the walls of upper rooms in the house and lay heaped together in a very fragile state. The principal elements of three or four whole scenes, besides a multitude of detailed features, have been reconstituted. Taken as a whole they are said to afford a unique illustration of the painter's art of the golden age of Minoan Crete at approximately 1600 B.C. The variety of naturalistic detail, which is described as going beyond anything yet brought to light among Minoan remains, includes marine growths, birds, and many flowering plants, some of which can be identified. Monkeys of the genus Cercopithecus, which are not found nearer than the Sudan, and a group of three warriors, of which two are negro mercenaries, point to close African connexions. Some of the painted fragments are partly filled with Minoan writing. The abutment on the Palace of the important prehistoric main road from the south has been established, and Sir Arthur Evans

has found traces of it extending to Phaestos and thence to the havens of the African Sea. It thus brought Knossos into direct connexion with the Nile valley, and explains the intimate relations with Egypt, going back to the earliest dynastic age and beyond, which are recorded in the series of Egyptian relics found in the excavations on this site.

A GALE of unusual severity for the time of year was experienced over England during the latter part of Wednesday, August 29, and the early part of Thursday, August 30. At places in the south-west of England and at Scilly, the strength of the storm reached the force of a "whole gale," the anemometers registering a velocity of 60 miles an hour, while elsewhere on the south and east coasts and in the central parts of England the force of a "strong gale" was experienced, the anemometers registering a velocity of 50 miles an hour. The intensity of the gale was very prolonged, the greatest strength of the storm continuing in many places for six or eight hours. On Tuesday evening, August 28, the centre of the storm was located about 500 miles west-south-west of Ireland and was approaching the British Isles at the rate of thirty-five miles an hour. The centre of the storm passed over Ross-on-Wye on Wednesday evening, when the barometer read 29·17 in., and it continued in a north-easterly track across England, reaching southern Norway by Thursday evening. A noteworthy feature of the storm was the rapid movement or progress of the storm area, which maintained a rate of thirty-five miles an hour for a distance of about 1500 miles from the Atlantic to the North Sea. Heavy rain accompanied the storm, falling most in the advance segment; the amount measured exceeded an inch in the south of Ireland, and at places on the south-west and west coasts of England. Little rain fell in the rear of the disturbance, the weather rapidly clearing as the barometer rose, and brilliant sunshine was fairly general, ten to eleven hours being registered on Thursday, August 30, over England, except in the north and north-west.

THE sixty-eighth annual international exhibition of the Royal Photographic Society will be opened at 35 Russell Square, on Saturday, September 15, at 3 P.M., by the Rt. Hon. Lord Riddell. The exhibition will be open free to the public on September 17-October 27.

WE much regret to announce the death, at fifty-four years of age, of Sir Henry Hayden, F.R.S., formerly director of the Geological Survey of India, in an accident while descending the Finsteraarhorn on August 13; also of Mr. E. K. Muspratt, vice-chairman of the United Alkali Company, president of the Society of Chemical Industry in 1885 and member of the council of the University of Liverpool, on September 1, aged eighty-nine.

THE London Press recently reported the fall of a meteorite during a storm at Immingham, in North Lincolnshire, and stated that it had been secured by the Vicar. The matter has been investigated by Mr. T. Sheppard, of the Municipal Museum, Hull, who finds that the alleged meteorite is a piece of slag from the local ironworks, though it certainly appears to have been fused by having been struck by lightning, which will account for an eye-witness's statement that it made a hissing noise in the water and that steam rose from it.

UNDER the auspices of the National Union of Scientific Workers a meeting is to be held in the Hartley Botanical Laboratories, University of Liverpool, on Friday, September 14, at 5.30 P.M., to which all members of the British Association are invited. A discussion on the relation of "Science and Industry" will be opened by Prof. J. M. Thompson: Mr. J. Sandeman Allen, chairman of the Liverpool Chamber of Commerce, is to preside. It is hoped that a large attendance of local business men will be secured, in order that their interest in the application of science and the claims of men of science for their sympathetic interest and support may be further stimulated.

THE Vancouver correspondent of the *Times* states that a member of the relief party sent in search of the Canadian expedition to Wrangel Island has returned to Nome, Alaska, bringing with him only the Eskimo cook of the party. The expedition, which was under the leadership of Mr. Alan Crawford, was financed by Dr. V. Stefansson, and set out in 1921. The relief party under Mr. Harold Noice found the body of one member of the expedition, who had apparently died of scurvy, and learned that Mr. Crawford with two companions started over the ice for Siberia in December. Nothing has been heard of them since. A bottle was found in Roger Harbour on the south of Wrangel Island containing the names of the party and claiming the island in the name of King George. The relief party left one man with thirteen Eskimos behind to colonise the island and to search for the bodies of the missing men.

IN connexion with the recent correspondence in NATURE on the forms of scientific terms derived from the Greek language, Mr. A. Stanley Pye-Smith sends us a copy of an interesting letter written to "Charles Lyell, jun., Esq., F.R.S." by Dr. J. Pye Smith, F.R.S. in April 1837, and returned by Lyell in 1851 when the biography of his correspondent was being prepared. Dr. Pye Smith protests in this letter against the use of "e" to represent "ai" in Eocene, Miocene, etc., and he points out that this letter leads to obscurity, since it might equally serve as a substitute for "oe," as is the case in the word "economy." He cites several cases in which a diphthong was dispensed with for about a century and was afterwards restored (Cæsar, Phœnicia, ægis, etc.). Curiously enough, he does not criticise the first syllable of Miocene and Pliocene, in which the use of the Greek "ei" has been courageously maintained by Prof. Boyd Dawkins. International usage probably now stands in the way of any changes in either of these well-established terms.

WE have received from Messrs. James Swift and Son, Ltd., 81 Tottenham Court Road, London, W.1, a copy of their "Petro 922" catalogue of petrological

and mineralogical accessories which includes a large variety of micrometers, goniometers, refractometers, spectroscopes and other microscope accessories necessary for routine work or for special research in petrology, mineralogy and crystallography. Among the more important items described are Dr. A. Hutchinson's universal goniometer; the recording micrometer designed by Prof. Shand for geometrical rock analyses; the stage refractometer of Dr. F. E. Wright by means of which the refractive index of a liquid may be easily ascertained, with an accuracy of one or two units in the third place of decimals, on any microscope fitted with a Bertrand lens and a micrometer scale in the ocular; and the tank refractometer designed by Mr. A. F. Hallimond and Dr. H. H. Thomas, which affords a convenient means for determining the refractive indices of liquids in bulk and is useful for expeditiously preparing standard fluids for testing the refractivity of minerals. A price list accompanying the catalogue shows a general reduction in the prices of the apparatus listed.

MR. H. LING ROTH, of the Bankfield Museum, Halifax, has in the press a work on "The Maori Mantle," with numerous illustrations and plates.

THE *Annali di Chimica* is a new journal published at Trieste, devoted to chemical and "astrochemical" matters. The second number, which we have just received, contains articles on the atom, on Sirius and other stars, and on the transmutation of base metals into gold (reprinted from another journal).

ALL who were attracted by the sumptuous art books of Mr. A. Thorburn on "British Birds" and "British Mammals" will be interested to learn that the same author and artist is bringing out through Messrs. Longmans and Co. in the autumn a further work entitled "Game Birds and Wild-fowl of Great Britain and Ireland," with 30 plates in colour showing 58 species. The volume will be issued in two forms, one being on large paper, limited in number to 155 copies.

AMONG the forthcoming books announced by the Cambridge University Press are the following: "Life," by Sir A. E. Shipley, which will form an introduction to biology for the general student; "Physical and Chemical Science," by W. C. D. Whetham; "The Structure of the Atom," by Dr. N. R. Campbell, being supplementary chapter No. 17 to "Modern Electrical Theory"; and a new and revised edition of Prof. G. H. Hardy's "Orders of Infinity: the *Infinitärcalcul* of Paul Du Bois-Reymond," in the Cambridge Tracts in Mathematics and Mathematical Physics.

Our Astronomical Column.

CALENDAR REFORM.—The League of Nations, in addition to the grave political problems which confront it, has found time to appoint a committee to deal with the question of Calendar Reform, more especially as relating to the determination of the date of Easter. The principal religious bodies are represented, the Church of England by Rev. T. E. R. Phillips, secretary of the Royal Astronomical Society. In this connexion an interesting letter, signed "Astronomicus," appeared in the *Times* for August 27. It quoted a resolution adopted by a council representing the Greek and allied churches that was held last May in Constantinople. This extended to ecclesiastical purposes the use of the Gregorian calendar, which had been adopted for civil purposes in Greece some months previously. Thus the Gregorian calendar has now finally superseded the Julian one in Europe, though the process has occupied 340 years.

The resolution also affirmed the willingness of the churches concerned to modify the method of determining Easter, provided it was kept on a Sunday subsequent to the first full moon after the equinox. This condition would be satisfied if Easter were the first Sunday after April 20; this is a later date than most of those suggested, which have been near the middle of the present range, extending from March 22 to April 25.

STELLAR POSITIONS AND THE EINSTEIN LIGHT-BENDING.—The idea has occurred to many people that the Einstein light-bending by gravitation, the existence of which was confirmed at the eclipses of 1919 and 1922, might produce appreciable displacements in the apparent position of stars if their rays passed close to other stars on their way to our system. Signor O. Z. Bianco, of Turin, in a note to us, quotes Bessel's view that the number of dark stars may greatly exceed that of the lucid ones, and infers that serious displacements of the positions of the latter may result. A little consideration will, however, make it plain that the number of cases where the necessary conditions prevail must be extremely small, and even in these cases the proper motions of the three bodies concerned (lucid star, dark star, sun) would quickly modify them, so that the large shift would be of very brief duration.

A ray passing at a distance of one astronomical unit from the sun is deflected through an angle of 0·01″. A study of the stellar masses shows that only a very small minority of the stars have masses greatly in excess of the sun's. Moreover, the majority of the stars are at such distances that an astronomical unit subtends an angle of less than 0·01″. Even if we supposed the dark stars to outnumber the lucid ones a thousand-fold, there would be very few cases of two independent stars approaching each other so closely in direction; but, as a matter of fact, dynamical researches on the stellar motions give no support to the existence of such a large preponderance of dark stars.

It may further be pointed out that shifts of the order of 0·01″ in the positions of stars are quite unimportant, being far below the probable errors of the best catalogues. As an illustration of this it may be mentioned that the correction of meridian observations for the Einstein shift due to the sun has not been suggested by any one, though it would frequently mount to 0·02″. It has already been mentioned in these notes that the Einstein displacement has no effect on the relative positions of binary stars, but only comes into play when one orb is far behind the other. The argument outlined above shows that Signor Bianco's fears are groundless, and that no appreciable errors in star-positions from the cause mentioned are to be apprehended.

Research Items.

EXCAVATIONS AT CIRENCESTER.—Some recent important discoveries at Cirencester are described by Mr. St. Clair Baddeley in vol. xliv., 1922, of the Transactions of the Bristol and Gloucestershire Archæological Society. The remarkable fact results from the excavation of the city wall that portions of it, at any rate, are not of Roman Imperial construction or formed, as has been hitherto accepted, during the late Empire, so as to enclose the then far-expanded, but not yet endangered, Corineum; but that they are of precisely identical character with the dry-walled mounds that encircle many other Cotteswold settlements, made by pre-Roman tribes. In the former case the probability seems to point to the Dobuni, or a previous people, the Cornavii, as the makers of the wall; these mound-enclosures have been in later times occupied in the extension of Roman Cornubium. Mr. Baddeley's paper is illustrated by photographs of the excavations.

TABLET-WEAVING IN ANCIENT EGYPT.—Mrs. Crowfoot and Mr. H. Ling Roth have reprinted a paper from the Annals of Archæology and Anthropology (vol. x., Nos. 1-2), entitled "Were the Ancient Egyptians conversant with Tablet-weaving (Brettchenweberei, Tissage aux Cartons)?" They dispute the theory that tablet-weaving is the origin of all weaving, as has been asserted by Herr H. Pralle. No authentic tablet-weaving tools have yet been found, and the art is not known to be depicted on any wall or other illustration in Egypt. After a careful review of weaving technique, illustrated by numerous drawings, the writers arrive at the conclusion that no tablets have been found earlier than Coptic days, and those of doubtful provenance, and all ancient Egyptian textiles examined by them were certainly not tablet-woven; there is at present no evidence whatever for tablet-weaving in ancient Egypt.

WOOD CARVINGS FROM THE CONGO AND WEST AFRICA.—Mr. H. V. Hall continues in the June issue of the Philadelphia *Museum Journal* his account of a collection of wood carvings from the Congo and West Africa. The article is fully illustrated and describes some remarkable specimens of West African work. The question of foreign influence on this type of native art is not clear, but most of the specimens seem clearly to be indigenous work. The Kroos, at any rate, have been for a long period closely associated with Europeans, and one image seems to represent a Captain Hunt, the master of a steamer, who is seated on a barrel which may have contained nails, or its contents may have been of a liquid nature. The characteristic carelessness of the Negro craftsman in matters of detail is shown by the fact that though the opening of the tight jacket is carefully indicated by a line down the front, yet this is shown folding right over the left, there are no buttons, and no division is marked between jacket and trousers.

APPRECIATION OF TIME.—"An Experimental Study of the Appreciation of Time by Somnambules" is the title of an article by Mr. Sidney E Hooper in the Proceedings of the Society for Psychical Research for July. It is known that some hypnotic subjects display what appears to be a supernormal power of appreciating the passage of time. If, for example, such a subject is told during hypnosis to perform some simple act at the end of 5000 minutes he will do so, at or about the correct time, although in the period intervening between the hypnosis and the performance of the act he has had no conscious knowledge of the suggestion that has been given to him. Experiments demonstrating this peculiarity of the hypnotic state have been recorded by Gurney, Delbœuf, Milne Bramwell, and Mitchell, and Mr. Hooper takes up the inquiry at the point at which it was left by these observers. Two main problems are presented by the results of these experiments: (1) the subliminal calculation by which the subject comes to know the time at which the suggested act is to be performed; (2) "true time-appreciation," by which the subject knows when the time so calculated arrives. When a long time-interval is given in minutes the subject usually calculates subliminally so as to find out when the suggested act falls due. Mr. Hooper's experiments corroborate this; but one of his subjects maintained that as soon as the suggestion was given she began to count rhythmically and continued to do so until the suggested number of minutes had elapsed. It is to such a capacity for accurate counting of seconds by a subconsciousness on which the pendular rhythm of the clock has been faithfully inscribed that Mr. Hooper looks for an explanation of "true time-appreciation."

CALIFORNIAN POLYCHÆTES.—Dr. J. Percy Moore (Proc. Acad. Nat. Sci. Philadelphia, vol. 75, 1923) completes the account of the polychætes dredged off the coast of South California. His three previous papers, published respectively in 1909, 1910, and 1911, dealt with the Nereidiformia, and the present report contains the systematic account of the other suborders. Thirty-three new species are described.

ALPINE WATER-MITES.—Dr. C. Walter's memoir on the Hydracarinæ of alpine waters (Denkschr. Schweiz. Naturforsch. Ges., Bd. 58, 1922), together with previous accounts of Italian investigators of the more southern forms, provides a fairly complete account, at least of the faunistic aspect, of these fresh-water mites. The author has been collecting since 1906, and specimens have been obtained from 433 localities in the basins of the Rhone, Rhine, Danube, and Po. The systematic descriptions are followed by a short comparative account of the eggs and of the larval and nymphal stages. Attention is directed to the great importance of the larval stage in regard to the distribution of many of the species; the larva fixes itself to some insect, inserts its mouth-parts through the skin of its host and so feeds, being meantime transported by the host. The author gives interesting notes on the adaptations met with, *e.g.* the rich development of hairs on the legs of swimming species, the dorso-ventral flattening of the body—and with this a hardening of the dorsal chitin—usual in fluviatile species. The Hydracarinæ of alpine waters may be divided into two groups—the eurythermic species, mostly living in still water, highly adaptive and resistant, and widely distributed; and the stenothermic species, not tolerant of extensive changes of temperature but finding their optimum in water of low temperature, more limited in their range, and found chiefly in the springs and on the shores of high alpine lakes. The author discusses the origin of these two groups—the first largely composed of species which in post-glacial times spread westwards from Central Asia, and the second for the most part a remnant of the glacial fauna. He puts forward anatomical and other evidence indicating the origin of these fresh-water mites from marine mites (Halacaridæ).

NEMATODES OF SHEEP AND CHICKENS.—The two principal communications in the current issue of the *Journal of Helminthology* (vol. i. pt. 3, 1923) are a careful account by T. W. M. Cameron of the anatomy

of *Monodontus trigonocephalus* of sheep, and a description by Dr. R. J. Ortlepp of the life-history of *Syngamus trachealis*, the gape-worm of chickens. The eggs in the uterus of the female Syngamus do not develop there beyond the 16-cell stage, and they are usually laid in this or in the 8-cell stage, and when kept in well aerated water at 25° C. they take about a week to develop into infective larvæ, undergoing one moult during this period. The second stage or infective larvæ are sheathed; they are non-climbers, do not penetrate the skin, and cannot resist desiccation. Larvæ swallowed by chicks reach the lungs in about 24 hours. The path of migration from the digestive tract has not been traced but the author inclines to the view that the larvæ are carried like those of Ascaris and Ancylostoma, *i.e.* in the blood-stream through the heart to the lungs. In the lungs the larvæ grow considerably and undergo two further moults, the final or fourth stage being reached in about five days after infection. The young worms then pair and migrate into the trachea, where, 10 to 14 days later, they attain sexual maturity. Thus the whole life-cycle is completed within a month. Dr. Ortlepp supports the view of Walker and Waite that, in Nature, chickens contract the infection by eating earthworms infected with the larvæ.

A Physiological Function of the Pituitary Gland.—The chemical constitution of the active substances extracted from the posterior lobe of the pituitary gland is still unknown, but their important pharmacological properties have received much attention and are accurately determined. Until recently, however, the physiological functions of the organ remained a matter for speculation. In a series of researches on pigmentary changes, L. T. Hogben and F. R. Winton have now succeeded in defining such an essential endocrine function in amphibia. They showed (Proc. Roy. Soc., 1922, B, vol. 93, 318-329) that injection of traces of posterior lobe extracts into pale frogs (melanophores contracted) induces profound darkening of the skin (expansion of the melanophores). This action is not elicited by other tissue extracts, whereas the minute posterior lobe of a single frog includes enough active substance to darken at least fifty other pale individuals. So sensitive is the reaction that it may serve as a method of detection or of rough estimation of the potency of such extracts (*Biochem. Journ.*, 1922, vol. 16, 619-630). This response is a direct action on the skin, as can be demonstrated by experiments on the isolated skin, and by the inefficacy of drugs with paralytic action to prevent it. Stimulation of nerve trunks and the administration of the drugs showed no direct evidence of nervous mechanism for pigment control (Proc. Roy. Soc., 1922, B, vol. 94, 151-162). After extirpation of the whole of the pituitary gland the skin always became quite pale and the melanophores completely contracted. This pallor persisted permanently even in the presence of the optimum conditions (cold, wet, and shade) for darkening of normal animals. Injection of posterior lobe extracts was followed by profound darkening with complete melanophore expansion, which lasted for a varying time according to dosage and other conditions, the animals then returning to permanent pallor. Frogs from which anterior lobes only were removed, or with the brains exposed, were indistinguishable from normal animals with respect to their pigmentary reactions. The failure of colour response associated with complete hypophysectomy is therefore due to absence of posterior lobe secretion, and not attributable to anterior lobe deficiency or to the operative technique employed (Proc. Roy. Soc., 1923, B, vol. 95, 15-30).

The Ethyl Alcohol Industry.—The *Chemical Trade Journal* for August 3 prints a review of the ethyl alcohol industry. The pioneer of synthetic alcohol was Hennell, who, in 1828, found that dilution and distillation of a sulphuric acid solution of ethylene yielded alcohol; this reaction also has considerable theoretical significance. During the War, much alcohol was made from acetylene, which in its turn was produced from synthetic calcium carbide. The acetylene was hydrated directly to acetaldehyde (in the presence of a mercury salt), which was then reduced to alcohol by the Sabatier and Senderens' reaction. Under present economic conditions Germany appears to make most of her alcohol by fermentation methods. The article includes a survey of the physical properties of alcohol, its industrial applications and future prospects for the synthetic product.

Celtium or Hafnium?—*Chemistry and Industry* for August 10 contains an important article by Prof. G. Urbain under the title "Should the Element of the Atomic Number 72 be called Celtium or Hafnium?" Prof. Urbain claims that he has had this element in his possession and under his observation since 1911, when he suggested the name celtium for it; that although Moseley in July 1914 (when Urbain visited him in Oxford) was not able to detect the characteristic lines of No. 72 in the fraction submitted for test, two of these lines were, in fact, detected by Dauvillier in 1922, in the spectrum of the same material, by making use of improved experimental methods; that if any doubt existed as to the identity of these lines, it was finally removed by a direct comparison (by the method of coincidences) of lines from the 1911 fraction with a newly-prepared fraction from a zirconium mineral, in which the presence of No. 72 is not now questioned, in view of the *six* characteristic lines recorded by Coster and Hevesy. Under these conditions the claim of the later workers to have discovered a new element (since it could scarcely be based on a more accurate measurement of the same physical property) appears to depend on the assumption that a "fourth group" element *could not have been present* in the rare earth fractions examined twelve years previously by Urbain. Prof. Urbain claims that it was actually there, and that there is no theoretical objection to its presence, since "there is no law which compels the elements to associate themselves strictly in accordance with their classification." Moreover, he had already, in 1921, himself agreed to Perrin's classification of celtium as a fourth-group element, in spite of the fact that he had found it in a rare-earth mixture. He points out that there are many elements with different valencies which cling together so closely that one cannot separate them except by very laborious treatment, and in particular that thorium, which is quadrivalent, is in fact always accompanied by the tervalent rare earths. Again, Bohr's theory only applies to free atoms and simple ions, and affords at present no guidance whatever as to the behaviour of complex ions; it is therefore not able to predict the chemical properties of elements which form double-salts in solution. These give rise, however, to many surprising cases of complete isomorphism, *e.g.* the fluorides of quadrivalent titanium with the oxyfluorides of quinquevalent niobium. Prof. Urbain claims that "no purely theoretical reasoning ought to prevail against a well-established question of fact," and that as regards the presence of celtium in his rare-earth fractions, the facts are not only well established but also in strict accord with general chemical experience.

The Gaseous Nebulæ.[1]

By J. H. REYNOLDS.

IT has been recognised for many years that the nebulæ fall into two great divisions—the spirals and the gaseous and diffuse. The distinction between the two is fundamental, for there can be no doubt now that the spirals are extra-galactic, and the gaseous inter-galactic formations, although it is impossible yet to define the scale and distances of the spirals with any certainty. Since Huggins's great discovery of the gaseous nature of certain nebulæ, the principal work on these objects has been done in America, especially at the Lick Observatory and at Mt. Wilson; and coupling up these investigations with recent advances in physics in which Great Britain has played no mean part, we are able to form a good general idea of the meaning and origin of the gaseous nebulæ.

The first important fact which emerges from the physical work of Fowler, Saha and others, is that only the elements of simplest constitution, such as hydrogen and helium, are known to be present, and that ionisation of known elements is probably responsible for all the unknown lines in the nebular spectrum. Then, again, the gaseous nebulæ are only associated with stars of the highest temperature, as the stars in the centre of the planetaries are usually "O" type stars of the Harvard scale, which yield an emission or absorption spectrum of ionised helium on a continuous background, and the "B" type stars, which are next in order of temperature, and are associated with the irregular gaseous nebulæ in Orion. Until recently it was supposed that the so-called "nebulium" identified with the nebular radiations at λλ5007 and 4959, and other well-known lines such as λλ4363, 4686 and 3727, represented unknown gases in the same sense that helium was unknown until identified in the laboratory by Sir William Ramsay, but the work of Moseley and Aston and the formation of the scale of atomic numbers has taught us that there is no room for any more unknown elements in the atomic scale of the lighter gases, and we must look in the direction of ionisation of the known elements in the first ten numbers of the scale for these unknown nebular lines.

These considerations and the progressive spectra of Novæ lead to the conclusion that in all gaseous nebulæ we are dealing with the same material, and that the differences found in the spectra are to be assigned to differences only in physical conditions, and the key to these conditions is to be found in the effective radiation of the star or stars involved in the nebula. Only the first two numbers of the atomic scale are definitely established as appearing in nebular spectra, the helium appearing often near the nuclear star in its ionised form. But Wright in Lick Observatory Pub., vol. xiii., provisionally identified certain nebular lines with carbon and nitrogen, and the presence of the former at any rate is now accepted.

The principal nebular lines of unknown origin are the following:

$$\left.\begin{matrix}\lambda 5007\\ \lambda 4959\end{matrix}\right\}\lambda\lambda 4363,\ 4340,\ 3868,\ 3727\text{–}9.$$

The first two are invariably found in all the gaseous nebulæ in the same relative strength, and they evidently form a doublet. λ5007 was the first gaseous radiation to be discovered in a nebula, and with its companion it exists in a very marked form in nearly all the objects of this class yet observed. The other radiations vary much in relative strength, and sometimes seem to be absent altogether. Perhaps the most remarkable is that usually described as λ3727, which has been found by Wright to be in reality a doublet separated by only two Ångström units. It is astonishingly brilliant in the Orion Nebula, and with the hydrogen radiations it is responsible for the great photographic effect of this object. It is always of maximum extension, and is found in regions far removed from the involved stars, where the hydrogen radiations are comparatively faint. An investigation by means of screened direct photographs was undertaken by the writer some years ago as to the relative distribution of the doublet λ(5007, 4959), the hydrogen series, and λ3727 in the Orion Nebula. The principal results were:

(1) To establish Keeler's conclusion that the doublet λ(5007, 4959) was conspicuous only in the central region surrounding θ Orionis, where it was very brilliant.

(2) To demonstrate that the hydrogen radiations extended faintly to the most remote regions of the nebula, and

(3) To show that the radiation λ3727 was much stronger than the hydrogen in these outer regions, besides giving certain differences in detail.

We have then in the Orion Nebula a bright central portion where the radiations λ(5007, 4959), the hydrogen and helium radiations and λ3727 are integrated together, an intermediate region where hydrogen and λ3727 appear of about the same strength, and an outer region where λ3727 predominates. There is no difficulty in ascribing λ(5007, 4959) probably to an ionised form of one of the elements heavier than helium, as we should expect the heavier elements to predominate in the central regions surrounding the star θ and the other involved stars. There is, however, very great difficulty in explaining the distribution of λ3727. It must be remembered that the "B" type stars involved in the nebula are not of the highest stellar temperature, and it is almost impossible to imagine the existence of ionisation at all in these remote regions. There is, indeed, good reason for thinking that λ3727 is a comparatively low temperature radiation, as it is either faint or absent altogether in the planetaries containing "O" type stars, and it does not occur so far as can be ascertained in the nebular stages of Novæ.

An interesting point bearing on the distribution of this radiation is brought out by an examination of the nebulosity surrounding the star Bond 734 to the north of the principal nebula. In the screened exposure of the radiations λ(5007, 4959) no nebulosity appears round this star, and the absence of these radiations is confirmed in Lick Observatory Pub., vol. xiii. The hydrogen image is quite strong, and is accompanied by an equally strong image in λ3727. It is quite evident, therefore, that there is no direct relation between the radiations λ(5007, 4959) and λ3727, and it is known that the hydrogen radiations occur without either. But the question arises as to whether λ3727 can exist independently of hydrogen. The differences of detail visible in the photographic images given by hydrogen and λ3727 certainly suggest an independent origin, but a complete answer to the question is impossible, as hydrogen is found in varying strength in all the gaseous nebulæ and novæ. If it were not an impossibility, one would naturally look for the radiation as some element actually lighter

[1] Substance of an address delivered before the Birmingham University Physical Society on March 14, 1923.

than hydrogen. The origin of this radiation λ3727, therefore, seems to be one of the most interesting and difficult problems which the astrophysicist has to deal with at the present time.

We will now pass on from the Orion Nebula, which may be taken as a good example of the irregular gaseous nebulæ connected with "B" type stars, to the planetaries. Here we have to deal with involved central stars which are of the "O" type, of a considerably higher temperature. In the spectra of these nebulæ the doublet λ(5007, 4959) is still usually the strongest radiation, and hydrogen is invariably present. On the other hand, λ3727 is often comparatively faint or absent altogether, although λ3869 is still one of the strongest radiations in the spectrum. Ionised helium at λ4686 is also very conspicuous, but does not extend far from the nucleus, showing that ionisation only takes place in the neighbourhood of the star itself. Another bright radiation at λ3426, found in the spectrum of Nova Cygni III, also makes its appearance, but its extension from the nucleus is small.

A very remarkable feature in the high dispersion spectra obtained with the three-prism spectrograph of the Lick 36-in. refractor was the character of the lines under magnification. When the slit of the spectrograph was placed across the major diameter of the planetary nebula disc, the resulting lines in the doublet λ(5007, 4959) were not parallel-sided, but spread out in the centre, each end being slightly curved in opposite directions. If we are to interpret this phenomenon on the Döppler principle in the usual way, this means that the gases are both receding from us and advancing towards us in the line of sight, coupled with a slight rotation of the gaseous spheroid as a whole. The only feasible explanation is that the gaseous shells forming the nebula are still expanding, and we are at once led to a comparison with novæ such as Nova Aquilæ III, which now has developed an expanding gaseous disc.

The later spectroscopic stages of novæ are comparable in every detail with the planetary nebulæ; the galactic distribution of both is similar, and the evidence is now overwhelming that the planetaries had their origin in novæ, and the gaseous shells of the planetaries are the remains of past outbursts. It may be asked why the planetary nebulæ are comparatively few in number, but it is evident that if the central star fell to a lower temperature than the "B" type, the radiation would be insufficient either to keep the gases in a state of equilibrium or to illuminate them, and the aspect of a planetary nebula would disappear. The same remarks apply to objects like the Orion Nebula connected with "B" type stars. Here the radiation energy is not so intense as the "O" stars, and we do not get an equal degree of ionisation: the strength of the radiation λ3727 is also evidence of different physical conditions, but the Orion and other irregular gaseous nebulæ have every appearance of being swept away from the involved stars, and they all probably indicate a former outburst of several stars culminating in an "O" type of spectrum and a high radiation pressure.

The old idea that the gaseous nebulæ were the primitive forms of matter from which stars were evolved must, it seems, be given up for the exactly contrary hypothesis that they had their origin in stellar outbursts, where matter passed from complex to simpler forms by atomic disintegration under the stress of extreme temperature development.

Plants in Relation to the Health of Man.[1]

CINCHONA, the plant which yields quinine, known under the name of "Jesuits' Powder" since 1655, was introduced into India about the years 1858 to 1862. Seedlings and seeds were brought to Great Britain from the Andes of Bolivia during those years, principally by Sir Clements Markham and Mr. Richard Spruce, and the plants, which were raised at the Royal Botanic Gardens, Kew, were taken to India and Ceylon. The cultivation in India was mainly established in the Nilgiri Hills and in Sikkim. The three species of Cinchona which are particularly valuable as sources of quinine are *C. Calisaya*, *C. Ledgeriana*, and *C. succirubra*.

The Dutch had also been experimenting with Cinchona and established the plant in Java about the same time as the English were introducing it to India. So successful have the plantations been in India, thanks to the labours of Dr. Thomas Anderson, Sir George King, Messrs. Wood and Gammie and Sir David Prain, that in every post office in India it is now possible to procure doses of from seven to ten grains of pure quinine for a *pice*, which is about equal to a farthing. In this way, more than eight thousand pounds avoirdupois of quinine are distributed yearly, and in addition to this a large supply is furnished to hospitals, etc. Efforts are now being made to extend the cultivation of quinine in Malaya and Burma. Substitutes for Cinchona as a source of quinine were used in the Cameroons by the Germans during the War.

Other interesting plants are Efwatakala grass, *Melinis minutiflora*, which is reported to be obnoxious to the tsetse fly; citronella grass, the source of lemon grass oil, which is repellent to mosquitoes; and *Ocimum viride*, the Basil plant, which at one time was thought to be repellent to mosquitoes, but now is known to be of no value for the purpose.

In connexion with the preservation of the health of man in temperate climates, the plants yielding india-rubber are of first importance. The principal source of india-rubber is *Hevea brasiliensis* (Para rubber). This again is a native of South America. Seeds and young plants have been distributed to the various tropical colonies of the British Empire since the year 1873, when it was brought over from South America, mainly owing to the successful efforts of Sir Henry Wickham. Other sources of rubber are the Ceara rubber, *Ficus elastica*, Funtumia, and the tropical African Landolphias. The rubber now used commercially is mainly derived from plantations in the East, to which region it was introduced through the Royal Botanic Gardens, Kew.

Another source of protection against damp and cold is furnished by the various plants which yield tannin, the preservative of leather. The principal sources of this are oak galls and bark, mangrove bark, Myrobalans, Quebracho, and *Acacia decurrens*. Extensive plantations of the latter plant, which is a native of Australia, have been made in South Africa and are a source of considerable wealth to Natal.

Cinnamomum Camphora, which is a native of Formosa, is the source of camphor, valuable as a drug and also a preservative of clothing against moth. Trade in camphor is a monopoly of the Japanese, but seeds have been freely introduced to British colonies, largely through the agency of Kew. It is now found there are two forms of camphor, but only one of these yields the solid camphor which is of value.

Erythroxylon Coca is the source of cocaine, the alkaloid which has so many useful as well as harmful effects.

[1] From a Chadwick public lecture delivered by Dr. A. W. Hill, F.R.S., at the Chelsea Physic Garden, on June 13.

The Liverpool Meeting of the British Association.

THE following Dominion and foreign representatives are expected to be present at the Liverpool meeting of the British Association which begins on Wednesday next, September 12. In the programmes of the various Sections, published in last week's issue, announcement was made of papers to be read by these visitors and of discussions in which they will take part.

Prof. F. D. Adams, McGill University, Montreal.
Prof. W. D. Bancroft, Cornell University, Ithaca.
Prof. N. Bohr, Institut for Teoretisk Fysik, Copenhagen.
Mr. S. C. Brooks, Hygienic Laboratory, Washington.
Dr. Herbert Bruce, University of Toronto.
Prof. A. H. R. Buller, University of Manitoba, Winnipeg.
Senatore Principe G. Conti, Florence.
Dr. D. Coster, Copenhagen.
Prof. P. Ehrenfest, University of Leyden.
Prof. E. Ekwall, University of Lund.
Prof. A. S. Eve, McGill University, Montreal.
Dr. K. G. Falk, New York.
Prof. J. C. Fields, University of Toronto.
Prof. V. M. Goldschmidt, Universitetets Mineralogisk Institut, Kristiania.
Prof. V. E. Henderson, University of Toronto.
Dr. G. Hevesy, Copenhagen.
Prof. D. R. Hoagland, University of California.
Prof. O. Jespersen, Copenhagen.
Prof. A. E. Kennelly, Massachusetts Institute of Technology, Cambridge, Mass.
Dr. P. L. Kramp, Zoological Museum, Copenhagen.
Dr. A. C. Kruyt, University of Utrecht.
Prof. P. Langevin, Collège de France, Paris.
Dr. V. Lebfelter, Folksgesundheitamt, Vienna.
Prof. F. S. Lee, Columbia University, New York.
Prof. G. N. Lewis, University of California.
Prof. A. B. Macallum, McGill University, Montreal.
Prof. J. C. McLennan, University of Toronto.
Prof. J. J. R. Macleod, University of Toronto.
Prof. R. Magnus, University of Utrecht.
Prof. A. P. Mathews, University of Cincinnati.
Prof. E. Merritt, Cornell University, Ithaca, New York.
Prof. A. R. Moore, Rutgers College, New Brunswick, N.J.
Dr. Th. Mortensen, Universitetets Zoologiska Museum, Copenhagen.
Prof. W. A. Noyes, University of Illinois.
Prof. Sven Oden, Kgl. Tekniska Hogskolan, Stockholm.
Prof. W. A. Parkes, University of Toronto.
Prof. M. I. Pupin, Columbia University, New York.
Prof. H. M. Quanjer, Institut voor Phytopathologie, Wageningen, Holland.
Prof. Roule, Musée d'Histoire Naturelle, Paris.
Prof. R. L. Sackett, State College, Pennsylvania.
Prof. J. Satterly, University of Toronto.
Dr. Johs. Schmidt, Carlsberg Laboratorium, Copenhagen.
Prof. J. Sebelien, Aas, Norway.
Prof. H. B. Speakman, University of Toronto.
Dr. V. Stefansson, Canada.
Prof. J. Tate, McGill University, Montreal.
Prof. W. Vernadsky, Paris.
Senatore Prof. V. Volterra, University of Rome.
Dr. G. S. Whitby, McGill University, Montreal.
Prof. A. Willey, McGill University, Montreal.
Prof. R. W. Wood, Johns Hopkins University, Baltimore.
Prof. H. Zwaardemaker, Universitas Rheno-Traiectina, Utrecht.

Relativity and Theory of Knowledge.

THE *Scandinavian Scientific Review*[1]—a new quarterly in English published in Norway—contains in its first number an original and important piece of philosophical research in an article entitled "The Theory of Relativity and its Bearing upon Epistemology," by Prof. Harald K. Schjelderup, the recently appointed professor of philosophy in the University of Christiania. The author is already distinguished in his own country, although he is probably the youngest occupant of a chair of philosophy, having been born in 1895.

The article begins with a lucid exposition of the principle of relativity which calls for no special remark, but it proceeds to examine the consequence of its acceptance in physics for theory of knowledge. It is obvious that it must make a clean sweep of all naïvely realistic theories, materialistic or spiritualistic, which assume the physical reality of the universe to be presented objectively to the mind of the observer for his discernment by means of sense discrimination. But does it accord with idealism? Does it deny that there is any objective universe to which knowledge can attain? Does it require us to be content with the subjective space-time universes of individual observers? Prof. Schjelderup answers emphatically, No. Relativity gives us not a relative but an absolute universe, a universe the scientific reality of which, however, is completely different in its nature from anything which men of science have hitherto imagined or thought it necessary to assume. The Minkowski four-dimensional space-time universe is absolute, in precisely the same sense in which Newton's three-dimensional space and independent variable time were absolute, and the world-lines of the Minkowski universe with their intersecting points determined by Gaussian co-ordinates are real in the objective sense, but the reality is not sense-presented, it is unimaginable and imperceptible. It consists, like the reality of Pythagoras, of numbers.

The point of special interest in the argument is the way in which the author brings out the deciding influence in physical theory which the epistemological weakness of the older mechanics has had. It was Galileo, the founder of modern physics, who, in his discrimination between what he called the accidental and the essential attributes of things, first suggested the distinction between secondary and primary qualities, which has played a determining part in later theories of knowledge. Galileo found his interpreter in Descartes, who reduced physical reality to extension and movement. The principle of relativity has eliminated even the primary qualities from the subject-matter of physics.

Similarly in the relation of Kant to Newton, we see the directive force of the epistemological weakness of a physical theory. The subjectivity of time and space in the Kantian theory meant their transcendental ideality. Abstracted from the subjective conditions of sensory observation they are invalid. But relativity goes further, it eliminates time and space not only from an unknowable thing-in-itself, but even from the subject-matter of physics. To us to-day the principle of relativity is not a return to older philosophical concepts, but a forward movement looking for a new philosopher to interpret a new epistemology.

[1] *Scandinavian Scientific Review*: Contributions to Philosophy, Psychology and the Science of Education by Northern Scientists. Vol. I., No. 1, September. Pp. 136. (Kristiania: Scandinavian Scientific Press A/S, 1922).

Pan-Pacific Science Congress, Australia, 1923.

WHILE not on so extensive a scale as, nor with the Imperial significance of, the Australian meeting in 1914 of the British Association for the Advancement of Science, the second triennial Pan-Pacific Science Congress, which has just met in Melbourne and afterwards in Sydney, may mean very much to the development of organised knowledge of, and in, countries bordering upon the Pacific Ocean. The first gathering of the kind was held in Honolulu in 1920, and as a matter of fact it was really the sequel to ideas that originated during the British Association visit to Australia and later were warmly fostered by Prof. W. M. Davis (Harvard), Prof. H. E. Gregory (Yale), Dr. T. Wayland Vaughan (U.S. Geological Survey), Mr. A. H. Ford, and others. The Pan-Pacific Union, a wide organisation with the general aim of promoting harmonious relations between the peoples of the Pacific, stood behind the Honolulu Congress, but future Science Congresses will undoubtedly all be under the general direction and control of the National Research Councils of the countries concerned.

The Commonwealth Government is acting as host for the 1923 gathering, the organisation being in the hands of the Australian National Research Council, of which Sir David Orme Masson is president. State Governments are generously supplementing the Commonwealth's financial and other assistance, and it has been possible in many cases to make grants helping to defray travelling costs for delegates from distant countries. The prevailing high rates for steamship travelling are a grave difficulty in the way of international assemblies in a region of such vast distances as the Pacific. Happily the interest of the Governments of the chief countries concerned has been aroused, and invitations, conveyed through the Colonial Office, to send official delegates, have met with much response. Unfortunately the South American Republics, with few exceptions, have regretted that their financial conditions do not permit the sending of official representatives. Even more unfortunate is it that France has not seen fit to send a delegation. Nevertheless, with eleven visitors from Great Britain, nineteen from the United States of America, three from Canada, eight from Hawaii, twelve from Japan and Formosa, nine from the Philippines, six from the Netherlands and the Dutch East Indies, eleven from New Zealand, and smaller delegations from British Malaya, Burma, Tahiti, Papua, Fiji, and Hong Kong, a very fairly representative gathering is assured. While in Australia, all visitors from overseas are the guests of private citizens or institutions and are receiving the privilege of free railway travelling before, during, and after the Congress.

To transfer a congress after ten days in one city to another some six hundred miles distant must militate against consecutive work and lead to a certain amount of overlapping; but the advantages in enabling visitors to see more of the country, and in increasing the numbers of local workers who come into personal contact with them, more than counterbalance the obvious disadvantages.

Needless to say, an extensive series of excursions has been arranged, the principal excursions, over long distances, necessarily coming after the official business in Sydney has been concluded. Visits to Broken Hill, Irrigation Areas, Artesian Water Areas, Great Barrier Reef, Northern Rivers to Brisbane, Canberra and other parts of the Commonwealth, are proposed.

The scientific work is being carried on in eleven Sections. As, however, it has been a deliberate object of the organisers to avoid a multiplicity of papers on single and more or less isolated topics, and to aim instead at broad general discussions, there are several joint meetings between Sections. The Sections comprise: I. Agriculture; II. Anthropology and Ethnology; III. Botany; IV. Entomology; V. Forestry; VI. Geodesy, Geophysics, Radiotelegraphy, etc.; VII. Geography and Oceanography; VIII. Geology; IX. Hygiene; X. Veterinary Science; and XI. Zoology.

The agriculturists are concerned chiefly with the problems presented by diseases in wheat and other cereals, sugar-cane, cotton, tobacco, bananas, etc., and on the serious difficulties to be faced in controlling weed pests. Proposals for plant quarantine regulations may represent an immediate practical outcome. Agricultural education and research, soil surveys, and irrigation questions are also being discussed, while much interest is being taken in a joint discussion with the zoologists and veterinarians upon genetics, with special reference to the improvement of farm animals.

In anthropology and ethnology the Congress is attacking the fundamental problem of how best to organise and carry out research work in the Pacific Islands before it is too late. The matter is very urgent indeed. Expressions of opinion have been invited from leading ethnologists in Great Britain who cannot be present in person, and it is hoped that, so far at least as the British islands are concerned, a practical working scheme may be evolved, to be submitted later, with the full weight of the Congress behind it, to the Commonwealth Government. Sir Baldwin Spencer, who has just returned from yet another visit to the interior, is bringing forward the allied, yet distinct, question of future research in regard to the Australian aborigines. Another wide topic under consideration, in common with the Hygiene Section, is the recent rapid decline in native population in the islands, while there are also discussions upon the physical anthropology of various Pacific types, and the race relations between them.

Botany, entomology, and forestry have much in common in several proposed discussions upon timbers, and with zoology the matter of introduced pests and their natural enemies is being taken up, especially the increasingly serious problem of checking the spread of tropical boring insects.

The physical work of the Congress centres mainly round geodesy, terrestrial magnetism, meteorology, and seismology, while the highly practical international matters of radiotelegraphic communications and determinations of longitude by wireless, are also being discussed. Solar physics research, for which many maintain that more is being claimed on the purely practical side than it will yield, and the need for its endowment by Governments, is a subject for vigorous debate.

Those members concerned with geography and oceanography are meeting with the physicists frequently, especially when discussing questions of cartography and meteorology. Definite proposals are being made for continuing and extending, by local effort, the invaluable hydrographic work of the Royal Navy, and for international collaboration in oceanographic work.

As might be expected, the largest Section is that devoted to Geology. The structure of the Pacific Basin, Post-mesozoic volcanic action in the Pacific, ore provinces, correlation of Kainozoic formations, coral reef formations, glaciation, Carboniferous and Permian problems in the Pacific Region, are among the more general matters before the Section.

Two main subjects discussed in the Hygiene Section,

at Melbourne, are mining hygiene and a general survey of the hygiene of the Pacific Region. The basis for discussion of the latter is a summary of replies received by the director of the Commonwealth Department of Health to a widely circulated *questionnaire* relating to yellow fever, malaria and filariasis, bubonic plague, small-pox, leprosy, beri-beri, hook-worm disease, and tuberculosis. In Sydney, the principal topics are climate in relation to human efficiency, meteorological standards in relation to comfort, and insects in respect to hygiene.

The work of the Veterinary Science Section is mainly in joint meetings with allied Sections, such as Agriculture and Zoology, in dealing with parasitological and other problems. Proposals are being put forward with regard to international notification of animal diseases.

Finally, the Section of Zoology is undertaking, in addition to much conjoint work with other Sections, a general survey of the many questions now arising in connexion with Pacific fisheries and the establishment of marine biological stations.

The main aim of the Congress is to deal with wide subjects, many of them of international significance, from a practical as well as a purely scientific point of view. A. C. D. RIVETT.

University and Educational Intelligence.

WE learn from the *Chemiker Zeitung* of the following appointments: Dr. W. Schumann, director of the Institute of Technical Physics at Jena University, to be professor of theoretical electrotechnics at the Munich Technical College; Dr. Julius Schmidt, of the Stuttgart Technical College, to be reader in chemistry at the Engineering College, Esslingen; and Dr. K. Fajans, to be assistant professor of physical chemistry at the University of Munich.

THE trustees of the Laura Spelman Rockefeller Memorial, founded in October 1918 by John D. Rockefeller in memory of his wife, have published a report on their appropriations, amounting to nearly 13 million dollars, up to December 31, 1922, on which date the corporation's assets amounted to 78 million dollars. Grants classified under the head "Education" amounted, in the four years 1919–1922, to 6000, 9000, 286,000, and 500,222 dollars respectively, and included 30,000 in 1921 for the American College for Girls at Constantinople, 110,530 dollars in 1922 for Robert College of Constantinople, the American University of Beirut, and the Constantinople Women's College, and 600,000 dollars for the Women's Union Christian Colleges in the Orient. For boy scouts and girl scouts grants amounting to 193,000 dollars were allocated, and an appropriation which will amount to more than 55,000 dollars was made for the inauguration of courses of instruction for scout leaders in universities and women's colleges. Such courses, it is noted, are given in 42 institutions, and in 13 of them the expense of instruction has already been taken over by the college. Scientific research interests the trustees because they "believe that knowledge and understanding of the natural forces that are manifested in the behaviour of people and of things will result concretely in the improvement of conditions of life," but grants for promoting it have hitherto been small: 13,000 dollars in 1921 and 37,500 in 1922, including 10,000 for the Mme. Curie Radium Fund. The Y.M.C.A. and Y.W.C.A. and other social welfare organisations received 3,299,000 dollars; religious organisations, 1,975,000; emergency relief, 1,543,000; and public health, 692,000.

A REPORT on the development of higher education in Poland has been issued by the Chief Statistical Office of the Polish Republic. For the five State universities the report shows the following student enrolments:

	Cracow.	Warsaw.	Lwów.	Poznań.	Wilno.	Total.
1920–21 . .	4136	5787	3639	2094	788	16444
1921–22 . .	4531	7518	4773	3273	1729	21824
1922–23 . .	5235	8939	5646	3416	2202	25438

For the technical State schools the respective numbers are:

	T.H. Sch., Warsaw.	T.H. Sch., Lwów.	Agric. Coll., Warsaw.	Sch. of Min., Cracow.	Total.
1920–21 . .	2931	2178	787	179	6075
1921–22 . .	4112	2305	761	282	7460
1922–23 . .	3868	2560	906	462	7796

The following figures show the number of students admitted in 1922–23 to other higher schools and professional colleges: Independent University, Lublin, 1120; Free Polish University, Warsaw, 1664; College of Commerce and Economics, Warsaw, 988; Veterinary College, Lwów, 327; Teachers' College, Warsaw, 124; School of Fine Arts, Cracow, 155. Of the total number of students, about 24 per cent. were women. Nearly 27 per cent. were enrolled in faculties of jurisprudence, 13 per cent. in faculties of medicine, 17 per cent. were engaged in the study of technology, mechanical and electrical engineering, etc., about 6 per cent. were students of agriculture, and 30 per cent. devoted themselves to the study of philology, history, mathematical and natural science, philosophy, and education.

LISTS of colleges and universities "accredited" by various agencies are published in Bulletin, 1922, No. 30, of the United States Bureau of Education. The standardising movement has advanced rapidly during the past ten years, and the lists published in 1917 already need revision. The agencies in question are: certain State universities and departments of education, the Carnegie Foundation for the Advancement of Teaching, the Association of American Universities and several other voluntary educational associations, and church boards of education. The Bureau is careful to announce in large type that "there is no comprehensive classification of collegiate institutions by any national governmental agency." The longest of the lists is that drawn up by the University of California of 286 institutions from which holders of bachelor degrees representing the usual college course of four years will be admitted to its own graduate division. Commenting on the lists, the compiler notes that the standards used are very various and the basis of classification in some cases is very vague, while "there is no practical consensus of opinion as to what constitutes that much-talked-of entity, the standard college." He finds ground for hope of a coming approximation to uniformity in this regard in the fact that a committee appointed for the purpose by the American Council on Education has formulated certain principles and standards for 4-year colleges and universities which have been adopted in whole or in part by some of the accrediting agencies. Among these principles are: "Teaching schedules exceeding 16 hours per week per instructor, or classes (exclusive of lectures) of more than 30 students, should be interpreted as endangering educational efficiency"; and "the minimum annual operating income, exclusive of payment of interest, annuities, etc., should be $50,000, of which not less than $25,000 should be derived from stable sources, other than students, preferably from permanent endowments."

Societies and Academies.

PARIS.

Academy of Sciences, August 6.—M. Guillaume Bigourdan in the chair.—A. **Lacroix**: Comparison of the chemical composition of two Iceland lavas, characterising eruptions of which the kind of dynamism is different. Analyses of five lavas and basalts from Katla and Hecla. As regards the distinction between quietly flowing and explosive eruptions, the author holds that the fluidity of the magma is not the only explanation of the different types of eruption, since a fluid magma, which according to Washington, should flow out quietly, if suddenly cooled on its egress into the air, may give rise to an explosive eruption. The eruption of Hecla is an example of this: the fluid lava had to force its way through the ice cap of the Myrdalsjökull glacier and the eruption throughout was of the explosive type.—G. **Bigourdan**: Project of a new catalogue of the French learned societies.—André **Blondel**: A rational method for tests and specification of triode lamps intended to work as valves. An outline of tests to be made partly at the works where the lamp is constructed, and partly at the laboratory where the lamp is to be used.—Charles **Nicolle**, E. **Conseil** and A. **Cuénod**: Preventive vaccination against acute conjunctivitis due to the Weeks bacillus. Its importance in the campaign against trachoma. Details are given of the preparation of the vaccine and of the results of experiments demonstrating the protective action of the vaccination.—Nilos **Sakellariou**: Oblique linear curvature and total geodesic curvature.—F. H. **van den Dungen**: Some technical applications of integral equations.—Rolf **Nevanlinna**: The theorem of M. Picard.—R. **de Fleury**: Elastic stability and modern materials of construction.—A. **Grumbach**: The superposition of electromotive forces in batteries with a fluorescent liquid.—R. **Levaillant**: Fluorescence and photochemistry. A certain number of fluorescent colouring matters (uranine, methylene blue, eosin, erythrosine) dissolved in glycerol or other polyalcohol, when submitted to light in the absence of air change colour, owing to hydrogenation by the alcohol. The original colour is more or less completely restored by the action of air.—G. **Vavon** and S. **Kleiner**: Catalytic hydrogenation and steric hindrance. The study of some heptenes. The addition of hydrogen to four isomeric heptenes (ethylpropylethylene, dimethylisopropylethylene, methyldiethylethylene and trimethylethylethylene) was studied in the presence of platinum black. It was found, in agreement with the theory of steric hindrance, that the hydrogenation was more difficult the greater the number of substituting radicles.—V. **Agafonoff**: The comparative study of some methods of chemical analysis of the humus in soils. A comparison of the amounts of carbon in soil determined by combustion, by the ordinary sulphochromic process, and by Simon's method (with silver bichromate). The dry combustion and Simon's method are in good agreement: the ordinary wet combustion with sulphuric and chromic acids only gives low results.—Pierre **Lesage**: Anomalies of the fruit of *Capsella Bursa-pastoris*, caused by the presence of salt in the soil.—L. **Blaringhem**: The biological control of the influence of manures: determination of the sensible periods.—Robert **Stumper**: The chemical composition of the nests of *Apicotermes occultus*. The nests are made of sand, cemented together with about 15 per cent. of organic secretion.—J. **Benoit**: The origin of the interstitial cells in the testicle of the domestic cock.—Et. **Burnet**: Irregular reactions of the filtrate from broth culture in goats infected with *Micrococcus melitensis*. If the infection by this Micrococcus renders the goat as sensitive as man to the inoculation of a small quantity of filtered culture, this reaction should afford a rapid and certain means of recognising infected goats and preventing the use of their milk. It has been proved, however, that the reaction is very irregular, and some goats, certainly infected, do not show the reaction at all. The conclusion is drawn that the filtrate creation cannot be used in practice as a means of diagnosing *Melitensis* in the goat.

August 13.—M. Guillaume Bigourdan in the chair.—A. **Lacroix**: The signification of the alkaline granites very rich in soda. The study of the rocks collected from the island of Rockhall has shown that rockallite, described as a persodic granite, has no real geological existence. Chemical analyses of various portions of the granite and its enclosures are given.—Torsten **Carleman**: Functions indefinitely derivable.—Jules **Baillaud**: Studies on the distribution of the energy in stellar spectra made at the Pic du Midi Observatory in 1920 and 1921. The spectra of nine stars have been studied. The arrangement of apparatus and method of carrying out the observations are described: the details of the experiments and the results will be published elsewhere.—R. **de Mallemann**: The theory of rotatory polarisation.—J. **Bathellier**: Correction relating to the nests of Eutermes. In a preceding note a series of fungus beds forming part of an ant nest have been described as belonging to *Eutermes matangensis*. It would appear that this view was incorrect: the structures are probably the work of an insect determined by M. Bugnion as *Microtermes incertus*.—E. **Bugnion**: Remarks on the note of M. Bathellier.—O. **Duboscq** and H. **Harant**: Sporozoa of the Tunicates.

Official Publications Received.

Egyptian Government. Almanac for the Year 1923. Pp. viii+256. (Cairo: Government Press.) P.T. 10.

Calendar of State Papers, Colonial Series, America and West Indies, June 1708–1709, preserved in the Public Record Office. Edited by C. Headlam. Pp. xliii+642. (London: H.M.S.O.) 40s. net.

Annual Report of the Meteorological Observatory of the Government-General of Chosen for the Year 1920 (Results of Observations). Pp. iii+141+1 map. (Zinsen.)

Results of the Meteorological Observations in Korea for the Lustrum 1916–1920. Pp. vii+48. (Zinsen).

N.S.W. Department of Mines. Geological Survey. Bulletin No. 3: Copper. By E. J. Kenny. Pp. 51. (Sydney: A. J. Kent.)

Annual Report of the Board of Regents of the Smithsonian Institution for the Year ending June 30, 1921. Pp. xii+638. (Washington: Government Printing Office.)

Smithsonian Institution. U.S. National Museum. Bulletin 120: The Opalinid Ciliate Infusorians. By M. M. Metcalf. Pp. viii+484. (Washington: Government Printing Office.)

Thirty-seventh Annual Report of the Bureau of American Ethnology to the Secretary of the Smithsonian Institution, 1915–1916. Pp. viii+560. (Washington: Government Printing Office.)

Aeronautics. Report of the Aeronautical Research Committee for the Year 1922–23. Pp. 48. (London: H.M.S.O.) 2s. net.

Fishery Board for Scotland. Salmon Fisheries, 1921, No. IV. Salmon Investigations in Scotland, 1921, IV. Summary of Results (with 7 diagrams). By W. J. M. Menzies, Jun. Pp. 18. (Edinburgh: H.M.S.O.) 1s. 6d. net.

Air Ministry. Meteorological Office. Geophysical Memoirs, No. 21. Pyrheliometer Comparisons at Kew Observatory, Richmond, and their bearing on data published in the *Geophysical Journal*. By R. E. Watson. Pp. 17. (London: H.M.S.O.) 2s. net.