THURSDAY, OCTOBER 2, 1873

ON MEDICAL STUDIES

As at the present time so many students have just assembled at the medical schools in London and the provinces to commence or continue their medical education, we think that notwithstanding the advice so freely given them in all directions by their friends, and especially by those who deliver the introductory addresses at the different hospitals, there are some few points to which their attention cannot be too frequently directed.

First, with regard to the range of subjects which is required by the higher examining boards, such as the University of London, in the earlier stages of the medical curriculum. There cannot be the least doubt, though several who have not participated in its advantages are fond of expressing an opinion to the contrary, that the wider and more extended the field of study that can be grasped by a student at the outset, the more chance he has of ultimate success; and he who has no higher object in view than that of passing the least difficult of the necessary examinations which give him a licence to practise, must ultimately find himself far behind in the race. In surgery, no doubt, there are a few who, without much scientific knowledge, have attained great eminence as operators, on account of their manual dexterity; but this position ought not to be the aspiration of the commencing student, as the reputation is generally of short duration, and is not much higher than that of a man who has rowed in a winning University boat-race.

One great argument in favour of a liberal medical education is that the mental capacities of the young men who commence it are very different; and if those who are the most gifted have but little chance of acquiring a knowledge of the facts and theories of Science, as they stand at the time at which they study, they are placed in a position of disadvantage for future research, and find it always difficult to make up for lost time. When all have to start on the extended course, which includes a knowledge of physics, botany, pure physiology, and chemistry, those who have the capacity for higher work in Science obtain an opportunity of developing their tendencies, and are often led to give up their original design of being medical practitioners, to become specialists in their favourite subjects, and an honour to Natural and Medical Science. This means of selecting the best men for scientific work would be a sufficient result in itself to justify the primary education of all medical students in the pure sciences that relate indirectly to medicine; for it must be remembered by those who hold the contrary opinion, that it is to its scientific supporters that the medical profession owes most of its dignity. If we look at the names of those who stand highest in the profession at the present day, it is readily seen that nearly all have their reputation based on a thorough scientific foundation. The lowering of the scientific standard would, therefore, undoubtedly lower the status of the profession amongst society at large, and it will be generally acknowledged that such a result is anything but desirable.

The recent thorough working out of the cause of the outbreak of typhoid fever in the west end of London this summer, shows how satisfactory are the results which follow the employment of a rigorous scientific method of observation. How long it would have remained undiscovered that the impurities in the milk-supply of a locality are the not unfrequent cause of an outbreak of typhoid fever it is impossible to say, if the subject had not been entered upon and carried through in a manner which does great credit to those who detected its origin, as Dr. Ballard had done on a former occasion in Islington.

A second point worthy of attention is the social position of the medical student. That he generally does not compare favourably with the undergraduates of Oxford and Cambridge is certain; but why this is the case does not seem to be so definitely settled. One of the great reasons is that the medical education does not include anything but the mental training: and although the medical student is like the average University undergraduate so far as age, preliminary education, and object of life are concerned, nevertheless after a curriculum of three years or more, the latter has made more progress as a social individual. The different natures of their studies cannot be proved to have anything to do with the difference in the results, and nearly all may be traced to the systems in which each participates. The University undergraduate is subject to two independent influences for good. A fixed code of University and College rules restrains him in many directions, as with regard to his conduct and the allotment of his time; at the same time that a much more stringent, but not written code, the result of his necessarily intimate relations with a large number of companions of his own age, regulates the details of his actions continually, the infringement of which code removes him from his most pleasurable source of enjoyment during leisure hours. Most medical students miss both of these. The absence of a Proctorial system and College rules makes him free to his heart's content; and the comparative smallness, as a rule, of the clique to which he belongs, helps to encourage rather than remove objectionable individual peculiarities, which would not be tolerated in general society. It is excessive freedom which is the bane of the young medical student, and the introduction of any system which provided a reasonable amount of restraint during the medical education would undoubtedly improve the social status of its undergraduates. Attempts have been made, but on too small a scale to be really successful. If the leading schools could be persuaded to invest money in building suitable apartments for their pupils, and spend part of the profits which must necessarily accrue to them, in giving scholarships, open only to those who resided in such buildings, a system might be developed which, after some time, from the convincing evidence it would give of its advantages, would cause all to participate in it.

Until there is much more co-operative feeling among the different schools in London, it is difficult to conceive how this or any other really marked improvement can be effected. Whilst things stand as they do, we are convinced that, in the long run, those will enjoy the most profitable studentship, and afterwards find themselves in the most advantageous position, who put themselves under reasonable restraint, and endeavour to extend their circle of acquaintance beyond the few sympathising "chums," who generally have but little influence for good, at the same time that they often, by an unconscious process of approval and persuasion, help to exaggerate bad qualities and develop worse.

LYELL'S "ANTIQUITY OF MAN"

The Geological Evidences of the Antiquity of Man, with an Outline of Glacial and Post Tertiary Geology, and Remarks on the Origin of Species, with special reference to Man's First Appearance on the Earth. By Sir Charles Lyell, Bart., M.A., F.R.S. Fourth Edition Revised. Illustrated with Woodcuts. (London: John Murray, 1873.)

SINCE the first volume of "The Principles of Geology" appeared—now more than forty-three years ago—Sir Charles Lyell has put forth an uninterrupted series of new works or new editions, and we have now arrived at the 11th edition of the "Principles," the 7th of the "Elements of Geology," and the 4th of the "Antiquity of Man." A most striking feature of these works is, that they give the fullest and most accurate scientific details, and the most philosophical discussion of principles and results, without for a single page ceasing to be interesting to any well educated and thoughtful man. Perhaps no author has attained in so perfect a degree the art of making science popular without ever attempting to popularise it, or has produced a series of works which are equally acceptable to the experienced geologist and to the general reader.

The present edition of the well-known "Antiquity of Man" will fully sustain the author's high reputation, since it is not a mere corrected reprint of former editions, but, in several important respects, a new work, embodying all the most recent discoveries and researches on the various subjects of which it treats, while several discussions of temporary or personal interest have been omitted. Almost every chapter contains either important new facts or new results derived from a more careful study of old ones; while some are almost wholly rewritten, as, for example, chap. xii., in which the most recent researches on the climate of the Crag period is very fully given; and it would need a very acute critic to discover in these any lack of that lucidity of arrangement and vigour of thought which have always distinguished Sir Charles Lyell's writings.

The most striking additional facts bearing directly on the Antiquity of Man are so well known and have been so often before the public, that it is unnecessary to enumerate them here; but it may be advisable to remark briefly upon a theoretical point of some importance on which the author's views seem open to question; and there are also a few matters connected with the general subject which seem worthy of attention.

Although Professor Gastaldi, of Turin, after a careful study of the Italian Alps, has adopted Professor Ramsay's view of the excavation of alpine lake basins by ice, Sir Charles Lyell is still strongly opposed to that view. He maintains that they have been produced by changes of level in valleys, producing depressions which have been preserved during the glacial epoch by being filled with ice, while at all other times they were either soon filled by *débris*, or their lower barriers were cut down as fast as they were formed. He thus accounts for the fact that lakes only occur in any abundance in glaciated districts. He further maintains that the erosive power of glaciers, as indicated by the muddy torrent that always issues from them has been overrated, because "the flour of rock" thus produced is due, not solely to the wearing down of the floor of the valley, but, "to a considerable extent," to the grinding up of the stones which fall upon the glacier and are engulphed in its crevasses.

There are doubtless many difficulties in Prof. Ramsay's theory, and much remains to be done to verify it, but it does seem to cover a larger portion of the facts than that now opposed to it. There is no evidence before us to show how much of the glacier mud is respectively due to the two sources above referred to, but the enormous bulk of many of the old moraines, where they have not been destroyed by subsequent denudation, seems amply sufficient to account for the *débris* which falls upon a glacier; while the wide extent of glaciated surfaces, and the manner in which the very hardest upturned strata are often planed off or *moutonnées*, is equally convincing proof that large masses of rock have been ground down by glaciers. The evidence of this is very remarkable also, in the case of the Loess, a deposit which covers an enormous extent of country, and in some parts of the valley of the Rhine reaches a thickness of near 1,000 feet, and which Sir Charles Lyell himself considers to be undoubtedly glacial mud. It is difficult to conceive how such an enormous amount of mud could have been formed except by a grinding power capable of producing most of the effects imputed to it by Prof. Ramsay. It is considered to be one of the most powerful arguments against the ice-erosion theory that no lakes exist in certain valleys which were undoubtedly filled with enormous glaciers; but the answer to this is, that a lake will only be produced when the erosion is considerably greater at one part of the valley than at another, and this inequality may be caused either by unequal hardness of the subjacent rocks or by the piling up of the ice to a greater thickness in certain spots by the convergence of several branch glaciers, as must have been notably the case over the site of Lago Maggiore, which received the icy streams descending from near 100 miles of the loftiest Alps. It must also be remembered, that at such points of convergnce the rate of motion of the glacier will be much more rapid than elsewhere, in order to discharge the accumulated ice-streams; and we shall thus have a double cause of increased grinding in such positions. A difficulty of a somewhat similar nature, and which cannot be so easily overcome, besets the unequal-subsidence theory, which can hardly be made to account for the thousands and tens of thousands of lakes so thickly scattered over the lowlands of Northern Europe and America.

It is somewhat remarkable that notwithstanding the numerous researches in post-tertiary caves and gravels in all parts of Europe, no human remains have been discovered which can be proved to be older than those found by Dr. Schmerling more than forty years ago in the caverns near Liége. After many years' labour this gentleman, a skilful anatomist and palæontologist, published, in 1833, a detailed account of his researches, copiously illustrated. It is curious to see, from Sir Charles Lyell's account of this work, how completely its author anticipated all the more important results of modern cave exploration, and how thoroughly he had worked out that doctrine of the antiquity of man which the great majority of geologists so long attempted to put down. Such wholly independent researches as those of Schmerling in Belgium, McEnery in Devonshire, and Boucher de Perthes in France, made by careful and conscientious observers, and all converging to the demonstration of one fact, were for many long years laughed at or ignored, solely because they clashed with preconceived opinions. When this occurred with the students of a science which had already fought and won many hard battles against popular and theological prejudice, and whose whole course of study should have taught them how to interpret the evidence adduced, we are bound to deal tenderly with the less unjustifiable prejudices of those who have had no such training.

Notwithstanding the lesson these long-ignored facts should have taught them, some geologists still exhibit a strange fear or hesitation in facing the whole results of modern inquiries on the subject. How is it that, whenever any estimate is made of the lapse of time (expressed in years) since any human remains or works of art were deposited, the lowest possible estimate is almost always chosen? One would think that, having once got beyond the traditional six thousand years, the period of man's past existence would be a matter of purely scientific inquiry, to be arrived at by careful estimates in a variety of ways. But how can we possibly arrive at the truth by always taking the lowest estimate? we might just as reasonably always take the highest. Is there any merit in arriving at a false result so that the figures are small? Is it really the "safe" side so to calculate that we shall almost certainly be wrong? Astronomers do not think those observations most likely to be correct which give the smallest distances and sizes of the heavenly bodies and it would be more dignified and more scientific if geologists, whenever any data exist on which to found a calculation, should insist on taking the mean result of various impartial estimates as that most likely to be the true one. From this point of view it may be interesting to give a summary of the more important attempts which have yet been made to determine the antiquity of human remains or works of art.

From observations at the delta of the Tinière and on the lakes of Neufchatel and Bienne, the bronze age in Europe has been determined with approximate accuracy to have been from 3,000 to 4,000 years ago, and the stone age of the Swiss Lake dwellings at from 5,000 to 7,000 years and an indefinite anterior period. The burnt brick found 60 ft. deep in the Nile alluvium indicates an antiquity of about 20,000 years, taking, from a calculation by Mr. Horner, the estimate of 3½ in. per century as the rate of deposit of the mud. Another fragment found at 72 ft. deep is estimated by M. Rosière to be 30,000 years old. Some human bones found in a lacustrine formation in Florida have been considered by Agassiz, after a careful examination of the locality, to be at least 10,000 years old. A human skeleton found at a depth of 16 ft. below four buried forests superposed upon each other, has been calculated by Dr. Dowler to have an antiquity of 50,000 years.

These latter estimates may be very uncertain, but we have no reason to think them improbable, from what we know of the great changes of physical geography that have undoubtedly taken place since man existed. Kent's Cavern at Torquay furnishes a good example of these, since the whole drainage of the surrounding country must have been very different when the great thickness of cave earth was deposited by floods rushing through the cavern which is now situated in an isolated hill. We have here indications of an immense antiquity from various sources. The upper stalagmitic floor itself marks a vast lapse of time, since it divides the relics of the last two or three thousand years from a deposit full of the bones of extinct mammalia, many of which, like the reindeer, mammoth, and glutton, indicate an arctic climate. It has been remarked that the varying thicknesses of the stalagmitic floor, from 16 in. to 5 ft. and upwards, closely correspond to the present amount of drip in various parts of the cave, so that the cave itself with its various fissures and crevices does not appear to have been materially altered since the stalagmite was deposited. It is true that the drip may once have been greater, but it may also have been less, and we do not know that a more copious drip would necessarily produce a more rapid deposit of stalagmite. But names cut into this stalagmite more than two centuries ago are still legible, showing that, in a spot where the drip is now very copious, and where the stalagmite is 12 ft. thick, not more than about one-eighth of an inch, or say one-hundredth of a foot, has been deposited in that length of time (British Association Report, 1869, p. 196). This gives a foot in 20,000 years, or 5 ft. in 100,000 years; and there is no reason whatever to consider this to be too high an estimate to account for the triple change of organic remains, of climate, and of physical geography. But below this again there is another and much older layer of stalagmite, generally broken up and imbedded in the cave earth. This older stalagmite is very thick and is much more crystalline than the upper one, so that it was probably formed at a slower rate. Yet below this again, in a solid breccia, very different from the cave earth, undoubted works of art have been found. A fair estimate will therefore give us, say, 100,000 years for the upper stalagmite, and about 250,000 for the deeper layer of much greater thickness, and of more crystalline texture. But between these we have a deposit of cave-earth which implies a different set of physical conditions and an alteration in the geography of the surrounding country. We have no means of measuring the period during which this continued to be formed, but it was probably very great; and there was certainly some great change in physical conditions during the deposit of the lower stalagmite, because the fauna of the county underwent a striking change in the interval. If we add 150,000 years for this period, we arrive at the sum of half a million as representing the years that have probably elapsed since flints of human workmanship were buried in the lowest deposits of Kent's Cavern. It may be objected that such an estimate is so loose and untrustworthy as to be altogether valueless; but it may be maintained, on the other hand, that such estimates, if sufficiently multiplied, are of great value, since they help us to form a definite idea of what kind of periods we are dealing with, and furnish us with a series of hypotheses to be corrected or supported by further observation, and will at last enable us to arrive at the antiquity of man within certain probable limits of error. Without laying stress on any portion of the above very rude estimate, it may, I think, be averred that it is not palpably too high, but is just as likely to be too low; and this last supposition will be rendered more probable when we consider the vast lapse of time implied by the position of some of the recently discoverd palæolithic weapons.

The flint tools found in the gravel at Bournemouth, in the Isle of Wight, and near Salisbury, at elevations of from 80 to 100 feet above the present valleys, imply, according to the best observers, that the whole series of surrounding river valleys have been excavated since they were deposited, and that the system of drainage and position of the coast-line have been very greatly altered. The hippopotamus of the Gower Caves implies changes equally great, since the peninsula of Gower now contains only small streams, and could not possibly have had a large river without very important changes in its relations to the adjacent country. The position of the flint weapons in the valley of the Somme, at Hoxne in Suffolk, and in many other places, all combine in indicating that very important changes in physical geography have taken place since they were deposited. We can hardly suppose that in all these different localities the changes were abnormally rapid, especially as in no case do records of the historic period indicate that any remnant of the process was then going on; and from what we do know of the rate of such changes, and their intermittent nature, we are entitled to affirm that the most extreme estimates yet made of the antiquity of the men who fashioned and used the palæolithic implements is quite as likely to be under as over the truth.

There is as yet no clear evidence that man lived in Northern Europe before the glacial epoch, and even if he did so the action of the ice sheet would probably have obliterated all records of his existence. Every evolutionist, however, now believes that he must have existed far back in the tertiary period, and that the proof of it will be found, if at all, in some of the warmer regions of the old world. Here is surely a problem of grand and absorbing interest awaiting solution at our hands. Geologists are not usually wanting in energy or enterprise, and they number in their ranks many wealthy men. It is to be hoped that they will soon energetically attack the problem; and no more promising field of research offers itself than the limestone caves of Borneo, which can be explored with perfect safety, and at a moderate expense. We can hardly now expect any great additions to our knowledge respecting the antiquity of man in Northern and Central Europe, and must go to warmer regions if we wish for new discoveries and startling revelations.

A. R. WALLACE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Fellowship at Magdalen College

I THINK the notice in NATURE of Sept. 25 respecting the election about to take place to a Natural Science Fellowship at Magdalen College requires some comment. The amount of academic preferment which falls to the share of science in Oxford is so small, that it might reasonably be demanded that what there is should be thrown open to as many candidates as possible. When, therefore, it was announced that the Fellowship would be given for proficiency in Biology, it might have been inferred that the electors had this object in view. Biology is held, elsewhere than in Oxford, to be the science which treats of the laws governing organization and vital activity; in other words, structure and function in *all* forms of life, whether vegetable or animal. It was not, perhaps, an unreasonable inference, therefore, to draw from the terms of the notice, that it was the intention of the College to make Biology in its widest sense the foundation of the examination, and to allow individual candidates to exhibit, in addition, such detailed knowledge as they might possess of Zoology, Botany, or even Palæontology. This would not have attributed to Biology a wider meaning than, for example, Mr. Herbert Spencer or the Science and Art Department attach to it. Thinking it desirable, however, to get some official information upon the subject, I wrote to the President, who, after some delay, replied that, in his opinion, as preference would be given to Biology, it would be useless to offer Botany as a special subject. This is not more reasonable than it would be to say, that because Physics was to be the subject of an examination it would be useless to offer Electricity or Heat as a special subject. But the terms of the President's reply were rather ambiguous, and I therefore made some further inquiries. I learnt, as the result, that the College considered it impossible to compare the merits of a candidate who stood on the Zoological, with one who stood on the Botanical, side of the general subject.

I think myself the difficulty is not one which should have been found insuperable; but, assuming that the College had sufficient grounds for a different opinion, then I think the electors should not have offered their Fellowship for Biology, when what they really had in view appears to be a detailed knowledge of the Zoological preparations in the University Museum.

W. T. THISELTON DYER

The Sphygmograph

THERE appears in NATURE, vol. viii. p. 330, a notice of a thesis for the M.D. Cantab. on the subject of Bright's disease, in which reference is especially made to some sphygmographic observations therein contained. It is apparently from the pen of Mr. Garrod, who is himself the author of interesting and important researches with the sphygmograph and cardiograph. While agreeing with a part of my explanation of the normal pulse tracing, as regards the points in which it differs from the view commonly received, he takes exception to the account which I have given of the tidal or first secondary wave. It may be well to say in reply a few words upon the point at issue, since the reference to it in the thesis was very brief and incidental, and I should not wish it to be taken as a full account of my views as to the mechanism of the pulse.

The explanation of Mr. Garrod himself is that the tidal wave is an instantaneous wave due to the closure of the aortic valves. This theory was first proposed by M. Marey to account for the tidal wave in many of its forms; but, so far as I know, it has not been adopted by any writer on the subject in England with the exception of Mr. Garrod. There is this difference, however, between them, that while M. Marey holds that the dicrotic wave has nothing to do with the aortic valves, but is a reflection from the periphery, Mr. Garrod considers that it is the wave of expansion from the closure of the aortic valves, which becomes separated from the instantaneous wave as it recedes from the heart. Thus the faculty of originating two waves of different velocity, which by most writers is attributed to the first impulse of the heart, combined with the closure of the mitral valve, is by Mr. Garrod denied to that event, but ascribed to the closure of the aortic valves. Now I believe it to be mechanically impossible for any wave to be propagated with a velocity different from that of the wave of expansion, except the purely vibratory wave of sound, and Mr. Garrod appears himself to hold that a mere vibration produces no elevation in the tracing. The question, however, may easily be determined experimentally. If there appear in the tracing two waves which are travelling with different velocities, their relative position will vary at different distances from the heart. Let, therefore, anyone who wishes to settle the question for himself take tracings of a good many markedly tricrotic pulses, say from the femoral and also from the dorsalis pedis arteries. According to the view of Dr. Burdon Sanderson and most other writers, the interval between the primary and tidal waves ought to be more than doubled in the dorsalis pedis; according to the view of Mr. Garrod, on the contrary, that between the tidal and dicrotic waves. It will be found that there is no such considerable and constant variation as would be required by either theory, although the tidal wave does not maintain its relative position so closely as does the dicrotic wave. The kind of pulse best of all suited for this experiment is fortunately * rather scarce; it is that of a young person who has a granular kidney, but is free from dropsy.

The theory of Mr. Garrod may appear at first sight suitable to one of the forms of healthy pulse, in which the tidal wave appears as a slight elevation preceding the dicrotic wave; but I do not think that it will be accepted by anyone who has watched its variation in a large number of diseased pulses, and has seen it pass through every gradation, from a separate and distinct wave to a mere convexity in the descending curve, which may commence immediately from the top of the primary upstroke. In the pulse of rigid arteries this latter form is often taken when the heart is quiet, but when it acts more vigorously the tidal wave becomes separated, owing to the development of the so-called "percussion element," which is really the effect of acquired velocity in the sphygmograph. The case which should afford the most crucial test is perhaps that very rare one in which the aortic orifice is closely obstructed, and scarcely any valves remain to produce a wave by their closure. The tidal wave should then, according to Mr. Garrod's theory, be at least greatly diminished, but, in point of fact, it is then more greatly developed than under any other circumstances whatever. Evidence to the same effect may be derived from the use of an artificial heart with experimental elastic tubes, for it is found that, under suitable conditions, the tidal wave may be greatly prolonged by a protracted contraction of the heart. This was first shown by Mr. Mahomed in the *Medical Times*, and although I believe his theory to be erroneous as to the relation between the primary and tidal waves, yet, with regard to the practical associations of the tidal wave, my experiments have led me to conclusions which are quite in agreement with his, namely, that three things contribute to the development of the tidal wave—increase of pressure, diminution of elasticity, and prolongation of the heart's contraction.

Mr. Garrod argues that the tidal wave cannot have anything to do with the inertia of the long lever, because it is shown in the reflecting sphygmoscope, in which that is absent. I do not, however, consider that the result is due solely, and possibly not even chiefly, to the inertia of the lever, but to that of the instrument altogether, and inertia is possessed likewise by the sphygmoscope. Moreover, since the latter does not record its indications, it would be difficult to ascertain whether the tidal wave shown by it corresponds precisely to that of the sphygmographic tracing. Another instrument has also been called a sphygmoscope, in which the motion of the pulse is shown by the variation of a gas flame. In this there appears indeed the counterpart of the tidal wave, but not in the form of a single wave; instead of this a series of small waves is shown. These may appear only as a slight quivering motion, and are evidently due to the oscillation of the elastic diaphragm upon which the pressure of the pulse is received.

Mr. Garrod maintains his own theory especially on the ground of observations with his cardio-sphygmograph, showing the commencement of the tidal wave in the radial pulse to be synchronous with the closure of the aortic valves. But the determination of the moment of that closure depends on the correctness of his interpretation of the minor elevations in the cardiographic tracing. These are numerous, and his interpretation of them all is most ingenious, but to accept it requires an implicit faith that the instrument itself has no part in producing any of the minor features of the curve. Now, that curve was drawn by a lever, moving on a pivot, and balanced between two springs, which would seem a contrivance peculiarly liable to oscillate. When therefore it is further found that in cardiac tracings published by other observers, or those obtained by applying the sphygmograph directly to the heart, there is no close correspondence either in the number or the position of the elevations, the conclusion can hardly be resisted that some of them are due to such oscillation. My own opinion is that neither in the cardiographic nor in the radial pulse tracing can the point corresponding to the end of systole be precisely determined.

The whole subject is one which it is difficult even to state intelligibly without a constant reference to diagrams of tracings, and therefore, for a fuller account of my views as to the theory of the pulse, particularly in reference to the complete explanation of the dicrotic wave, I must refer to a paper to be published in the next number of the *Journal of Anatomy and Physiology*.

While I consider that the construction of the sphygmograph has some influence on the tracing produced, yet I believe that, by a fortunate chance, the result is more practically useful than if the pulse-wave were recorded with perfect accuracy, for I think that slight differences in it, which would then perhaps escape notice, are, as it were, magnified and made manifest to the eye.

I may say in conclusion that I do not quite agree in the view that we must wait for the practical application of the sphygmograph until physiologists are agreed about the theory of the pulse, for, according to present appearances, that consummation is distant indeed. There is, however, among sphygmographers an agreement about practical inferences which is almost as notable as the confusion which prevails as to mechanical causes. It is possible therefore for a person to use the sphygmograph for diagnosis and prognosis, who does not even attempt to understand the cause of the waves seen in its tracings. But it must be allowed that the settling of the mechanical question is much to be desired, and that, without it, the sphygmograph cannot afford that service, which otherwise it would be capable of doing, to the solving of all general physiological problems relating to the vascular system. And, from a practical point of view, these may perhaps be regarded as among the most important in physiology, for it is probably through the agency of the vascular system that many of the greatest effects of remedies are produced.

A. L. GALABIN

* [We have omitted the prefix *un*- from this word; we hope Mr. Galabin will forgive us.—ED.]

On the Origin of Nerve-Force

IN a paper on this subject, by Mr. A. H. Garrod, in NATURE, vol. viii. p. 265, the author states that in cold-blooded animals, nerve-force must be generated by the difference between their own temperatures and that of the medium by which they are surrounded. Now, to take the case of a frog as a common example of a "so-called" cold-blooded animal: A few days ago, when the thermometer was standing at 71°, I took the temperature of two frogs, one was 69°, and the other 67°; the difference between their temperature and that of the surrounding air was practically *nil*. Now, on a day of this sort of temperature, it would seem that the pervious integument of the frog is continually exhaling moisture, and that in consequence the temperature falls, and would continue to fall below that of the surrounding air, were it not that it was raised by the heat generated "by the destruction of tissue that is continually going on within the body of the animal;" so between these two contending forces a state of equilibrium results, and the temperature of the animal and the surrounding air are the same. But, if this be true, it follows that the whole of the heat from the animal is used up in keeping up its temperature, and therefore none can be spared for conversion into nerve-force. Therefore, a frog at rest on a summer's day ought to have no nervous energy. Now, suppose our frog takes to leaping vigorously, he will develop a certain amount of heat, and then he ought to have a great deal of nerve-force; but it is not found that an active frog is more "nervous" than a quiescent one.

Again, the nervous irritability of a frog, though perhaps not acting with the instantaneous energy with which it acts in a mammal, still persists far longer than in other vertebrates, and will continue much longer after the somatic death of the animal, when it is quite clear that the temperature of the body and the surrounding medium will be the same. Now in this case the nerves may be so irritated as to lose all irritability, and yet, after a period of rest, this irritability will be regained, clearly, to my mind, showing that nervous energy must be generated after the death of the animal, when all differences of temperature have ceased.

Finally, it must be admitted, without the aid of any hypothesis, that the difference between the temperature of a frog and the surrounding air is, at any time, very slight; and yet this animal possesses what we call an extremely "persistent" form of nerve-force.

R. LYDEKKER

On the Polarisation of Light in the Rainbow

As I do not remember seeing any notice, in books on light and colour, about the polarisation of light in the rainbow, I think it my duty to relate the following facts, although I can scarcely think the appearance has been unobserved till now.

Three times I have tested the rainbow-light this summer, and each time I found it wholly polarised. On the first occasion, while looking at the rainbow, I thought I would examine it with a tourmaline, which I kept in my pocket. I looked at the bow, through the tourmaline, and saw the bow; but on rotating the tourmaline the bow alternately disappeared and reappeared at every quarter turn: while the light from a stack of chimneys which stood within the bow remained apparently unchanged. From this I inferred that the light of the rainbow was wholly polarised, while the other light in its neighbourhood was not so.

I have observed the vanishing and reappearing of the light of the rainbow on rotating the tourmaline on two occasions since that. I have waited for these additional occasions to make sure of the fact, as I was called away from the first observation; and when I could go back the rainbow had vanished.

The date of the second and third times are August 28 and September 4, 1873.

Leicester, Sept. 5 GEO. FINLAY

[The polarisation of the light of the rainbow was observed by Biot in 1811, and by Brewster in 1812. (See "Brewster's Optics," art. 185.) With respect to rainbows by reflection, there are two kinds—(1) that observed by X. Z. Y., in which the light comes to the eye from the water. This is not thought worthy of special mention by Brewster. (2) That in which the light of the sun reflected from water strikes the shower and forms a bow not concentric with the common bow. (See "Brewster's Optics," art. 186.) It is very easy to see that these two kinds of bow form parts of the same cone whose axis is at the same altitude as the sun, but in the opposite azimuth.—J. C. M.]

Autumnal Typhoid Epidemics

THERE appear to be two types of these,—first, the malignant and dangerous, which breaks out in isolated spots and is usually traceable either directly or indirectly to some sins of sewerage; and a second or milder form, which extends over far larger areas, is much more general, and apparently unconnected with sewage exhalations or liquid contaminations. Some observations I have lately made suggest an explanation of the origin of this latter form. We have had just a moist and rather warm summer, followed by an unusually wet autumn. Turnips, swedes, beets, mangolds, cabbages, potatoes, peas, &c., put forth luxurious foliage, and much of this, especially the lower leaves of turnips, swedes, and cabbages, have been rotted by the recent rains—so much so, that many a country lane that should have exhaled sweet balmy odours has been] the abode of most unromantic stink. This is especially the case in the flat market garden areas that lie by the side of the Thames, and in these the most especially where cabbages are cultivated. I have no doubt that the partridge shooters of 1873, who have largely availed themselves of the cover of turnip-fields, will confirm my observation of their offensive odour.

Modern agriculture is, in England, chiefly developing and extending in the direction of root crops for cattle feeding, and the foliage of these is very liable to offensive decomposition under the conditions above named. When the autumn is hot and dry, their outer leaves, and also those of kitchen vegetables, drop off and return to the soil in a dry, crisp, and inodorous condition.

That the moist decomposition of such vegetable matter should supply nourishment to disease germs analogous to those which are fed by sewage, and that the exhalations of the decomposing vegetables should spread them after the manner of sewage exhalations, is obviously probable.

If I am right, the widely extended and milder forms of autumn epidemics should be most prevalent in such years as the present, and should prevail more especially in market-garden and cattle-feeding districts.

So far as my own means of observation extend, this appears to be the case, but as these are too limited to justify any positive conclusion, I throw out the above as a merely suggestive explanation, demanding further confirmation, which some of the readers of NATURE may be able to supply.

Woodside, Sept. 8 W. MATTIEU WILLIAMS

Venomous Caterpillars

OBSERVING a letter in NATURE respecting venomous caterpillars, I venture to offer a few remarks from personal experience.

The rough hairy caterpillars have a bad reputation everywhere. As a boy, the nurses told me if one got tight round my finger, *it* (and of course I understood the finger) would have to be cut off. In Switzerland they are regarded by the common people as poisonous, though, as far as I know, without foundation.

In Portugal there is a most remarkable gregarious species, known as the "procession caterpillar," from the great numbers that may be seen advancing in a body. This kind has undoubtedly the power of causing very considerable irritation to a tender skin. A specimen once crawled up the arm of my little girl, then one year old, leaving the skin-surface red and inflamed along its track; and there was a tradition at Lisbon of a child that had fallen into a mass of these larvæ, and subsequently died from the consequent inflammation.

In Brazil there is a species in the neighbourhood of Rio that, with regard to the formidable nature of its external clothing, is a veritable porcupine. It corresponds remarkably with the description of the Burmese specimen, both in size and colour. The hairs, in a state of repose, are, however, but slightly erect, and it is only when irritated or alarmed that it raises them in hostile guise. There can be no question as to the stinging properties of these hairs, to which my wife, among others, can bear testimony; but as our experimental ardour did not induce us to grasp the creature, the consequences were never serious. The largest hairs must be nearly an inch long, and the points of all have a lighter appearance, as though singed. It was interesting to watch their elevation by the animal on the approach of the finger, as though by some electric attraction. The stinging sensation is analogous to that caused by a nettle. I am inclined to think that in this case the cause was likewise analogous. It is, however, possible that the hairs are brittle, or armed with articulated branches.

With reference to the power of detaching hairs possessed by some caterpillars, a remarkable instance came under my notice in Tiguca (Brazil). It was observed in the larva of a beautiful black and white butterfly with conspicuous yellow tail. The determining principle of its existence appeared to be rather economy than defence. Consequently the hairs with which its body was covered were utilised in the construction of its cocoon. For this purpose it was first clearly necessary to shed them; after which they were dexterously crossed and recrossed over the creature's body ensconced under the shadow of some convenient leaf. In this process, if thread was used at all, it was with the greatest economy.

As it was evident that such hairs must be well adapted to their purpose, I examined them under a good microscope, when I found them armed with short barbs on all sides, especially towards the extremities. The spines were tolerably thick, giving under the lens much the appearance of a sprig of juniper.

Berne, Switzerland C. EDEN

IN reference to the article on venomous caterpillars in NATURE of the 14th inst., I beg to offer you, if the subject is not closed, my own very unpleasant experience.

On the 19th of June last, as I was sitting in my drawing-room near an open window, looking on the garden, I suddenly felt an itching sensation in my throat and arm, and on examining my dress I found a large brown long-haired caterpillar. In a few moments my skin, on the parts affected, was covered with a strong eruption attended with intense heat. Thinking it impossible that the insect could have produced this inflammation, I sent for a doctor. After examining the skin he assured me he could see no other cause, and that the eruption resulted from the hairs of the caterpillar remaining in the skin.

He ordered me some simple applications, telling me that a few hours would bring relief. In this he was totally mistaken. The inflammation increased to the extent of producing general fever; I passed a sleepless night, and the next day it continued unabated. After that it very gradually subsided, but the traces of the eruption were visible ten days afterwards.

The insect could not, I imagine, have bitten me, as I felt nothing at the moment.

I have frequently been bitten by tropical insects, but in no one instance have I suffered so severely, or been so disfigured. The sensation reminded me somewhat of the prickly heat, only it was infinitely more intolerable.

There was no predisposing cause, as I was at the time in good health, and had no tendency to fever, although the temperature was remarkably high for the month of June.

I have not seen a similar accident during my fifteen years' residence in France, but I presume they are not unfrequent here, or there would be no reason for the vulgar French expression "Mauvaise comme une chenille." A. GILLANDER

7, Rue St. Claire, Passy, Paris

The Glacial Period

PROF. TYNDALL has several times called attention to a point in regard to the height of the snow-line, which seems to be steadily overlooked by those who speculate on the causes of the great prevalence of snow during the glacial epoch. It is of course well known that the height of the snow-line at any place is determined mainly by two things, viz., the depth of annual snow-fall, and the temperature of the place. If the amount of snow falling over the whole earth is to be increased, the evaporation must also be increased. ("Heat as a Mode of Motion," pp. 206-7. New York, 1866.) This would also raise the temperature, but the snow-line might nevertheless descend. We have a case of exactly this kind in the Himalayas. On the warm southern side of these mountains the snow-line is, nevertheless, 3,000 ft. lower than on the northern side, where the temperature is very much colder. This is evidently due to a difference in the amount of annual precipitation. Assume that the sun was at one time much warmer than now, and that since then it has been steadily cooling; and I believe you have the key to the solution of the questions asked by J. H. Rohrs, as well as to such questions as the widespread occurrence of tropical vegetation during the past ages.

Iowa City, U.S. FRANK E. NIPHER

RECENT RESEARCHES ON THE LOCALISATION OF THE CEREBRAL FUNCTIONS

THE fifth part of Dr. Brown-Séquard's new "Archives of Scientific and Practical Medicine" contains an excellent report by Dr. Neftel, "on some of the recent researches in neuropathology" embracing a digest of several important modern methods, recently introduced, for the purpose of analysing the functions of the different parts of the cerebral hemispheres, together with a succinct account of the results arrived at by their employment. An abstract of this report forms the substance of the present notice.

The researches of Longet, Magendie, Matteucci, and others have led to the assumption by most physiologists, that the cerebral hemispheres, especially their cortical substance, are destitute of sensibility, being the seats of origin of higher mental phenomena only. The experiments from which these conclusions were arrived at, consisted in the irritation of the hemispheres in living animals by mechanical, chemical, and electrical means; and in none were they succeeded by muscular contractions. As if to put the question beyond a doubt, Flourens removed the entire hemispheres without disturbing the muscular mechanism.

But the tendency of modern observation is in a different direction; the new researches have been made independently by several investigators, with entirely different methods, nevertheless the results are the same, contrary to that of the earlier workers; the evidence going to prove that the cortical substance of the cerebral hemispheres is in close relation with certain muscular groups, forming the "psychomotor centres" of Gudden.

Fritsch and Hitzig commenced these researches, the latter having observed that galvanic excitation of the hemispheres in the living man produced contraction of the eye-muscles. This aberrant result suggested further experiments. They irritated the cerebral hemispheres in a dog with an extremely weak current, and found that movements of certain groups of muscles followed the excitation of definite spots on the anterior convex portion of the brain, always upon the side opposite to that which was acted on; whilst the same excitation of portions of the hemispheres situated more posteriorily, produced no effect. Thus they found the centre for the extensor and adductor muscles of the anterior extremity at the external end of the pre-frontal convolution; and somewhat behind it the centre for the flexor and rotating muscles of the same extremity. The irritation of these centres by metallic closing of a very weak galvanic current produces a single contraction, whilst the interrupted current produces tonic and gradually disappearing contractions of these muscles, followed by epileptiform movements. The anode has much more influence in producing these results than the cathode, so much so, that with a current of minimal intensity contractions can only be produced by the anode.

When Fritz and Hitzig removed in dogs the centre for the anterior extremity, this latter did not become entirely paralysed, the animal could use it, but imperfectly, and seemed quite unconscious of the condition of the limb, which could be placed into any position without attracting its attention.

Nothnagel employs a new method for the determination of the functions of the brain. His observations are made mostly on rabbits. An incision is made in the scalp, the skull is perforated with a needle. Through the canal thus formed in the bone a very small drop of a concentrated solution of chromic acid is injected by means of a hypodermic syringe with a very slender nozzle. The scalp wound is then united by suture, and the animal does not seem to be affected, except with regard to the functional derangement incidental to the lesion. Generally they survive the operation two or three weeks, and die from causes which Nothnagel cannot explain, no constitutional symptoms being developed. However, when the chromic acid is injected into the lateral cerebral ventricles death is the immediate result. On post-mortem examination, where the chromic acid was injected a minute circumscribed place appears, of a green colour, resistant and hard.

In methods employed previous to this, many causes acted to impair the value of the results arrived at; there was considerable hæmorrhage, refrigeration of the brain surface, and modification of the intra-cranial pressure, in addition to which the animal died very shortly. These are obviated by the new means just described; many fresh facts have therefore been brought to light. In one of his experiments Nothnagel made a chromic acid lesion on the surface of the cerebral cortex, which penetrated very slightly into its substance, in a spot corresponding exactly to the outer end of the post-frontal convolution. The animal appeared healthy, but it was found on careful observation that it had lost the muscular sense in the anterior extremity, on the opposite side to the cerebral lesion, it being possible to put, and retain for some time, the affected paw in strained positions. This condition passed off before death, which seems to indicate that the terminal station or the real centre for the muscular sense exists elsewhere, and that after a time other ways to it become developed.

Nothnagel found, further, a circumscribed locality in the cerebral cortex, the lesion of which produces a partial and transient hemiplegia of the opposite extremity. This spot is in front of that for the muscular sense, and deeper than it. In no other portions of the cerebral cortex, except those above mentioned, have the chromic acid lesions been followed by paralytic symptoms.

Gudden has introduced another method by which the function of the different parts of the cerebrum may be studied. He finds that newly-born animals, as rabbits, will undergo a very great amount of mutilation without interfering seriously with the nutritive functions, so that portions of the brain may be removed, and the animal will grow to full size, with no peculiarities excepting those resulting from the absence of the parts removed. The slight sensibility, rapid coagulation of the blood, and the quick growth, are all in favour of operations. The following are the results of his experiments on the cerebral hemispheres:—"Very convincing facts are obtained by removing the cerebral hemispheres in new-born animals, and allowing them to grow up. The result is idiotismus. There is also reason to locate the organic conditions of voluntary movements in the cortical substance of the brain, but there is no reason to accept the corpus striatum as a motor ganglion. The hemiplegion following the destruction of the nucleus lenticularis can be satisfactorily explained by the rupture of fibres passing through the internal capsule. But admitting the cerebral cortex as the organ for voluntary movements, there is no necessity to have another motor ganglion. Indeed, Gudden's experiments on new-born rabbits, by removing portions of the hemispheres, have demonstrated that the organ of voluntary motion is located in the frontal part of the cerebral cortex."

Dr. Ferrier, whose results are referred to in another column, is working in a similar field of observation, with the view of elucidating the relations between certain convolution centres, and definite sets of muscles at the periphery.

FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE second meeting of the French Association for the Advancement of Science was held at Lyons from the 21st to the 28th of August, under the Presidency of Prof. Quatrefages. This Association bids fair to become as popular in France as the British Association in this country. The work done in the sections which I visited, those of Anthropology and Geology, was, to say the very least, as valuable as that done by our own sections. Among the papers brought before the former, the pleistocene station of Solutré excited considerable interest, and was subsequently visited by the section. The site has been used by man for habitation and burial, as late as the Merovingian times, in which it was a cemetery, and the strata are to a considerable extent *remanié*. The association of remains on that spot of varying age, Palæolithic, Neolithic, and Frankish, seems to throw a doubt on the precise date of the human skeletons, buried at full length, and generally believed to be of the same age as the associated implements of reindeer, and bones of mammoth. Dr. Gosse also read a paper on the reindeer-cave of Veyriers, Switzerland, and exhibited carved implements of reindeer antler, usually called "batons de commandement," which are of the same form as the arrow-straighteners of the Eskimos. Here, as in the caves of Belgium explored by M. Dupont, they presented but one perforation. The debates were very animated, and drew out many valuable remarks from the eminent anthropologist, Dr. Paul Broca.

In the Geological section, papers were contributed by the Comte de Saporta, MM. Dumortier, Beboux, and others, and in the debates Prof. Carl Vogt of Geneva took a prominent part. MM. Falsan and Chantre exhibited and described an elaborate map of the glacial phenomena of the middle basin of the Rhone, drawn on a large scale. They traced the glaciers of the Alps, and of the Jura, as far to the west as the Saône, and as far to the south as Valence; and they proved that there were two epochs of glaciation, the one during which the area in question was covered by a great ice-sheet, conveying Alpine blocks over the Jura into the valley of the Saône and middle basin of the Rhone, and the other during which the glaciers were isolated, and local moraines accumulated in the river valleys. These two periods correspond with those which have been noted in Great Britain and Ireland, by Prof. Ramsay, Hull, and others. The map presented a combination of artistic skill, with careful work in the field, which is very rarely met with.

In the evening three popular lectures were given to the public, one of which, by M. Janssen, on the Constitution of the Sun, was admirably illustrated.

The times of meeting of the sections differ from ours, the programme of the day being, first, a morning sitting from 8.30, or 9 to 11 A.M.,—*dejeuner;* and, an afternoon sitting from 3 to 5 P.M.—then dinner; and sometimes an evening sitting commencing at eight, when there were no lectures going on. The sections were 15 in number, and comprised Agriculture and Medicine, as well as those represented in the British Association. There were excursions down the Rhone, and to Geneva; a grand *fete* given by one of the merchants, and a magnificent entertainment given by the City of Lyons in the Town Hall.

In writing this short notice the extreme courtesy and consideration of the French Association to the strangers should not be omitted. Their hospitality to the only English guest present was too great to flow from any personal motive, and evidently was intended as a mark of respect to the British Association. W. B. D.

THE METEOROLOGICAL CONGRESS AT VIENNA

THE Meteorological Congress which met at Vienna during the past month worked very hard amid many difficulties, and we believe will have good results. The Congress sat from Sept. 2 to Sept. 16. The protocols and appendices are in the press, and will appear officially in French and German; while Mr. R. H. Scott has undertaken an English translation, which will appear as soon as possible. The following is a list of the delegates from the various countries:—Antonio Aguilar, Spain; H. Buys-Ballot, Netherlands; Carl Bruhns, Germany; Alexander Buchan, Great Britain and Ireland; J. D. Campbell, China; Giov. Cantoni, Italy; Aristide Coumbary, Turkey; v. Czelechowsky, Austria; F. Doergens, Germany; Prof. Ebermayer, Bavaria; Fradesso da Silveira, Portugal; M. Gloesener, Belgium; Julius Hann, Austria; Hoffmeyer, Denmark; Carl Jelinek, Austria; Josef Lorenz, Austria; Heinrich Mohn, Norway; Robert Müller, Austrian-Hungary; Albert Myer, United States; Georg Neumayer, Germany; E. Plantamour, Switzerland; Ernst Quetelet, Belgium; R. Rubenson, Sweden; Guido Schenzl, Hungary; Julius Schmidt, Greece; H. Schoder, Germany; Robert H. Scott, Great Britain and Ireland; Carl Sohncke, Germany; H. Wild, Russia; F. Winnecke, Germany; A. Zamara, Austria. The following is the programme of subjects discussed:—

I. *Instruments.*—1. What is the construction of the barometer most suitable for stations of the second order? Is the use of aneroids at such stations advisable? 2. What mode of exposure of thermometers for the observation of air temperature is the best and most suitable for general adoption? 3. What is the best construction for maximum and minimum thermometers? 4. What instruments should be used for determining intensity of radiation, and in what way can the comparison of the results obtained be secured? 5. What is the best apparatus for observing earth temperatures? At what depths ought they to be made, in order that the desired agreement may be attained? 6. What instruments should be used for ascertaining the state of moisture of the atmosphere? Does the psychrometer suffice for this purpose? Can the hair hygrometer be made applicable, and with what limitations? 7. In what way can an agreement in the signs for the directions of the wind be attained? Is the deduction of the mean direction of the wind according to Lambert's formula desirable? Is it desirable or not to include very light winds (force 0) in constructing wind roses for the direction of the wind? 8. What scale is to be used for the force of wind where it has to be estimated without the aid of an instrument? 9. Is the introduction of simple counting instruments for ascertaining the rate of the wind desirable? What units should be fixed upon as a basis for observing the rate of the wind? 10. What is the most suitable form, size, and position for rain-gauges? At what time of day should the measurement of rainfall be made. 11. Should days of rain and snow-fall be separated from each other, or be counted as the same? 12. Is it desirable in recording the amount of hail to separate the falls of sleet (*graupel*) from those of hail proper? 13. In reckoning thunderstorms, are the storms only to be recorded, or the days in which they occurred? How is sheet-lightning to be regarded? 14. What apparatus is to be recommended for measuring evaporation? What is the most suitable exposure for the vaporimeter? 15. How should the amount of cloud be estimated and recorded? Is it desirable to introduce for clouds, hydrometeors, and for other extraordinary phenomena, a nomenclature which shall be independent of local language, and therefore universally intelligible? 16. Moreover, should other elements which are reckoned meteorological, *e.g.* atmospheric electricity, ozone, &c., be included in the circle of normal observations, and what are the most suitable instruments for observing them. 17. For meteorological measurements should the same units of measure (units of length, degree, time, &c.), be introduced into all countries? or is it sufficient to establish fixed rules for the reduction of the measurements used in different countries?

II. *Taking and calculation of the observations.*—18. Could corresponding times of observation be established at all meteorological stations. 19. According to what rules, periods of time, &c., are the mean values of the various meteorological observations to be calculated? Is it expedient to begin the meteorological year with the month of January, or with the month of December? 20. In what way, and for what periods of time are the normal values of the several meteorological elements to be deduced?

III. *Weather telegrams.*—21. Does the interchange of weather telegrams appear so useful that a wider circulation and more complete organisation should be given to it?

IV. *Maritime Meteorology.*—22. In what way would maritime meteorology be best introduced into the system of general meteorology?

V. *Organisation.*—23. Is it desirable that in each country one or more central stations for the superintendence, collection, and publication of meteorological observations, should be established? 24. In reference to the verification of instruments and the inspection of meteorological stations, can any adequate general rules be laid down? And is it advisable to introduce general instructions for taking and calculating meteorological observations? 25. In what way can the agreement of the standard instruments of the various central establishments be best secured?

VI. *Publication of Observations.*—26. Is it desirable and practicable to publish the meteorological observations of a limited number of stations in each country in a uniform manner and within a reasonably short time after the observations have been made? 27. How is the interchange of meteorological publications of various institutions and countries to be organised most simply, speedily, and certainly?

VII. *The Carrying Out of the Decisions of the Congress.*—28. What measures should be adopted for the accomplishment of the decisions and purposes of the Meteorological Congress? For this purpose, is the establishment of a permanent committee and the arrangement of further meteorological Congresses necessary?

BIRMINGHAM NATURAL HISTORY AND MICROSCOPICAL SOCIETY

ABOUT twenty members of this society, including several ladies, proceeded to Teignmouth in the beginning of September, in fulfilment of the proposed marine excursion, and took up their quarters according to agreement at the Queen's Hotel. The yacht *Ruby* had been chartered for the occasion, and proved a most seaworthy and serviceable craft. Dredging operations commenced on Monday, Sept. 1, and were continued daily throughout the week, in depths varying from $5\frac{1}{2}$ to 20 fathoms. The atmospheric, surface, and bottom temperatures were taken at each sounding, the maximum and minimum results being as follows:—

Atmospheric temperature,	Maximum			66°	Minimum	64°
Surface	,,	,,	,,	61°	,,	$58\frac{1}{2}$°
Bottom	,,	,,	,,	$60\frac{1}{2}$°	,,	58°

The averages were: atmospheric, $65\frac{1}{4}$°, surface, $59\frac{1}{2}$°, bottom, $58\frac{3}{4}$°. A Miller-Casella thermometer was used. On the whole the results of the dredging were very satisfactory. The weather was fine, but cloudy, with occasional rain, and sometimes a little too calm for the work. About 30 hauls of the dredge were made, and specimens of many of the marine invertebrate animals in the neighbourhood secured. The tangles attached to the bag of the dredge sometimes came up literally swarming with echinoderms. By far the most noteworthy capture was *Comatula rosacea*, the feather-star, two individuals of which were taken in the larval pedunculate condition attached near the base of a frond of *Laminaria*, which was torn off by the dredge.* The specimens measured about one-third of an inch each in length. Five young Comatulæ in a free condition, the largest about an inch across, were also taken. A subsequent haul on the following day brought up from the same locality three adults. The members of the Society had the unusually rare opportunity of seeing under the microscope the young feather-stars in the living state. They were but little thicker than sewing-silk, of graceful, erect, lily-like form, and very lively, bending and waving on the peduncle; the arms vigorously contracting in an inward direction. Drawings of the larval Comatulæ in the living state were made to scale by Mr. Wills, with the camera lucida, and the specimens mounted by him for exhibition to the Society. A full description will be communicated to the Society in a report of the excursion. During the evenings the members had the opportunity of examining under the microscope the pedicellariæ of the star-fishes and sea-urchins, and the whip and bird's head processes of certain of the polyzoa, also the structure of Botryllus and other tunicates, the larval forms of crustacea, &c.; objects always interesting, but specially so to a society carrying on its work in an inland neighbourhood far removed from the sea. In the course of the week very enjoyable excursions were made by some of the members down the River Dart to Berry-Pomeroy Castle, Lustleigh, Becky Falls, Moreton Hampstead, Chagford, Exeter, Torquay, &c. On the whole, the excursion has proved a most successful experiment, quite fulfilling the expectations of those who projected it, and it is to be hoped may be succeeded by others in a wider field. The members received much kind attention from the Rev. R. Cresswell, Mr. W. G. Ormerod, Rev. R. C. Douglas, Mr. Adams, and other gentlemen. Most of the party returned to Birmingham by train on Monday, having had a most delightful excursion.—The members of the society who remained in Devonshire after the marine excursion had a great treat on the following Friday, when they were escorted through the famous cavern by W. Pengelly, F.R.S., who courteously explained to them the mode of conducting the explorations, the contents of the flora, and their relation to geological time. Mr. Pengelly also showed them at his own house the collection of bones, teeth, &c., of man, and the extinct bear, hyæna, dog, and other animals, and the flint implements of earlier and later manufacture found therewith in the cavern.

* They were taken in the vicinity of Torbay on Thursday, Sept. 5, at a depth of 12 fathoms on a limestone bottom, the bottom temperature registering 59°.

THE COMMON FROG

I.

WHAT is a Frog? At first, almost all persons will think, on meeting with this question, that they can answer it readily and easily. Second thoughts, however, will show to most that such is by no means the case.

Indeed many a man of education and culture will find himself entirely at a loss, if suddenly called upon for a reply to what is in fact a problem by no means easy of solution.

"The Frog is a small saltatory Reptile" will probably be the reply of the majority. But *is* it a Reptile? At any rate it begins life (in its Tadpole stage) like *a Fish!*

By the great Cuvier, however, as by very many naturalists since, it has been regarded as a Reptile and classed with Lizards, Crocodiles, and Serpents; and yet it may be a question whether the murine affinity connubially assigned to it in the Nursery tale, be not the lesser error of the two.

If the Frog was only known by certain fossil remains it would be considered one of the most anomalous of animals.

Many persons are accustomed to make much of the distinctive peculiarities of the human frame. In fact, however, Man's bodily structure is far less exceptional in the animal series, is far less peculiar and isolated than that which is common to Frogs and Toads.

The number and nature of both the closer and the more remote allies of the Frog; its distribution both as to space and as to time; its relationships whether of analogy or affinity * to very different animals; its bony frame-work; its muscles and nerves; its brain and sense-organs; its respiratory and excretory structures; its various changes from the egg to maturity, together with peculiarities of habit in allied forms; are all matters which will well repay a little attentive consideration.

Indeed it is probable that no other existing animal is more replete with scientific interest of the highest kind, than is the Frog.

About it are gathered biological † questions which bear upon the origin of species, and upon the course and mode of organic development, as well as other speculative problems to which answers are as yet far to seek.

If it is a fact that all the various species of animals have arisen through ordinary generation one from another by a process of development, the life history of the Frog may with reason be expected to have some bearing upon such a process, since every Frog begins its free existence with the organisation of a Fish, and after undergoing a remarkable "Metamorphosis," attains the condition of an air-breathing quadruped, capable of easy and rapid terrestrial locomotion.

There is a matter with respect to which the zoologist can hardly avoid regarding the botanist with envy. The creatures sought after by the latter may be rare or inhabitants of stations difficult of access, but at any rate they are incapable of flight or concealment, and specimens of some kind or other generally present themselves in plenty.

On the other hand not only does the townsman of a thickly-peopled land like our own, often meet with fewer animals in his country walks than he anticipated, but the explorer of tropical lands and virgin forests has frequently to endure disappointment from the contrast between the richness of a known local fauna and the little to be actually seen of the animal population of the place.

Frogs and Toads, however, are often enough seen both at home and abroad, and when perceived generally fall a far more ready prey to the collector than do the swift-running Lizards and small Beasts which are the commonest ground-animals met with besides. The group is also rich in species as well as in individuals, and it is spread over the far greater part of the habitable globe. Neverthele[s] Frogs and Toads have few admirers even amongst professed zoologists, and meet with no little neglect.

While the term "Ornithologist" ‡ is familiar to everyone, and the title "Erpetologist" § is so to all naturalists; the name "Batrachologist" ‖ has not yet been conferred on or assumed by any one worker in Science.

* Analogous relationship refers to the uses to which parts are put. Relationship of affinity refers either to such a relationship as that of kindred or to an ideal affinity reposing on similarities of structure.

† Biological questions are questions referring to living beings "Biology" being the science which treats of all living things, including both plants and nimals.

‡ Ὄρνιθος, a bird, and λόγος, a discourse.

§ Ἑρπετόν, a reptile, and λόγος.

‖ Βάτραχος, a frog, and λόγος.

Economically, Frogs are of little esteem in England save occasionally for bait and as the staple food of certain rare and interesting animals preserved in our menageries. Our American cousins indeed have given one more evidence of their French sympathies by the introduction of the Frog into their *cuisine*, and, as suits that land of the longest rivers and the largest lakes, it is no less a creature than the gigantic Bull-frog which figures in the *menu* of Transatlantic gourmets.

If zoologists and economists have neglected the Frog, the same assertion can by no means be made with respect to physiologists.

The Frog is the never-failing resource for the physiological experimenter. It would be long indeed to tell the sufferings of much-enduring frogs in the cause of Science! What Frogs can do without their heads? What their legs can do without their bodies? What their arms can do without either head or trunk? What is the effect of the removal of their brains? How they can manage without their eyes and without their ears? What effects result from all kinds of local irritations, from chokings, from poisonings, from mutilations the most varied? These are the questions again and again addressed to the little animal which perhaps more than any other deserves the title of "the Martyr of Science."

To return to our question at starting, "What is a Frog?"

To answer this, it will in the first place be well to make a certain preliminary acquaintance with the frog absolutely.

Fig. 1.—The Common Frog, *Rana temporaria*.

Secondly, to study those creatures which are most like it, and are, therefore, as we shall directly see, its "class fellows," living and fossil.

Thirdly, to investigate its anatomy so far as to be able to institute fruitful comparisons between its organisation and that of all other creatures belonging to the same great primary group of animals to which it pertains.

Fourthly, to sum up the results in a series of successively wider and wider comparisons, and by the light thence derived to answer as fully as the present state of Science allows the question first asked.

We shall then be able to answer that question, because we shall have ascertained how various parts of this creature form one organic whole as a system of mutually related structures; and how this whole and its parts are related to the entire series of animal existences from the monad up to man. Then, and then only shall we be able to say what a frog is.

In the first place it is necessary to acquire a general notion of the way in which animals are distinguished and segregated into groups, as well as the general system of arrangement of those groups and the mode of bestowing names which has been adopted by zoologists in common with botanists.

When we have acquired an adequate general notion of zoological classification we shall be able to see with what creatures the Frog is now admitted to be, in various degrees, allied.

The whole mass of animals of all kinds from man down to the lowest animalcula) is spoken of by the fanciful term *king-*

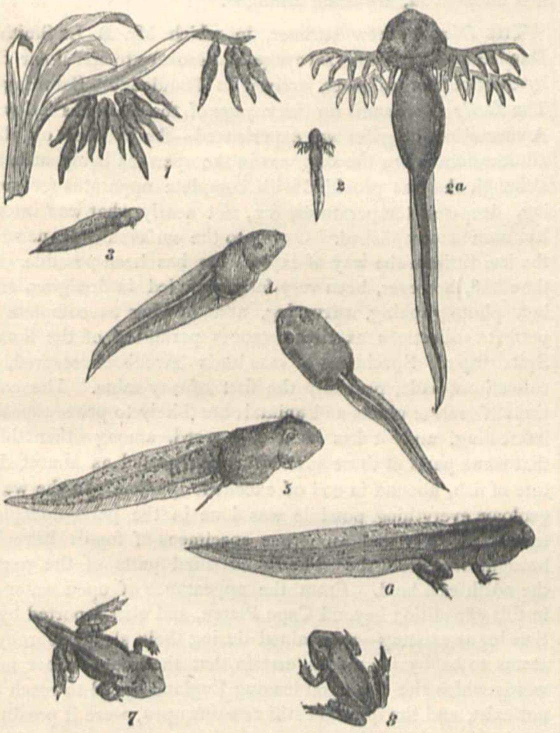
dom. Thus we have the animal kingdom in contrast with and in distinction from the vegetable and mineral kingdoms.

This great whole, the animal kingdom, is subdivided into seven great groups or *sub-kingdoms*, to one or other of which every animal known to us belongs.

Each of these sub-kingdoms (like every more subordinate zoological group) is characterised and defined by certain points of structure possessed by the animals which compose it and which serve to distinguish them.

Thus, if we take up an earthworm we see that its body is composed of a series of similar segments or rings placed one behind the other, and we know that it belongs to that great sub-kingdom of ringed animals termed *Annulosa*.

If we examine a thousand-legs or a wood-louse we see that here again the body is evidently composed of a series of rings or segments, to most of which jointed legs are attached. A successive survey of a lobster, a scorpion, a bee, a beetle, or a butterfly will reveal to us that all these creatures, however different in other respects, all belong to the same ringed type, *i.e.*, that they are all members of the sub-kingdom *Annulosa*, which



G. 2.—Tadpoles in different stages of development, from those just hatched (1) till the adult form is attained (8).

contains all such animals, all insects, together with spiders, earthworms, and leeches.

Another great sub-kingdom called *Mollusca* contains all snails, slugs, cuttle-fishes, and creatures of the oyster and scallop class. Such animals have not the body composed of a series of similar segments, but are united by characters less obvious indeed, but as distinctive.

A third sub-kingdom called *Molluscoida* is made up of the sea-squirts, or Ascidians (sometimes called Tunicates) and lamp-shells, together with minute animals living in water in compound aggregations, like the *Flustra* (or Sea-mat) so common on our coasts, the surface of which is pitted with minute depressions, in each of which a minute animal had in life its abode—as doves in a dove-cot, if we imagine each fastened in its cell by natural growth.

A fourth sub-kingdom, *Annuloida*, is composed of such animals as star-fishes and sea-urchins, together with internal parasites (tape-worms, &c.) and their allies.

The fifth sub-kingdom is named *Cœlenterata*, and contains all sea-anemones, jelly-fishes, Portuguese men-of-war, polyps, and coral animals, these being the little creatures which have formed the atolls (or coral islands) of southern seas, and the vast reefs which stretched for so many hundred miles on the earth's surface.

The sixth sub-kingdom, *Protozoa*, comprises the Sponges, the Infusoria, and all the lower forms of animal life.

Now the whole of these six sub-kingdoms may be contrasted with the last and seventh, which bears the name *Vertebrata*, from which they all differ in several important particulars, and therefore they are often spoken of by the common and convenient term *Invertebrata*.

When we examine a fish (such as a sole, a herring, or a mackerel), one of the first things likely to be noticed by us on dividing it, is a solid structure—the backbone—extending from the head to the tail, and coated externally by the flesh.

This backbone is soon seen to be made up of a number of pieces jointed together. Each piece is called in natural history a *vertebra*, and every animal in which such a structure is found, is called, on that account, a Vertebrate animal.

Now every kind of beast and reptile agrees with these fishes in the possession of the vertebrate backbone, as well as in a variety of other important characters, which constitute the definition of the sub-kingdom *Vertebrata*.

Thus in the development of the egg of every Vertebrata (such *e.g.* as in that of the fowl), the first indication of the future animal, is the appearance on part of its surface of a minute longitudinal furrow called the *primitive groove*. Next the margins of this groove ascend to meet together above, thus enclosing a canal, the lining of which becomes thickened and transformed into no less important a structure than the brain and spinal marrow.

Concomitantly with the development of this canal, there is found, immediately beneath it, a little gelatinous rod enclosed in a membraneous envelope, and called the *notochord*, or *chorda dorsalis*. It is this structure which is subsequently developed and becomes the backbone.

Another singular condition is invariably presented in the development of every vertebrate, whether the structures formed are transitory or permanent.

This condition is the appearance of a certain series of openings formed at the side of the neck, and which, in fishes, remain permanent as the gill openings. These openings are termed *visceral clefts*, and lead from the exterior into the throat. The solid pillars (or intervals) between the clefts are called *visceral arches*, and in creatures (*e.g.* fishes) which develop gills upon them, *branchial arches*.

In all vertebrates again (unlike insects or spiders) there are never more than four limbs, and these are supported by bones, or cartilages, which are clothed externally with flesh, and are not moved by muscles placed *within* the hard parts, as is the case with lobsters, insects, and all their allies.

The heart in all vertebrates, consists of at least two distinct cavities, and sends forth blood into a system of arteries, thence it is brought back again to the heart by other vessels termed the veins. On its way back to the heart, however, some of the veins carry blood to be redistributed in the liver, forming what is called the *portal* circulation.

In all the points above enumerated, the Frog (as we shall shortly see) fully agrees with beasts, birds, reptiles, and fishes, and thus shows that it differs from the immense majority of animals—the *Invertebrata*—and pertains unmistakeably to the seventh sub-kingdom of animals—the *Vertebrata*.

Now every sub-kingdom of animals is further divided into a greater or lesser number of subordinate (though still large) groups, termed *classes*. Each class is again subdivided into a certain number of smaller and more subordinate groups, each of which is termed an *order*. Each order is made up of *families*, each family being of course, smaller, and more subordinate than an order. Every family consists again of still more subordinate groups, each of which is termed a *genus*. And every genus comprises one or more *species*.

In zoology, every animal bears a name composed of two words. The first of these is a substantive, and denotes the genus to which any given animal belongs. The second word is an adjective—or a word used in an adjective sense—and denotes which species of the genus that given animal is. Thus the Chimpanzee is called *Troglodytes niger*, it is the species *Niger* of the genus *Troglodytes*, which genus contains also another species, namely, the Gorilla.

ST. GEORGE MIVART

(To be continued.)

NOTES

It would be well if our men of science were to be found more frequently distributing prizes and taking an interest in the schools in which, thanks to the wisdom and energy of Mr. Cole, so many thousands of our people are learning science. In this Prof. Williamson has just set a good example by distributing the prizes at the Keightley School of Science and Art on Thursday last. Prof. Williamson, at the end of his speech, remarked that "We in this country give a peculiar position to Science in relation to material affairs. If we find a coal-seam we look upon it as wasteful not to work it and make the most of it, but what he said was, that to leave the clear heads and true hearts of our countrymen left useless was a greater waste, because he believed that they were infinitely more valuable than any coal-seam that ever was discovered."

An anonymous donor has placed a large sum in the hands of the Committee of the Birmingham and Midland Institute, for the foundation of a Lectureship on the Laws of Health, and also for a prize fund in connection with the class. Dr. Corfield has been offered the post for this year, has accepted it, and will deliver an inaugural lecture in the Town Hall, Birmingham, on Thursday, October 9, at 8 P.M., on "Sanitary Progress." The course will begin on Tuesday, October 14, at 8 P.M., and be continued on succeeding Tuesdays until some time in April. It is intended more especially for the working classes, and both men and women will be admitted.

The programme of the Birmingham and Midland Institute for Session 1873-4 is a very full one, and, to judge from what is set down, is well organised in its departments, and doing a thoroughly good educational work among all classes of the populous and important district in the midst of which it is established. At a merely nominal fee it places valuable scientific instruction within the reach of the poorest artisan.

Sir Samuel and Lady Baker left Alexandria for London on Tuesday.

We would draw our readers' attention to a letter from Professor Thiselton Dyer, in this week's number, on the Oxford Fellowships in Science about to be competed for. We hope that, at any rate, the matter of Research will be taken into consideration.

Next year's meeting of the American Association for the Advancement of Science will be held at Hartford, Conn., and the officers elect are :—President, Dr. John L. Le Conte, of Philadelphia; Vice-President, Prof. C. S. Lyman, of New Haven; Gen. Sec., Dr. A. C. Hamlin, of Bangor; Treasurer, Mr. W. S. Vaux, of Philadelphia.

The Italian Association for the Advancement of Science meets on the 20th inst.

The business of the Social Science Congress opened at Norwich yesterday, with a meeting of the Council, after which there was a special service in the Cathedral; and in the evening the inaugural address was delivered by the President. To-day the exhibition of sanitary and educational apparatus and appliances at the Drill Hall, kindly lent for the occasion, will be opened with an address by the High Sheriff of Norwich. The address of the President of the Council, Mr. G. W. Hastings, will follow, after which the departments will meet in their respective rooms, and in the evening a *soirée* will be given by the local Executive Committee in St. Andrew's Hall. On Friday morning Mr. Joseph Brown, Q.C., will deliver his address as president of the Department of Jurisprudence and Amendment of the Law; and after the meetings of the various departments for the reading and discussion of papers, a working men's meeting in St. Andrew's Hall, at which the Mayor will preside, will conclude the business of the day. On Saturday an address on education will be delivered by Prof. W. B. Hodgson, LL.D., and after the rising of the departments the President of the Congress will distribute the certificates and prizes to the successful candidates at the last Cambridge middle-class examination. The address of Capt. Douglas Galton, C.B., F.R.S., president of the Health Department, will be given on Monday morning. The departments will meet as usual in their respective rooms, and in the evening a grand concert will be given in St. Andrew's Hall. Mr. Thomas Brassey, M.P., will deliver his address on Economy and Trade on Tuesday, and after the business of the departments a *soirée* will be given in St. Andrew's Hall by the Mayor, and the concluding meeting, preceded by a meeting of the Council, will be held on the Wednesday. In connection with the Congress there will be a conference on female education, and in the Exhibition short addresses will be delivered daily in the afternoon on the subject of the articles exhibited in the various classes. Excursions to various places, it is understood, are being arranged.

The *Diana*, screw steamer, in which Mr. B. L. Smith left Dundee in May last on a voyage of discovery to the Polar Seas, by the Spitzbergen route, arrived in Dundee on Saturday last. The *Daily News* sums up the voyage of the *Diana* as follows:—A succession of gales was experienced—the weather on almost all occasions when the ship was in the open sea being such that, although she was provided with complete apparatus for sounding, deep-sea temperatures, &c., not nearly what was intended has been accomplished. Owing to the unfavourable nature of the ice, little in the way of exploration has been possible. The time had, however, been very fully occupied in dredging, trawling, photographing, surveying, and making as complete and perfect collections as circumstances permitted of the flora of Spitzbergen. Specimens of rare birds have been secured, and collections made, probably the first of any value. The collections of marine plants and animals are likely to prove especially interesting, and it has been discovered, among other things, that some parts of those seas hitherto reported as almost destitute of fish, abound in cod of excellent quality. In the way of geology everything possible was done in the parts unexplored by the Swedes, and numerous specimens of fossils have been brought back from the hitherto unvisited parts of the coast of the north-east land. From the appearance of open water seen in this expedition beyond Cape Platen, and also reported by the Swedes as existing—ascertained during their sleigh journey—it seems to be by no means certain that the route farther northwards which the *Diana* on leaving England hoped to reach does not exist, and the question still remains open, were it possible to reach this early in the season, whether a means of reaching a higher latitude to the north-east of Spitzbergen is not available. Mr. Smith has ascertained that the North Cape is situated on an island separated by a sound from the main land, and to this extent a knotty point has been determined. The expedition never got beyond 81°, while Mr. Smith in his expedition of 1871 got to 81·24°. He states that the *Diana* behaved admirably, but he did not realise his anticipations which would be achieved by the substitution of steam for sailing power.

With reference to our announcement of the forthcoming work by Mr. Boyd Dawkins on Cave Hunting,—the new line of inquiry which has added so much to our knowledge of ancient man,—we may now state the work will comprise the physical history of caves and their relation to the general physical geography of the district, as well as the history of their contents; and will treat of the men who have inhabited the caves of France, Spain, and Britain, during the historic, pre-historic, and pleistocene ages. The subject bristles with problems ethnological, archæological, and geographical, and demands a careful criticism that will sift the certain from the uncertain. The evidence will be given from which it may be concluded that the Eskimos lived as far to the south as the Pyrenees in the palæolithic age, and that the Basque or Iberic population ranged as far north as the British Isles.

THE "Astronomical Observations taken during the years 1870—72, at the private observatory of Mr. Joseph Gurney Barclay, Leyton, Essex," by Mr. C. G. Talmage, contains well-arranged tables of double star observations, followed by copious notes on the observations, and occultations, and phenomena of Jupiter's satellites. Mr. Barclay thinks it so advisable to reduce and print observations at short intervals, that he has determined, wisely we think, to adopt the plan without waiting for a number to form a large volume.

AMONG Messrs. Smith, Elder and Co.'s announcements of forthcoming works, we observe the following:—A translation of Prof. "Hermann's Elements of Physiology," by Dr. Arthur Gamgee; and "A Text Book of Pathological Anatomy," by John Wyllie, M.D., Lecturer on General Pathology at the School of Medicine, Surgeons' Hall, Edinburgh.

AMONG Mr. Robert Hardwicke's autumn announcements we notice the following scientific books—"Man and Apes:" an Exposition of Structural Resemblances and Differences bearing upon questions of affinity and origin, by St. George Mivart, F.R.S. This work will be published simultaneously in America and England. "Waste Products and Undeveloped Substances:" a synopsis of progress during the last quarter of a century at home and abroad, by P. L. Simmonds, the editor of the "Journal of Applied Science." "Where there's a Will there's a Way; or, Science in the Cottage," by James Cash; being an account of the labours and lives of some north-country botanists in humble life. "The British Hepatica," with descriptions by Dr. Carrington, and drawings by J. E. Sowerby. This will be issued in twelve monthly parts. "Hooker's Synopsis Filicum," a new edition brought up to the present time by J. G. Baker, Royal Herbarium, Kew. "On Mounting Microscopic Objects," by Thomas Davies. A new edition, much enlarged, by John Matthews, M.D., F.R.M.S. This last-named work is nearly ready for publication.

THE library of the Manchester Athenæum was destroyed by fire on Sept. 24. The damage, estimated at 10,000*l.*, is said to be wholly covered by insurance.

WE have received the programme of the Edinburgh Veterinary College. We hope that, under the superintendence of the new Principal, Prof. Fearnley, this important institution will become more prosperous than it has ever been, and that the principles of the veterinary art will be taught in a thoroughly scientific way. That this is likely to be the case may be seen from the following list of professors:—Dr. Balfour, F.R.S., Dr. Murie, Mr. Dewar, F.R.S.E., Dr. Young, and Mr. Wally.

THE following are some of the most important recent additions to the Brighton Aquarium:—2 Octopus (*O. vulgaris*); 1 Group of Barnacles (*Lepas Hillii*); 30 Sea-horses (*Hippocampus ramulosus*); 5 African Crocodiles; 2 Alligator Terrapins (*Chelydra serpentina*); 1 Edible Turtle (*Chelonia midas*); 1 Sturgeon (*Acipenser sturio*).

THE additions to the Zoological Society's Gardens during the past week include a Brown Capuchin (*Cebus fatuellus*) from Guiana, and two Bonnet Monkeys (*Macacus radiatus*) from India, presented by Lord Louth; two Crested Ground Parakeets (*Calopsitta novæ-hollandiæ*) from Australia, presented by Miss L. E. Lyon, and two hatched; four Alpacas (*Lama pacos*), two Llamas (*Lama peruana*) from Peru, a Vicuna (*Lama vicugna*) from South America; a Cuvier's Gazelle (*Gazella cuvieri*) from Muscat; a Sultry Hermipode (*Ortyxelos meiffreni*) from West Africa; a Southern Mynah (*Acridotheres mahrattensis*) from S. India, deposited; a Philantomba Antelope *Cephalophus maxwelli*) from Sierra Leone, received in exchange.

MOLECULAR EVOLUTION

AT quite uncertain times and places
  The atoms left their heavenly path,
And by fortuitous embraces
  Engendered all that being hath.

And though they seem to cling together
  And form "associations" here,
Yet, late or soon, they burst their tether,
  And through the depths of space career.

So we, who sat, oppressed with Science,
  As British Asses, wise and grave,
Are now transformed to fierce Red Lions,
  As round our prey we ramp and rave.

Thus by a swift metamorphosis,
  Wisdom turns wit, and Science joke;
Nonsense is incense to our noses,
  For when Red Lions speak they smoke.

Hail, Nonsense! dry nurse of Red Lions,*
  From thee the wise their wisdom learn,
From thee they cull those truths of science
  Which into thee again they turn.

What combinations of ideas
  Nonsense alone can wisely form,
What sage has half the power that she has
  To take the towers of Truth by storm?

Yield, then, ye rules of rigid reason!
  Dissolve, thou too, too solid sense!
Melt into nonsense for a season,
  Then in some higher form condense.

Soon, ah! too soon, the chilly morning
  This flow of soul will crystallise,
And those who nonsense now are scorning
  May learn too late where wisdom lies.

$\frac{dp}{dt}$

THE BRITISH ASSOCIATION

WE are glad to say that the attendance at the Bradford Meeting was considerably larger than was at first stated. The total number of persons who attended the meeting is 1,983, and the total amount received, 2,102*l.*

The following is a list of the grants of money appropriated to scientific purposes by the General Committee:—

	£	s.	d.
Mathematics and Physics			
Cayley, Prof.—Mathematical Tables	100	0	0
Cayley, Prof.—Printing Mathematical Tables ...	100	0	0
Glaisher, Mr. J.—Efficacy of Lightning Conductors (renewed)	50	0	0
Balfour Stewart, Prof.—Mauritius Observatory ...	100	0	0
Balfour Stewart, Prof.—Magnetism of Iron ...	20	0	0
Brooke, Mr. C.—British Rainfall	100	0	0
Glaisher, Mr. J.—Luminous Meteors	30	0	0
Tait, Prof.—Thermo-Electricity (renewed) ...	50	0	0
Williamson, Prof. A. W.—Testing Siemens' Pyrometer (renewed)	30	0	0
Chemistry			
Brown, Prof. Crum.—High temperature of Bodies (partly renewed)	70	0	0
Williamson, Prof. A. W.—Records of the Progress of Chemistry	100	0	0
Gladstone, Dr.—Chemical Constitution and Optical Properties of Essential Oils	10	0	0
Armstrong, Dr.—Isomeric Cresols and their Derivatives	20	0	0
Carried forward	£780	0	0

* "Leonum arida nutrix."

Brought forward	£780 0 0
Geology	
Herschel, Prof.—Thermal Conducting Power of Rocks	10 0 0
Phillips, Prof.—Labyrinthodonts of the Coal Measures	10 0 0
Bryce, Dr.—Collection of Fossils in the North-West of Scotland	10 0 0
Wiltshire, Rev. T.—Investigation of Fossil Corals	25 0 0
Willett, Mr. H.—The Sub-Wealden Exploration...	25 0 0
Lyell, Sir C.—Kent's Cavern Exploration ...	150 0 0
Harkness, Prof.—Mapping Positions of Erratic Rocks and Boulders	10 0 0
Woodward, Mr. H.—Record of Geological and Palæontological literature	100 0 0
Lubbock, Sir J.—Exploration of Victoria Cave ...	50 0 0
Biology	
Lane-Fox, Col. A.—Forms of Instruction for Travellers (25*l.* renewed)	50 0 0
Stainton, Mr.—Record of the Progress of Zoology	100 0 0
Jeffreys Mr. Gwyn.—Dredging off the Coasts of Yorkshire	30 0 0
McKendrick, Dr.—Physiological Action of Light	20 0 0
Brunton, Dr.—The Nature of Intestinal Secretion	20 0 0
Foster, Dr. M.—Methods of Breeding the Embryos of Delicate Marine Organisations	30 0 0
Statistics and Economic Science	
Houghton, Lord.—Economic Effects of Trades Unions	25 0 0
Mechanics	
Froude, Mr. W.—Instruments for Measuring the Speed of Ships and Currents (renewed) ...	50 0 0
	1,495 0 0
Widow of the late Mr. Askham (Clerk to the Association)	50 0 0
	£1,545 0 0

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICS

Report of the Luminous Meteor Committee of the British Association on Observations of Shooting-stars in 1872-73.

Shooting-stars and large fireballs have appeared during the past year in more than usual varieties. Large meteors have presented themselves in considerable numbers, and ordinary shooting-stars in a more striking manner as regards the explanation of their origin than has often been the case in former years. Of all these kinds of shooting-stars, both large meteors and meteoric showers, much accurate information has reached the committee; but the extent of the knowledge acquired on all hands, has at the same time advanced so rapidly, that a smaller amount of attention has this year been directed towards the discussion of the individual descriptions, than the committee have hitherto bestowed upon them, and a more complete reduction of the separate observations will accordingly be attempted when the opportunities of the committee allow of their closer examination.

Those meteors, however, which have been observed simultaneously at more than one observing station have been selected from the collection for transcription in suitable columns in this report, and a list of large meteors is added, among which some have occurred that have without doubt been noticed, and may have attracted attention in other directions, than has hitherto come to the knowledge of the committee. Two of the largest fire-balls seen in Great Britain were aërolitic, or burst with the sound of a violent explosion on November 3 and February 3 last. The first passed over the central part of Scotland, and the second burst over Manchester and its neighbourhood at half-past five, and at 10 o'clock respectively on the evenings of those days. Aërolitic meteors and aërolites have also been noticed in the scientific journals of other countries, which have given rise to experiments on the composition of aërolitic substances, both chemical and microscopical, the conclusions of which continue to extend the range of our speculations regarding the origin of these bodies. Thus the existence of carbon and hydrogen in the atmosphere from which the largest iron meteorite yet found (a few years since upon the shores of Greenland) was expelled, confirms the discoveries of Grahame and Professor Mallet, in America, of the existence of the same gases in other meteoric irons. Dr. Wöhler has thus detected the oxides of carbon as gases in the vast meteoric iron of Ovifak found in Greenland, and brought to Stockholm during the last few years by Prof. Nordenskiöld, and the same gas was found by Prof. Laurence Smith in the siderite which fell recently in the United States. A connection between comets and meteorites appears to be indicated by these discoveries, in the spectra of some of which gases containing carbon appear to have been certainly recognised by Dr. Huggins.

The past year was distinguished by the occurrence of a most remarkable star shower on the night of November 27 last, to the expected appearance of which astronomers were looking forward with especial attention from the unexplained absence of the double comet of Biela (to which it belongs) from its accustomed returns in the last three of its periodical revolutions. The probability of the comet's path being marked by a meteoric stream into which the earth might plunge on or about Nov. 27 every year was already become a certainty, by the observation of such a meteoric stream on Nov. 30, 1867. On that night M. Zezioli of Bergamo, observed a distinct star-shower, according to Schiaparelli, no doubt of whose belonging to the missing comet could be entertained. Although the exact date of the shower could not be accurately foretold with certainty from the want of recent observations of the comet, yet every probability of its being seen was favourable to its reappearance last year, and those who awaited it, as well as many unexpected watchers of meteor-showers, were surprised by the brilliant spectacle which it suddenly presented. At the first approach of darkness on the evening of Wednesday the 27th of November last, the cloudy state of the sky unfortunately deprived observers in the south of England from witnessing the sight, but in Scotland, and north of the Midland Counties of England many uninterrupted views of it were obtained. On the European continent and in the United States of America, as well as in the East Indies, at the Mauritius and in Brazil observers were equally fortunate in recording its appearance, and few great star-showers have hitherto been more satisfactorily observed, or indeed more abundantly described. In an astronomical point of view the agreement of the time and other circumstances of its appearance with the supposed path of the lost comet is so exact as to prove that the calculations made by astronomers of that comet's orbit cannot be affected by any errors of a large sensible amount, and a proof almost certain is thus obtained that the disappearance of the comet is owing to no unexplained disturbances of its path; but that like some former comets of variable brightness, it has not improbably faded for a time out of view, and that at a future time a reasonable expectation may be entertained of re-discovering it pursuing its original path in repeated visits to the earth's neighbourhood, and to the field of telescopic observation.

Only partial views of the ordinary periodical meteor showers of December, January, and April last were obtained, of which some descriptions are contained in the Report.

Reductions of the scattered meteor-observations on ordinary nights of the year are an important subject of the Committee's inquiries, which have been kept in view in their operations of the past year. Captain Tupman having obligingly placed a list of nearly 6,000 such observations (made by himself) at their disposal, the greater part of which he has reduced to their most conspicuous radiant points, the present purpose of the Committee is most effectually obtained by the publication of the valuable meteor list which has thus unexpectedly come into their possession; and a graphic projection of the radiant points has been prepared, which will be printed as an illustration of the copious information that will be gathered by observers from the contents of Captain Tupman's list. The catalogue will be distributed this year to observers interested in the research; and to enable suitable lithographic charts to be added to it, it is hoped that the members of the British Association will assist the Committee with such liberal communications of their observations as they have hitherto abundantly supplied.

Note on a Natural Limit to the Sharpness of the Spectral Lines, by Lord Rayleigh, F.R.S.

In the explanation usually given of the broadening of the fixed lines with increased pressure, it appears to be assumed that their finite width depends on the disturbance produced by the mutual influence of the colliding molecules. I desire to point out that even if each individual molecule were allowed to execute its vibrations with perfect regularity, the resulting spectral line would still have a finite width, in consequence of the motion of the molecules in the line of sight. If there is any truth at all in the kinetic theory of gases, the molecules of sodium, or whatever the substance may be, are moving in all directions indifferently, and with velocities whose magnitudes cluster about a certain mean. The law of distribution of velocities is probably the same as that with which we are familiar in the theory of errors, according to which the number of molecules affected with a given velocity increases, the nearer that velocity is to the mean.

By the principles of this theory of gases the mean square of the velocity of the molecules can be deduced from the known pressure and mass. If v denote the velocity whose square is equal to the mean, it is found that for air at the freezing-point, $v = 485$ metres per second.

At the temperature of flame, the velocity may be about three times greater. For the purposes of a rough estimate it will be accurate enough to take the mean velocity of the molecules at 1,500 metres per second, and that of light at 300,000,000 metres per second. The wave-length of the light emitted by a molecule moving with the mean velocity from the eye will therefore be greater by about five millionths than if the molecule were at rest. The double of this will be a moderate estimate of the width of the spectral line, as determined by the cause under consideration. We may conclude that however rare the gas, and however perfect our instrument may be, a fixed line cannot be reduced to within narrower limits than about a hundredth part of the interval between the sodium lines. I must leave it to spectroscopists more practised and skilful than myself to say whether this result is in agreement with the appearance of the spectrum.

SECTION B.—CHEMISTRY

The report of the Committee appointed to examine the *Methods of making Gold Assays and stating the Results thereof*, was read by Mr. W. C. Roberts.

The report stated that although the amount of alloy in gold could be ascertained to within a maximum error of 0·01 per cent., or one ten-thousandth part, yet there was an amount of difference between the results obtained by different assayers which required an explanation. The committee considered that the difference between different assayers was too great to be accounted for by the ordinary causes of error in analysis, and they had therefore come to the conclusion that the nominally assayed gold must have contained some impurity which had escaped the assaying process. The committee had precipitated eighty ounces of gold from no less than a hundred gallons of chloride of gold, and they suggested that the gold thus obtained might be used as a standard with which the gold assayed by different assayers might be compared.

Mr. A. Vernon Harcourt, F.R.S., and Mr. F. W. Fison, F.C.S., explained a *Continuous Process for Purifying Coal Gas and obtaining Sulphur and Ammonium Sulphate.*

Mr. Vernon Harcourt said that the usual method of freeing coal gas from sulphuretted hydrogen was by passing it through lime. But oxide of iron was also employed in place of the lime, the advantage possessed by the oxide being that whilst the lime, after it had served its purpose, was useless and difficult to get rid of, the oxide of iron could be used repeatedly for the same purpose. The chemical changes involved were, that when the gas had passed through the oxide the latter was changed into sulphide of iron; when the sulphide was exposed to the air, the sulphur separated and the oxide was re-formed, thus enabling the oxide to be again used. This was called a *continuous* process, because the oxide could be continuously used. But the process was not quite continuous, for after the oxide had been used some thirty times, it became so clogged with sulphur as to be useless. The advantage of the process he was about to describe was that the oxide could be used over and over *ad infinitum*; and, besides, the ammonia was secured in a marketable form. The present method of freeing gas from ammonia by "scrubbing," or passing it through a large receiver containing a small quantity of water spread over a large surface, had one or two defects. It did not secure the ammonia in a good form, and it probably diminished the illuminating power of the gas, for olefiant gas was soluble in water. The new process was applicable wherever oxide of iron could be used in the purifying process. The difference from the old process was that the oxide during revivification was moistened with a solution of ferric sulphate (per sulphate of iron), and a portion of the oxide was removed from time to time, and treated as follows:—It was first extracted with water by the use of a well-known arrangement. The soluble salts were sulphate of ammonia—formed in the purification by the reaction of ammonia upon ferric sulphate—and, in smaller quantities, sulphocyanide, hypo-sulphite, and probably sulphate of ammonia. This extract was mixed with a small excess of sulphuric acid; and yielded when concentrated by evaporation, crystals of ammonium sulphate. The remainder of the substance was then boiled with dilute sulphuric acid, which dissolved the oxide and left a residue of sulphur. The actual process of extraction by acid consisted in treating the substance successively with (1) a solution of ferric sulphate containing some free sulphuric acid; (2) with a more dilute solution of ferric sulphate to which sulphuric acid had been added; (3 and 4) with more dilute solutions of ferric sulphate—all these liquids being the product of a former extraction—and (5) with water. The liquid resulting from the first of the treatments enumerated above was a strong solution of ferric sulphate, which was used as already mentioned, by being mixed with the charge of oxide before it was replaced in the purifier. The residue of the final washing consisted almost entirely of sulphur, and required only to be dried. It would be evident that all the oxide which had been freed from sulphate of ammonia and sulphur by this treatment passed into the condition of ferric sulphate, and in this condition it was replaced in the purifier. There it again became oxide by the action upon it of the ammonia in the gas, which it completely removed, fixing it as sulphate. This system had been brought into use as a manufacturing process, and had been found to be, as far as could be judged, a complete success.

Mr. Fison explained at length the apparatus by which the process was carried into effect.

Mr. J. Spiller, F.C.S., gave a short communication on *Artificial Magnetite*, the object of which was, first, to point out an error in the statement of a chemical reaction occurring in several standard works of reference; and, in the second place, to indicate the formation of crystallised magnetic oxide of iron (magnetite) in the ordinary process of manufacturing aniline from nitro-benzol by the reducing action of metallic iron. Reference was made to "Reimann's Aniline and its Derivatives," and to Wagner's "Chemical Technology," where the action of iron upon nitrobenzol in the presence of acid (Béchamp's process) is stated to give ferric oxide, or a "hydrated oxide of iron." The author pointed to the fact that the ordinary residual product in this operation was *black*, and could be so far purified by washing and elutriation from the excess of iron, usually remaining in admixture, as to give a fine black pigment, which appeared under the microscope as minute octohedra, and was strongly magnetic. Chemical analysis showed this to consist almost entirely of magnetic oxide of iron, with such impurities as were inherent to the process, or previously existed in the cast iron. The physical properties of this form of oxide were further described, and its analogy to the native varieties of magnetic ore (Cornish and Dannemora) shown.

Mr. W. C. Roberts exhibited some specimens of artificial horn silver which he had formed by mixing strong solutions of silver nitrate and common salt.

Prof. Schafarick, of Prague, read a paper *On the Constitution of Silicates*, in which he developed his views as to the manner in which certain members of this class of bodies might be graphically represented.—Prof. Crum Brown, whilst complimenting the author on the importance of the step taken, pointed out that we should guard against confusing graphic formulæ, as applied to minerals, with those applied to organic substances, because they do not represent the same kind of knowledge. Structural formulæ in organic substances represented reactions, and not merely composition; in the case of minerals we had as yet no method of following their reactions.

Prof. Crum Brown then read a paper *On the Action of Sulphide of Methyl on Bromacetic Acid.* He said bromacetic acid dissolved readily in sulphide of methyl. The solution soon became warm and separated into two layers, the lower of which solidified into a white crystalline mass. The crystals were easily purified by washing with absolute alcohol, in which they were very sparingly soluble. Analysis had given a result for this compound which showed it to be a compound of one molecule of bromacetic acid, and one of sulphide of methyl. The compound was obviously analogous to hydrobromate of betain.

Mr. Jesse Lovett described an improved gas-burner.

simply a modification of Wallace's gas-burner. The improvement consisted in a simple mechanism whereby the air and gas could be shut off by one movement.

SECTION C.—Geology

Second Report on the Discovery of Fossils in certain remote parts of the North-western Highlands, by W. Jolly.

During the past year search has been made at various points along the great limestone strike of the North-western Highlands, but, with the exception of the Durness basin, from which the fossils already collected have been alone obtained, none have been found at any new locality. It is most desirable that continued search should be made for fossils, and to determine if the fossiliferous Durners limestone be the same as that in the line of strike from Eribol to Skye.

Report on Earthquakes in Scotland, by Dr. J. Bryce, F.G.S.

Last year a report on this subject was read at Brighton, stating that there had been but little to record during the year then reported on; but whilst the Association was sitting a shock occurred in the Comrie district, an account of which is given in the report now presented. The earthquake occurred on Aug. 8, 1872, at from 8m. to 10m. past 4 o'clock in the afternoon. The successive phases, according to almost all the observers, were:—a noise or sound, loud, heavy, and rumbling; a shock with a shaking and rattling of objects; and a wave-like motion of the ground. The undulations appear to have come from the W. or N.W.; according to some observers, from the opposite direction; but these probably did not distinguish between the first impulse and the recoil.

The extent of country through which the shock was felt is greater than that of any which has occurred since this inquiry was undertaken. The limits are marked by Stirling and Blair Logie on the S.E., and by St. Fillans on Loch Earn, and Glen Lednock on the N.W. The shock was feebler at their limits than in the country between, as about the Bridge of Allan, Dunblane, &c. The breadth of the disturbed area does not appear to have extended more than two or three miles from the Allan Water; the shock seems to have emanated near Comrie. The geological formations of the district are very various in character, and it does not appear that any connection can be traced between the nature of the rock forming the surface and the severity of the shock.

Another shock, which occurred at 9.55 P.M. on April 16, 1873, is briefly described. This was in the South of Scotland, in the parishes of Tyrone, Glencairn, and others adjacent. According to one observer, there was another shock in this district at 2.46 A.M. on the following morning.

Report of the Committee for Exploring the Settle Cave, by W. Boyd Dawkins, F.R.S.

This cave is of great interest, and is being explored by a local committee, aided by a grant from the British Association. In the newest layers there is evidence of human occupation during the historic period; but in the older cave earth, which contains the remains of extinct mammalia, no trace of man has yet been discovered. The exact age of the cave earth is a matter of dispute. Mr. Tiddeman, from the physical evidence alone, regards it as pre-glacial, or rather as older than the great ice-sheet of that district. Mr. Dawkins, whilst doubting the physical evidence afforded by the cave alone, is inclined to regard the fauna as pre-glacial, and he remarks:—"It is obvious that the hyænas, bears, mammoths, and other creatures found in the pleistocene stratum could not have occupied the district when it was covered by ice; and had they lived soon after the retreat of the ice-sheet, their remains would occur in the river-gravels, from which they are absent throughout a large area to the north of a line drawn between Chester and York, whilst they occur abundantly in the first glacial river deposits south of that line. On the other hand, they belong to a fauna, that overran Europe, and must have occupied this very region, before the glacial period. It may, therefore, reasonably be concluded that they occupied the cave in pre-glacial times, and that the stratum in which their remains lie buried, was protected from the grinding of the ice-sheet, which destroyed nearly all the surface accumulations in the river-valleys, by the walls and roof of rock, which has since, to a great extent, weathered away."

Report of the Boulder Committee, by Rev. H. W. Crosskey, F.G.S.

This committee was appointed at the Brighton meeting to collect and tabulate information upon the distribution of erratic blocks throughout England and Wales. Good work has already been done in Scotland by a committee formed for a similar purpose. It is evident that some steps should at once be taken to record the existence of remarkable blocks, and if possible to take some steps to ensure their preservation.

The report, which is necessarily chiefly preliminary, describes the distribution of boulders around Charnwood Forest, and refers to the existence of Charnwood Forest boulders in Shropshire. It also contains a notice, by Mr. Pengelly, of a large granite boulder below the raised beach in Barnstaple Bay. An account is given of the place adopted by the Geological Section of the Birmingham Natural History. Society for mapping the boulders of their district, a plan so effective that we reproduce the paragraph referring to it in the hopes that other districts may follow the good example here set. "The Ordnance map of the neighbourhood of Birmingham has, in the first place, been divided by ruled lines with squares of one inch wide, each square enclosing a representation of one square mile of country. Enlarged maps, on the scale of six inches to the mile, were prepared from this. On these enlarged maps the boulders are to be marked by circles, the number of concentric circles representing the diameter of one boulder in feet. For collecting specimens of the rocks of which the boulders are composed, bags were made and numbered, corresponding to each square on the map. At the same time notes were to be made of any specimee that was of unusual interest. Finally it was proposed to represent, on a duplicate map, the number of boulders and the character of the rocks by discs of colour, so that a graphic representation of the boulders as to position, numbers, and kind of rock, would be given, and the source of any class of boulders, as granite *e.g.*, could be readily traced. It was further proposed to make a rough relief map of the district, so as to judge in what way the configuration of the country had affected the distribution of the boulders.

On the Whin Sill of Northumberland, by W. Topley, F.G.S., and G. A. Lebour, F.G.S.

This paper, the result of work by the authors during the progress of the Geological Survey, was laid before the section by permission of the Director-General of the Survey.

The basaltic rocks of the North of England occur in two forms, either as *dykes* cutting vertically through the rocks, or as *beds* lying amongst them. The intrusive character of the dykes is undisputed, but there is much uncertainty prevailing as to the character of the beds of basalt. The authors endeavoured to show that it too is intrusive, and has been forced in a melted state through the rocks long after their deposition and partial consolidation.

The Whin Sill is best known in Teesdale and along the face of the great Pennine escarpment. This district was only briefly alluded to, partly because it has already been often described, especially by Professors Sedgwick and Phillips, but also because the intrusive character of the rock is less evident there than in Northumberland.

An account of the literature of the subject was then given, and a MS. section of the Northumberland coast, made in 1822, by Sir Walter C. Trevelyan, Bart., was exhibited. Although the Whin Sill of more southern districts had been mentioned by earlier writers, it was not till the publication of Sir Walter Trevelyan's paper in the Wernerian Transactions for 1823, that attention was drawn to the intrusive character of the rock.

The Whin Sill is a true basalt, and does not differ in appearance or composition from the whin dykes of the district. In Teesdale it is very uniform in its position amongst the sedimentary strata; for this reason, and because it generally alters but slightly, if at all, the rocks above, Prof. Phillips, and most geologists who have given most attention to the Teesdale district, believe the whin to be of the same date as the beds amongst which it lies.

The object of the paper was to show that through Northumberland the Whin Sill is not so constant in position, that it frequently very greatly alters the beds above it as well as those below, and that, in numerous instances, it can be shown to cut through the strata in a manner that would be impossible with a contemporaneous bed. It also varies in position to an extent of more than 1000 feet, and often comes up, not in true beds, but in bosses.

Nothing can be certainly known as to the age of this Whin Sill. That it is later than the beds with which it is associated is certain, but many considerations lead to the inference that it may not be later than the latter part of the carboniferous period.

SECTION D.—Biology.

Department of Anatomy and Physiology.

The Localisation of the Functions in the Brain, by Professor Ferrier.

All are agreed that it is with the brain that we feel, and think and will, but whether there are certain parts of the brain devoted to particular manifestations is a subject on which we have only imperfect speculations or data too insufficient for the formation of a scientific opinion. The general view is that the brain as a whole subserves mental operations, and that there are no parts specially devoted to any particular functions. This has been recently expressed by so high an authority as Professor Séquard. The idea rests chiefly on the numerous facts of disease with which we are acquainted. There are cases where extensive tracts of brain are destroyed by disease, or removed after a fracture, apparently with no result as regards the mind of the individual. Along with these facts we have others which are very curious, and which hardly seem to agree with this doctrine. One of these is that when a certain part of the brain is diseased, in Aphasia, the individual is unable to express himself in words. Other curious phenomena have been well described by Dr. Hughlings Jackson, viz., that certain tumours or pathological lesions in particular parts of the brain give rise, by the irritation which they keep up, to epileptiform convulsions of the whole of one side, or of the arm or leg or the muscles of the face; and from studying the way in which these convulsions show themselves he was able to localise very accurately the seat of the lesion.

The great difficulty in the study of the function of the brain has been in the want of a proper method. When we study the function of a nerve, we make our experiments in two ways. In the first place, we irritate the nerve by scratching or by electricity, or by chemical action, and observe the effect; and in the second place, we cut the nerve, and observe what is lost. In regard to the brain and nervous system, the method has been almost entirely, until recently, the method of section. It has been stated by physiologists that it is impossible to excite the brain into action by any stimulus that may be applied to it, even that of an electric current; they have, therefore, adopted the method of destroying parts of the brain. This method is liable to many fallacies. The brain is such a complex organ that to destroy one part is necessarily to destroy many other parts, and the phenomena are so complex that one cannot attribute their loss to the failure only of the parts which the physiologists have attempted to destroy.

About three years ago, two German physiologists, Fritsch and Hitzig, by passing galvanic currents through parts of the brains of dogs, obtained various movements of the limbs, such as adduction, flexion, and extension. They thus discovered an important method of research, but they did not pursue their experiments to the extent that they might have done, and perhaps did not exactly appreciate the significance of the facts at which they had arrived.

I was led to the experiments which I shall have to explain by the effects of epilepsy and of chorea, which have been explained to depend upon irritation of parts of the brain. I endeavoured to imitate the effects of disease on the lower animals, and determined to adopt the plan of stimulating the parts of the brain by electricity, after the manner described by Fritsch and Hitzig.

I operated on nearly a hundred animals of all classes—fish, frogs, fowls, pigeons, rats, guinea pigs, rabbits, cats, dogs, jackalls, and monkeys. The plan was to remove the skull, and keep the animal in a state of comparative insensibility by chloroform. So little was the operation felt that I have known a monkey, with one side of the skull removed, awake out of the state induced by the chloroform, and proceed to catch fleas or eat bread and butter. When the animal was exhausted I sometimes gave it a little refreshment, which it took in the midst of the experiments.

First, as to the experiments on cats, I found that on applying the electrode to a portion of the superior external convolution the animal lifted its shoulder and paw (on the opposite side to that stimulated) as if about to walk forward; stimulating other parts of the same convolution, it brought the paw suddenly back, or put out its foot as if to grasp something, or brought forward its hind leg as if about to walk, or held back its head as if astonished, or turned it on one side as if looking at something, according to the particular part stimulated. The actions produced by stimulating the various parts of the middle external convolution were a drawing up of the side of the face, a backward movement of the whiskers, a turning of the head, and a contraction of the pupil respectively. A similar treatment of the lower external convolution produced certain movements of the angles of the mouth; the animal opened the mouth widely, moved its tongue, and uttered loud cries, or mewed in a lively way, sometimes starting up and lashing its tail as if in a furious rage. The stimulation of one part of this convolution caused the animal to screw up its nostrils on the same side; and, curiously enough, it is that part which gives off a nerve to the nostril of the same side.

Results much of the same character were produced by the stimulation of the corresponding or homologous parts of the rat, the rabbit, and the monkey. Acting upon the anterior part of the ascending frontal convolution the monkey was made to put forward its hand as if about to grasp. Stimulation of other portions acted upon the biceps, and produced a flexing of the fore-arm, or upon the zygomatic muscles. The part that appeared to be connected with the opening of the mouth and the movement of the tongue was homologous with the part affected in man in cases of aphasia. Stimulation of the middle temporo-sphenoidal convolution produced no results; but the lower temporo-sphenoidal, when acted upon, caused the monkey to shut its nostrils. No result was obtained in connection with the occipital lobes.

These experiments have an important bearing upon the diagnosis in certain kinds of cerebral disease, and the exact localisation of the parts affected. I was able to produce epileptic convulsions of all kinds in the animals experimented upon, as well as phenomena resembling those of chorea or St. Vitus's dance. The experiments are also important anatomically, as indicating points of great significance in reference to the homology of the brain in lower animals and in man, and likewise served to explain some curious forms of expression common to man and the lower animals. The common tendency, when any strong exertion is made with the right hand, to retract the angle of the mouth and open the mouth on the same side, had been stated by Oken, in his *Natur-geschichte*, to be due to the homology between the upper limbs and the upper jaw; the true explanation being that the movements of the fist and of the mouth are in such close relation to each other that when one is made to act powerfully the impression diffuses itself to the neighbouring part of the brain and the two act together.

The experiments have likewise a physiological significance. There is reason to believe that when the different parts of the brain are stimulated, ideas are excited in the animals experimented upon, but it is difficult to say what the ideas are. There is, no doubt, a close relation between certain muscular movements and certain ideas which may prove capable of explanation. This is supported by the phenomena of epileptic insanity. The most important guide on the psychological aspect of the question is the disease known as Aphasia. The part of the brain which is the seat of the memory of words is that which governs the movements of the mouth and the tongue. In Aphasia the disease is generally on the left side of the brain, in the posterior part of the inferior frontal convolution, and it is generally associated with paralysis of the right hand, and the reason might be supposed to be that the part of the brain affected is nearly related to the part governing the movements of the right hand.

It is essential to remember that the movements of the mouth are governed bi-laterally from each hemisphere. The brain is symmetrical, and I hold it to be a mistake to suppose that the faculty of speech is localised on the left side of the brain. The reason why an individual loses his speech when the left side of the brain is diseased is simply this. Most persons are right-handed, and therefore left-brained, the left side of the brain governing the right side of the body. Men naturally seize a thing with the right hand, they naturally therefore use rather the left side of the brain than the right, and when there is disease, there the individual feels like one who has suddenly lost the use of his right arm.

I may, finally, briefly allude to the results of stimulating the different ganglia. Stimulation of the corpora striata causes the limbs to be flexed; the optic thalami produces no result: the corpora quadrigemina produce, when the anterior tubercles are acted upon, an intense dilatation of the pupil, and a tendency to draw back the head and extend the limbs as in opisthotonos;

while the stimulation of the posterior tubercles leads to the production of all kinds of noises. By stimulating the cerebellum various movements of the eye-balls are produced.

In the discussion which ensued Dr. Geo. Harley alluded to the effect of mental emotion on the bodily functions, and the possibility of producing disease by simply acting on the nervous system. Referring to phrenology, he said it was one thing to localise function in the interior of the brain, and quite another to specify functions by manipulating the external cranium; and he quoted a saying of Flourens with reference to phrenology: "Les hommes qui la pratiquent sont des charlatans, et les hommes qui la croient sont des imbeciles."

Dr. Carpenter remarked that the great work of the brain is done in the cortical substance, and in Dr. Ferrier's experiments the first effect of the stimulus is upon that particular substance, producing an intensification of the circulation through it; being in that respect different from the ordinary stimulation of a nerve which acts upon the fibrous substance of the medullary matter of the brain. He had long since expressed his disbelief in phrenology, which maintained that the animal functions were placed at the back of the head, and the intellectual at the front. Dr. Ferrier's experiments tended to show that the real seat of the intellectual functions was in the posterior part of the brain.

Dr. Brunton, however, alluded to the faculty of will and of self-restraint as distinguishing man from the lower animals, and said that this was probably situated in the *anterior* part of the brain. It was noticeable that criminals, who were deficient in that faculty, possessed only a small portion of brain in front of the head.

Prof. Burdon Sanderson said that the stimulus in Dr. Ferrier's experiments was, contrary to Dr. Carpenter's supposition, exactly like the ordinary excitation of a nerve, and that the effect was produced in an extremely short space of time.

Note on Huizinga's Experiments on Abiogenesis, by Dr. Burdon-Sanderson.

Under the title of a "Contribution to the Question of Abiogenesis," Prof. Huizinga has very recently published (Pflüger's Archiv. vol. vii. p. 549) a series of experiments which deserve notice as constituting a new and carefully worked out attempt to support the doctrine of spontaneous generation.

Prof. Huizinga begins his paper with the words *Multa renascentur quæ jam cecidere*, using them as an expression of the recurring nature of this question. He then proceeds to say that he was induced to undertake his inquiry by the publication of the well-known work of Dr. Bastian (whom he compliments as having awakened the exhausted interest of physiologists in the subject), his special object being to repeat the much-discussed turnip-cheese experiment.

Everyone knows what Dr. Bastian's observation is. It is simply this, viz. that if a glass flask is charged with a slightly alkaline infusion of turnip of sp. g. 1015, to which a trace of cheese has been added, and is then subjected to ebullition for ten minutes and closed hermetically while boiling, and finally kept at fermentation temperature, Bacteria develop in it in the course of a few days. This experiment has been repeated by Huizinga with great care, and the accuracy of Dr. Bastian's statement of fact confirmed by him in every particular; yet notwithstanding this he thinks the evidence afforded by these results in support of the doctrine so inadequate, that he, desiring to find such evidence, has thought it necessary to repeat the experiment under what he regards as conditions of greater exactitude.

Huizinga's objections to Bastian's experiment are two. First, that when a flask is boiled and closed hermetically in ebullition, its contents are almost entirely deprived of air, and (2) that cheese is a substance of mixed and uncertain composition. To obviate the first of these objections, he closes his flasks, after ten minutes boiling, not by hermetically sealing them, but by placing over the mouth of each, while in ebullition, a porous porcelain plate which has just been removed from the flame of a Bunsen's lamp. The hot porcelain plate is made to adhere to the edge or lip of the flask by a layer of asphalt with which the edge has been previously covered. The purpose of this arrangement is to allow air to enter the flask, at the same time that all germinal matter is excluded. It is not necessary to discuss whether this is so or not.

To obviate the second objection he alters the composition of the liquid used: he substitutes for cheese, peptone, and for turnip infusion, a solution containing in a litre of distilled water:—

Grape sugar	25	grammes
Potassium nitrate	2	,,
Magnesium sulphate	2	,,
Calcium phosphate	0·4	,,

The phosphate is prepared by precipitating a solution of calcium chloride with ordinary sodium phosphate, taking care that the chloride is in excess. The precipitate of neutral phosphate so obtained is washed and then added to the saline solution in the proportion given. On boiling it is converted into soluble acid phosphate, and insoluble basic salt, of which the latter is removed by infiltration. Consequently the proportion of phosphate in solution is less than that above indicated.

To the filtrate, peptone is added in the proportion of 0·4 per cent.

The peptone is obtained by digesting egg-albumen at the temperature of the body in artificial gastric juice made by adding the proper quantity of glycerin extract of pepsin to water acidulated with hydrochloric acid. The liquid so obtained is first rendered alkaline by the addition of liquor sodæ, then slightly acidulated with acetic acid and boiled. The syntonin thus precipitated is separated by infiltration from the clear liquid, which is then evaporated to a syrup and poured in a thin stream into strong alcohol, with constant agitation. The precipitated peptone is separated after some hours and washed with alcohol, and redissolved in a small quantity of water. The solution is again precipitated by pouring it into alcohol in the same way as before, and the precipitate washed and dried.

Flasks having been half filled with the liquid thus prepared (in 1,000, 2 each of nitre and Epsom salts, a trace of phosphate of lime, 25 parts of grape sugar, and 4 parts of peptone), each is boiled for ten minutes, closed while boiling, with the earthenware plate as above described, and placed as soon as it is cool in the warm chamber at 30° C. The experiment so made "gave, without any exception, a positive result in every case. After two or three days the fluid was crowded with actively moving Bacterium termo."

The readers of NATURE are aware that in June last I published a repetition of Dr. Bastian's experiments with a variation not of the liquid but of the mode of heating (see NATURE, vol. viii., p. 141). Instead of boiling the flasks for ten minutes over the open flame and closing them in ebullition, I boiled them, closed them hermetically, and then placed them in a digester in which they were subjected to ebullition under a pressure of two inches or more of mercury. The result was negative. There was no development of Bacteria.

Since the publication of my experiments Huizinga's have appeared. His result, regarded as a proof of spontaneous generation is clearly not superior to Bastian's. The substitution of a soluble immediate principle for an insoluble mixed product like cheese, and the use of a definite solution of sugar and salts are not material improvements. The question is not whether the germinal matter of Bacteria is present, but whether it is destroyed by the process of heating. Consequently what is necessary is not to alter the liquid but to make the conditions of the experiment as regards temperature as exact as possible. In this respect Huizinga's experiment is a confirmation of Bastian's and nothing more.

I have recently repeated it with the same modifications as regards temperature as those employed in my repetition of the turnip-cheese experiments. The result has been the same. In all other respects I have followed the method described by him in his paper.

I have prepared the solution of salts, grape sugar, and peptone in exact accordance with his directions. To obviate his objection as to the absence of air, I have introduced the liquid, not into flasks, but into strong glass tubes closed hermetically at each end and only half filled with liquid, the remainder of the tube containing air at the ordinary tension. Each of these tubes, after having been subjected to the temperature of ebullition under two inches of mercury for half an hour, has been kept since September 10 at the temperature of fermentation (32° C.). Up to the present time, no change whatever has taken place in the liquid.

As a control experiment I opened one of the tubes immediately after boiling, and introduced a drop of distilled water. It became opalescent in twenty-four hours.

In conclusion let me observe that I still maintain my resolution to take no side whatever in this controversy. I do not hold that spontaneous generation is impossible. I do not regard heterogenists as scientific heretics. All I say is, that up to the present moment I am not aware of any proof that they are right.

On the Electrical Phenomena which accompany the Contractions of the leaf of Dionæa muscipula, by Dr. Burdon-Sanderson.

It is well known that in those structures in the higher animals which are endowed with the property of contracting when stimulated—viz., nerve and muscle—this property is associated with the existence of voltaic currents which have definite directions in the tissue. These currents have been the subject of very careful observation by physiologists. They require delicate instruments for their investigation, but the phenomena dependent on them admit of the application of the most exact measurements. The constant current which can be shown to exist in a muscle is called the normal current. The most important fact with reference to it is that it exists only so long as the muscle is alive, and that it ceases during the moment that the muscle is thrown into action. Other characteristics of the muscle currents were referred to, which we have not space to mention.

In certain plants said to possess the property of irritability, contraction of certain organs on irritation occur which strikingly suggest a correspondence of function between them and the motor organs of animals. Among the most remarkable are those of Drosera and some other plants belonging to the same natural order, particularly the well-known Venus' Flytrap (*Dionæa muscipula*). The Sensitive Plant, the Common Monkey Flower, the Rock Cistus, afford other examples.

Strange as it may seem the question whether these contractile movements are accompanied with the same electrical changes as those which occur in the contraction of muscle and in the functional excitation of nerve has never yet been investigated by vegetable physiologists. Mr. Darwin, who for many years has devoted much attention to the animal-like functions of Dionæa and Drosera, kindly furnished plants for the purpose of the necessary experiments, which have been made by Dr. Sanderson in the laboratory of University College, London. The result has been that the anticipations he had formed have been confirmed as to the existence of voltaic currents in these parts, and particularly in the leaf of Dionæa. By a most remarkable series of experiments (which will be published subsequently) made with the aid of Sir W. Thompson's galvanometer, he has shown that these currents are subject, in all respects in which they have been as yet investigated, to the same laws as those of muscle and nerve.

On Physiological Researches on the Nature of Cholera, by Dr. Brunton.

Without entering into the question of the nature of cholera poison, the writer regarded it as probable that its effects might be counteracted in the same way as those of other poisons—by appropriate antidotes. He supposed that if a poison could be found having a similar action to that of cholera, an antidote to the former might prove a remedy for the latter. The condition of cholera collapse has been attributed by Parkes and Johnson to contraction of the vessels in the lungs, and their theory is generally adopted. The writer fround that muscarin—an alkaloid derived from a species of poisonous mushroom—caused contraction of the vessels of the lungs and some of the symptoms which are counteracted by atropia. It therefore seems probable that atropia might be useful in cholera, and in fact an American practitioner has recently employed large doses of it with success. The fact that nitrate of amyl, which also relaxes the pulmonary vessels, is useless as a remedy in cholera, as well as the absence of distension of the right side of the heart in cholera patients during life, shows that Parkes and Johnson's theory is imperfect, and that one of the most important conditions in cholera is active dilatation of the large veins in the interior of the body. The condition might be relieved by digitalis. The effect of this poison was at once observed in cholera. The rice water stools in cholera were stated to have exactly the same composition as the fluid secreted after the division of the intestinal nerves in Moreau's experiment, and the profuse secretion from the intestines in cholera was therefore attributed to paralysis of some of the intestinal nerves. Injection of Epsom salts into the intestines also produced a profuse secretion, though this might be due to irritation and not to paralysis of the nerves. This is not lessened in the least by atropia, and it seems therefore probable that atropia will not prove a perfect remedy for cholera. Dr. Brunton is still endeavouring to find a remedy which will arrest this secretion.

On the Movements of the Glands of Drosera, by Alfred W. Bennett, F.L.S.

The peculiar movement of the glands which cover the margin and the upper side of the leaf of the Sundew has often attracted the attention of botanists. The observations were all made on the commonest species, *Drosera rotundifolia*.

It should be noted in the first place that the glands of Drosera are in no sense hairs, *i.e.* cellular expansions of the epidermis of the leaf. They have been shown by Groenland and Trécul to be an integral part of the leaf itself, penetrated by a fibro-vascular bundle with spiral threads (in other words by a vein or nerve of the leaf) from one end to the other, and even furnished with stomata on their surface. They terminate in a pellucid knob within which is found their peculiar viscid secretion. Under a low magnifying power this secretion may be seen collected about the knobs, and stretching in thin glutinous strings from one to another. The secretion has probably an attraction for flies and other small insects, as, if the plant is examined in its native bogs scarcely a leaf will be found in which an insect is not imprisoned, and one leaf will very often show as many as three or four. The experiment was made of placing a very small insect, a species of Thrips, on a leaf at that time quite unencumbered beneath a low power of the microscope. Immediately on coming into contact with the viscid secretion it made vigorous efforts to escape, but these efforts only seemed to entangle it all the more deeply. The contact of the insect appeared to excite a stronger flow of the secretion, which soon enveloped the body of the animal in a dense almost transparent slime, firmly glueing down the wings, and rendering escape hopeless. It still, however, continued its struggles, a motion of the legs being still clearly perceptible after the lapse of three hours. During all this time the insect was sinking lower and lower down among the glands towards the surface of the leaf, but only a slight change had taken place in the position of the glands themselves, which had slightly converged so as to imprison it more completely. But after the struggles of the prisoner had ceased, a remarkable change took place in the leaf. Almost the whole of the glands on its surface and its margin, even those removed from the body of the insect by a distance of at least double its own length, began to bend over, and point the knobs at their extremities towards it, though it was not observed that this was accompanied by any increased flow of the secretion from them. The experiment was made in the evening; and by the next morning almost every gland of the leaf was pointing towards the object in the centre, forming a dense mass over it. The sides of the leaf had also slightly curved forwards so as to render the leaf itself more concave. The nearly allied Venus's Fly-trap, or *Dionæa muscipula* of the United States, which imprisons flies by a much more sudden motion of the sides of the leaf, collapsing when irritated on the upper surface, is said to digest and absolutely consume the insects thus entrapped. What becomes eventually of the prisoners of the sundew, my experiments have not been carried sufficiently far to ascertain. It will be seen that the most singular feature in the phenomena here described is that the motion of the greater number of the glands did not begin till after the insect had become comparatively motionless; and therefore it is very difficult to attribute it to the excitement caused by the struggles on any "contractile tissue" at the base of the glands, an explanation which has been offered for the sudden and rapid motions of the stamens of Berberis or the leaves of Mimosa. It is also quite certain that the impinging of raindrops on the surface of the leaf causes no similar motion, a peculiarity similar to that which Darwin has observed in the case of the motions of tendrils and of climbing stems. In order to determine what share in these motions of the glands was due to the organic nature of the substance imprisoned, and to its power of motion, the following experiments were also made:—A small piece of raw meat was placed on another leaf similar to the first. No immediate change was observable, and no increased flow of the secretion; but after the lapse of a few hours a perceptible inclination of the more distant glands towards the object took place. The next morning the piece of meat was found, like the fly, sunk down on to the surface of the leaf, with almost the whole of the glands converging towards it and above it in just the same manner. The changes here were therefore perfectly of the same kind as in the case of the fly, though apparently somewhat slower. After the lapse of twenty-four hours the piece of meat appeared decidedly lighter in colour; but an accident prevented the process of digestion being further traced. On other leaves were placed a minute piece of wood and a small piece of worsted: and in neither of these cases was the least change perceptible after the lapse of a considerable time in the position of the object, nor in that of any of the glands, either those in contact with it or the more remote ones. It would appear, therefore, that the organised structure of the fly and of the piece of raw meat had some power of exciting this motion which is not possessed by matter of a different description.

SCIENTIFIC SERIALS

Poggendorff's Annalen der Physik und Chemie, No. 6, 1873.—This number commences with a paper by M. Seebeck, on the motion of sound in bent and branching tubes. He finds, among other things, that the gradual bending of a tube has little effect on the size of wave-length, but if a tube be suddenly bent to an angle, the sound-motion is considerably affected; it would seem that the motion of the air-particles did not suddenly alter in direction with the tube.—A series of experiments on the electromotive and thermo-electric forces of some metallic alloys, on contact with copper, is detailed by M. Sundell. The alloys examined were bismuth-tin, bismuth-antimony (in various proportions), and German silver; the method employed in the case of electromotive force being that of Edlund, based on the fact, that a galvanic current, passing through an electromotor, produces in it, proportionally to its electromotive force, an absorption or production of heat, according as the current is in the same direction as that of the electromotor, or contrary to it. The alloys, like the pure metals, have the same order in electromotive as in thermo-electric series; and it appears that the proportion of thermo-electric to electromotive force is constant, and equal to that for the combinations iron-copper, and copper-bismuth. Comparative experiments on various pyrometric methods—air thermometer, expansion of solid bodies, calorimeter, dissociation of a compound, and electrical resistance, lead M. Weinhold to a preference for the last (or Siemens'), as the most reliable. The calorimeter, properly used, also gives good results.—M. Lorenz, of Copenhagen, furnishes a new determination of the electrical resistance of mercury, in absolute measure. He attributes the discordance in previous results to the employment of induced *currents*, of *variable* strength, and he adopts an ingenious method in which a *constant* electromotive force *without* current, is applied. The result of five experiments is 1 mercury unit = 0·9337 Ohm's unit, or the mercury unit equal to $0·9337 . 10^{10}$ absolute units.—Of the remaining papers we may note one by Kohlrausch on the electrochemical equivalent of silver, and mineralogical notes on wolfram, and on a new mineral, ardenite.

SOCIETIES AND ACADEMIES

LONDON

Royal Horticultural Society.—General Meeting, Aug. 20.—W. A. Lindsay, Secretary, in the chair.—The Rev. M. J. Berkeley said Kerson's seedling gooseberry, a fine variety which gained a first-class certificate at the last meeting, turned out to be not a garden seedling but one originally taken from a common hedge in the neighbourhood of Peterborough. This was not a solitary instance of a fine variety of fruit being found in such places—the Bess Pool apple having been discovered in a plantation at Nottingham. Mr. Berkeley then alluded to a disease of the crocus very destructive to the gladiolus, and which also attacked the saffron crocus and the narcissus; it was first described by Montague under the name of *Tacon*. He concluded by remarking that vegetables treated with sewage were apt to be much deteriorated in flavour.

Sept. 3.—General Meeting.—Dr. Kellock in the chair.—Adverting again to the subject of *Tacon* in the Gladiolus, the Rev. M. J. Berkeley was inclined to attribute it to "sunstroke."—A bunch of grapes was exhibited from the parent plant of the Hampton Court vine; it dated from 1761.—A fungus (*Lentinus lepideus*) was sent by Sir Gilbert Scott, from the roof of a church at Croydon.

PARIS

Academy of Sciences, Sept. 15.—M. Bertrand in the chair.—The following papers were read:—An answer to Father Tacchini's last note, by M. Faye. The author replied to the objections raised by the Italian observer to the cyclonic theory on the ground of the appearance of prominences where there are no spots. M. Faye considered that the pores, which are vertical cyclones, are the cause of the circulation of the solar hydrogen, and hence of the prominences. He also replied to some objections relating to the direction of the circular motion in cyclonic spots.—New researches on the analysis and the theory of the pulse in normal and abnormal states, by M. Bouillaud. The author announced the discovery of a secondary beat in the pulse, which he ascribed to a contraction and expansion of the arteries themselves.—On choleraic dejections as agents in the propagation of cholera, by M. Ch. Pellarin.—On the changes of form exhibited by Comet IV., 1873, by MM. Rayet and André.—On the movement of an elastic wire one end of which has a vibratory motion, by M. E. Mercardier.—On the products of the oxidation of meteoric irons and a comparison of them with the terrestrial magnetites, by M. Stan. Meunier.—Process for the preparation of a new aniline red, by M. E. Ferrière. The new colour is prepared by acting on acetate of aniline with ammoniacal cupric hydrate, and then saturating with sulphuric acid. On concentration ammoniac sulphate is deposited, and the colour remains. It is a purple red.

Sept. 22.—M. Bertrand in the chair.—On the chairman taking his seat, he at once proceeded to announce the deaths of M. Coste, of the Section of Anatomy and Zoology, and of M. Nelaton, of the Section of Medicine and Surgery; and to express in a few words the sorrow of the Academy at the grievous loss it had thus sustained. At the conclusion of the chairman's remarks, M. le Baron Larrey at once proposed that, to mark its sense of the double loss, the Academy should not hear any papers at the meeting, and that the correspondence only should appear in the *Comptes Rendus*. The following papers were accordingly printed:—Thermic researches on the condensation of gases by solids—continuation: absorption of hydrogen by platinum-black, by M. P. A. Favre.—Certain observations on the winged form of the *Phylloxera vastatrix* in connection with the propagation of the insect, by M. Max. Cornu.—On the proper time for the application of the submersion treatment to vines tainted by Phylloxera, by M. L. Faucon.—On the proportion of carbonic anhydride in atmospheric air, and on its variation with the altitude, by M. P. Truchot. The author finds that the quantity of this gas diminishes as the altitude increases.—On coralline, by M. Commaille.—Note on a meteorite with a phosphorescent train seen on the night of September 28, 1873, by M. Chapelas.—The second part of M. Mercadier's note on the movement of an elastic wire, one end of which is endued with a vibratory motion.

BOOKS RECEIVED

ENGLISH.—Centrifugal Force and Gravitation: John Harris (Supplement A.).—Half Hours with the Microscope. New Edition (Hardwicke).—Zoological Record, Vol. viii, Edited by Prof. Newton (Van Voorst).—Chapters on Trees: M. and E. Kirby (Cassell).—The Amateur Greenhouse and Conservatory: Shirley Hibberd (Groombridge).—Proceedings of the Literary and Philosophical Society of Liverpool, Vol. xxvi. (Longmans).—A Discourse on the Pursuit of Truth: A. Elley Finch (Longmans).

FOREIGN.—Mikroskopische Physiographie: H. Rosenbusch (Williams & Norgate).

CONTENTS