THURSDAY, OCTOBER 5, 1899.

BERTHELOT'S AGRICULTURAL CHEMISTRY.

Chimie végétale et agricole. Par M. Berthelot. Four volumes. Pp. xvi + 511, vi + 441, vi + 517, vi + 528. (Paris: Masson, 1899.)

THE ancient Château de Meudon, Seine-et-Oise, which was left in ruins at the end of the war of 1870, was thirteen years later converted into an agricultural experiment station by the French Government, and permanently attached to the Professorship of Organic Chemistry in the Collège de France. In the four bulky volumes now before us, the professor, M. Berthelot, has brought together an account of the various investigations carried out at the station between 1883 and 1899, under his direction, with the assistance in many cases of M. G. André. Besides these reports, the volumes contain an account of several earlier investigations by M. Berthelot. We have in fact brought before us the whole of his investigations on plants, soils and various cognate matters, carried out during the last forty years.

Agricultural chemists will heartily welcome the publication in such a convenient form of this great mass of original investigation. M. Berthelot is well known as a first-class man of science, and as one of the most prolific and versatile workers of the present age. The new ideas he has brought forward concerning many obscure points in agricultural chemistry will be highly valued. Nevertheless, those acquainted with the peculiarities of M. Berthelot's work will not be surprised that a cautious critic is unable always to accept the conclusions to which he has apparently too easily arrived. The more startling and novel are the conclusions brought before us, the more thorough and unmistakable ought surely to be the basis of fact on which these conclusions are built. A few experiments, relating to only a part of the facts in question, must fail to carry conviction when new laws are propounded, or we are asked to surrender as a mistake views previously arrived at after much patient research.

It will be gathered from what has just been said that the papers in the volumes before us are of unequal value. All the investigations are indeed highly suggestive, and no experimental investigator would desire one of them to be omitted; but students of agricultural chemistry will not unfrequently find it advisable to examine with much care the evidence brought forward before accepting all the conclusions of the author.

That we may do no injustice to the book we will, in the first place, call attention to the very valuable investigation upon the nature and properties of humus, which occupies more than one hundred pages in the fourth volume. The elucidation of the chemical nature of humus has been regarded as an almost hopeless problem by the ordinary agricultural chemist. Berthelot has brought to bear upon the subject the methods and conceptions of modern organic chemistry, and his work has resulted in a considerable increase to our knowledge.

Berthelot has carefully studied the composition and properties of the simple nitrogen-free humus obtained by boiling sugar with hydrochloric acid. It appears to be a mixture of a condensed anhydride and hydrate, the simplest expression for the former being $C_{18}H_{14}O_6$. It swells up in water, forming a colloid body. It absorbs a considerable quantity of alkali from an aqueous solution. One third of the potash or soda thus absorbed is permanently retained in a practically insoluble condition after long washing with water. Placed in contact with ammonia an insoluble amido-compound is produced, from which ammonia is not recovered by boiling with magnesia. The oxidation of humus under the influence of light, and its more rapid oxidation in the presence of alkali are also studied. The heat relations of the principal reactions have also been ascertained. All this is fundamental work of very great importance, and throws much light upon the behaviour and functions of humus in a soil.

The natural humus in soil is also studied, and the action of acids and alkalis upon it investigated. The gradual formation of ammonia when the nitrogenous humus of soils is boiled with weak acids, soluble nitrogenous compounds being simultaneously produced, is pointed out as in full agreement with the assumed amido nature of the humic matter. The humus of soils is, however, a very complex substance; it may contain a very distinct amount of sulphur, and even phosphorus, in a state of organic combination. It will certainly be a novel fact for most agricultural chemists to hear that a soil may yield 0·183 per cent. of phosphoric acid when boiled with strong hydrochloric acid, 0·222 per cent. when the silica has been entirely removed by hydrofluoric acid and 0·292 per cent. when the soil is burnt in oxygen gas and the products retained by sodium carbonate. The excess obtained by combustion in oxygen is regarded by Berthelot as representing the phosphorus in organic combination. This part of the subject clearly requires much further investigation. Phosphorus, if present, is possibly a survival of the nuclein occurring both in the animal and vegetable kingdom.

We take our next example from one of the less satisfactory of M. Berthelot's investigations, in which the evidence brought forward seems quite insufficient to warrant the conclusions which he seeks to establish.

He has determined the quantity of nitrates present in certain plants, and has conceived the idea that plants have the power of producing nitrates abundantly in their own tissues. This assumption, if proved, would clearly furnish an entirely new departure in vegetable physiology One would have thought that to establish such an hypothesis the plant would have been grown in a medium supplying no nitrates; any appearing in the plant would then clearly be due to the work of the plant itself. M. Berthelot makes no such experiment. To establish his position, he grows the plant (borage or *Amaranthus*) in the open field, without any knowledge of the quantity of nitrates produced in the soil during the season of growth, and without taking into account the upward movement of subsoil water containing nitrates during the dry summer of his experiment. He is satisfied by ascertaining that on September 25 a square foot of soil contained only about 1/20 of the quantity of nitrate contained in the plant pulled up from it, and that a similar bulk of soil taken at the beginning of the season, from another part of the field, contained a similar quantity of nitrate to that found in the exhausted soil around the plant at the end of the season. The next year he finds that the soil of the field, when deprived of vegetation, doubled its contents in nitrates between June 4 and "the end of the season"; but this rate of increase was insufficient to account for the nitrates found in the crop the *previous year!* Finally, to prove that the plant contains a nitrifying agent, a single experiment is made by introducing a fragment of the stem of *Amaranthus* into a flask containing 300 grams of sterilised and exhausted soil. At the end of eleven weeks six milligrams of saltpetre were found in the soil. A blank experiment, made with soil only, was for some reason only continued for six weeks.

Data such as these are quite insufficient to convince a critical reader. Our confidence in the investigation is not increased by reading that the growth of a *single* crop in the field diminished the nitrogen in the soil from ·275 to ·173 per cent., and the potash of the soil in the neighbourhood of the roots from ·64 to ·47 per cent. Nor by remarking that the same figures for nitrates in the soil are first quoted as kilograms, and are afterwards always spoken of as grams.

The whole of the first volume is occupied with an account of investigations on the fixation of atmospheric nitrogen by soil and plants. M. Berthelot has been a pioneer in this branch of inquiry. The peculiar function of the organism forming the nodules on the roots of leguminous plants is now universally recognised. A similar case of symbiosis between a nitrogen-assimilating organism and certain algæ is also well known. Not so well known is the isolation of a bacillus from the soil by Winogradsky, which when supplied with sugar, and protected from the action of oxygen, is capable of assimilating atmospheric nitrogen. This organism succeeds in assimilating nitrogen from ordinary air when it is associated with ærobic organisms which appropriate the oxygen, and thus produce conditions suitable for the growth of the bacillus assimilating nitrogen.

Both in the case of the reaction in the leguminous rootlets and algæ, and in the case of the reaction *in vitro*, studied by Winogradsky, we have a clear indication of the source of the chemical energy which accomplishes the difficult task of bringing nitrogen into a state of organic combination; in every case we have carbohydrates abundantly present, and in Winogradsky's experiments we have a demonstration that the quantity of sugar fermented is a measure of the quantity of gaseous nitrogen assimilated.

With this principle before us we should suppose that a soil entirely destitute of vegetation could fix nitrogen only at the expense of its own organic matter; carbon would, in fact, be lost in the operation of fixing nitrogen. If, on the other hand, certain green algæ or leguminous plants were present, fixation of nitrogen might be accompanied by an actual gain of organic matter.

According to Berthelot's experiments, soils destitute of visible vegetation may gain large quantities of nitrogen when exposed to air. Even subsoils of argillaceous sand or clay, containing mere traces of carbon or nitrogen, are capable of gaining considerably in nitrogen when exposed to air. From an agricultural point of view, the quantities of nitrogen fixed are very considerable. Layers, 7 inches deep, of three surface soils from Meudon, fixed in 11 weeks from 70 lbs. to 130 lbs. of nitrogen per acre, quantities equivalent to 6–11 tons of farmyard manure. If this enrichment of soil by mere exposure to air is a fact, we shall be very anxious to know what are the precise conditions and limitations of such a beneficial action. Scientific agriculturists will be loath to admit that the exposure of a soil uncovered by vegetation tends to its permanent enrichment; the process of weathering tends, on the contrary, to the exhaustion of soil capital, and not to an increase of nitrogenous organic matter.

Berthelot's trials of various organisms yielded results of a similar favourable character. Out of seven organisms tried five produced an active fixation of nitrogen. The composition of the medium was apparently indifferent, for a mixture of certain bacilli from soil with kaolin determined an increase of 32 per cent. of the original nitrogen in one case, and an increase of 150 per cent. in another. Among the organisms fixing nitrogen, Berthelot includes the common mould *Aspergillus niger*.

In the last section of this volume Berthelot describes experiments which lead him to the conclusion that the natural electrical conditions, both of soil and plant, aid in bringing about the fixation of nitrogen from the air.

It is to be regretted that the large amount of work contained in these volumes is not of a more thorough and definite character, but we are very thankful that the investigations have been published. R. W.

OUR BOOK SHELF.

Bird Life in an Arctic Spring; the Diaries of Dan Meinertzhagen and R. P. Hornby. Edited by Mrs. G. Meinertzhagen. Pp. iii + 150. Illustrated. (London: Porter, 1899.)

A PATHETIC interest attaches to this volume, as being practically a memorial to a most promising and talented young ornithologist, whose life was unhappily cut short almost at the outset of his career. The late Mr. D. Meinertzhagen was essentially a lover of bird-life, and thus a naturalist in the very best sense of that somewhat abused word. But he was much more than this, being an artist of great talent, whose sketches and etchings of birds form some of the most beautiful delineations of feathered life it has been our fortune to see. In addition to those illustrating the text itself, nearly thirty of these talented sketches have been photographically reproduced as an appendix to the present volume, and serve not only to enhance the general interest of the latter, but likewise to convey an excellent idea of the artistic capacity of the author of the journal which constitutes its main claim to attention.

As we gather from the preface, the book is mainly intended for private circulation, and only a limited number of copies are offered to the general public. On the whole, the editor has exercised a wise discretion in endeavouring to preserve the journal of her son as much as possible in its original form, although it must be confessed that a little fuller supervision on the part of a trained ornithologist than has been permitted would have been advantageous in a few instances.

The journal is divided into two portions, the first and longer by Mr. Meinertzhagen, and the second by his companion Mr. Hornby. The trip to Lapland, of which these form the chronicle, was undertaken in 1897; and the journal of the originator breathes out the enthusiasm of an ardent bird-lover. The two companions appear to have visited spots to which few if any Englishmen have penetrated since Wolley's time; and in collecting eggs they suffered almost from *embarass des richesses* on account of the numbers that were brought in by the natives. One of the objects of their desire was to obtain a clutch of Smew's eggs, but it was not an easy matter to identify these without some of the down from the nest. At length they succeeded in obtaining what they thought were the right eggs; and their acumen was confirmed on arrival in England by the identification of the specimens from the down.

A section of the volume is also devoted to an account of the magnificent collection of raptorial birds maintained by the late author of the first journal at his father's seat, Mottisfont Abbey, Romsey. This collection, which is stated to be one of the finest in England, is still maintained; and the account shows how it is possible to keep such splendid birds in perfect condition. Altogether, the bird-lover will find much to interest him in this charming little volume. R. L.

Progressive Lessons in Science. By A. Abbott, M.A., and Arthur Key, M.A. Pp. xix + 320. (London: Blackie and Son, Ltd., 1899.)

THIS book consists of two parts—the first, by Mr. Abbott, dealing with the non-metallic elements found in animal and vegetable substances; the second, by Mr. Key, on the detection and distribution of the elements in animal, vegetable and mineral substances. The former part contains a course of experimental work in chemistry of a kind with which many text-books have made us familiar. All that need be said of it is that most of the experiments are suitable for performance in the laboratory by beginners in chemistry, and that the book will assist the progress of rational methods of science teaching. With regard to the second part, though the plan has something to commend it, the execution is open to criticism. Mr. T. G. Rooper, who generously endeavours to assist the volume by his introduction, remarks upon the idea to which we refer. "The most original feature in the book is the set of experiments which illustrate the composition of food-stuffs. Starting with a table of the chief constituents of the blood, the author proves the presence of each by the use of an ingeniously-devised test. He then traces each constituent through animal life to the vegetable life on which animal life is supported, and thence to the soil from which the plant derives it, and finally to the rock, by the disintegration of which the soil is formed." There are several grave objections to this method of procedure as it is here presented. Students are *told* the tests which have to be applied to detect different substances, hence the experiments are not in advance of the test-tube practice which is fast giving place to more intelligent practical instruction. Moreover, the object of the experiments is too complicated to be of real educational value to beginners; and, finally, very few students have the time to do so much experimental work. Originality in text-books is a very commendable characteristic, but the authors should know that practicability is an even more important factor to consider. In its present form the book may be of service to a few teachers of domestic science and hygiene, but we do not think any other useful purpose will be served by its publication.

De la Méthode dans la Psychologie des Sentiments. By F. Rauh. Pp. 305. (Paris: Félix Alcan, 1899.)

THIS is a valuable monograph the merit of which is unfortunately partly concealed by a singularly obscure and unattractive literary style. M. Rauh's principal object is to enter a warning against the growing tendency of psychologists to neglect the adequate description of complicated facts, and to corrupt their science in its infancy by excessive reliance upon over-simple metaphysical and psychophysical theories. Psychology, as he well points out, possesses as yet no such simple and universal generalisation as that of the conservation of energy; in the present state of the science any single theoretica generalisation is premature; for the full description of the facts of mental life we need many points of view, each represented by a different tentative hypothesis. Thus the emotions, which form the immediate subject of the essay, may be studied as concomitants of physiological changes in the organism, as embodying a *quasi*-judgment on the part of the organism as to what is beneficial or harmful, as manifestations of the "will to live," or finally as special phenomena calling for independent description and classification. Each of these points of view throws light upon some characteristic of human emotions, and none of them can be neglected in a complete psychology of sentiment. In the course of the argument many one-sided theories, especially that of Prof. James as to the organic concomitants of emotion, receive really trenchant criticism. Like most French writers, M. Rauh is particularly happy in what may be called "psychological diagnosis"; his too rare descriptions of the various emotional "temperaments" are subtle and illuminating. On the other hand, he makes occasional slips which partly vitiate his reasoning. In his deductions from the supposed existence of special "pain-conducting" nerves, for instance, he forgets to allow for the possibility that what the nerve conducts is the special presentative element, the "racking," "stabbing," or "burning" sensation rather than the painfulness of it. Again, he scarcely lays enough stress on the fact that our emotional state at any moment depends, not on isolated sensations, but upon the total complex of our sensations at the moment. And, finally, to the present writer at least, the conception of "psychical forces," of which M. Rauh makes great use, is exceedingly obscure. It is a pity that terminology, which has led to so many confusions, even in dynamics, should be needlessly transported into psychology. A. E. T.

Histoire Abrégée de l'Astronomie. Par Ernest Lebon. Pp. vii + 288. (Paris: Gauthier-Villars, 1899.)

THIS book, as its title implies, is not intended as a complete history of the progress of astronomical science from the earliest day, but is devoted to rendering a brief account of the main steps in this progress, and at the same time giving us short biographical sketches of the chief workers in this branch of science. The subject is divided into three parts. The first deals with the ancient period which ends in the middle of the sixteenth century: only eighteen pages are devoted to this portion, so that the reader can rightly conclude that only a very general sketch has been attempted. The second or modern period, extending to the middle of the nineteenth century, commences with the system of Copernicus, and ends with an account of the state of the science at the time of the death of the illustrious astronomer of the Königsberg Observatory, Friedrich-Wilhelm Bessel. The last, or contemporary, period is contained in 125 pages. M. Lebon divides this portion of the book into ten chapters, dealing in each with the progress made in separate branches of the subject. Thus we find first an account of the advance made in celestial mechanics, then the progress in observational astronomy, spectroscopy, geodesy, photography, &c. Each of these reviews is brought well up to date, and contains a good general survey of the progress made. A useful addition to the book will be found in the biographical and bibliographical dictionary which follows this last portion. Besides a small chart of the northern hemisphere, which apparently has little utility in such a book as this, the illustrations include a set of sixteen processed reproductions of portraits of celebrated astronomers. Not only should astronomical readers find this book a welcome addition to their libraries, but those interested in the welfare of this, the oldest, of sciences, will peruse these pages with advantage.

LETTERS TO THE EDITOR.

The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.*]

The Intake of Carbon Dioxide—a Correction.

WILL you give me the opportunity of making the following correction in my Presidential Address to the Chemical Section of the British Association.

I stated incidentally that Mr. F. F. Blackman, in his well-known experiments on the intake of carbon dioxide into the two sides of an assimilating leaf, employed air enriched with that gas to the extent of 4 per cent. and upwards.

Mr. Blackman has pointed out to me that in the experiments in question he used air containing only from 1·8 to ·33 per cent., and that since the publication of his earlier results he has still further reduced this amount. In fact he is of the opinion that his method is applicable to the measurement of the intake of carbon dioxide of even a much greater degree of dilation.

The error was an inexcusable one on my part, but does not affect the main argument that the natural rate of intake cannot be directly deduced from experiments in which the carbon dioxide content of the air materially departs from the normal amount 0·03 per cent. HORACE T. BROWN.

52 Nevern Square, Kensington, S.W., September.

Geological Time.

IN his Presidential Address to Section C at Dover, Sir A. Geikie has offered a bold challenge to Lord Kelvin and those who agree with him by calling upon them to give due weight to geological phenomena in forming an estimate of geological time. Permit me to say what I think about it.

It seems to me probable that, when the grand idea of the universal dissipation of energy had occurred to Lord Kelvin, he saw that the principle must be applicable to the earth, and that, if the law of conduction of heat could be used, he might from it obtain an estimate of the world's age. He then instituted his important experiments to determine the conductivity of rocks *in situ*, and found the value 400 (more or less), the units being a foot a year, and a degree Fahr. But it was necessary, for his calculation to succeed, that he should assume the earth to be solid. If I do not misjudge him, I think he then sought for arguments to prove this point. Now, I cannot but think that the proofs of solidity on which physicists rely are by no means convincing; and if the earth is not solid, Lord Kelvin's estimates are without foundation. Moreover, it is not sufficient that the earth should be solid at the present, to which these proofs refer, but it needs to have been so from the beginning of the time to which his estimates go back.

Prof. G. Darwin, in his book on the tides (p. 237), has done me the honour of referring to my "Physics of the Earth's Crust" as if I am an arch-heretic on this question of solidity. Whether my arguments are beneath notice, or whether there is a difficulty in answering them, I do not know; but they have never been refuted, while they are held to be of decided force by some geologists, and among these by the Indian Geological Survey.

Harlton, Cambridge, September 25. O. FISHER.

The Terrestrial Gegenschein.

I DO not know whether the phenomenon I am about to describe has ever been noticed. The circumstances under which it is noticeable must occur rarely. They are these:—

I spent some time of the summer of 1898 on an isolated mountain peak, surrounded by lower mountains whose sides were densely wooded. The result was that near the time of sunset the shadow of my own mountain peak was visible on a mountain side which might have been three or four miles distant. One evening I amused myself by watching the shadow of the peak as the sun was descending. My attention was attracted by an illumination in the direction opposite the sun so strikingly resembling the astronomical gegenschein, that at the first glance I saw in it an explanation of the latter. It consisted of a somewhat bright glow, which might be a degree or two in diameter, but which shaded off by such imperceptible gradations that a definite extent could not be assigned. A little study, however, showed an explanation. As I have said, the mountain on which the glow was seen was densely wooded. In such a case the shadows of those leaves and branches which the sun's rays first reached fell upon the interior foliage and obscured it. But an observer looking from the exact direction of the sun will see through the foliage as far as the sun's rays extend. In other words, the visible surface on which he is looking will be entirely illuminated by the sun's rays, whether this surface is formed of the outer strata of foliage or of a strata ever so far inside, which can be seen only through the crevices in the outer stratum. The shaded interior will be entirely invisible to him. But if his point of view is in a direction ever so little oblique, he will see only the outer foliage illuminated, while more or less of the interior foliage which he sees will be in the shadow. Thus the region exactly opposite the sun will be seen in its full brilliancy, while the neighbouring region will be a mixture of light and darkness. At a distance of several miles this compound of light and darkness will be fused into a single half-shade, strongly contrasting with the full brilliant light of the opposite point.

It is clear enough that we cannot have such a state of things as this in the case of the astronomical phenomena. Yet the phenomenon seems to be of sufficient interest to warrant its being placed on record. S. NEWCOMB.

The Cause of Undercurrents.

IN NATURE of August 3, p. 316, is given a letter from Rear-Admiral Sir William Wharton, in which he states that he is diametrically opposed to my opinion about the double currents in the Straits. He says that "Admiral Makaroff considers that difference of density of the water is the primary, and, indeed I gather he thinks, the only cause of these opposing currents; but he brings no evidence beyond theoretical considerations in support of his belief"; further, in his letter, Admiral Wharton refers particularly to the double current of the Bosporus, of which I spoke in my lecture at the Royal Society of Edinburgh. I cannot leave unnoticed remarks from so distinguished a hydrographer, who, during his long work, has contributed so much to the advance of science. My researches about the Bosporus are published only in Russian, in a book named "On exchange of water between Black Sea and Mediterranean" (St. Petersburg, 1885). Should Admiral Wharton know my language, he would easily come to the conclusion that my opinion about double currents in the Bosporus are based upon the observations made in 1881 and 1882. I then invented an instrument for measuring the current at different depths, and gave the name of "fluctometer" to it. The instrument consists of a propeller revolving on a horizontal spindle. A bell is attached to the propeller, and at every revolution of the propeller it strikes twice. As water is a very good conductor of sound, the number of revolutions could be counted through the bottom of the ship (provided the ship is not sheeted with wood) at all depths to which the instrument was lowered (40 fathoms). I used to anchor in the middle of the Bosporus for a couple of days at a time, and make a series of observations every two hours. In order to obtain more detailed data, I used to take the samples of water from the same depth to which the fluctometer was lowered. Twice I used to go along the Bosporus from the Black Sea to the Marmora Sea in order to learn in what depth is the limit of two currents. In volume xxii. of the *Proceedings* of the Royal Society of Edinburgh, Plate I. shows a position of the limit of two currents, mean velocity of both currents, and specific gravity of water. In Plate II. is given a sketch of my "fluctometer."

I am sorry that the limits of this paper do not allow me to give particulars of my observations, but I believe some of my deductions, worked out from direct observations, would be interesting to English readers.

Mean velocity of the upper current, 3¼ feet per second. It varies from 0 to 10 feet per second in certain places.

Velocity of the upper current diminishes with every fathom of depth.

Limit between two currents close to the Marmora Sea is at 11 fathoms. It gradually goes down to 27 fathoms close to the Black Sea. Limit between two currents is influenced by winds and by barometrical pressure, but not very much.

Lower current has close resemblance with the river. Its velocity does not vary very much. We never found anywhere lower current less than 1·84 and more than 3·22 feet per second. Mean velocity of the under current was 2·32.

The upper current does not everywhere occupy the full breadth of the strait. It flows in some places under the north coast of the strait, and in other places under the south coast. Lower current also does not flow over the whole breadth, and occupies a certain part of the bed of the strait. In my book I give a chart of the Bosporus, where I show direction of the upper and lower currents. A glance at that chart will show that there are places where both currents can be found, and there are places where the instrument will show the existence of only one of them, and in some places the explorer will not find either upper direct current or under current.

Difference of level of Black and Marmora Seas, calculated from difference of specific gravity of water, is 1 foot 5 inches. Mean specific gravity of water $\left(S\frac{17·5}{17·5}\right)$ entering from the Black Sea into the Bosporus is 1·0140. Mean specific gravity of water entering from the Marmora into the Bosporus is 1·0283.

By upper current pass 370,000 cubic feet per second. By lower current pass 200,000 feet per second. Difference between these two figures being 170,000 feet per second is due to the excess of fresh water in the Black Sea.

I hope that after reading these figures and deductions Admiral Wharton will change his opinion, and come to the conclusion that my idea about double current in the Bosporus is based, not only on theoretical considerations, but also upon direct measurement.

Admira Wharton expresses also his opinion about double current of the strait of Bab-el-mandeb, and says that "there are none of the differences of specific gravity." I may be permitted to refer to my other book, "Le Vitiaz et l'Ocean Pacifique," where I give (p. 136, plate xxvii.) specific gravity of the strait of Bab-el-Mandeb measured by myself. Examination of figures given there by me shows that there is a difference of specific gravity which produces double current in that strait.

Nobody can deny that the wind has a great influence upon the movement of surface water; but I hope that Admiral Wharton will agree with me that differences of specific gravity has also some influence upon the circulation of water in the seas generally and in the straits particularly. S. MAKAROFF.

Ermack, Newcastle-upon-Tyne, September 23.

Movement of Sea-Gulls with a Coming Change of Weather.

THE suggestion that sea-gulls may have some meteorological sense would be best tested by inquiry as to whether there was, at the time of their westward flights, observed by your recent correspondents, any other possible motive for their journeys; and particularly what food was available in the Channel at the time.

Neither of your correspondents needs to be told that every sea-gull is a semaphore to every other sea-gull in sight of him, nor that one gull "working on" fish will presently be surrounded by others from all sides.

In clear, fine weather the news of abundant sprats would be passed along the Channel in this way, faster than by military signallers, and answered by a concentration of gulls much speedier than any possible to troops.

Moreover these birds, and vultures, as I know from repeated observation, do not merely *follow each other* round the headlands. If a gull in a bay sees another gull hurrying from the offing into the next bay, he does not fly round the headland between, but rises over it, well knowing that from the upper air he will see whatever hurried the outer gull. As he does so his motions will be observed, and probably acted on by others further within the first bay; and if food be failing in the Thames, and abundant off the Wight, there will be plenty of gulls flying across Kent, and some across even Surrey. Here in Chelsea I seldom see large gulls on the river. But it is a common enough thing to see them flying high overhead to the south-westward.

Westerly and south-westerly gales are so common in the Channel that neither beast nor bird can make any movement without a good off-chance of finding one on his way. It has to be remembered that most of our migratory sea fish are apt to run *up* Channel in the warm half of the year, so that the message, "Plenty of fish on the surface," is probably most often passed from west to east.

We shall, I think, need a good many simultaneous observations at various points and of various matters before we make it even probable that sea-gulls can foretell a south-west wind, and will then go to meet it. They cannot eat it; and, if strong, it will give them little leave to eat anything else; so the motive is not apparent. W. F. SINCLAIR.

102 Cheyne Walk, Chelsea, S.W., September 22.

On the Use of the Fahrenheit Scale for Observations on Sea Temperatures.

IN addition to the Fahrenheit scale being so much more practical for observation in meteorology than the Celsius, allow me to point out that in observations for ocean temperature it is even more so, and especially when we come to deal with observations taken in the polar regions. Here with the Fahrenheit scale we have never to deal with a minus reading at all; whereas with the Celsius scale it is a constant change from plus to minus and minus to plus. This introduces a source of very serious error both in observation and calculation, besides adding to the work, and therefore the cost of working up results. The boon of never having to think of a minus in such work is not to be lost sight of for the sake of fashion. As one who has taken part in extensive observation and calculation work at Ben Nevis Observatory, on board ship, and in connection with the Scottish Fishery Board, I would also urge the use of the Fahrenheit scale for meteorological observations on the same grounds as Mr. J. Y. Buchanan and Mr. H. Helm Clayton.

Joppa, Edinburgh, September 25. WILLIAM S. BRUCE.

Cave Shelters and the Aborigines of Tasmania.

I HAVE just received news from Mr. J. B. Walker, of Hobart, of the discovery of some interesting relics of the aborigines of Tasmania. Mr. Walker accompanied Mr. R. M. Johnston, the Government Geologist, on an expedition in search of some remains of Tasmanians, and the party were rewarded by finding a hitherto unknown so-called quarry where the natives manufactured some stone implements, also a cave which showed considerable evidence of having been used by the aborigines, as well as a tree notched by them for climbing purposes. The sandstone cave or rock shelter i situated in Hutton Park, near Lovely Banks. The quarry is situated at Coal Hill, two miles north of Melton Mowbray, about 40 miles N.N.W. of Hobart, and 1100 feet above the sea-level.

The discovery of this quarry makes the tenth known quarry used by the aborigines, and the first mention of their use of cave or rock shelters. H. LING ROTH.

Halifax (Yorks.), September 27.

The Darjeeling Disaster.

UNUSUALLY large seismograms were obtained in the Isle of Wight on September 3, 10, 17, 20 and 23. The first three refer to disturbances originating in Alaska. The fourth refers to disasters in Asia Minor, and the last to an earthquake having an origin as distant as Japan. Since the 23rd in the Isle of Wight, and I believe also at Kew, not the slightest movement has been recorded. The inference is that the great earthquakes reported as having taken place at Darjeeling on the night of September 25–26 are at the most small and local, and are not likely to have been recorded outside the Indian Peninsula. It is extremely likely that the tremors noticed in Darjeeling were due to landslides, and seismic phenomena were entirely absent. J. MILNE.

Shide Hill House, Shide, Newport, Isle of Wight, September 27.

Lectures at the Royal Victoria Hall.

I SEE in your issue of September 21 (p. 513) the statement that I am to lecture at the Royal Victoria Hall on "Photographs taken in the dark." I beg to say that the title I gave for my lecture was "Pictures taken on a photographic plate in the dark." I suppose the authorities at the Hall consider the titles identical. I do not. W. J. RUSSELL.

St. Ives, Ringwood, Hants, September 26.

Vole.

I THINK NATURE should take note of a short article by Prof. Skeat in the number of *Notes and Queries* for September 16, wherein he points out that *vole* is corrupt Norwegian for field, and that therefore a water-vole is a water-field, a field-vole a field-field, and a bank-vole a bank-field.

Exeter. JAMES DALLAS.

THE INVESTIGATION OF THE MALARIAL PARASITE.

PENDING the arrival here of Major Ross and part of the Malaria Expedition connected with the Liverpool School of Tropical Diseases, which is expected about October 7, we may, from information already to hand, forecast some points in his report without in any way detracting from the interest with which it will be received.

In the issue of this journal for September 7 we recorded the fact that a species of Anopheles was found to be concerned in the transference of all the forms of malaria. In the barracks of Wilberforce, a suburb of Freetown, Sierra Leone, out of four hundred men there was a daily average of forty ill in hospital with all three forms of malaria. The place seems to have been infested with mosquitoes, but only the genus Anopheles was found, and of those examined one-third were found to contain zygotoblasts.

In searching for the haunts of the Anopheles larvæ the members of the expedition found them chiefly in small stagnant pools in which green algæ were growing. The larvæ appear to live upon this, for larvæ hatched from eggs did not grow unless they were given some of the algæ to feed upon. They infer that the conditions under which algæ will grow, namely, in stagnant puddles, are the same as those under which Anopheles larvæ will hatch out and thrive; the larvæ of Culex were found in every receptacle for stagnant water, even in old sardine tins. Stagnant puddles are only found during the rains on low-lying ground, and near a stream or spring, from which they can be replenished in the dry season. So far, only one experiment on the action of kerosene oil on larvæ has been reported; one drachm of the oil was poured on the surface of a pool of water of about a square yard in area, and all the Anopheles larvæ it contained were found dead after six hours.

Ross considers the Anopheles to be the genus concerned in propagating malaria, and seems to rely on being able to exterminate them from a locality to free it from the disease.

Koch (*Erster Berichtüber die Thätigkeit der Malaria Expedition*, April 25 bis August 1, 1899) found *Culex pipiens* to be concerned in propagating malaria in Tuscany, but to a lesser extent than the Anopheles. The German Commission find that the parasite requires a temperature of 80° F. to develop in the mosquito, and it is only found in these insects during the summer months. At the commencement of the hot weather the mosquito draws the parasite with the blood from a patient who has a relapse. Human beings with the parasite in their blood they consider to be the connecting link during the nine months of the year when the temperature does not allow of the parasite developing in the mosquito; they think relapses can be stopped by the use of quinine; so by this means it would become possible to stamp out the disease.

It is evident we want a large series of observations made in different parts of the world, for, if the genus Culex can propagate the disease, it would be almost impossible to exterminate these if they breed wherever water lies. On the other hand, should relapses of fever be prevented by a proper use of quinine, malaria would not be stamped out in countries where the temperature is sufficiently high all the year round to allow the parasite to develop in the mosquito.

MR. PERCY S. PILCHER.

MANY of our readers who were acquainted with Mr. Percy S. Pilcher, and others who have only heard of him through his great enterprise and keenness in constructing and using aërial machines, will be very sorry to hear that his accident on Saturday last has proved fatal, and that he died at 2.40 on Monday morning.

Mr. Pilcher, during the last few years, had been making a considerable number of experiments with the object of constructing a soaring machine which would propel itself. The writer of this note was present at one of his trials in August 1897, at the time when he was at work in designing a small light engine for propelling his machine, and communicated to this journal an account (with illustrations from photographs) of his experiments on that occasion (NATURE, vol. lvi. p. 344).

Like his forerunner Otto Lilienthal, Mr. Pilcher has come to the same sad end, and now his name must be added to that already long list of pioneers in aërial navigation.

The experiments causing the fatality took place on Saturday last at Stanford Hall, the seat of Lord Braye, near Market Harborough.

We gather from the *Times* that after several ineffectual attempts to start, a signal was given about twenty minutes past four, and Mr. Pilcher rose slowly in the machine until he had travelled about 150 yards, and had risen to a height of about 50 feet or 60 feet. Then a sharp gust of wind came and the tail of the apparatus snapped. Instantly the machine turned completely over and fell to the earth with a terrible thud, Mr. Pilcher being underneath the wreckage. His devoted sister was one of the first to reach the scene of the accident. Mr. Adrian Verney-Cave, Mr. Everard Fielding, and Dr. Stuart, all friends and companions of Mr. Pilcher, removed him from the machine and found that he was unconscious. Raising his left leg it was discovered that it was fractured above the knee. Mr. Pilcher was carried to his room in the house, and Dr. Stuart and Dr. Nash carefully examined him, another surgeon being summoned by telegraph from Rugby.

W. J. S. L.

NOTES.

PROF. SIMON NEWCOMB has been elected president of the recently established Astronomical and Astrophysical Society of America. The secretary is Prof. G. C. Comstock.

THE seventh International Geographical Congress began a series of successful meetings on Wednesday, September 27, at Berlin. Papers have been read by, among others, the Prince of Monaco, on his Greenland Deep-sea Expedition, and Dr. Nansen, on "The Hydrography of the Polar Sea." At one of the sittings of the Congress a telegram was read from Mr. H. J. Mackinder, the Reader of Geography at Oxford, announcing that he had succeeded in reaching the summit of the hitherto unscaled Mount Kenia in British East Africa, and that some fifteen glaciers were found upon the mountain. It will be remembered that Mr. Mackinder left England in June last in charge of an exploring expedition.

A BUOY bearing the inscription "Andrée's Polar Expedition," found on the north side of King Charles Island, north-east of Spitsbergen, 80° latitude and 25° east of Greenwich, on September 11, has been brought to Stockholm and there opened in the presence of several experts and Ministers. It was found to be the so-called "North Pole buoy" which the explorer was to have dropped when passing the North Pole. So far as the examination extended no message from the explorer was revealed. Prof. Nathorst declared that the buoy could not have been carried from the Pole to King Charles Island, and Captain Svedenborg was of opinion that it had been thrown out empty. A search is, it is stated, to be made next year at King Charles Island.

A MARBLE bust of Prof. Emil Du Bois Reymond has been presented to the Physiological Institute of the University of Berlin by the professor's widow.

THE new Paris Institute of Biological Chemistry, facing the Pasteur Institute, towards the erection and endowment of which Baroness Hirsch gave 80,000*l.*, is now, so far as the exterior is concerned, completed.

PROF. R. BURCKHARDT, of Bâsle, and Prof. V. Uhlig, of Prague, have been elected members of the Academy of Sciences of Halle.

THE death is announced, at the age of forty-five, of Dr. Kowalowsky, professor of hygiene in the University of Warsaw, and of Canon Carnoy, professor of the natural sciences at the University of Louvain.

IN connection with the Glasgow Lecture Association a special science lecture will be given to school children during the Christmas holidays by Prof. McKendrick, F.R.S.

PROF. GEORG STEINDORFF, the director of the Aegyptologische Sammlung at Leipzig, has, it is stated, obtained leave of absence for six months to enable him to undertake a scientific journey to Africa.

DR. L. A. BAUER, of the U.S. Coast and Geodetic Survey, is at present in Europe for the purpose of inspecting various magnetic observatories and the comparison of the Coast and Geodetic Survey instruments with observatory standards.

THE announcement is made that Mr. W. H. Twelvetrees has been appointed geologist to the Government of Tasmania. During recent years Mr. Twelvetrees has devoted considerable attention to the geological formation of Tasmania, with special reference to mining operations, and has been a frequent contributor of papers to the Australian Institute of Mining Engineers and to the Royal Society of Tasmania.

THE Corporation of Glasgow has just appointed Dr. R. M. Buchanan bacteriologist to the city. He will devote the whole of his time to the duties of the office, and a laboratory has been assigned to him in the Sanitary Chambers.

IT is announced in the *Pioneer Mail* that Mr. Douglas Freshfield has arrived at Darjeeling, accompanied by two Swiss guides, intent on exploring the great snowfields of Kinchinjunga.

THE tenth International Congress of Hygiene and Demography will be held in Paris from August 9 to 17, 1900.

THE second International Congress on Hypnotism will be held in Paris from August 12 to 16, 1900, under the presidency of Dr. Jules Voisin. The questions proposed for discussion are: (1) The formation of a vocabulary concerning the terminology of hypnotism and the phenomena connected therewith; (2) the relations of hypnotism with hysteria; (3) the application of hypnotism to general therapeutics; (4) the indications for hypnotism and suggestions in the treatment of mental disease and alcoholism; (5) the application of hypnotism to general pædagogy and mental orthopædics; (6) the value of hypnotism as a means of psychological investigation; (7) hypnotism in relation to the (French) law of November 30, 1892, as to the practice of medicine; (8) suggestion and hypnotism in relation to jurisprudence; (9) special responsibilities arising from the practice of experimental hypnotism.

IN compliance with the request made by Russian men of science to the Russian imperial authorities, the scientific exploration of the coast-line of the Pacific in the Far East is to be undertaken. It has been arranged that a distinguished zoologist and member of the Imperial Russian Geographical Society shall undertake the exploration at the cost of the Society, in conjunction with the Ministry of Agriculture. The expedition intends to make investigations with a view to classifying the marine fauna and flora on the coast of the Russian territory, and the conditions of zoological life will also be investigated upon the Liao-Tong peninsula, and in the adjacent regions of Manchuria and Korea. The period for these investigations has been fixed at two years, and the cost of the expedition is estimated at 12,000 roubles. The Geographical Society has promised to make a grant of 7500 roubles towards this sum, and the Ministry of Agriculture and Imperial Domains will contribute the remaining 4500 roubles. The Ministry of Agriculture has been led to take a part in this expedition in the expectation that its results will be of great service in developing the coast industries of the Amur and the Island of Saghalien, and also in the districts which have been acquired lately by the Russian Government. The Geographical Society also entertains great hopes of the successful results of this expedition, in view of the fact that the previous expeditions sent by it to investigate the Black, Azov, and Marmora seas were particularly successful. The expedition to the Far East will work in conjunction with the Society for Exploring the Amur Territory, and intends to establish at Vladivostock a zoological station for studying the marine fauna of the district.

Science states that the late Richard B. Westbrook, of Philadelphia, has made a bequest of 10,000 dollars, taking effect on the death of his widow, to the Wagner Institute of Science. The sum is to be used as an endowment of a special lectureship to "secure the full and fearless discussion by the most learned and distinguished men and women in our own and other countries of mooted or disputed questions in science, and especially the theories of evolution."

A YEAR ago Cornell University secured 30,000 acres of woodland in the Adirondack Mountains for the exclusive use of the University's forestry department. The land has been divided into a number of sections, and several seed beds have been laid out in which there has been planted over a million small trees of different varieties. The students of forestry will study the theory of the subject from October to April, and from then until Commencement they will study the practical side of forestry. Cornell University is, according to the *Scientific American*, the only college in the United States which has a forestry department.

THE *Scientific American* states that the men of science who have been investigating the Wyoming fossil beds are having remarkable success, and a large number of boxes containing fossil remains have been sent to the State University, and the work of restoration will soon be begun under the direction of Prof. Wilbur C. Knight.

DURING this summer a number of field parties in connection with the United States Fish Commission have been engaged, in various States, in ichthyological and other investigations. A camping party under the direction of Dr. Charles H. Gilbert has, says *Science*, systematically examined the coastal streams of Oregon, with reference to their fish fauna; the eastern tributaries of the Sacramento have been visited by Mr. C. Rutter; a comprehensive study of the biological and physical features of the Wabash basin has been begun under the direction of Prof. B. W. Evermann; a party in charge of Mr. W. P. Hay has explored the Monongahela basin in West Virginia; Dr. P. H.

Kirsch has been collecting and studying the fishes of the San Pedro River, Arizona; in connection with the biological survey of Lake Erie, Prof. J. Reighard and assistants have cruised along the northern and southern shores of the lake in a special steamer; Dr. H. M. Smith has visited Seneca Lake, N.Y., for the purpose of determining the character of its fish fauna; a study of the variations of the mackerel of the east coast has been conducted by Mr. M. C. Marsh, and in the interesting Sebago and Cobbosseecontee lake regions of Maine, Dr. W. C. Kendall has made some special investigations regarding salmon and other fishes.

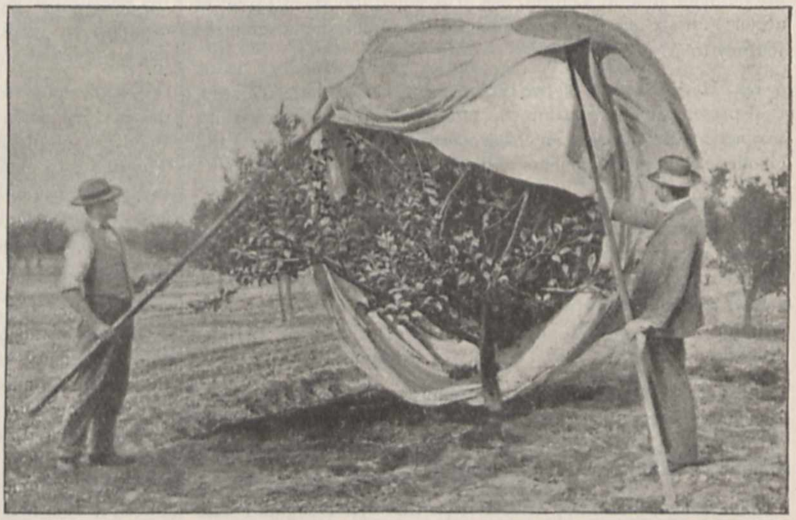
THE return of the schooner *Julia E. Whalen*, after an absence of nearly a year, is announced in *Science*. The vessel was sent out by Stanford University on a scientific cruise among the Galapagos Islands, &c., and carried members of a scientific expedition under the direction of R. E. Snodgrass, entomologist, and E. Heller, zoologist. A large collection of specimens, including birds, mammals, invertebrates, and fish, was obtained. On board the vessel were eighteen live land tortoises taken from Duncan and Albemarle Islands, some of them weighing four hundred pounds; also 220 fur sealskins and 2300 skins of hair seals.

A REMARKABLE demonstration of the success of inoculation against the plague is to be found in the statement recently made to the Indian Imperial Research Laboratory by Dr. Chinoy, the medical officer of the Southern Mahratta Railway at Hubli. In Hubli itself 4961 persons were inoculated once, 7840 persons twice, and 1346 persons thrice (these having been twice inoculated last year), or a grand total of 14,147. In the district 1849 persons were inoculated once, and 1967 twice, or altogether in Hubli and the district a total of 17,963 persons. In the words of Dr. Chinoy: "There are about 1000 people living in the railway chawl, Hubli, which was seriously infected last year. *All of them are inoculated*, and I am glad to be able to state that *not a single case of plague* has occurred amongst them since plague broke out in the town, although they freely move about and mix with people in the town, where plague is increasing daily."

THE report of the Imperial Bacteriologist at Calcutta for 1898–99 states that a considerable quantity of mallein and tuberculin are being manufactured in the laboratory for veterinary use. The hope is expressed that arrangements may be made for dairy and other cattle to be tested with tuberculin, so as to ascertain the extent of tuberculosis amongst cattle in India. Attention has been given to the further investigation of "surra," and the report says it is more than ever probable that this disease is identical with the South African tsetse-fly.

THE Vienna correspondent of the *Times* reports some experiments in a new system of telegraphy made in Budapest and Berlin on Friday last. These are alleged to have given the extraordinary result of a transmission of no fewer than 220 words in ten seconds without prejudicing the clearness of the message. According to the reports from Budapest, the impression made upon the technical experts who had an opportunity of following the trial was favourable. A perforated roll of paper, similar to that at present in use, is employed for the despatch of the message, which is made visible and fixed photographically at the receiving station. Instead of the dashes and dots of the Morse alphabet there are rising and falling strokes starting from a horizontal line. The receiver consists of a telephone fitted with a small concave mirror, upon which are reflected, in the form of streaks of light, the impulses marked on the membrane. By an ingenious arrangement, recalling in some respects that of the cinematograph, the streaks of light reflected upon the mirror are reproduced upon a roll of sensitised paper, thus giving a narrow oblong picture, which in the present stage of the invention is developed and fixed like any ordinary photograph.

THE fumigation of trees for the destruction of insect pests has for some time been extensively used in California and other parts of the United States. The process will probably soon come into use in New South Wales, for Mr. W. J. Allen describes in the *Agricultural Gazette* of the Colony some very successful experiments in spraying and fumigating for red and other scales on orange trees. The tree to be treated is completely covered with a tent, such as is shown in the accompanying illustration, and is subjected for nearly an hour to the fumes of hydrocyanic acid, produced by the combination of sulphuric acid and potassium cyanide. The number of men generally employed in a fumigating gang is four or five, according to the size of the trees. One man introduces the chemicals, another looks out for the generator and measures the acid, and two or three handle the tents. Such a gang can handle from thirty to forty medium-sized tents, and cover four to six acres of orchard in a night. Fumigation is to be preferred above spraying, because the trees are not in any way damaged by the fumes, except in the case of a few of the tender leaves, while the solution used in the sprays must to a certain extent close the pores of the tree and slightly weaken it.



Placing tent over tree to be fumigated.

ACCORDING to the *Engineer*, the signalling on the whole of the Pennsylvania Railroad system is now operated electrically. When a train passes a signal bridge it closes an electric circuit, which moves a signal semaphore to the "danger" position. When the train passes beyond the next bridge a circuit is opened, and the signal indicates that the block from which the train has just passed is clear. Finally, when the train passes beyond the third bridge, another signal arm on the same post drops, showing the driver of an approaching train that there is nothing on the next two blocks ahead.

THE connection between fleas and the permanency, or otherwise, of continents might not at first sight be very apparent, but, nevertheless, some important evidence on the latter point is afforded in a paper by the Hon. N. C. Rothschild published in *Novitates Zoologicae* for December last. In this contribution the author, who is making fleas his special study, describes a new species of those insects on the evidence of a single specimen from Argentina, which is believed to take up its abode on a rat. Now this Argentine flea, which is remarkable on account of the helmet-like shield covering the head, is provisionally assigned to a genus (Stephanocircus) hitherto represented by a single Australian species infesting the spotted dasyure (*Dasyurus maculatus*). And we presume it may be taken for granted that, whether or no they are rightly regarded as congeneric, the two species are evidently very closely allied. Now this being so, it is difficult to see how they reached their respective habitats except by means of a direct land connection between Australia and South America; and they accordingly serve to confirm the evidence afforded by the occurrence of the chelonian genus Miolania in both areas, to which attention has been recently drawn in these columns.

THE discovery of a new generic type oι marine gastropod, represented by a species whose shell is over six inches in length, is such a rare event, that Mr. G. B. Sowerby's description of *Neptuneopsis gilchristi*, from the Cape seas, demands a brief notice. In general form the shell (which is described in the publications of the Cape Department of Agriculture for 1898) is so like some of the *Buccinidae*, such as *Neptunea* (*Chrysodomus*) that, were it not for its curiously swollen apex, it might be referred to the genus mentioned. On the other hand, the tricuspid tooth-ribbon, or radula, is of the characteristic volute type; and Mr. Sowerby consequently infers that the new genus should be placed in or near the *Volutidae*. If included therein, it will represent an interesting annectant, and therefore generalised type. The generic name selected scarcely seems to us a happy one.

THE close study of the smaller mammals of Europe on the lines followed by the American naturalists for those of their own country is gradually bringing to light the existence of numerous local races of species hitherto quite unsuspected. A remarkable instance of this is Mr. Barrett Hamilton's recognition of two new forms of mice from St. Kilda, which are described in the June number of the *Proc. Zool. Soc.* The first of these (*Mus hirtensis*) is nearly allied to the wood mouse (*M. sylvaticus*); while the second (*M. muralis*) is as closely related to the common house mouse. The former the author regards as indigenous to St. Kilda since the period when that island was connected with the mainland; while the latter he considers to have been derived from individuals of *M. musculus*, introduced by human agency not more than a few hundred years ago. Yet both differ from their mainland prototypes to the same degree; and thus indicate the different inherent variability of different species. The variation displayed by *M. muralis* is probably in the direction of the wild ancestor of *M. musculus*.

IN the *Indian Meteorological Memoirs*, vol. vi. part v., Mr. Eliot contributes a very important discussion of the air movement at Simla and in the Western Himalayas, deduced from anemometric observations recorded at Simla during four years ending August 1896. Some fifty years ago Sir Richard Strachey made a lengthened series of observations, chiefly in Kumaon, and in his description of the diurnal variation of the wind he considered the most important feature of the air movement in the Western Himalayas to be the up and down valley winds. Mr. Eliot's discussion entirely confirms this view. He states that the examination of the wind data from every point of view shows that the most important and unique feature of the air movement is the alternating currents between the hills and plains. He states that it is a permanent feature, independent of the change of seasons, and also of the air movement in the plains of Northern India, and is due to the changes of pressure vertically produced by the unequal expansion and contraction of the lower and middle strata of the atmosphere over the plains of Upper India and the Western Himalayan mountain zone.

THE *U.S. Monthly Weather Review* for June contains particulars of the increased usefulness of the Canadian Weather Service. For the year 1896 (the last published) the Report consists of two large quarto volumes, instead of one octavo volume published a year or two previously. During that year there were more than three hundred stations of observation of various classes. At the chief stations (eight in number), the telegraphic reporting stations, and a few of the special stations, the observers are paid, but the great bulk of observers are volunteers. There are thirty telegraphic stations, whose reports are received in Toronto before 9.30 a.m., and which, combined with fifty-four reports received by exchange from the United States, enable the director at Toronto, Prof. Stupart, to issue daily weather maps and forecasts similar to those published by other meteorological offices. The second volume is wholly taken up with details and results of the observations made at the eight chief stations. For each of these the Report gives for every hour and day the complete record of all the principal meteorological elements, in a form closely corresponding to that recommended for international meteorological publication of detailed observations. Prof. Cleveland Abbe's comment upon the work is that it is a noble contribution of data needed for the study of climatology in its relation to every matter that interests civilised humanity.

AN investigation of the emission and absorption of platinum-black and soot, and their dependency on the thickness of the layer employed, forms the subject of a paper by F. Kurlbaum in *Wiedemann's Annalen* (67). It would appear that both substances closely approach a black body in their behaviour towards waves of the length emitted by a black body at a temperature of 100°. For shorter waves the power of absorption increases. Both platinum-black and soot satisfy the Stefan-Boltzmann law when a sufficiently thick layer exists at high temperatures; and any deviations from this law are due to the radiating surface, or the bolometer being too thinly coated. Several further results of interest are found, and the author considers that for several reasons platinum-black is better than soot in all experiments.

IN the *Transactions* of the Institution of Mining Engineers, M. A. Rateau, Professor at the School of Mines, St. Etienne, describes experimental investigations on the theory of the Pitot tube and Woltmann mill, which are employed by engineers in gauging the rate of flow of air and water. Although the Pitot tube and mill and fan anemometers measure accurately the velocity of currents when these currents are uniform, they give, under opposite conditions which usually prevail, indications of the mean velocity which are always exaggerated, and are the more exaggerated the more marked the irregularity of the current itself. M. Rateau finds that the equation $v = a + bn + c/v$ which holds for low velocities in connection with such meters should, for velocities unrestricted in magnitude, be written in the form $bn = v - c/v - f(v)/v$, where $f(v)$ is a function of the resistance of the fluid with regard to which our knowledge is still somewhat deficient. In a note added later attention is drawn by the author to subsequent experiments by Mr. Epper, bearing on the same subject.

As there are insufficient data on the normal relations of voluntary movement to consciousness, Dr. R. S. Woodworth has set himself to this study, and his results are published as a monograph—"The Accuracy of Voluntary Movement"—in *The Psychological Review* (vol. iii. 1899). The following are some of his conclusions: When the eyes are used, the accuracy of a movement diminishes as the speed increases; but it does not vary so much when the eyes are not used; the right hand is slightly more accurate than the left. When the interval between successive movements is kept constant and the speed of the motion alone varied, the accuracy diminishes rapidly as the speed increases, the accuracy also diminishes on keeping the speed constant and varying the interval alone; the accuracy of initial adjustment is favoured by a short interval, accuracy of current control by a low speed; fatigue increases the variability of a performance, but practice decreases it, variability means improvability. Finally, the author advocates a new mode of writing, as he finds that the side to side movement of the wrist and forearm possesses advantages in point of ease and of speed over the usual thumb-and-finger movement, or a movement of the whole arm from the shoulder.

DR. MARIO BARATTA has contributed a preliminary sketch of his work on the Latian earthquakes to the *Rivista Marittima* for August. He shows that the more important shocks are connected with certain definite seismic centres, the positions of which he determines. An interesting comparison is made between the earthquakes of an extinct volcanic region, such as the Alban Hills, and those which precede, accompany and follow an eruption of an active volcano such as Etna. In both, the disturbed areas are as a rule extremely small, and yet near their centres the shocks may be strong enough to damage buildings. Also, in successive shocks, there are many changes in the positions of the epicentral areas.

M. A. DE GRAMONT sends a reprint of an article describing a method he has devised for varying the scale and dispersion of a laboratory spectroscope (*Comptes rendus*, vol. cxxviii., p. 1564, June 1899). The scale is adapted in the usual way by placing it at the end of a collimator tube and arranging that the light from the scale shall be reflected from one face of the prism into the observing telescope; but instead of having only one lens in the scale tube, fixed at its principal focus from the scale, there are two lenses there, whose distances from each other and from the scale can be varied by known amounts. It will be at once apparent that the effect of this will be to alter the magnification of the scale as seen in the observing telescope, and in practice the scale is so altered that a certain number of divisions always correspond to the distances between the same two spectrum lines, whatever kind of prism or dispersive arrangement is being used. The variation in the dispersion is obtained in a well-known but little used manner, by rotating the prism about its refracting edge, and using positions more or less removed from that of minimum deviation, the dispersion being increased or decreased accordingly as the prism is turned towards the collimator or telescope from the mean position. A useful diagram is given bringing together the various effects on the dispersion by gradual displacements of the prism. The author is investigating more closely the variation of dispersion with the angle of incidence, and will communicate results later.

AN observation of some interest in connection with recent discussions on heredity is recorded by M. Casimir de Candolle in a paper read before the *Société de physique et d'histoire naturelle de Genève*. He points out a constant difference between the normal and the adventitious buds of trees. The latter he regards as new individuals of the same species as the tree from which they spring, or as apogamic embryos, while the former are simply organs for prolonging the life of the individual. It is quite common for the first leaves of a woody plant to differ in form or structure from the later leaves. Examples are given in *Eucalyptus globulus*, the walnut, the horse-chestnut, and the hornbeam. In all these cases the first leaves from adventitious buds resemble, not those from normal buds, but the first leaves of the young plant.

A SECOND edition of Dr. M. M. Richter's tables of carbon compounds is in course of publication by the firm of Leopold Voss, Hamburg and Leipzig (London: Williams and Norgate), under the revised title of "Lexikon der Kohlenstoff-Verbindungen." How tremendous has been the advance in the knowledge of carbon compounds since the original work appeared may be judged by the fact that the total number of compounds registered in the new edition is 67,000, as compared with 16,000 known in 1883. The work will be an index to Beilstein's handbook of organic chemistry, for all the compounds—about 57,000—contained in Beilstein are dealt with. Papers published up to the end of last year have been used in the preparation of the volume, and it is intended to publish annual supplements in order to keep the work up to date. The "Lexikon" will be completed in about thirty-five parts, twelve of which have been issued.

MESSRS. WHITTAKER AND CO. have published a second edition, thoroughly revised, of "An Introductory Course of Practical Magnetism and Electricity" by Mr. J. Reginald Ashworth. The book contains an admirable course of experimental work suitable for students in "Schools of Science" and other institutions where physics is taught by laboratory practice. A number of new illustrations have been added to assist students to understand descriptions of experiments.

CALENDARS, for the session 1899-1900, have reached us from the Merchant Venturers' Technical College, Bristol, and the Birkbeck Institution, London, in each of which all necessary information is to be found as to the various activities in connection with the two institutions. We notice that at the Merchant Venturers' College certain extensions and improvements have recently taken place; *e.g.* a new optical laboratory has been opened in connection with the Department of Applied Physics and Electrical Engineering, the amount of space available for the dynamo and testing-room has been doubled, and a battery, &c., have been added to the equipment. In addition to these, other changes have either taken place or are in progress.

TWO articles of scientific interest are to be found in the current issue of the *Humanitarian*—one, by Mr. E. W. Brabrook, entitled "Anthropology, 1863-1899"; the other, by Dr. D. Somerville, on "The Rise of Bacteriology." Each gives in brief outline some idea as to what has been accomplished in the two branches of science. A reproduction of a striking photograph of Mr. Brabrook forms a frontispiece to the number.

WE are asked to say that the lectures to young people, referred to in the last issue of NATURE, p. 538, are to be delivered by Mr. Cecil Carus-Wilson during the months of October and November, and not as was stated.

THE additions to the Zoological Society's Gardens during the past week include five Barbary Turtle Doves (*Turtur risorius*) from Africa, presented by Mrs. J. A. Moore; one Ruffed Lemur (*Lemur varius*) from Madagascar, two Westermann's Eclectus (*Eclectus westermanni*) from Moluccas, a Two-spotted Paradoxure (*Paradoxurus binotata*) from West Africa, a Rufous Tinamon (*Rhynchotus rufescens*) from Brazil, a Grey Ichneumon (*Herpestes griseus*) from India, four Blanding's Terrapins (*Emys blandingi*) from North America, deposited; a Black-necked Swan (*Cygnus nigricollis*) from Antarctic America, a Hoopœ (*Upupa epops*), two Sandpipers (*Tringoides hypoleucus*), European, two Lanceolated Jays (*Garrulus lanceolatus*) from the Himalayas, purchased.

OUR ASTRONOMICAL COLUMN.

ASTRONOMICAL OCCURRENCES IN OCTOBER:—

October 7. 10h. 18m. Minimum of Algol (β Persei).
9. 14h. Saturn in conjunction with the moon (♄ 1° 27′ N.).
10. 7h. 7m. Minimum of Algol (β Persei).
10. 7h. 3m. to 8h. 10m. Occultation of 4 Sagittarii (mag. 4·6) by the moon.
11. 5h. Mars in conjunction with Jupiter, ♂ 1° 11′ S.
14. Vesta (mag. 6·5) in opposition to the sun.
15. Venus. Illumination portion of disc 0·991; Mars, 0·981.
16. 6h. 3m. to 7h. 2m. Occultation of 16 Piscium (mag. 5·6) by the moon.
16. 11h. 53m. to 13h. Occultation of 19 Piscium (mag. 5·2) by the moon.
18-20. Epoch of the October Meteors (Radiant, 91°+15°).
21. 8h. 37m. to 9h. 35m. Occultation of K^1 Tauri (mag. 4·6) by the moon.
21. 8h. 38m. to 9h. 34m. Occultation of K^2 Tauri (mag. 5·5) by the moon.
26. 6h. Venus in conjunction with α Libræ. ♀ 0° 6′ N.
29. 13h. Venus in conjunction with Jupiter. ♀ 0° 33′ S.
30. 8h. 50m. Minimum of Algol (β Persei).

COMET E. GIACOBINI.—A telegram has been received from the Centralstelle at Kiel announcing the detection of this comet at the Nice Observatory for the first time during the present apparition. The observation was as follows:—

1899. Sept. 29d. 8h. Nice Mean Time. {R.A. 16h. 26m. 32s. Decl.—5° 10′

The daily motion in right ascension is + 2m. and in north polar distance − 10′, so that the comet is moving slowly in a north-easterly direction. The position at the time of discovery on September 29 was about 3° north of the 5th magnitude star, v Ophiuchi. The comet is merely described as "faint." Previous appearances of this visitor took place in September 1896 and June 1898.

A later telegram from Kiel announces that the comet has been observed at the Konigsberg Observatory, the measured position being:—

1899. Oct. 1d. 8h. 0·5m. {R.A. 16h. 31m. 0·7s. Decl. −4° 39′ 50″

TWO NEW VARIABLE STARS.—Dr. T. D. Anderson, of Edinburgh, announces in *Astr. Nach.* (Bd. 150, No. 3594) his discovery of two new variables.

(1) *In Hercules.*—A star, not mentioned in the B.D., whose position is

R.A. = 17h. 53m. 27s.
Decl. = + 19° 30′ } (1855)

was found in August to have a variation amounting to about 0·9 of a magnitude. The star is about 2′ or 3′ *north-preceding* the 9·2 magnitude star B.D. + 19°·3489.

(2) *In Cygnus.*—A star, not mentioned in the B.D., whose position is

R.A. = 20h. 9m. 44s.
Decl. = + 30° 37′ } (1855)

is at present (September 21) rapidly diminishing in brightness. Comparisons with the neighbouring stars B.D. + 30°·3958, 3963, 3964 showed the variation in magnitude to be from 8·5 to 9·2.

THE MELBOURNE OBSERVATORY.—The thirty-third report of Mr. P. Barrachi, the Government Astronomer at the Melbourne Observatory, Victoria, has recently been distributed, showing the work undertaken and the progress made during the period July 1, 1898, to February 28, 1899. The observatory is reported in good order, the instruments well cared for and in good working condition.

With the 8-inch transit circle 1571 observations have been made in right ascension, for determinations of azimuth, clock corrections and catalogue stars; also 1017 observations in north polar distance have been made in connection with latitude determination, catalogue stars and special zodiac stars. The catalogue stars were intended chiefly to be used in the reduction of the plates for the astrophotographic catalogue. The zodiacal stars have been observed at the request of Dr. Gill, of the Cape Observatory, in connection with his heliometer observations of Neptune and the other major planets at opposition. All the reductions are well in hand.

Astrophotographic Catalogue.—The series of plates for the catalogue is now completed, and 387 plates for the Chart have been passed as satisfactory. Special series have been taken for the region round the South Pole, and seven plates have been exposed for the Oxford chart type. The measurement of the plates is being undertaken by six young ladies, using three micrometers. The probable error of a measured coordinate is now no more than 0″·1, which is within the prescribed limit. The progress of this part of the work is rather slow, but trials with Prof. Turner's scale, as adopted at Greenwich and Oxford, although permitting of much greater speed, showed the error to be as great as 0″·5, and therefore all the measurements are to be made with the filar micrometer. A new instrument, similar to that designed by Dr. Gill for the Cape, has been ordered from Messrs. Repsold and Söhne.

The various operations connected with the time service, meteorological observations, and inspection of outlying depôts have been carried out as in previous years.

Terrestrial Magnetism.—The photographic registration of the horizontal and vertical components and of the magnetic declination have been continued, absolute measurements and redeterminations of scale zeros being made *five* times. The measurement and reduction of the curves obtained since 1867, numbering some 30,000, have been commenced.

The photo-heliograph has been employed on sixteen days for solar pictures; 264 pairs of cloud photographs have been obtained with cameras placed at different points round the observatory buildings.

The great telescope and south equatorial have been used for comet and planetary observation, and for the use of visitors, 189 persons being admitted on Wednesday afternoons and 195 at night during the year.

SIR ANDREW NOBLE ON THE BEST EDUCATION FOR ENGINEERS.[1]

WHEN your Dean first did me the honour to ask me to address you on the opening of your session, I had grave doubts as to whether I was a proper person to accept the invitation. On the one hand, I have had little or nothing to do with the education of others, and in some points my views, at all events so far as regards primary education, are at variance with much that is being done at the present day, but as, on the other hand, I have had exceptional opportunities of observing, both in this and other countries, certain points which seem to me to be of importance to those who propose to uphold the industrial supremacy of this country in the struggle which year by year other countries are rendering more and more severe, you therefore see me here to-day, and I shall consider myself amply rewarded if I can tempt but one of you to enter, for the sake of knowledge itself, the boundless fields which science day by day is opening up to you. I can promise that the pursuit will give you happiness. I hope it may give you wealth and distinction; but I remember the words of the Preacher, that riches are not always given to men of understanding, nor favour to men of skill, but that time and chance happen to us all.

Technical education is a phrase that has been so often misused, perhaps so often misunderstood, that many of those who, like myself, are engaged chiefly in trying to solve the practical problems of engineering are in the habit of hearing it either with impatience or of regarding it as a fad of lay theorists, or sometimes, I fear, as a cloak for educational shortcomings in other directions. And I am bound to confess, if their experience has been the same as mine, that there is some excuse for them. You can form but little idea of the number of persons of both sexes who have assured me that their sons had no taste for books, but had shown a marvellous talent for engineering. I need hardly tell you that the marvellous talent generally turns out to be an incapacity, possibly from defective education, for seriously applying the mind to any subject whatever.

But technical education, properly considered, is of the highest

[1] Inaugural Address of the Session 1899-1900 of the City and Guilds Central Technical College, given at the College, Exhibition Road, by Sir Andrew Noble, K.C.B., F.R.S., on Tuesday, October 3.

importance both to you and to England. It is only its abuse that we have to guard against.

Now one of the great abuses I take to be that technical education is often begun too early in life, that is, that it is substituted for a general education, and a boy attempts to put his knowledge to practical use before he has learnt how to learn.

Another abuse is the divorcing of practice from theory, and the danger of elevating practical application above scientific knowledge.

I shall try, therefore, to-day to say a few words, firstly, about the necessity of acquiring a sound general education before any special work is attacked, and, secondly, about the necessity of basing all practical work on theoretic knowledge.

I attribute the compliment which has been paid me in the invitation to speak at the opening of the present session to the fact of my having been connected, for many years past, with the management of probably the largest engineering firm in England. That position has afforded me exceptional opportunities for observing what educational antecedents are likely to produce the best results in the engineering field. I say "exceptional opportunities" advisedly, for we at present employ in our various works not far removed from 30,000 hands. Of these a large number are youths; often sons of workmen, but not unfrequently drawn from the class which I see represented before me.

I am continually asked what education I should recommend for a lad entering Elswick. I always say, "Send your son to as good a school as you can, keep him there as long as you can, do not curtail his time of schooling, do not stunt his early intellectual growth by narrowing it down to any special study as taught at elementary schools."

Science, mechanical drawing, and such like are no doubt very useful (as all knowledge is useful) in their way. These studies may prove an irresistible attraction to minds with a strong bent towards scientific subjects, but I would fancy most employers would rather that a lad came to us blankly ignorant of both, so long as he had had a good education, had been taught, and had ability to think, and to concentrate his attention on any subject brought to his notice.

Some of you may have heard, no doubt, the answer of the Duke of Wellington to a father who asked him what was the best education for his son, preparatory to his joining the army: "The best education you can give him."

It was a very pregnant utterance, terse and to the point, as nearly all the great Duke's were; and it remains as true for any other profession as for the army.

In nine cases out of ten, I should say, any knowledge acquired by a boy before he is sixteen can have but a slight intrinsic value. Up to that age, it is not *what* he learns that we have to look at, but *how* he learns; it is the habit of discipline, of mental application, of power in attacking a subject, that are so valuable; not, generally, any definite piece of knowledge he may have gained.

According to my experience, the most valuable knowledge that a man has at his disposal is that which he has taught himself. That a special technical education is not an absolute necessity is not difficult of proof. My own chief, Lord Armstrong, commenced life as a solicitor; James Watt was an instrument maker, and was prevented from opening a shop in Glasgow because he had not served a full apprenticeship. George Stephenson was an assistant fireman to his father at Killingworth Colliery. Faraday was brought up as a bookbinder. I cite the cases of these great men simply to show how men without trained assistance have taught themselves, and what can be done by the dauntless energy, untiring industry and patient search after truth which were the great characteristics of all of them, and which enabled them to do such great things.

My own impression with regard to early education is that, as a sharpener of the young intellect, and as a mental discipline, it would be difficult to improve upon the curriculum which is now in force at our public schools, and which, in the main, has been in force for so many centuries.

I am not in accord with those who think that modern languages should supersede the classics as a means of education, and I should regret more than I do the attempts which have been made in this direction, did I think that these attempts were likely to be successful. Men of science will remember that practically the whole of our scientific nomenclature is borrowed from the Greek and Latin languages; and, personally, I have found my own knowledge of the classics—which represents, no doubt, that of a very ordinary schoolboy—stand by me, and enable me to enjoy, as I would not otherwise have done, that noble literature, which, as Lord Macaulay says, is the most splendid and perhaps the most durable of the many glories of England.

But, whatever may be the fate of the classics as a means, I must take up my parable against a course of education I have seen in several primary schools where an attempt is made to teach boys, often little better than children, rudimentary chemistry, rudimentary geology, also physiology and electricity.

Occasional popular lectures on these sciences may be of very great value to some boys in interesting them in these great subjects, and in leading them, at some later date, seriously to study them, but these sciences as taught in the schools I refer to can have but little value in encouraging habits of thought, of application, and of mental discipline; and to knowledge so acquired the words of Pope are peculiarly applicable:—

> "A little knowledge is a dangerous thing,
> Drink deep or taste not the Pierian spring,
> There shallow draughts intoxicate the brain,
> And drinking deeply sobers it again."

I am aware that many people say that the years a boy wastes on Greek and Latin might be better employed in learning German and French. It may be so, but it is not difficult to teach these most important languages colloquially at a very early age; and with regard to technical subjects, speaking from my own observation, I may say that I do not think I have known any man at twenty-eight or thirty who was the better for having abandoned his general education for technical subjects at too early an age.

Those men who, with fair abilities, have received a really good education, have been taught to use their minds, and who, by contact with other students, have acquired habits of application, amply make up for their late start by the power of mind and grip that they bring to their work. They are fresh and keen when others, who have been hammering away at semi-technical work from early boyhood, have become stale and are less vigorous, and that reflection moves me to deprecate strongly any attempt to teach seriously practical or electrical engineering in preparatory or elementary schools. As an excellent recreation, such studies are no doubt to be encouraged, but to make them a systematic part of education, to the exclusion of studies which have a more direct effect in developing the understanding, seems to me to be entirely wrong. I would go further and say that even in public schools, and their equivalents, for older boys, what are termed engineering shops are generally a failure, so far as any efficient knowledge to be gained in them is concerned. Except as a reasonable diversion for recreation hours, such "shops" have, I fear, but little value, and in nine cases out of ten the hours spent in them are subtracted from the time due to more valuable studies.

In my judgment, the age at which a boy should seriously begin any special studies, with a view to fit him technically for the profession he may have decided to follow, should not be earlier than seventeen or eighteen.

And in any discussion as to the age at which a boy should leave school, the great incidental advantages that he gains from a reasonable prolongation of his schooldays must never be lost sight of. A stricter discipline, a wiser supervision, a more authoritative yet sympathetic advice as to conduct, are more possible at school than can ever be the case in after life, and a more constant and generous association with his equals rubs off angularities and leads to amenity of disposition. It is seldom, indeed, that one cannot trace the difference between a lad who has had a full public school training and another who has been less fortunate. Speaking as an employer of labour, I should say that we find a pleasant speech and manner, tact in dealing with others, and some power of organisation of the utmost value; and it is precisely those qualities which a boy acquires, or ought to acquire, in his *later* years at a public school. Without such qualities even the highest scientific attainments will never make a captain of industry, and in selecting candidates for appointments the man-of-business distinctly prefers a youth who has had the benefit of some years at a good school.

So much for the necessity of grounding technical studies on the basis of a sound general education.

The next point I should like to urge is that any practical technical instruction and any practical knowledge acquired in the workshop should be based upon sound theoretic knowledge. I am driven to enforce this question because (speaking again from my own observation) I find that in this country far too much weight is given to practical skill and what is called the "rule of thumb"; far too little to sound theoretic knowledge.

In the middle of this century English machinery was immeasurably superior to any other. To our remaining content with this state of things, and to our seriously neglecting technical instruction, I attribute the very much greater comparative progress that Germany, the United States and Switzerland have made in the last fifty years, and, if I am not very greatly mistaken, we shall have before many years, in the East, an important commercial rival in Japan, since that country is developing its manufacturing powers with an energy that is as remarkable as it is unexampled.

Turning to other departments of industry, no Englishman can observe without regret how certain branches have almost altogether abandoned this country, and been in a great measure left to those who have paid more attention to technical instruction.

Nearly every requirement of a drawing office can be better and more economically obtained from Germany. From what source do all our pure chemicals come, our filter papers and most of our glass apparatus? I admit that the workmanship of many articles made in England cannot be surpassed, but if we require any original or special piece of apparatus we are frequently compelled, as I have been, to go to Germany or France for their manufacture.

I do not desire to press my point too far, and admit that a portion of this transference of work, which I so much regret, may be due to cheaper labour. But the English mechanic is second to none, and if that false trade unionism, which endeavours to prevent the most intelligent and skilled from reaping the full benefit of their abilities, be abandoned, I do not despair of seeing this country regain much that it has now lost.

But it is to theoretic and technical knowledge that we must chiefly look. Consider, as an illustration, electricity in the service of man. Think of its innumerable applications, and of the number of hands dependent upon its industries. But for one man capable of designing or improving these powerful machines or delicate instruments, there are a thousand ready and able to carry out their designs. But it is the former who are the salt of the earth, and those who have the management of large concerns know well how to value them.

It was to meet the want that I am referring to that your Technical College was founded. Its objects are admirably stated in its programme, and your attention is drawn to the undoubted fact that no theoretic or technical instruction can supersede the necessity of obtaining practical experience in the workshop and factory. But, on the other hand, I believe that no genuine success in the higher walks of industry is probable without thorough theoretic or technical knowledge.

In my experience I do not think I have ever known a man rise to the top of the tree without it. I may, perhaps, be forgiven if I refer to one great engineering genius, Lord Armstrong, with whom it has been my privilege to be so long and so intimately connected. In whatever investigation he was engaged, he added to sound theoretic knowledge an intensity of application and an apparently intuitive perception of the results to be expected that I have rarely seen equalled.

Of him it may be truly said that "whatever his hand found to do, he did it with his might."

Sir William Harcourt, speaking a fortnight ago, attributed the immense commercial advance which has recently been made by Germany to the better teaching of languages, and to the German merchant being able to speak to the English buyer in a tongue which he can understand. I very much doubt if that has much to do with the matter, and I am sure that houses where business is done on a large scale very much prefer that all letters should be in the languages of the respective writers, and not in the doubtful English that is not unfrequently thrust upon us. There is no doubt that Germany is competing with us, as she has a right to do, successfully; and, so far as I am aware, with respect to her manufactures, perfectly honestly.

I say "honestly," because I do not believe in any attempt to enhance the value of one's own wares by depreciating those of other people; and I entirely differ from those who would attribute the success of our German competitors to their putting on the market inferior goods specially got up to imitate those of a superior class. It was some idea of this kind, no doubt, that led to the most ill-advised regulation that foreign-made goods should be stamped so as to show their origin. It doubtless does this, but its effect is, I believe, in the direction of an advertisement for foreign goods, and there is some danger that if our own manufacturers relax their efforts the "made in Germany," which was, I think, meant to be a reproach, should become, on the contrary, a hall-mark of excellence, as when the *Wilhelm der Grösse*, one of the finest steamships afloat, steamed into Southampton water with a facetious placard, "Made in Germany," hanging on her side.

In many articles, and especially with the apparatus of scientific research to which I have referred, this is already the case.

Manufacturing progress has in Germany gone hand in hand with material progress, and any one who has travelled much must be astounded with the extraordinary improvement which has been going on in recent years, not only in German railways, shipbuilding and steel-working, but also in the buildings, order and general amenities of life of the great German cities, such as Berlin, Frankfurt and Cologne. In the competition of manufacture we are pressed very hard from steel to watches, from marine engines to scientific instruments. In nothing, indeed, have German manufacturers made more progress than in the making of all exact instruments. In these departments Germany certainly excels us, so far as original and inventive improvement is concerned.

Now, all this improvement, I feel inclined to attribute, not, with Sir William Harcourt, to any linguistic superiority, but to the far greater opportunities of technical study which are afforded in Germany. If we are to hold our own, we older men must try to multiply these opportunities of study in our own country, and you younger men must do your part by seeking to avail yourselves to the uttermost of any such opportunities provided.

To you, gentlemen, who are about to commence the studies which will be useful to you in your future career, I venture to say a few words. Consider the marvellous progress that has been made in the physical and practical sciences during the century now rapidly drawing to a close. At the commencement of the century steam navigation and railways were unknown and unachieved. Our knowledge of the science of electricity was confined to a few isolated phenomena, and chemistry was in its infancy. Now the latter science has spread its branches until it seems likely it may bring into a common brotherhood the whole of the physical sciences. Consider, further, that knowledge and progress appear to be increasing in a geometric ratio; who then can predict what will be the progress made at the conclusion of the twentieth century, or even during the first half of it? In forwarding that progress I sincerely trust that many of those whom I now address may be prominent workers. We have never wanted in this country the men whom I would call the captains of the scientific army, but I think we are much inferior to Germany in the rank and file, in the number of men who are willing to follow particular lines of investigation, and who thus do invaluable service to science.

We older men, whose careers are approaching their termination, cannot but look with envy on the career which may be open to some of you. It was said of the telescope, which opened to our vision infinite space, that it was balanced by the microscope, which showed us the infinitely small; but small as are these objects, the kinetic theory of gases opens up to our appreciation, I had almost said to our view, molecules whose dimensions are inconceivably smaller. It would be vain to name to you the limiting dimensions of these molecules which have been revealed to us by the labours of Maxwell, Lord Kelvin, Clausius and others, but I have seen somewhere, possibly in the columns of NATURE, a statement which may be more intelligible. It was something like this:—That though the molecules of hydrogen gas are so small that it would take about 50 millions touching one another to make an inch, they are so numerous in a cubic inch of gas at 0° Centigrade and atmospheric pressure, that if the whole of them were formed into a row, they would go round the circumference of the earth more than a thousand times. The molecules also, as you probably know, are in violent motion. The highest velocity I have obtained with a projectile nearly reached 5000 f.s., but the average velocity of the hydrogen molecules at the temperature and pressure I have named is somewhat more. I once calculated that a few molecules, I forget in how many millions, might exceed 50,000 f.s.

We smile, and justly smile, at the seekers after what was called perpetual motion. Modern science seems to show that it is equally vain to seek for anything that is perpetually and absolutely at rest.

I have alluded to the kinetic theory of gases because we know more of the constitution of that form of matter than we do of any other, but having regard to the progress of science to which I have referred, is it too much to hope that some of you will live to see a second Newton, who will give you a second Principia, which shall clear away the difficulties which surround the constitution of matter whether ponderable or imponderable?

One word more, bring enthusiasm to your studies; without it the best instruction (this you will have) and the best apparatus will do nothing for you. Make your work the first aim, and do not let athletics, or anything else, take precedence of it. Here, again, I cannot help thinking that the Germans get a little the better of us. With them work is absolutely in the forefront; I am not at all sure that it is so with the average young Englishman of to-day. No one appreciates the value of athletics, when kept in their proper place, more keenly than I do. But against the substitution of athletics for the more serious objects of life, I should like to enter my strongest protest, and it will be a sorry day for England if such a change ever takes place.

Lastly, I would say to you, while giving the acquiring of knowledge that may assist your own business or profession the first place, not be too utilitarian, do not narrow the search for knowledge down to a search for utilitarian knowledge, for knowledge that you think will pay. I remember a strong protest of De Morgan's against the number of men who take their station in the business of life without ever having known real mental exertion; he put it that knowledge which ought to open the mind was decided on solely by its fitness to manure the money tree.

Therefore, above all things, pursue knowledge. It is that pursuit which will stand by you to the end as at once the greatest and the most enduring of pleasures. Friends may die; the most tender attachments must be severed; advancing years will very soon debar you from any serious pursuit of athletics; the acquisition of wealth will take away from you the pleasure of "making a position," which is probably the keenest, and surely the most legitimate, incentive of middle life; but the pleasure of acquiring knowledge will console you to the last, so long as you have strength to open a book, or to hold a test-tube. Cry after knowledge; seek for her as silver; and search for her as for hidden treasure.

THE BRITISH ASSOCIATION.

SECTION H.

ANTHROPOLOGY.

OPENING ADDRESS BY C. H. READ, PRESIDENT OF THE SECTION.

THE difficulties that beset the President of this Section in preparing an address are chiefly such as arise from the great breadth of our subject. It is thought by some, on the one hand, to comprehend every phase of human activity, so that if a communication does not fall within the scope of any other of the Sections into which the British Association is divided, it must of necessity belong to that of anthropology. On the other hand, there are many men, wanting neither in intelligence nor education, who seem incapable of grasping its general extent, but, mistaking a part for the whole, are fully content with the conclusions that naturally result from such a parochial method of reasoning. The Oxford don who stated, a year or two ago, his belief that anthropology rested on a foundation of romance can only have arrived at this opinion by some such inadequate process, and the conclusion necessarily fails to carry conviction. The statement was, however, singularly ill-advised, for anthropology gives way to no other branch of science in its reliance upon facts for its existence and its conclusions. Had the reproach been that the facts were often of a dry and repellent character we might have pleaded extenuating circumstances, but I fear it must have been admitted that there was some justice in the complaint, though we could fairly point to instances where master minds have made even the dry bones of anthropology live, and that without trenching upon the domain of romance.

It is not, however, my purpose to-day to enter upon a general defence of anthropology as a branch of science. It has taken far too firm a hold upon the popular mind to need any such help. I intend rather to treat of one or two special subjects with which I am in daily relation, in order to see whether some practical means cannot be found to bring about a state of things more satisfactory than that at present existing.

The first of these branches is that of the prehistoric antiquities of our own country. It will not be denied that there can be no more legitimate subject of study than the remains of the inhabitants of our islands from the earliest appearance of man up to the time when written history comes to the aid of the archæologist. There is no civilised nation which has not devoted some part of its energies to such studies, and many of them under far less favourable circumstances than ours. The chiefest of our advantages is to be found in the small extent of the area to be explored—an area ridiculously small when compared with that of most of the continental nations, or with the resources at our command for its exploration. The natural attractions of our islands, moreover, have also had a great influence on our continental neighbours, so that their incursions have not been few, and no small number of them decided to remain in a country where the necessaries of life were obtainable under such agreeable conditions. The effect of these incursions, so far as our present subject is concerned, is that there is to be found in the British Islands a greater variety of prehistoric and later remains than is seen in most European countries, a fact which should add considerably to the interest of their exploration. At the same time also it must be borne in mind that it is by such researches alone that we can arrive at any true understanding of the conditions of life, the habits and religious beliefs, or the physical characters of the varied races who inhabited Britain in early times.

It may seem unnecessary to urge, in face of these facts, that all such memorials of the past should be, in the first place, preserved; and, in the second, that any examination of them should be undertaken only by properly qualified persons. Unfortunately, however, it has never been more necessary than it is at the present time to insist upon both points, and the fact that these prehistoric remains are scattered impartially over the whole country, with the exception, perhaps, of the sites of ancient forests, makes it almost impossible to devise any special measures for their preservation. An additional difficulty is to be found in the fact that many ancient remains, such as the barrows of the early Bronze Age, are altogether unrecognised as such, and in the process of cultivation have been ploughed down almost to the level of the surrounding surface, until at last the plough scatters the bones and other relics unnoted over the field, and one more document is gone that might have served in the task of reconstructing the history of early man in Britain.

Such accidental and casual destruction is, however, probably unavoidable, and, being so, it is scarcely profitable to dwell upon it. We can, perhaps, with more advantage protest against wilful destruction, whether it be wanton mischief or misplaced archæological zeal. An enlightened public opinion is our only protection against the first of these, and will avail against the second also, but we are surely entitled to look for more active measures in preventing the destruction of archæological monuments in the name of archæology itself. It is a far more common occurrence than is generally realised for a tumulus to be opened by persons totally unqualified for the task either by experience or reading. An account may then be printed in the local journal or newspaper. When such accounts do appear it is often painfully obvious that an accidental and later burial has been mistaken for the principal interment, while the latter has been altogether overlooked, and no useful record has been kept of the relative positions of the various objects found. The loss that science has suffered by this indiscriminate and ill-judged exploration is difficult to estimate, for it should be borne in mind that an ancient burial, once explored, is destroyed for future searchers—no second examination can produce results of any value, though individual objects overlooked by chance may repay the energy of the later comers. So much varied knowledge is, in fact, required for the proper elucidation of the ordinary contents of a British barrow that it is almost impossible for any single person to perform the task unaided. A wide experience in physical anthropology must be combined with an acquaintance fully as wide with the ordinary conditions of such interments and the nature, material, and relative positions of the accompanying relics, all of which must be brought to bear, with discriminating judgment, on the facts laid bare by the digger's spade. Added to this, the greatest precaution is needed that nothing of value be overlooked. In some soils, such as a stiff clay, it is almost impossible to guard against such a casualty, especially when the barrow is of large size and vast masses of earth have to be moved. The amount of profitable care that may be bestowed on scientific work of this character can nowhere be better seen, I am glad to say, than in our own country, in the handsome volumes produced by General Pitt-Rivers as a record of his investigations in the history of the early inhabitants of Dorsetshire. The memoirs contained in them are unsurpassed for scientific thoroughness, and they will probably long stand as the model of what archæological investigation should be. It is very seldom, however, that circumstances conspire so favourably towards a desired end as in the case of General Pitt-Rivers, where a scientific training is joined to the love of research, and finally ample means give full scope for its practice under entirely favourable conditions. While it is, perhaps, too much to expect that all explorations of this character should be carried through with the same minute attention to detail that characterises General Pitt-Rivers's diggings, yet his memoirs should be thoroughly studied before any work of the same kind is entered upon, and should be kept before the mind as the ideal to be attained. It is not too much to say that a diligent study of the works of the two foremost explorers of prehistoric remains in this country—Canon Greenwell and General Pitt-Rivers—will of itself suffice to qualify any intelligent antiquary to conduct the exploration of any like remains. At the same time, it must not be forgotten that exploration is one thing and a useful record of it is another, and here the explorer would do well to invite the co-operation of specialists if he would get the full value out of his work, and there is generally little difficulty in getting such help.

I have ventured to point out, in moderate terms, the dangers to which a large number of our prehistoric sites are liable, and to state under what conditions they should be investigated. It is not unreasonable to expect, if the danger is so obvious, that a remedy should be forthcoming to meet it. In most of the continental States it would be easy to institute a scheme of State control by which such sites would vest in the Government to just such an extent as would be necessary to prevent their being destroyed, and such a scheme might be cheerfully accepted and applied with success in any country but our own. Here, however, we are so accustomed to rely upon individual influence and exertion in matters of this kind, that an appeal to the Government is scarcely thought of; while, on the other hand, the rights of property are fortunately so safeguarded by our tradition and law that nothing but a futile Act of Parliament would have the least chance of passing. Moreover, experience teaches us that it is not to State control that we must look. The Ancient Monuments Bill, which was intended to protect a special class of monuments, and was framed with a full regard to the rights of owners, still stands in the Statute Book, but for years past it has had no effective value whatever. That being so, we must look to private organisations, and preferably to those already in existence, for some effectual moral influence and control, and, in my judgment, the appeal could best be made to the local scientific societies. Many of these are very active in their operations, and could well bear an addition to their labours; others, less active, might become more energetic if they had a definite programme. The plan I would propose is this:—Each society should record on the large scale Ordnance map every tumulus or earthwork within the county, and at the same time keep a register of the sites with numbers referring to the map, and in this register should be noted the names of the owner and tenant of the property, as well as any details which would be of use in exploring the tumuli. I am well aware that a survey of this kind has been begun by the Society of Antiquaries of London, and is still in progress; but this is of a far more comprehensive character, and is, moreover, primarily intended for publication. The more limited survey I now advocate would in no way interfere with it, but, on the contrary, would provide material for the other larger scheme. Once the local society is in possession of the necessary information just referred to, it would be the duty of its executive to exercise a beneficent control over any operations affecting the tumuli, and it may safely be said that such control could in no way be brought to bear so easily and effectively as through a local society.

Some of the arguments in favour of some such protection for our unconsidered ancient monuments have been already briefly stated, and, in conclusion, I would only urge this in their favour, that while the more beautiful monuments of later and historic times are but little likely to want defenders, the less attractive early remains are apt to disappear not so much from want of appreciation as from want of knowledge, and I would repeat that it is from them alone that we can reconstitute the life-story of those who lived in what we may, with truth, call our dark ages.

I will now ask you to turn your attention to another matter in which it seems to me that this country has opportunities of an unusually favourable kind. I refer to the collection of anthropological material from races which still remain in a fairly primitive state. It is somewhat trite to allude to the extent of our Empire and the vast number of races either subject to our rule or who look to us for guidance and protection. The number may be variously computed according to the bias, philological or physical, of the observer, but it will not be contested that our opportunities are without precedent in history, nor that they greatly exceed those of any existing nation. That being so, it may not be useless to see how far these opportunities are utilised. While it will not be denied that the Indian Government and the Governments of some of our Colonies have done excellent work in the direction of anthropological research and publication, and that exhaustive reports from our Colonial officials are frequently received and afterwards entombed in parliamentary papers, yet it is equally clear that work of this kind is not a part of our administrative system, but rather the protest of the intelligent official mind against the monotony of routine. The material, the opportunity, as well as the intelligence and will to use both, are already in existence, and all that is now wanted is that the last should be encouraged, and the work be done on a systematic plan, and, as far as may be, focussed on some centre where it may be available for present and future use. It was for this end that I ventured to bring before the British Association at the Liverpool meeting a scheme for the establishment of a central Bureau of Ethnology for Greater Britain. Frequent appeals had been made to me by officials of all kinds in distant parts of the Empire to tell them what kind of research work they could most usefully undertake, and it seemed a pity not to reduce so much energy and good will into a system. Hence the Bureau of Ethnology. The Council of the Association, on the recommendation of the Committee, invited the Trustees of the British Museum to undertake the working of the Bureau; this they have accepted, with the result that if the Treasury will grant the small yearly outlay it will be under my own supervision. If I had foreseen this ending I might have hesitated before starting a hare the chasing of which will be no sinecure.

It was considered necessary, before attempting to begin the work of the Bureau by communicating with commissioners and other officials in the various Colonies and Protectorates, to lay the matter before Lord Salisbury and to invite his approval of the scheme. The whole correspondence will appear in the Report of the present meeting, but I may be pardoned for quoting one paragraph of the circular letter from the Foreign Office to the several African Protectorates. It is as follows: "Lord Salisbury is of opinion that Her Majesty's officers should be encouraged to furnish any information desired by the Bureau, so far as their duties will allow of their doing so, and I am to request you to inform the officers under your administration accordingly." When it is remembered that this is strictly official phraseology, its tenor may be considered entirely satisfactory, and there can be little doubt that other departments of the Government will recognise the utility of the Bureau in the same liberal spirit. Thus we shall have within a short time an organisation which will systematically gather the records of the many races which are either disappearing before the advancing white man, or, what is equally fatal from the anthropological point of view, are rapidly adopting the white man's habits and forgetting their own.

The Bureau of Ethnology, however, will only perform a part of the task that has to be done. While there is no doubt of the value of knowledge as to the religious beliefs and customs of existing savages, it is surely of equal importance that anthropological and ethnological collections should be gathered together with the same energy. The spear of the savage is, in fact, far more likely to be replaced by the rifle than is his religion to give way to ours. Thus the spear will disappear long before the religion is forgotten. It may be said that we have collections of this kind in plenty, and it is true that in the British Museum, at Oxford, Cambridge, Liverpool, and Salisbury, there are indeed excellent collections of ethnology, while at the College of Surgeons and the Natural History Museum there are illustrations of physical anthropology in great quantity. Whatever might be the result if all these were brought together, there can be no question that no one of them meets the requirements of the time. Here also there is a want of a system that shall at once be worthy of our Empire and so devised as to serve the ends of the student. Where, if not in England, should be found the completest collections of all the races of the Empire? It must be admitted, however, not only that we have no national collection of the kind, but that other nations are ahead of us in this matter. This could be readily understood if their sources of supply were at all comparable to ours. But this is, of course, very far from being the case. The sources are ours in great part, and if we stand inactive it is not unlikely that some will be exhausted when we do come to draw upon them. It is, perhaps, better to give here a case in point rather than to rely on general statements. In the summer of last year I arranged, with the approval of the Trustees, that Mr. Dalton, one of the officers of my department, should make a tour of inspection of the ethnographical museums of Germany, with a definite object in view, but at the same time that he should make a general survey of their system and resources as compared with our own. The report which he drew up on his return was printed and has recently been communicated to the newspapers; it is therefore not necessary to allude to it now, except to quote one instance confirming my statement that it is to a great extent from our Colonies that material is being drawn. Mr. Dalton says: "On a moderate estimate the Berlin collections are six or seven times as extensive as ours. To mention a single point, the British province of Assam is represented in Berlin by a whole room and in London by a single case." But even this, forcible though it is, does not adequately represent the vast difference between the material at the disposal of the two countries. For it is the habit of the collectors for the German museums to procure duplicates or triplicates of every object, for the purposes of exchange or study. It is thus not unlikely that the whole room referred to represents only a part of the Berlin collection from the British province of Assam. In making these observations, I should be sorry if it were thought that I wish to advocate a dog-in-the-manger policy, or that I consider it either desirable or politic to place any restriction upon scientific work in our Colonial possessions, even if such restrictions were possible. I would prefer to look at the matter from an entirely different point of view. If the German people, who are admittedly practical and business-like, think it worth while, with their limited Colonies, to spend so much time and money on the establishment of a royal museum of ethnography, how much more is it our duty to establish and maintain one, and on a scale that shall bear some relation to the magnitude of our Empire. The value of such museums is by no means confined to the scientific inquirer, but they may equally be made to serve the purpose ot the trader and the public at large.

How can we best obtain such a museum? That is the question that we have to answer. It is scarcely profitable to expect that the Government will be stirred to emulation by the description of the size and resources of the Museum für Völkerkunde in Berlin. In the British Museum there is at the present time only the most limited accommodation even for the collections already housed there, and I am well aware that these form a very inadequate representation of the subject.

It may be thought that the solution of this difficulty is easy. It is well known that the Government has purchased the rest of the block of land on which the British Museum stands, and it may seem that such a liberal extension as this will form should be enough for, at any rate, a generation or two, and that a little additional building would meet immediate wants, and enable the collections, now so painfully crowded, to be set out in an instructive and interesting way. I admit that if the whole of the contemplated buildings were at this moment complete, and at least double as much space given to the ethnographical collections as they occupy at present, the difficulty would be much simplified. The collections could at any rate be then displayed far more worthily and usefully. Even this, however, would hardly meet the case, even if there were a certainty of the buildings being immediately begun. Such works as these, however, can only be executed in sections during the course of each financial year. Thus, even if a Chancellor of the Exchequer could be found to fall in entirely with the views of the Trustees, it would still be an appreciable number of years before the completion of the entire range of galleries that is contemplated. For this reason alone I do not look forward to obtaining the space that is even now urgently wanted for some time. Meanwhile the natural and legitimate increase of the collections at the rate of about 1 to 2 per cent. per annum still goes on, and the original difficulty of want of room would still face us, though in a lesser degree. This estimate of the rate of increase may seem a high one; but it should not be forgotten that the science is new, and that it is only within the last few years that such collections have been made on scientific lines, instead of being governed only by the attractive character or rarity of the object. The gaps that exist in such a series as that of the British Museum, made in great part on the old lines, are relatively more numerous than would be the case in museums more recently founded. Another reason, equally cogent, for allowing far more room than is required for the mere exhibition of the objects is that, in my judgment, ethnographical collections, to be of real value, need elucidation by means of models, maps and explanatory descriptions, to a far greater extent than do works of art, which to the trained eye speak eloquently for themselves. Such helps to understanding necessitate a considerable amount of space, though the outlay is fully justified by their obvious utility, and in any general scheme of rearrangement of the national collection they should be considered an essential feature.

There is yet another factor to be considered. It has been the fashion in this country to consider remains illustrating the physical characters of man to belong to natural history, while the productions of primitive and uncultured races generally find a place on the antiquarian side. Thus the skull of a Maori will be found at the natural history branch of the British Museum, while all the productions of the Maori are three miles distant in Bloomsbury. Such an arrangement can perhaps be defended on logical grounds, but its practical working leaves much to desire, and the arguments for a fusion of the two are undoubtedly strong. For instance, the student of one branch would be unlikely to study it alone without acquiring a knowledge of the other, while the explorers to whom we look for collections usually give their attention to both classes of anthropological material. Here again, in such a case, there would be a call for still more space at Bloomsbury.

If I may be permitted to add one more to the requirements of what should be an attainable ideal, I should like to say that courses of lectures on anthropology delivered in the same building that contains the collections would form a fitting crown to such a scheme for a really Imperial museum of anthropology as I have endeavoured to sketch. There is but one chair of anthropology in this country, and admirably as that is filled by Prof. Tylor, he would himself be the first to admit that there is ample room and ample material to justify the creation of a second professorship.

It will be admitted that if my premisses are well founded the conclusion must necessarily be that we cannot look to the British Museum to furnish us eventually with the needful area and other resources for the installation of a worthy museum of anthropology. The difficulties are far too great for the Trustees to overcome, unless by the aid of such an exhibition of popular enthusiasm as I fear our branch of science cannot at present command. Failing the British Museum, which may be called the natural home of such a collection, we must look elsewhere for the necessary conditions, and I think they are to be found, although it is possible that, however favourable these conditions may seem from our point of view, difficulties may already exist or arise later.

It is not the first time that a scheme has been thought out for the establishment of a museum or kindred institution which should represent our Colonies and India. In the year 1877 the Royal Colonial Institute made a vigorous effort in this direction, and, in combination with the various chambers of commerce throughout the country, advocated the building of an "Imperial Museum for the Colonies and India" on the Thames Embankment, with the then existing India Museum as a nucleus. The arguments then brought forward were in the main commercial, but they are, if anything, more forcible now than they were twenty years ago. The competition with foreign countries has become keener on the one hand, while the bonds between the Colonies and the parent country are notoriously closer and more firm than at any previous time. No moment could thus be more opportune than the present for the foundation of a really Imperial Institution to represent our vast Colonial Empire.

The last sentence has, perhaps, given an indication of my solution of the question. The Imperial Institute at South Kensington has now been in existence for some time, and has passed through various phases. But its most enthusiastic supporters will scarcely claim for it entire success in its mission. Whatever may be the underlying causes, it must be admitted that such popular support as it possesses is scarcely founded on the performance of its functions as an Imperial Institute. It would seem, therefore, that something more is wanted—a more definite *raison d'être*—than it has at present, and this I think it will find in being converted into such a museum of anthropology as I have indicated, but, of course, as a Government institution. I am by no means an advocate of the creation of new institutions, if the old ones can adequately do their work, nor do I think that anything but ill would result from a general partition of the contents of the British Museum. The separation of the natural history from the other collections was painful, though inevitable, and no such severe operation can be performed without loss in some direction. But the removal of the ethnographical and anthropological collections from the British Museum to the galleries of the Imperial Institute would possess so many manifest advantages that the disadvantages need scarcely be considered. The Government has already taken over a portion of the building for the benefit of the University of London. The remaining portion would provide ample accommodation for the anthropological museum, as well as for the commercial side, that might properly and usefully be continued; its proximity to the natural history branch of the British Museum would render control by the Trustees easy; the Indian collections, which formed so important a feature in the scheme of 1877, are at this moment under the same roof; and finally the University of London has but to found a chair of anthropology, and the whole of the necessary conditions of success are fulfilled.

I have but little doubt that, wherever it might be placed, the creation of a distinct department of anthropology would of itself tend to the enrichment of the collections. It must be remembered that it is only since 1883, when the Christy collection was removed to the British Museum, that the ethnographical collections there can claim any kind of completeness. Until then one small room contained the few important objects of this kind that had survived from the voyages of Cook, Wallis and the other early voyagers. The public did not expect to find ethnography in the British Museum, and it is, in fact, only within the last few years that it has been generally realised that a gallery of ethnography exists there. If it were placed in such a building as the Imperial Institute, it would still remain part of the British Museum, and be under the guardianship of its Trustees; but it would obviously command more attention and support from the public than can be expected while it remains an integral part of a large institution which has as many aims as it has departments.

I began this address by stating that it would have a practical application. I trust that to others it may seem that what I have ventured to suggest is not only possible of achievement, but would also be beneficial to the branch of science that we represent. I should like to add that, as far as possible, I have tried to state the case as it would appear to one who regarded the situation from an entirely independent standpoint, and wishing only to discover the most practical solution of what must be admitted to be a difficult question. My allegiance to the British Museum, however, may well have tinged my views, unnoticed by myself. There are many other subjects that might well have formed the subject of an address at the present time. On such occasions as these, however, it is, I think, advisable to select a subject with especial reference to the needs of the time, and I know of nothing that is at the present moment more urgent in this particular direction, and in my judgment it will tend greatly towards the true advancement of science, the object we all have at heart.

SECTION I.

PHYSIOLOGY.

OPENING ADDRESS BY J. N. LANGLEY, F.R.S., PRESIDENT OF THE SECTION.

ONE might suppose that physiology, dealing as it does for the most part with structures—such as nerves, and muscles, and glands—which every one has and has heard of, would be eminently a science the newer aspects of which every one could readily understand. And in this supposition one would be encouraged by the frequency of the references in English literature to some part of our inner mechanism. More than a century and a quarter ago we find: "If 'tis wrote against anything, 'tis wrote an' please your worships against the spleen, in order by a more frequent and more convulsive elevation and depression of the diaphragm, and the succussations of the intercostal and abdominal muscles in laughter, to drive the gall and other bitter juices from the gall-bladder, liver and sweetbread of his Majesty's subjects, with all the inimicitious passions which belong to them, down into their duodenums."

It must, however, be recognised that many subjects which are most interesting to the physiologist either involve so much special knowledge, or are so beset with technical terms, that it is difficult to make clear to others even their general drift.

I am not without uneasiness that my subject to-day may be found to fall within this category. I propose to consider some relations of the nerves which pass from the brain and spinal cord, and convey impulses to the other tissues of the body—the motor or efferent nerves; and in especial the relations of those efferent nerves which run to the tissues over which we have little or no voluntary control. It is as well to say at once that none of the general conclusions which I lay before you are encrusted with universal acceptance. One or two have been subjects of controversy for the last fifty years; others are too young to have met even with contradiction. I do not propose to give you an account of the various theories which have been put forward on the questions I touch upon, nor do I propose to point out how far the views I advocate are due to others. I am concerned only to state what seems to me to be the most probable view with regard to certain problems which have been emerging from obscurity in recent year.

Limitations in the Control of the Nervous System over the Tissues of the Body.—In view of the conspicuous manner in which nervous impulses affect every-day life, we are perhaps apt to over-estimate the character and range of the influence exercised directly by the nervous system.

From the early part of this century one way of regarding the body has been to consider it as made up of tissues grouped together in varying number and amount. Each tissue has its characteristic features under the microscope. We need not enter into the question as to which of the commonly recognised tissues of the body are to be regarded as forming a class by themselves and which are to be regarded as subdivisions of a class. The point I wish to lay stress on is that in any broad classification not more than two tissues are known to be supplied with approximate completeness with efferent nerve-fibres. The striated muscular tissue, which forms, amongst other parts of the body, the muscles of the limbs and trunk, receives in all regions nerve-fibres from the brain or spinal cord. And the unstriated muscular tissue, which forms, amongst other parts of the body, the contractile part of the alimentary canal and of the blood-vessels, is in nearly, and possibly in all, regions similarly supplied.

The glandular division of epithelial tissue in some parts responds promptly and strikingly to nervous impulses, but in some parts the response is feeble, and in others no nervous impulse has been shown to reach the tissue. The connective tissue which exists all over the body, and which in its varied forms of connective tissue proper—cartilage, bone, teeth, epithelioid cells—makes so considerable part of it, is in mammals, so far as we know, destitute of efferent nerve-fibres. The epidermic cells, which form a covering for the body, the ciliated cells, the reproductive cells, do not visibly respond to any nerve stimulus. And the myriads of blood corpuscles, which in different ways are in incessant action for the general welfare, are naturally out of range of nervous impulses. According to our present state of knowledge, large portions of the organism live their own lives uninfluenced, except indirectly, by the storms and stresses of the central nervous system. No nervous impulse can pass to them to make them contract or to make them secrete, or to quicken or slacken their inherent activity. The nervous system can only influence them through the medium of some other tissue by changing the quantity or quality of the surrounding fluid.

Regarding, then, the body from the point of view of the control exercised by the nervous system on the other constituents, we have first to recognise that this control is in considerable part indirect only, that the several tissues are in varying degree under direct control, and that different parts of one tissue may be influenced by the nervous system to different extents.

Limitation in the Control of the Nervous System over the different Activities of the Cell.—Even when nervous impulses can strikingly affect the vital activity of a tissue, their action is limited. They cannot modify the activity in all the various ways in which it is modified by the inherent nature of the tissue and the character of the surrounding fluid. Thus the submaxillary gland which pours saliva into the mouth is in life ceaselessly taking in oxygen and giving out carbonic acid; it does this without pouring forth any secretion. So far as we know, no nervous impulse can hasten or retard this customary life of the gland by a direct action upon it without producing other changes. The nervous system can only do this indirectly by modifying the blood supply. The nervous impulse which reaches the gland cells causes them to secrete, to take up fluid on one side and to pour it out on the other, and it does not, and so far as we know it cannot, confine its influence to those changes ordinarily going on in the gland cells. The essential effect of a nerve impulse appears to be to modify the amount of energy set free as work; usually it causes work to be done, as in the contraction of a muscle, or in the secretion of fluid by a gland; sometimes it diminishes the work done, as in the cessation of a heart-beat, or the decrease of contraction of a blood-vessel. Other changes often go on side by side with this setting free of energy as work, but there is no unimpeachable instance in which these other changes take place by themselves as the result of nervous excitation. Physiologists have sought for long years in all parts of the body for nerves—calorific or frigorific nerves—which cause simply an increase or decrease of the heat set free by a tissue; and for nerves—trophic nerves—which cause simply chemical changes in the tissue associated with a setting free of heat or not. Probable as the existence of such nerves seems to be, the search for them cannot, I think, be said to have been successful.

Somatic or Voluntary Tissues.—When we look at the question of nervous control subjectively, and consider in ourselves what tissues are at our beck and call, we find that we have immediate and prompt governance over one tissue only, the one which, as we have already seen, is most universally supplied with efferent nerve-fibres—namely, the (fibrous) striated muscular tissue. The parts of the body composed of this muscular tissue we move, as we say, at will. We exercise a control over it that we cannot exercise over any other tissue. The tissue is supplied with a special system of nerves. In other vertebrates there is a tissue of similar microscopical characters, and having a similar system of nerves. And we can be certain that in all vertebrates the fibrous striated muscle and the nervous system belonging to it form a definite portion of the body which can be properly placed in a class apart from the other tissues of the body. The tissues in this class are spoken of as "somatic" tissues, or sometimes, in view of our own sensations, as "voluntary." "Voluntary" is not a word which physiologists care much to use in this context, because it readily gives rise to misconceptions. It will serve, however, if we bear in mind that the primary distinguishing characters of the system are microscopical, anatomical and developmental; that other tissues than "voluntary" can be put in action by the will, though in a different fashion; and that "voluntary" tissues are also put in action involuntarily. That is to say, the word will serve if we rob it of much of its ordinary meaning.

The somatic or voluntary nervous system has in its essential features long been known. We may leave it and pass on to a more obscure field.

Autonomic or Involuntary Tissues.—In putting on one side the voluntary system, you will notice that we have disposed of one only of the several tissues, differing microscopically from one another, which go to make up the various organs of the body. Of the rest some, as we have said, either do not receive nerve-fibres from the brain and spinal cord, or, if they do, practically nothing is known about them in our own class of vertebrates—the mammalia. These I shall say a word or two about later. For the present we must confine our attention to the tissues which are known to be supplied not too illiberally with nerve-fibres. These are unstriated muscle, and its allied cardiac muscle, and certain glands. Since the voluntary striated muscle has a nervous system of its own, it might be imagined that the unstriated tissue and the glandular tissue, differing as they do, would also have separate nervous systems. This, however, is not the case. The nervous supply of these two tissues have common features and belong to the same system. There is, unfortunately, no satisfactory term by which to designate it. On the whole the term "autonomic" seems to me best adapted for scientific use. But it is not of the first importance for our present purpose to insist upon a proper nomenclature, so that I think I shall not do much harm if I use the familiar "involuntary" for the unknown, or nearly unknown, "autonomic."

I need hardly point out how widespread are both the glandular and the unstriated muscular tissues. In man practically the whole surface of the skin is supplied with sweat-glands, lachrymal glands lie hid behind the eye, small glands are thick in the respiratory tract from the nose to the smaller bronchial tubes, and glands stretch along the whole of the digestive tract. Most of these can be set in action by nerve-fibres. There are a number of others in which such action has not been shown, so that they do not concern us at present. Unstriated muscle forming, as it does, part of the walls of the arteries and veins, penetrates to every part of the body. It forms a large part of the coats of the stomach and intestines; it is present in the spleen and in parts of the lymphatic vessels; it is present in the iris and in other parts of the eye; it occurs in greater or less amount in different animals in the deeper layers of the skin.

Consider some of the ways in which these tissues in the several organs or structures affect the working of the body. The heart contracts and supplies the driving force for the circulation of the blood; the arteries contract less or more, here or there, and regulate the amount of the blood to each region; the digestive tract secretes solvent and disintegrating fluids in the food, churns it into pulp, absorbs some and rejects the rest; the skin-glands pour out their tiny beads of perspiration, and so aid in regulating the temperature of the body; the iris commands the aperture of the pupil and determines the amount of light falling on the retina; the ciliary muscle, by its varying contraction, brings about the focussing necessary for distinct vision.

But the involuntary tissues do not confine themselves to actions of such flagrant utility as those just mentioned. The contraction of small bundles of unstriated muscles in the skin will cause the flesh to creep; other similar small muscles are attached to the hairs; 'tis these will make

> "Thy knotted and combinèd locks to part,
> And each particular hair to stand on end,
> Like quills upon the fretful porpentine."

The involuntary tissues, although not under the prompt and immediate control of the will, are under the control of the higher centres of the brain. They are particularly responsive to the emotions; and in so far as we can call up emotions, we can play upon them at will. The ease with which nervous impulses pass along given tracts depends, amongst other things, upon use. And so it appears that our great-grandfathers wept and our great-grandmothers fainted with an ease which we should require assiduous practice to attain.

Further, you may note that the contraction of involuntary muscle caused by an emotion may in its turn set up nervous impulses, which pass back to the brain and give rise to vague and curious feelings, feelings often lending themselves to effective literary expression:—

> "Where our heart does but relent, his melts; where our eye pities, his bowells *yearn*."

I must ask your forgiveness for mentioning so many well-known facts in the sketch which I have just given of the involuntary tissues. But I hope it will take from you all excuse for not understanding the rest of what I have to say.

The arrangement of the involuntary nervous system presents some peculiar characters. The most distinctive of these is that the nerves, after they leave the brain or spinal cord, do not run interruptedly to the periphery; they end in nerve-cells, and the nerve-cells send off the fibres which run to the periphery. The most direct proof of this lies in the fact that a certain amount of nicotine prevents the central nervous system from having any influence on the peripheral structures—*i.e.* the line is somewhere blocked; it can be shown, speaking generally, that there is no block on either side of the ganglia, so that it must be in them. The actual point of attack of the nicotine appears to be the connections made by the central nerve-fibres with the peripheral nerve-cells. Thus all nerve-impulses, which pass from the brain or spinal cord to unstriated muscle or glandular tissue, pass through an intermediate station on their way. In this, as in some other respects, the arrangement of the involuntary nervous system is more complex than that of the voluntary nervous system ; in the latter the motor nerve-fibres run direct to the tissue and have no nerve-cells on their course. The nerve-cells which form the intermediate stations for the involuntary nerve-fibres are grouped together into ganglia ; and so we may call the nerve-fibres which run from the brain or spinal cord to the nerve-cells pre-ganglionic fibres, and the nerve-fibres which run from the ganglia to the peripheral tissues post-ganglionic nerve-fibres.

The involuntary nervous system is divided into at least two subdivisions. The most extensive of these is what is called the *sympathetic nervous system.* The pre-ganglionic fibres of the sympathetic arise from a limited portion of the spinal cord. They arise from that part of the spinal cord which is in the region of the chest and the small of the back—*i.e.* roughly from the part which lies between the origin of the voluntary nerves for the arms and the origin of the voluntary nerves for the legs. The fibres given off by the ganglia of this system—*i.e.* the post-ganglionic fibres—run to the involuntary tissue in all parts of the body.

The Cranial and Sacral Systems.—The second division of the involuntary nervous system consists of two parts : one part—the cranial—arises from the brain—*i.e.* above the origin of the sympathetic ; the other—the sacral—arises from the end of the spinal cord—*i.e.* below the origin of the sympathetic.

Each supplies a limited and different part of the involuntary tissue of the body, but both together supply a portion only of it. Taking the distribution broadly, they supply the muscular coats of the alimentary canal and certain structures connected developmentally with the anterior and posterior portions of it. They are especially connected with these terminal portions ; they send numerous nerve-fibres to them ; whereas they send but few to the intervening portion, and none at all to its blood-vessels. Thus parts of the involuntary tissue of the body receive a double supply of nerve-fibres, whilst parts receive a single supply only. Amongst the latter are all the involuntary tissues of the skin, the blood-vessels of the limbs and trunk, and of most of the viscera.

The cranial and sacral divisions of the involuntary nervous system are considered by some observers to be simply portions of the sympathetic system separated from it by the development of the nerve-centres for the arms and for the legs. I may give one reason why I do not take this view. The middle portion of the spinal cord, which is the region that sends fibres to the sympathetic, always sends fibres to a given spot by more than one nerve, and usually by four or five. The fibres passing by the several spinal nerves never differ in the kind of effect they produce, but only in the degree of effect ; the difference is in quantity and never in quality. If, then, regions above and below were mere separated parts of this sympathetic region, we should expect that when one of these regions and the sympathetic region sent nerves to the same spot, the effect produced by both sets of nerves would be the same in kind, though it might differ in extent. But this is often not the case. Thus certain blood-vessels may receive nerve-fibres from four spinal nerves in the sympathetic region and from three spinal nerves in the sacral region ; all the former cause contraction of the blood-vessels, all the latter cause dilation. And thus it seems to me probable that in the evolution of mammals the sympathetic nerves have developed at one time, and the cranial and sacral involuntary nerves have developed at another time.

Inhibition.—A striking feature of the involuntary nervous system is its possession of nerve-fibres which, when excited, stop some action at the time going on. The most striking example is perhaps the cessation of the heart-beats brought about by excitation of the vagus nerve. Such nerve-fibres are called inhibitory nerve-fibres, and the stopping of the action is called inhibition.

So far as has been definitely proved inhibitory nerve-fibres only run to involuntary muscle and to nerve-cells, and to these, so far as has been certainly shown, only in particular cases. It is true that when fear or other emotion causes the tongue to cleave to the roof of the mouth, there is a cessation of the customary flow from certain glands, but this flow is itself the result of nervous impulses passing in ever rising and falling intensity from the central nerve-cells, and its cessation is due to inhibition of nerve-cells, and not to inhibition of glandular cells.

The inhibition of nerve-cells has only been proved to take place in the central nervous system. When a group of nerve-cells of the central nervous system is engaged in sending out nervous impulses, other nervous impulses reaching them by way of other nerve-cells may diminish or stop their activity. The theory which is commonly advocated now to explain this inhibition makes the activity of the nerve-cells depend upon their receiving stimuli from the minute endings of other nerve-cells, and the cessation of the activity to depend upon these minute endings, either withdrawing themselves out of range, or having something interposed between them and the nerve-cells, so that the impulses can no longer pass. This theory I do not wish to discuss to-day ; it is sufficient to say that if it is true, the inhibition of nerve-cells is an entirely different process from that of the inhibition of involuntary muscle.

Turning to the inhibition of involuntary muscle, there is a source of confusion which we must first guard against. Nearly all the unstriated muscle in the body is kept in a state of greater or less tone, or contraction, by the central nervous system. A diminution or cessation of this contraction may then be caused by a diminution or cessation of the activity of the central nervous system. This cessation of contraction is, of course, not what we mean by an inhibition of the unstriated muscle. It is usually spoken of as an inhibition of the nervous centre. The inhibition we mean is that which is caused by stimulating the peripheral end of a nerve outside the spinal cord.

I have said that this inhibition can only be obtained in certain cases, and it is not easy to find anything in common with regard to these cases. But on the whole it appears that the more a tissue is able to work by itself, the more likely it is to be under the control of inhibitory fibres. The heart, stomach and the intestines work when no longer connected with the central nervous system, and these are especially liable to inhibition.

There has been a marked tendency amongst physiologists, in considering the question of inhibitory nerve-fibres, to take what may be called *the view of the equal endowment of the tissues.* Because some arteries have inhibitory nerve-fibres, therefore it is to be held as in the highest degree probable that all have. And many would go further and say that it is therefore in the highest degree probable that all unstriated muscle, and glands, and even the voluntary muscles, have such fibres. This view seems to me a mistaken one. There is hardly room for doubt that the motor fibres are supplied in most unequal measure to the unstriated muscle and glands of the body. There are veins in the body containing unstriated muscle, which show no visible contraction from any nerve stimulation. And there are a number of glands which no nerve—so far as we know—excites to secretion. Since in the course of the evolution of the organism, a universal development of motor fibres has not occurred, it is, I think, to be expected that the development of inhibitory fibres should be still less universal. For up to a certain point the results of inhibition can be obtained in most cases without inhibitory nerve-fibres, by a simple diminution in the impulses travelling down the motor fibres. The only, and the final, test is of course experiment. But not all experiments are decisive, and theory inevitably colours interpretation. This theory of the equal endowment of the tissues has, it seems to me, caused a number of quite inconclusive experiments to be accepted as offering satisfactory evidence for the existence of inhibitory nerve-fibres.

Passing from this question, we may consider briefly how far we can get on the way to understand what occurs during inhibition. The external characteristic feature of inhibition is that a certain state of activity ceases ; a muscle contracting at short intervals ceases to contract, or a muscle in a steady state of contraction loses this state. The tissue in either case becomes flabby.

The activity of a tissue may obviously be due to its receiving some stimuli from the nervous system or to its own inherent qualities. In the former case, if the tissue were only active when receiving nervous impulses, we should naturally look to some interference with these impulses as being the cause of inhibition. The blood-vessels of the sub-maxillary gland appear to me to offer sufficiently clear evidence with regard to the inhibition of blood-vessels. The superior cervical ganglion is the local centre from which the nerve-fibres bringing about contraction run to the blood-vessels of the gland. When this ganglion has been removed and the nerve-fibres from it have degenerated, the vessels receive no nervous impulses causing them to contract. But stimulation of the inhibitory nerve will still cause dilation—*i.e.* inhibition of the blood-vessels. The inhibition must then be due to a direct action on the tissue, and not to an interference with other nerve impulses. The evidence with regard to the inhibition of the beat of the heart and of the tone or peristalsis of the alimentary canal is more complex, but there is good reason to believe that the contraction is in both cases due to their inherent qualities. And if this be granted, it follows that here also inhibition must be due to a direct action upon the tissue.

The contraction of a muscle is due to a chemical change in it. In this chemical change some energy is set free as work—shown by the contraction of the muscle—and some as heat. It is conceivable that the nervous stimulus which causes inhibition should cause all the energy set free by the chemical change to take the form of heat. In that case the inhibitory nerve would be a calorific nerve. The amount of chemical change is indicated by the amount of carbonic acid given off to the blood. No experiments have been made as to the amount of carbonic acid given off to the blood by an inhibited tissue, but it appears very unlikely that the amount is increased, and we may take this view of the action of an inhibitory nerve as improbable.

If the nervous impulse does not act in this way it must in some way stop the particular chemical change associated with contraction from taking place. It does not stop all chemical change, for blood passing through an inhibited tissue loses some of its oxygen. The simplest way for a nervous impulse to prevent a particular chemical change is to induce a different one. We have seen that the tissues which are inhibited have a great tendency to contract of themselves—that is, they form certain very unstable substances. In closely related tissues which are not inhibited this tendency exists but little or not at all. The proximate cause of inhibition might then be that the nervous stimulus causes certain molecules of the tissue to form more stable combinations. This need not be associated with any general assimilation; it would simply make the muscle adopt for a time a mode of life more like that of other closely related muscle.

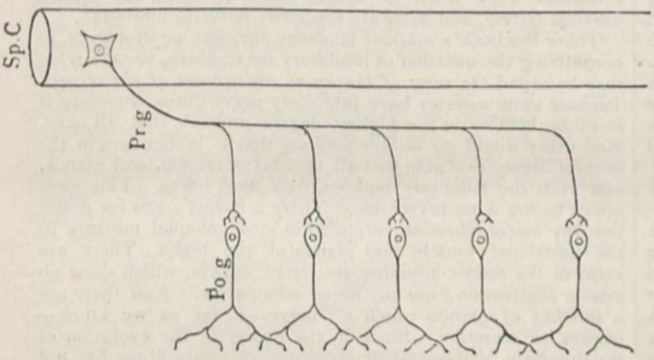


FIG. 1.

Number of Relay Stations.—I have already mentioned that the nerve-fibres which pass from the central nervous system to the involuntary tissues do not run to it direct, but end in groups of nerve-cells or ganglia from which fresh nerve-fibres are given off. Now, in most cases, there are anatomically several ganglia on a nerve in its course from the spinal cord to the periphery. For example, the nerve-fibres which cause the hairs of a cat's tail to stand on end, giving the tail the appearance of a bottle brush, leave the spinal cord in the lower part of the back, and enter a nerve-strand which is beaded with ganglia. They leave this strand near the root of the tail. Between the point where the nerve-fibres enter and the point where they leave the strand there are seven or eight ganglia. The fact offers us a problem of some difficulty. With how many of these ganglia are the nerve-fibres connected? Or, in other words, how many relay stations are there—eight or one, or some intermediate number? Further, do all kinds of involuntary nerve-fibres in all parts of the body have the same number of relay stations, or do some have one, some two, some three, and so on? It would take too long to discuss this question here. But the experimental evidence is, I think, fairly decisive in favour of the simple view that the nerve-impulse passes through one relay station only. There is, however, evidence that the nerve-fibres which pass from the spinal cord branch, so that we may take the element by reduplication of which the involuntary nervous system is built up to be diagrammatically as in Fig. 1.

Reflexes.—Another point of view is given by a comparison of the groups of nerve-cells of the peripheral ganglia with the groups of nerve-cells of the brain and spinal cord. The proper working of the body depends upon an agile response by the central nervous system to what is going on in the periphery. Now the peripheral ganglia are made up of nerve-cells and nerve-fibres which differ less in general characters from some of the cells of the central nervous system than these differ from one another. The nerve-cells of the spinal cord can receive impulses from many groups of nerve-cells both near and remote; they do not simply receive impulses from one quarter alone—say, the cortex of the cerebral hemispheres—but from many quarters, and notably direct from the periphery. Hence it has been supposed that the peripheral ganglia have similar wide connections, that they receive impulses direct from the periphery, that each is connected with other ganglia, and that impulses received from the periphery, or elsewhere, bring separate ganglia into coordinate action. And this view, which has been taken on general grounds, has been supported by microscopical observations.

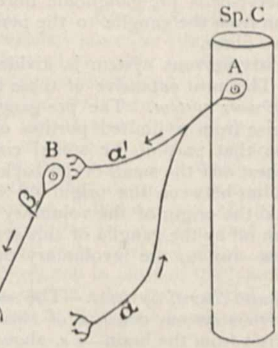


FIG. 2.

The evidence against this view is of two kinds. In the first place, it can be shown that in a number of individual cases the nerve-cells of one ganglion have no connection with the nerve-cells of another ganglion, so that anything like a universal scheme of connection is out of the question. And, secondly, it can be shown that whenever an action occurs, which might be referred to such connection, it is an action which is bound to occur in consequence of some other known arrangement, and that therefore it is unnecessary to seek for a further cause.

The evidence of the first kind we need not enter into; the evidence of the second kind we may hastily touch on. If we accept the conclusion stated above, that the pre-ganglionic nerve-fibres branch, and the branches run to different nerve-cells, it follows that a stimulus applied to one branch will stimulate a number of nerve-cells; this follows since a nerve-impulse set up in any part of a nerve travels over the whole of it. Thus actions, resembling reflex actions, will inevitably be obtained whenever nerve-fibres are stimulated which send branches to different ganglia. The mechanism in this case is confined to motor nerve-fibres and nerve-cells. The action, for lack of a con-

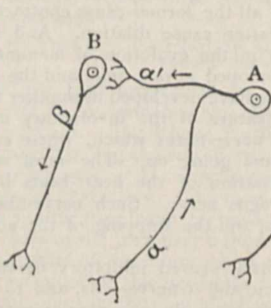


FIG. 3.

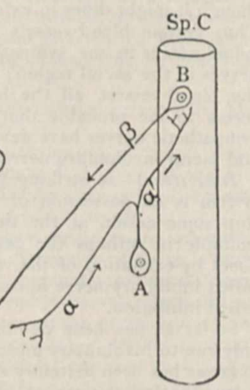


FIG. 4.

venient term, was spoken of by Dr. Anderson and myself as a reflex action. It is perhaps better to call it a *pseudo-reflex action*.

Regarded from the customary point of view, a pseudo-reflex differs widely from a reflex action. The one is brought about by stimulating an efferent or motor fibre, and the other by stimulating an afferent or sensory fibre.

But suppose we compare them from another point of view. Fig. 2 is a diagrammatic representation of a pseudo-reflex. A nervous impulse passes up one branch α of a cell A, passes to another branch α', so excites a cell B and its nerve-fibre β.

Fig. 4 is a diagrammatic representation of a simple true reflex in the voluntary muscle. A nervous impulse passes up one branch α of a cell A, passes to another branch α', so excites a cell B and its nerve-fibre β.

You see the two can be described in exactly the same terms, and both are reducible to the diagram of Fig 3. It is true that the cells A and B are not similarly situated in the two cases; in the pseudo-reflex A is in the spinal cord, and B is outside it in a peripheral ganglion; whereas in the true reflex A is outside the spinal cord, in a spinal ganglion, and B is inside the cord. But then no one has even suggested that the position of a nerve-cell determines whether an action in which it takes part is a reflex or no. So that this point is irrelevant. And so it might be urged that the one action has as good a title to be called a reflex as the other. I do not, however, wish to insist too much on this comparison. I am inclined to say, after Touchstone, "An ill-favoured thing, sir, but mine own."

If, as some think is the case, the spinal ganglion cell receives the nerve-impulse conveyed by the peripheral nerve process, and modifies it before passing it on to the central process, this establishes a distinguishing character for the true reflex. It would be probably an axon plus dendron reflex, the pseudo-reflex being simply an axon reflex. The important known functional difference between the reflex and the pseudo-reflex is that in the former case the nerve-endings of the primarily affected nerve-fibre are specially differentiated for receiving nerve-impulses, and in the latter case these endings are specially differentiated for imparting nerve-impulses. And, on the whole, it is probable that the pseudo-reflex is not a normal part of the working of the body, but comes into play only as it were by accident. I do not, however, regard this as quite certain.

The pseudo-reflex I have spoken of is caused by the excitation of nerve-fibres before they reach the ganglia—*i.e.* of pre-ganglionic fibres. But the fibres which are given off by the ganglia also branch, so that it appears inevitable that we should have in certain circumstances an action related to a reflex caused by a stimulation set up in one of these branches spreading to the rest—*i.e.* a spreading out of impulses in post-ganglionic fibres similar to that which occurs in pre-ganglionic fibres. Turning to the diagram, Fig. 1, a nervous impulse set up in one branch—possibly by a contraction of muscle-cells to which it runs—would spread to other branches and cause contraction of the muscle-cells in connection with them. You will notice that this spreading out of impulses does not necessarily involve the stimulation of any nerve-cell; it might perhaps be distinguished as *irradiation*. It would, probably, be very local in action, unless there were overlapping of the districts supplied by the several nerve-cells, in which case a not inconsiderable spreading out of a local contraction might take place, giving rise to a peristaltic wave.

It must be pointed out that it has been assumed that in the sympathetic nervous system an impulse cannot pass from a motor fibre through the nerve-cell from which the fibre arises and affect any other nerve-fibre or nerve-cell. There is good ground for this assumption, but the experimental evidence might certainly be more complete.

To return to our main line of argument, we have good evidence that nervous impulses set up in one spot may affect regions more or less remote by a mechanism which does not involve the presence in the sympathetic system of special sensory nerve-cells with peripheral sensory nerve-endings. And so far as investigation has gone at present, I think that all the apparent reflex actions can be explained without reference to such sensory apparatus. And so I take the analogy of the peripheral ganglia with the central nervous system to be misleading, and consider that all the nerve-cells of which we have been speaking are motor nerve-cells, and that they all conform to the simple plan shown in Fig. 1. Thus the whole consists of a duplication of one type; a cell in the spinal cord which branches, each branch ending in a single cell; each of these cells sends off a nerve-fibre which branches, the branches ending in a group of involuntary muscle or gland cells.

That I regard as the real working mechanism, but there are two reservations to make. All the tissues of the body may be looked upon as engaged in a lifelong process of carrying out experiments, and I am prepared to believe that there are in the body what may be spoken of as the residues of these natural physiological experiments, either the beginnings of experiments which have not succeeded, or the melancholy ends of those which once partially successful have failed later. Such possibly may be the nerve-cells which have been described in sympathetic ganglia as sending their nerve-fibres to other nerve-cells.

Secondly, in this account I have not included the nerve-cells which exist in the wall of the alimentary canal, and the cells of Auerbach's and Meissner's plexuses. These "enteric" nerve-cells belong, I hold, to a system different from that of the other peripheral nerve-cells. With regard to their connections I do not think anything can be said with certainty.

Regeneration. Specific Nerve Energy.—One other problem presented by this involuntary system we may say a few words about. You know that when a nerve in the hand or arm is cut the nerve will in proper conditions grow again; and the lost feeling and the lost power over the muscles will return. The recovery is brought about by the part of the nerve which is attached to the spinal cord growing along its old track and spreading out as before in the muscle, skin and other tissue. At any rate, that is the method for which there is most evidence. You may know also that when the nerve-fibres in the spinal cord are similarly injured, they do not recover function. Regeneration in the latter case implies that the nerve-fibres have to form fresh endings in connection with nerve-cells. If this were more difficult than the formation of nerve-endings in muscle and other non-nervous tissues, the difference which exists as regards recovery of function between the nerve-fibres of the limb and nerve-fibres of the spinal cord would be readily explainable. But recent experiments show that the nerve-fibres which run from the spinal cord to the peripheral ganglia—*i.e.* pre-ganglionic fibres—re-form with ease their connection with nerve-cells, so that we may probably seek in mechanical conditions for the reason of the absence of regeneration of the fibres in the spinal cord. Possibly some way may be found of improving the mechanical conditions, and so obtaining regeneration. That question, however, we need not enter into.

The regeneration of the pre-ganglionic nerves presents some very remarkable features. The nerve-fibres which end in a sympathetic ganglion are rarely, if ever, all of one kind—that is to say, they do not all produce the same effects. Thus, of those which run to the ganglion in the upper part of the neck, some cause the eyelids to move apart, some cause the pupil to dilate, some cause the face to become pale, some cause the glands of the mouth or skin to secrete, and others have other effects. These different kinds of nerve-fibres run, in large part at any rate, to different nerve-cells in the ganglion. There are in the ganglion several thousands of nerve-cells closely packed together. And it would seem hopeless for each kind of nerve-fibre as it grows again into the ganglion during regeneration to find its proper kind of nerve-cell. Nevertheless, nearly all of them succeed in doing this. The nerve-fibres which normally cause separation of the eyelids, or dilatation of the pupil, or pallor of the face, or secretion from the glands, produce the same effects after several inches of their peripheral ends have formed anew.

The fact offers at first sight a striking proof of a specific difference between the different classes of nerve-fibres and different classes of nerve-cells. Through the matted mass formed by the delicate interlacing arms of the nerve-cells, the ingrowing fibres pursue their tortuous course, passing between and about hundreds of near relations until they find their immediate stock, whom they clasp with a spray of greeting tendrils and so come to rest.

Absolute laws seem unfitted for a workaday world. For closer observation shows that the fibres have not always this marked preference for their own stock. The nerve-fibres of the cervical sympathetic, the nerve I have spoken of above, do not often go astray, at any rate so far as is known. But they do sometimes; thus it may happen that some nerve-fibres which ought to find their home with nerve-cells governing the blood-vessels, take up with nerve-cells governing the dilator structures of the pupil.

And if we turn to other nerves, greater aberrations are found. We have seen that the nerves running from the central nervous system to involuntary structures may be divided into two sets: the sympathetic nerves on the one hand, and the cranial and sacral nerves on the other. An important cranial nerve is the vagus; it causes, when in action, cessation of the heart-beat, contraction of the œsophagus, contraction or inhibition of the stomach, and various other effects. It does not send nerve-fibres to any of those structures of the head which we have seen the sympathetic ganglion at the top of the neck—the superior cervical ganglion—so liberally supplies. And yet the vagus nerve, if it has a proper opportunity of growing into the superior cervical ganglion, will do so, and there establish connections with the nerve-cells. Thus the nerve which properly exercises control over certain viscera in the thorax and abdomen is capable of exercising control over structures in the head, such as the iris, the blood-vessels and the glands. The details of the process, with which I will not trouble you, do not afford any clear evidence that the nerve-fibres of the vagus pick and choose amongst the nerve-cells of the superior cervical ganglion; the fibres appear rather to form their terminal branches around any kind of nerve-cell, so that, in fact, the action which the nerve-fibre will in future bring about depends, not on any intrinsic character of its own, but upon the nature of the action carried on by the nerve-cell. The nerve-cell may cause secretion from a gland, or contraction of a blood-vessel, or dilation of the pupil, or movement of hairs; whichever action it causes, the nerve-fibre which joins it from the vagus nerve can cause for the future, and it can cause no other. In this case, then, we arrive at results which are hopelessly at variance with the view that the nerve-fibres and nerve-cells of the involuntary nervous system are divided into classes which are fundamentally different. In other words, that theory which is spoken of as the theory of specific nerve-energy does not apply here.

But if this is so, how are we to account for the selective power shown by the sympathetic nerve-fibres which I have mentioned earlier? That the different classes of nerve-fibres and nerve-cells with which we are dealing have not those deep and inherent differences which are required by the theory of specific nerve-energy is, it seems to me, certain. Nevertheless, there may be some differences of a comparatively superficial nature which suffice to explain the selective activity observed. We may suppose that a re-growing nerve-fibre will in favourable circumstances join a nerve-cell the function of which is the same as that of its original cell, but that if there are hindrances in the way of this return to normal action, and if the conditions are favourable for joining a nerve-cell acting on some other tissue, why then it will join this. It is as if it had a preference, but did not care overmuch. We might perhaps express the facts by saying that there are different varieties of pre-ganglionic fibres, but no species.

We have been speaking so far of the nerve-fibres which run from the brain and spinal cord to the peripheral nerve-cells. The nerve-fibres which run from the peripheral nerve-cells have also, there is reason to believe, a large measure of indifference as to the kind of work they perform. The limits of this indifference have yet to be investigated.

I have said earlier that in mammalia nerve-fibres are not known to run to connective-tissue cells or to epidermic cells. But in some lower vertebrates certain connective-tissue cells are under the control of the central nervous system. Thus in the frog the pigmented connective-tissue cells, which play a large part in determining the colour of the skin, can be made to contract or to rearrange their pigment granules—and so change the colour of the skin—by excitation of certain nerves. In all probability, the motor nerve-fibres to the pigment-cells belong to the same class as the nerve-fibres which run to the arteries and to the glands—*i.e.* they belong to the autonomic system. We have seen that unstriated muscle-cells and gland-cells in different parts of the body are by no means equally supplied with motor nerve-fibres, and it may be that in mammals there are certain connective-tissue cells which receive motor nerve-fibres. Further, if it is true, as it well may be, that nerve-fibres which run to a gland are capable in favourable conditions of making connections with a blood-vessel, it is not beyond hope that either kind of nerve-fibre may experimentally, by offering it favourable conditions, be induced to join connective-tissue cells.

The factors which determine whether a particular tissue or part of a tissue is eventually supplied with nerve-endings, and the degree of development of these, are the factors which determine evolution in general. In the individual, it is exercise of function which leads to the development of particular parts; in the race, it is the utility of this development which leads to their preservation. And so it is conceivable that in some lower vertebrate at some time, the autonomic nervous system may have developed especially in connection with those tissues which appear in ourselves to be wholly unprovided with motor nerve-fibres.

I am tempted, before ending, to make a slight digression. Those who have occasion to enter into the depths of what is oddly, if generously, called the literature of a scientific subject, alone know the difficulty of emerging with an unsoured disposition. The multitudinous facts presented by each corner of nature form in large part the scientific man's burden to-day, and restrict him more and more, willy-nilly, to a narrower and narrower specialism. But that is not the whole of his burden. Much that he is forced to read consists of records of defective experiments, confused statement of results, wearisome description of detail, and unnecessarily protracted discussion of unnecessary hypotheses. The publication of such matter is a serious injury to the man of science; it absorbs the scanty funds of his libraries, and steals away his poor hours of leisure.

Here I bring my remarks to a close. I have endeavoured to give as clearly as possible what seem to me to be the conclusions which logically follow from certain data, but I would not have you believe that I regard them as representing more than the immediate point of view. As the wise man said: "Hardly do we guess aright at things that are upon earth, and with labour do we find the things that are before us."

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE presidential addresses delivered before the Sections of Zoology and Botany of the American Association, by Profs. S. H. Gage and C. R. Barnes, respectively, are printed in *Science* of September 8. The subjects were "The Importance and the Promise in the Study of Domestic Animals" and "The Progress and Problems of Plant Physiology" and the subjoined extracts show some of the points dealt with. Abstracts of several other sectional addresses have already appeared in NATURE, p. 515.

Prof. Gage described a few ways in which the study of domestic animals has thrown light on the problems confronting mankind in his social ideals, in preventive medicine, in physiology and hygiene, in embryology and comparative anatomy and in the doctrine of the evolution of organic forms. He showed that, with the higher forms at least, that is the forms most closely related to man, and with whose destiny his own economic, hygienic and social relations are most closely interwoven, the domestic animals have in the past and promise in the future to serve the best purpose because of the abundance of the material in quite widely separated groups of animals which long have been and still are under greatly differing conditions and surroundings; and, finally, because this material is plentiful and under control, and thus may be studied throughout the entire life cycle.

There has been and still is too great a tendency in biology to study forms remote and inaccessible. This is, perhaps, partly due to the fascination of the unknown and the distant, and the natural depreciation of what is at hand. But study of these supposedly generalised types has proved more or less disappointing. No forms now living are truly primitive and generalised throughout. They may be in parts, but in parts only. The stress of countless ages has compelled them to adjust themselves to their changing environment, to specialise in some directions so far that the clue through them to the truly primitive type is very much tangled or often wholly lost. Indeed, every group is in some features primitive.

As any complete study requires much material at all stages the higher forms must be of the domesticated groups, or wild forms must be practically domesticated for the time being to supply the material.

It may be objected, also, that in the investigation of domesticated forms sordid interests will play too prominent a part. No doubt to the true scientific man the study of zoology for its own sake, that is, for an insight into the fundamental laws of life, is a sufficient incentive and reward. Judging from the past, the study of the domestic animals in any other way than in a scientific spirit and by the scientific method will prove barren; but studied in that spirit and by that method the result has always justified the effort, and has thrown as much, if not more, light upon biological problems than an equally exact study of a wild form.

Therefore, while purely practical ends can never supply the inspiration to true scientific work, still surely no scientific man could feel anything but happiness that his work had in some ways added to the sum of human well-being. Perhaps no one has expressed so well the sympathy of a scientific man with his fellow-men as Pasteur in the preface to his work on the silkworm diseases: "Although I devoted nearly five consecutive years to the laborious experimental researches which have affected my health, I am glad that I undertook them. . . . The results which I have obtained are perhaps less brilliant than those which I might have anticipated from researches pursued in the field of pure science, but I have the satisfaction of having served my country in endeavouring, to the best of my ability, to discover a remedy for great misery. It is to the honour of a scientific man that he values discoveries which at their birth can only obtain the esteem of his equals, far above those which at once conquer the favour of the crowd by the immediate utility of their application; but in the presence of misfortune it is equally an honour to sacrifice everything in the endeavour to relieve it. Perhaps, also, I may have given young investigators the salutary example of lengthy labours bestowed upon a difficult and ungrateful subject."

In conclusion Prof. Gage summarised his address by saying: However necessary and desirable it may have been in the past that the main energy of zoologists should be employed in the description of new species and in the making of fragmentary observations upon the habits, structure and embryology of a multitude of forms, I firmly believe that necessity or even desirability has long since passed away, and that for the advancement of zoological science the work of surpassing importance confronting us is the thorough investigation of a few forms from the ovum to youth, maturity and old age. And I also firmly believe that, whenever available, the greatest good to science, and thus to mankind, will result from a selection of domesticated forms for these thorough investigations.

In the Section of Botany, Prof. Barnes discussed the chief features of plant physiology in which notable progress has been making during the last decade. The great advances in plant chemics and physics; the progress in the investigation of causes of plant form: the widening ideas of the property of irritability; the investigation of the social relations of plants, and the minute study of cell action in spite of their diversity, have one great end in view. This is nothing less than the solution of the great problem—the fundamental problem—of plant physiology as of animal physiology, namely the constitution of living matter. Entrenched within the apparently impregnable fortress of molecular structure this secret lies hid. The attacks upon it from the direction of physical chemistry and physiological morphology, of irritability, of ecology and of cytology are the concentrating attacks of various divisions of an army upon a citadel some of whose outer defences have already been captured. The innumerable observations are devised along parallel lines of approach, and each division of the army is creeping closer and closer to the inner defences, which yet resist all attacks and hide the long-sought truth.

One outer circle of defences yet remains untaken, and until that falls it would seem that there is little hope of capturing the inner citadel. More must be known of the constitution of dead substances chemically related to the living ones. When the students of chemistry can put the physiologists into possession of the facts regarding dead proteids, the attacks will be renewed more directly, with greater vigour and greater hope of success.

It is not possible to prove to-day that life and death are only a difference in the chemical and physical behaviour of certain compounds. It is safe to say that the future is likely to justify such an assertion.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Eleven county and borough councils have arranged with the Board of Agricultural Studies to make grants for the maintenance of the new department of Agriculture established under the direction of Prof. Somerville. The first list of lectures issued by the Board includes some seventeen courses.

IN the valedictory address delivered by Dr. Hill on vacating the office of Vice-Chancellor, reference was made to the fact that before the close of the academic year the contributions to the Benefaction Fund amounted to upwards of 50,000*l*.; also that a commencement has been made with the new Geological Museum. The Museum will cost about 44,000*l*., of which sum the fund raised as a memorial to Prof. Sedgwick will supply 27,000*l*.

A TECHNICAL and mining college is to be established at Wigan at an estimated cost of 40,000*l*.

THE Rev. J. F. Cross has been appointed professor of mathematics at St. John's University, Winnipeg.

PROF. A. MCADIE has been appointed honorary lecturer on meteorology in connection with the Berkeley Astronomical Department of the University of California.

MR. THEODORE MORISON has been appointed principal of the Aligarh Mahomedan College. The new principal, who is at present in this country, has been authorised to select two new professors to take out with him.

PROF. WAGSTAFF will lecture on geometry at Gresham College from October 10 to 13, and the Rev. E. Ledger's course of lectures on astronomy at the same institution will take place from November 14 to 17.

THE degree of Doctor of Pharmacy has just been conferred by the University of Paris for the first time. The recipient is M. Lacourt, whose graduation thesis was entitled "Historical, Chemical and Bacteriological Study of the Versailles Water."

THE fifteen universities of France together have a total of 27,080 students, of whom 12,059 belong to Paris. The total expenditure is 13,859,500 francs, so that the average cost of the education of each student is 511 francs (a trifle over 20*l*.). To meet this expense the universities have revenues amounting collectively to 2,093,700 francs; legacies, donations, &c., amount to 1,511,600 francs; therefore a deficit of 10,524,200 francs (equivalent to nearly 15*l*. for each student) has each year to be made up by the State.

AT the half-yearly meeting of the court of governors of Owens College, Manchester, held on Tuesday last, the following resolution was carried by a majority of two:—"That, subject to such limitations and conditions as the council may from time to time determine, and subject to the council being able to make satisfactory provision for a separate instruction in such cases as the council consider necessary, the court is of opinion that it would be desirable to admit women students to the course of study which would qualify them for medical degrees and practice."

ACCORDING to the Allahabad *Pioneer Mail*, during the past year no fewer than 11,000 candidates presented themselves for the various examinations of the Madras University, and of these slightly over 4000 were successful. The fees paid by candidates amounted to nearly Rs. 1,87,000; while sundry items, including about Rs. 10,000 interest on Government securities, swelled the income of the University to a little over two lakhs of rupees. The total expenditure for the year came up to Rs. 1,80,000, of which sum Rs. 1,38,000 were absorbed by examiners' fees. The Arts Examinations, as usual, yielded the greatest portion of the University income—the total fees realised from candidates amounted to over one and a half lakhs of rupees, while payments to examiners came up to Rs. 90,000. The Law Examinations yielded a quarter of a lakh of rupees, while the examiners fees only amounted to slightly over half this sum. The Medical and Engineering Examinations, however, are conducted at a loss; but, after balancing receipts and expenditure, the University realised a net profit during the past year of Rs. 10,000, without reckoning the Rs. 10,000 accruing as interest from Government securities.

WE learn from a memorandum that has just reached us that the number of students who attended the City and Guilds of London Institute Central Technical College last session was 245. Of these 220 were following the Diploma Course, eighty-eight attending the First Year Course, seventy-eight the second, and fifty-seven the third. Twenty-five other students were either engaged in research work or were following a special course. During the past year the council has conferred the diploma of Fellowship of the City and Guilds of London Institute upon two of the past students: Mr. W. J. Pope and Mr. A. E. Childs. Siemens Medals were awarded to Mr. F. E. Whittle and Mr. F. C. Hounsfield. Mr. T. M. Lowry and Mr. E. C. Jee, were successful in gaining the D.Sc. degree of the University of London for research work done in the Chemical Department of the College. Twelve students of the College were successful in passing the intermediate B.Sc. examination of the London University. In addition to the students admitted on the results of the Matriculation examination, several others have been admitted to special courses of instruction, and the number in the College at the commencement of the new session will be about 260. Those in special courses number 20. As built the College was intended to accommodate only 200 students. To make adequate provision for Electrical Engineering, a large portion of the basement floor in the adjoining new building of the School of Art Needlework is to be used. The suite of rooms now occupied by the Technological Examinations Department will also become available for teaching purposes, as more extensive quarters are to be found for the Examinations Department in the new building. In connection with this institution, our readers may be referred to the address delivered to the students by Sir Andrew Noble, K.C.B., F.R.S., on Tuesday last (see p. 551 of the present issue).

WHEN the history of education in rural districts comes to be written, the school of science established by the united efforts of the Countess of Warwick and Prof. Meldola, at Bigods, near Dunmow, in Essex, will be given an important place in it. The claims of science to form a part of every national system of education are becoming more and more recognised in our cities, but the forward movement has not been much felt in rural districts, hence the school at Bigods is of the nature of an experiment, and much depends upon the success attained. The curriculum followed in the school meets the requirements of modern education in a most efficient way. The school is a continuation or secondary one in which the ordinary "humanitarian" subjects are by no means neglected, but are carried to higher stages. Modern languages are included, and grammar, geography and history find their places. But the noteworthy characteristic of the school lies in the fact that students devote fifteen hours a week to science, which is not taught in the old-fashioned way, by means of books and blackboard and chalk, but by real work and by observations carried on by the pupils themselves in the laboratories and in the fields. The reasoning faculty is developed by scientific methods at the very commencement of the pupil's education at the school; and students who stay at Bigods for three or four years will have acquired knowledge which will be of the highest value in after life, whether they pass into an agricultural college or enter at once into rural or other industries. For the sake of British agriculture, it is to be hoped that parents in East Anglia will appreciate the efforts being made at Bigods to provide a system of education which will assist both individual and national progress.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, September 25.—M. Maurice Lévy in the chair.—Studies on trimethylene, by M. Berthelot. Preliminary experiments were made on the preparation of trimethylene in the pure state, free from propylene, and the gas obtained, believed to be pure, was characterised by its slow reaction with bromine. Propyl alcohol dropped upon hot zinc chloride gives propylene mixed with hydrogen and propane, but almost free from trimethylene; isopropyl alcohol behaves similarly, and the substitution of strong sulphuric acid for the zinc chloride does not result in the formation of any trimethylene.—On the Neomylodon, by M. Albert Gaudry. An account of the discovery of fossil remains in a cave in Terra del Fuego by Dr. Otto Nordenskjöld, the chief being the skin of a large animal resembling the Mylodon, and which has been named Neomylodon by M. Ameghino.—An account of the ceremony organised at Como to celebrate the discovery of the galvanic battery by Volta.—Observations of the sun made at the Observatory of Lyons with the 16 cm. Brunner equatorial during the first quarter of 1899, by M. J. Guillaume. The results are expressed in three tables giving the number of spots, their distribution in latitude, and the distribution in latitude of the faculæ.—A comparison of the times obtained for the contacts of partial eclipses of the sun by direct observation and by measurements of the lengths of common chord, by M. Ch. André.—On fixed transformation points, by M. H. Le Chatelier.—On the diurnal variation of atmospheric electricity, by M. A. B. Chauveau. From the results of observations made at the summit of the Eifel Tower, it is found that the true law of variation is given by a simple oscillation with a maximum in the day time, and a very constant minimum at 4 to 5 a.m. The more complicated curve obtained by observations in an ordinary building are probably due to the influence of water vapour.—On a particular mode of reproduction of appendices of insects in course of regeneration after artificial section, by M. Edmond Bordage.—On the lateral cephalic organs in *Glomeris*, by M. N. de Zograf.—Some phenomena of cellular disorganisation, by M. Vital Boulet. The asmotic pressure in the cells of a leaf severed from the plant and left in the same water as that in which the original plant was growing was found to regularly increase from 2·2 on the first day to over 6·0 on the twenty-second day.—On the formation of secreting canals in the seeds of certain species of *Garcinia* and *Allanblackia*, by M. Édouard Heckel.

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