THURSDAY, JUNE 14, 1877

THE ENDOWMENT OF RESEARCH

WE are authorised to publish the accompanying list of the sums to be paid by the Government, on the recommendation of the Royal Society, during the present year in aid of Scientific Research.

We might well leave the list to speak for itself, but it would be ungrateful not to point out that the Duke of Richmond and Lord Sandon have by their action, beyond all doubt, inaugurated a new era in the scientific activity of our country, and one which is sure to be fostered by corporate bodies and individuals now that the Government has set so noteworthy an example.

PERSONAL PAYMENTS.

Mr. J. A. Broun.—For Correction of the Errors in the published Observations of the Colonial Magnetic Observatories . . . . . . 150*l.*

Dr. Joule.—For Experimental Investigations into the Mechanical Equivalent of Heat . . . . 200*l.*

Prof. Parker.—For Assistance in Researches on the Morphology of the Vertebrate Skeleton and the Relations of the Nervous to the Skeletal Structure, chiefly in the Head . . . . 300*l.*

Rev. W. H. Dallinger.—For Microscopic Investigations of Monads, Bacteria, and other Low Forms of Life 100*l.*

Rev. F. J. Blake.—For compiling and publishing a "Synopsis of the British Fossil Cephalopoda" . 100*l.*

Prof. A. H. Garrod.—For Aid in preparing for Publication an Exhaustive Treatise on the Anatomy of Birds . . . . 100*l.*

Dr. Murie.—For completing and publishing three Memoirs:—"Anatomy of the Kingfisher," 4to., with five plates; on "Extinct Sirenia," 4to., with six plates; "Osteology of the Birds of Paradise," folio, three plates 150*l.*

Mr. H. Woodward.—For Continuation of Work on the Fossil Crustacea, especially with reference to the Trilobita and other Extinct Forms, and their Publication in the Volumes of the Palæontographical Society . . 100*l.*

Prof. Schorlemmer.—For Continuation of Researches into (1) the Normal Paraffins, (2) Suberone, (3) Aurin . 200*l.*

Dr. H. E. Armstrong.—For Continuation of Researches into the Phenol Series, and into the Effect of Nitric Acid on Metals . . . . . . 300*l.*

Profs. King and Rowney.—For Researches to Determine the Structural, Chemical, and Mineralogical Characters of a Certain Group of Crystalline Rocks represented by Ophite . . . . 60*l.*

Mr. W. J. Harrison.—Towards the Expense of collecting and describing Specimens of the Rocks of Charnwood Forest . . . . 50*l.*

NON-PERSONAL PAYMENTS.

In aid of Apparatus, Materials, and Assistance.

Dr. J. Kerr.—For aid in Electro-Optic and Magneto-Optic Researches . . . . . . 200*l.*

Mr. J. E. H. Gordon.—For Experimental Measurements of the Specific Inductive Capacity of Dielectrics 50*l.*

Prof. Guthrie.—For Apparatus and Assistance in (1) the Determination of the Latent Heats of the Cryohydrates and the Vapour Tensions of Colloids; and (2) the Examination of Heat Spectra and Radiant Heat by means of varying Electrical Resistance in Thin Wires . 150*l.*

Mr. J. T. Bottomley.—To aid in carrying out a Series of Experiments for determining the Conductivity for Heat of Various Liquids and Solutions of Salts . . 100*l.*

Sir William Thomson.—For Assistance and Materials for a Continuation of Experiments on the Effects of Stress in Magnetism . . . . . . 100*l.*

Mr. W. Crookes.—For Assistance in continuing his Researches connected with "Repulsion resulting from Radiation" . . . . 300*l.*

Messrs. Rücker and Thorpe.—For a Comparison of the Air and Mercurial Thermometers . . . 50*l.*

Mr. F. D. Brown.—For an Investigation of the Physical Properties, the Specific Gravity, Expansion by Heat, and Vapour Tension, of the Homologous and Isomeric Liquids of the $CnH2n + 1$ Series . . . . 100*l.*

Prof. Roscoe.—For Continuation and Extension of the Experiments on the Self-registering Method of measuring the Chemical Action of Light . . . . 100*l.*

Sir William Thomson.—For Investigation and Analysis of Tidal Observations and Periodic Changes of Sea Level . . . . 200*l.*

Dr. J. B. Balfour.—For the Expense of Illustrations for a "Monograph of the Pandanaceæ" . . . 50*l.*

Mr. H. T. Stainton.—For Aid in publishing the "Zoological Record" . . . . . . 100*l.*

Dr. J. G. M'Kendrick.—For Apparatus for a Research into the Respiration of Fishes . . . . 75*l.*

Prof. Gamgee.—For a more Complete Survey than has yet been made of the Physiological Action of the Chemical Elements and their more Simple Compounds, with the Object, in the first instance, of establishing a Physiological Classification of the Elementary Bodies . 50*l.*

Dr. Brunton.—For Researches into the Physiological Action of the most important Compounds of Nitrogen, and into the Action of certain Poisons, and for Apparatus . . . . 80*l.*

Mr. E. A. Schäfer.—To pay the Wages of an Assistant to give Mechanical Aid in Histological and Embryological Research . . . . . . 50*l.*

Dr. Burdon Sanderson.—For an Investigation of the Normal Relation between the Activity of the Heat-producing Processes, and the Temperature of the Body 70*l.*

Prof. Schorlemmer.—For continuation of Researches into (1) the Normal Paraffins, (2) Suberone, (3) Aurin . 100*l.*

Mr. W. N. Hartley.—For Researches into the Photographic Spectra of Organic Substances, into the Phosphates of Cerium, the Conditions under which Liquid Carbonic Acid is found in Rocks and Minerals, the Double Salts of Cobalt and Nickel, and for other Investigations, and for Assistance . . . . . 100*l.*

Dr. Burghardt.—For a Research into the Origin of the Ores of Copper and (if possible) of Lead, their Mode of Formation, and the Chemical connection (if any) between the Ore and its Matrix . . . . . 50*l.*

Prof. Church.—For a Research into the colouring matters of Colein, of Red Beet, and for the Study of Plant Chemistry . . . . . . 50*l.*

THE "CHALLENGER" COLLECTIONS

THE preliminary steps have been taken for the completion of the great work of the *Challenger*, and the vast collections made during the voyage are now being distributed among experienced workers for determination and description.

The director of the scientific staff has been at great pains in endeavouring to secure the services of men most competent for the task, and we are sorry to see that some of our English naturalists, and notably the president of the Geological Society, have thought it necessary to remonstrate against the course which the director has taken in the selection of the men to whom he is about to entrust the examination of the collections. We have already had occasion to refer to what we felt obliged to characterise as an unwarranted attack on Sir Wyville Thomson, and it is with much regret that we observe an attitude of hostility to the mode of distribution which has been deemed most conducive to the reputation of the expedition and to the interests of science.

It would seem that while almost all the great zoological groups which the *Challenger's* dredges have brought to light have been handed over for examination to naturalists in this country, a few have been placed in the hands of American and German workers; and it is this association of foreign zoologists with the men to whom in this country by far the largest portion of the work has been assigned that has excited the indignation of the individuals referred to.

Now every one who has kept himself *en rapport* with recent zoological research, must know that the foreign zoologists, to whom Sir C. Wyville Thomson has intrusted these collections, stand before all others in the amount and thoroughness of their work in the special departments of zoology for which their aid is asked, and the narrowest nationalism cannot deny that it was the duty of the director to see that the specimens were placed in the hands of men most competent to secure for science the results which have been obtained at the cost of so much labour, skill, and public expenditure.

If this country can be shown to enjoy the unique distinction of possessing in every department of zoological research men at least as good as can be met with elsewhere, the advocates of a national science may find an argument in favour of having the work absolutely confined to Englishmen; but if we cannot assume a position which no other nation in the world would think of claiming, it is plainly for the interests of science that we should supplement from abroad those departments of research in which foreign workers may excel us.

That the naturalists to whom we have referred will not receive much support from their fellow-workers will be evident from the subjoined letter to the Editors of the *Annals* now in process of signature, which has already received the adhesion of the presidents and secretaries of the Royal, Linnean, and Zoological Societies, and of other leading men in this department of knowledge:—

"*Zoology of the 'Challenger' Expedition.*

"As in a letter upon this subject in the number of the *Annals of Natural History* for May last Dr. P. Martin Duncan, writing as president of the Geological Society, has stated that he speaks 'at the instance of a very considerable number of members of learned societies,' we, the undersigned, wish to state that we do not agree in the strictures passed by Dr. Duncan upon the manner in which Sir C. Wyville Thomson has distributed the specimens collected by the *Challenger* Expedition for description. So far as we have had an opportunity of judging we are perfectly satisfied that Sir C. Wyville Thomson, in the arrangements which he has made as regards these collections, has acted consistently with the best interest of science.

"It was, in our opinion, Sir C. Wyville Thomson's duty to secure the aid of the most competent naturalists without regard to their nationality; and, even if it were proper that national jealousies should be imported into science, Sir C. Wyville Thomson can hardly be reproached on this score, when it is considered that two-thirds at least of the naturalists whose aid he has obtained are Englishmen.

"J. D. HOOKER.	W. H. FLOWER.
T. H. HUXLEY.	P. L. SCLATER.
CHARLES DARWIN.	OSBERT SALVIN.
ST. GEORGE MIVART.	A. H. GARROD.
FRANCIS DAY.	GEO. A. ALLMAN.
GEO. BUSK.	TWEEDDALE."
WILLIAM B. CARPENTER.	

It is of importance that no misunderstanding should exist as to the real state of the controversy which has arisen on a subject in which zoological science is so deeply interested, and we believe we cannot do better than lay before our readers the correspondence which had taken place between Sir Wyville Thomson and Dr. P. Martin Duncan before a word of hostile criticism had as yet shown itself in print.

"Scientific Club, Savile Row, London, W.
"24th March, 1877

"MY DEAR SIR WYVILLE THOMSON,

"You can hardly imagine the strong feeling of disappointment which has arisen amongst a very large section of the naturalists and palæontologists who study the invertebrates, in consequence of a letter which was published in the *Ann. and Mag. of Nat. Hist.* for March, 1877. In this letter the scientific world is informed by our mutual friend, A. Agassiz, that the Echini, Ophiurans, Radiolaria, and a part of the Spongida collected in the expedition of the *Challenger* have been given to American and German naturalists for description, and that the United States have a 'fair share' of the work. So great is the feeling that English workers should have been thus passed over, that a conference has been held on the subject, and I have been asked to write to you in the friendliest spirit of remonstrance. I need hardly state that I should not have taken this liberty did I not happen to hold a position which entails action in everything relating to the progress of geological science. Writing then on the part of many men whose capabilities as naturalists and palæontologists I am well aware of, I express their and my own opinion that in this distribution your amiability and want of personal acquaintance with English workers have led you astray. We recognise the great merits of those foreign gentlemen to whom you have sent collections and the exceeding liberality of A. Agassiz; but we do not think that you are justified in giving them the results of the greatest natural history expedition which has ever sailed from this country, unless there is a want of that power amongst English workers which will enable them to treat the subjects in the broadest sense, and to compare the recent and geological faunas satisfactorily. There is no such deficiency. I am asked to urge upon you a reconsideration of the matter, and to leave a fair portion of work in the hands of our friends, giving the rest to men of your own country. Assuring you that we appreciate your difficulties, and that we will assist you in every way consonant with the dignity of English science, I remain,

"Yours sincerely,
(Signed) "P. MARTIN DUNCAN

"Sir C. WYVILLE THOMSON"

"MY DEAR DR. MARTIN DUNCAN,

"I must ask you to consider this note as written to yourself personally, for I cannot, of course, in any way recognise this nameless 'Conference.' I may mention, however, at starting, that in this matter I have consulted several of the first English naturalists, and that they entirely approve of my selection.

"I take up my pen rather hopelessly, for your letter does not touch any of the considerations on which I have acted. My duty was to have prepared an official account of the voyage to the best of my power within a certain time. I endeavoured to select to assist me in this (1) those who had most successfully made certain branches their special study and were generally regarded as *authorities*; and (2) those whom I knew by experience to be likely to do the work within the time to which I was tied down, and to return the specimens in good order to be lodged in the British Museum. In all cases where I considered that these conditions were fairly fulfilled by Englishmen I at once and fully recognised the great advantage of avoiding the risk of sending things abroad, but except for this consideration I confess I saw and see no objection, but rather the reverse, to making a great work of this kind somewhat more catholic. The result has, however, been, that by far the greater part of the work will be done in England. I do not mean to go into special cases, but I give a general sketch of the arrangements as they now stand:—

Sea Mammals	Prof. Turner.
Birds	Dr. Sclater.
Fishes	Dr. Günther.
Cephalopoda	Prof. Huxley.
Gastropoda	Rev. R. B. Watson.
Lamellibranchiata	Rev. R. B. Watson.

Brachiopoda ...	... ...	Mr. Davidson.
Higher Crustacea	... ...	Probably Prof. Claus.
Ostracoda	... ...	Prof. G. Brady.
Copepoda	... ...	
Isopoda	... ...	Mr. Henry Woodward.
Cirripedia	... ...	Mr. Darwin.
Annelida	... ...	Dr. McIntosh.
Gephyrea	... ...	Prof. Ray Lankester.
Bryozoa	... ...	Mr. Busk.
Echinoidea	... ...	Mr. A. Agassiz.
Ophiuridea	... ...	Mr. Lyman.
Crinoidea	... ...	Dr. Carpenter and myself.
Hydromedusæ	... ...	Prof. Allman.
Corals	... ...	Mr. Moseley.
Sponges	... ...	Prof. Oscar Schmidt and myself
Rhizopods	... ...	Mr. Henry Brady.
Radiolarians	... ...	Prof. E. Haeckel.

"Now the only foreigners in this list are Dr. Günther, Prof. Claus, Prof. Agassiz, Mr. Lyman, Prof. Oscar Schmidt, and Prof. Haeckel. If there is a better English authority than Dr. Günther on fishes, I beg his pardon for having overlooked him. The crustaceans were to have been done by the late Dr. v. Willemöes-Suhm and certain considerations come in as to the use of his plates and notes, which I need not discuss. I am not aware that there is any one in this country who can be considered at present an authority on recent Echinoidea. The choice perhaps lay between Agassiz and Lovén, but the reference collection at Cambridge is the best in the world in this department. There is, so far as I know, no English authority on Ophiurids at present. I prefer Oscar Schmidt's mode of treating the sponges to that of any other author. I am not aware that any Englishman knows the Radiolarians so well as Haeckel. There are a good many departments not yet settled, and one or two other foreigners may be added to the list. I should of course have most heartily asked your assistance with the corals had Moseley not undertaken them, but he has the preference as one of our staff, and he has done excellent work.

"I have submitted the principles on which I am working to the best of my ability to the Treasury, and they have received their sanction and that of the Council of the Royal Society. I cannot recognise the importance of the geographical distribution of naturalists, and with all respect for the dignity of British science I must say I think that in this selection, which I considered entirely open, I have done it ample justice.

"Believe me, very truly yours,
"C. WYVILLE THOMSON

"20, Palmerston Place, Edinburgh, March 27"

To this letter no reply has been received, and the subject might well have ended here.

The objectors to the course pursued by Sir Wyville Thomson would hardly advocate our assumption of a spirit more narrow and illiberal than that of any other country, and they will perhaps be interested in knowing how a foreign Government has acted under quite similar circumstances.

The results of the two great recent scientific expeditions fitted out in the United States, that of the "Haslar," and the Exploration of the Gulf Stream, have been distributed among special workers without any regard to nationality. Of this we need no further evidence than that afforded by the arrangements which have been adopted for the examination of the very rich collections made during the Gulf Stream Expedition. These collections have been allocated as follows:—

Halcyonaria ...	A. Kölliker	Würzburg.
Annelides	E. Ehlers	Göttingen.
Sponges (part) ...	O. Schmidt	Strassburg.
Sponges (part) ...	E. Haeckel	Jena.
Holothurians ...	E. Selenka	Leiden.
Polyzoa	F. A. Smitt	Stockholm.
Mollusca	J. Gwyn Jeffreys	London.
Hydroids	G. J. Allman	London.
Starfishes	E. Perrier	Paris.
Crustacea	Alph. Milne Edwards...	Paris.
Fishes	F. Steindachner	Vienna.
Cephalopods ...	J. P. Steenstrup	Copenhagen.
Brachiopods ...	W. H. Dall	Washington.
Corals	L. F. Pourtales	Cambridge, U.S.
Ophiurans	T. Lyman	Cambridge, U.S.
Echini	A. Agassiz	Cambridge, U.S.

It will be thus seen that out of the twenty-two zoologists among whom the collections of the *Challenger* have been distributed *seventeen are English*; while out of the sixteen to whom the American collections have been assigned, *four are American*.

ELEMENTARY PHYSICS

Matter and Motion. By J. Clerk-Maxwell. (Society for Promoting Christian Knowledge. London, 1876.)

THE recent appearance of a swarm of elementary books on physics, some of which at least are written by well-known authors, leads to some very curious inquiries and speculations: for, though treating in the main of the same parts of the same subject as does the work we are specially dealing with, and addressed professedly to the same class of readers, they have comparatively little in common with it. To a certain, even a considerable, extent, this difference is of course due to the idiosyncrasies of the authors; but, after all allowance is made for these, there is still a most notable divergence. It will be both interesting and profitable carefully to consider in what this divergence consists, and what is its probable origin. For it is not too much to say that an intelligent reader of Clerk-Maxwell's book, had he no other source of information, would be utterly unable to answer any one of hundreds of questions which might be framed (without "dodge" or "trap") by a qualified examiner, *directly* from the text of the others. It is true that such questions would be artificial rather than natural—bearing more upon old and cumbrous dogmatic fallacies than upon the actual facts of science. But if the reader of Clerk-Maxwell's book would be at a loss when examined from any of the others, the student who relies merely upon one (or even *all*) of these would hardly even understand the meaning of a question put directly from Clerk-Maxwell's. The main origin of this divergence is to be found in the steady progress of knowledge in all departments of true science; even the most elementary. And, bearing this in mind, we may give an almost complete statement of the case by saying that Clerk-Maxwell's book properly belongs to the second half of the present century, while his rivals give us that of the first half only. These give us again the elementary "*Mechanics*" of our student days (more than a quarter of a century ago) very little changed—though where changed, often changed for the better—the first gives us what is emphatically the science of *to-day*. Possibly enough, in the beginning of the twentieth century even Clerk-Maxwell's book may appear a little antiquated; but it is hardly to be imagined that the text-book of that not very distant future will differ from Clerk-Maxwell's to anything like the extent to which that differs from its competitors. At least if there be anything like so a great difference it will depend upon some wholly new information as to the intimate nature of matter or energy, certainly not upon a mere difference in the mode of treatment.

The immense steps taken by Galileo and Newton (to mention only two of the chief workers) in the simplification and orderly arrangement of the fundamental conceptions and laws of physical science are too often lost sight of in comparison with the vast extensions which these men gave to our knowledge—though their actual *value* is probably little inferior. No doubt it is scarcely possible for any one (except by accident, of which some notable instances will occur to every reader) to make real extensions of our knowledge unless he possesses a clear conception of elementary principles. But great discoverers are generally too much engrossed with their higher studies to bestow much time on the explanation or co-ordination of the more elementary parts of their science. All the more honour, then, to those who, like Galileo and Newton, have made every step of their progress clearly intelligible to the student from its very foundations.

Immediately after Newton's time the progress of physical science was almost arrested in Britain (mainly, it seems, from the want of men of a high order of genius), and the really great foreign Mathematicians and Physicists of the time were entirely absorbed in the rapid development of their subjects. The disastrous consequence was that the elementary parts of science were left almost entirely to the second-rate men, or to the mere sciolists, men whose representations of science, even at the best, were mere caricatures—in the sense in which an orrery mimics the solar system, or an automaton a living animal;—and though, since that time, really first-rate men (*e.g.*, Cavendish and Young) have occasionally appeared in Britain, the pernicious influence of generations of smatterers was not easily shaken off. Thus an absurdly artificial, and unnecessarily complex system, based to a certain extent on Newton, but altogether devoid of his wonderful precision, simplicity, and completeness, came to be generally adopted here. This artificial system may be said to have reached its climax in the works of the late Dr. Whewell, perhaps the only brilliant writer of what is known now as *Paper Science*, by far the most pestilent weed against which the true scientific cultivator has to contend. The omniscient Master of Trinity might quite probably have been able to hold his own with Aristotle, had he lived in days when science had but a scanty development ; but it is impossible for any one nowadays to hold relatively to human knowledge any such position. And he who tries to do so, even had he the genius of Newton to start with, will simply *do nothing*.

Clerk-Maxwell (wisely, we think) appears to prefer Newton and Rowan Hamilton to Whewell, on whom or the like of whom his rivals mainly rely. And this alone accounts for a great deal of the extraordinary dissimilarity between the works to which we have alluded. What is Whewell, the universal genius, with all his book-learning, in comparison with Newton, the special genius, with his close and patient study of material phenomena themselves ? Nothing. Men consulted Whewell as they would a dictionary or an encyclopædia, simply to save themselves trouble. But when was an encyclopædia ever seen to add a volume of new matter to itself as an appendix ? To use an old comparison, Newton, as it were, studied Chinese metaphysics in China itself, at head-quarters ; Whewell and those who do like him read all the European works on China and all the European works on metaphysics, and "combine their information." Thus almost all Clerk-Maxwell's rivals —whom therefore we need not specially name—give us the sacred *Three Classes of Levers*, the various *Systems of Pulleys*, the altogether imaginary *gold* shell of the Florentine Academicians, the *Principle of Repulsion*, the *Transmissibility of Fluid Pressure*, and what not. Weight and specific gravity are usually put forward in preference to mass and density—the accidental property before the inherent or essential one ! We have the old confusing statements about a co-efficient of *elasticity* in the impact of balls. In one of the most pretentious of these works we are told that the "strict" definition of a *level surface* implies that "at all points of it the force of gravity has the same value, and its direction is at right angles to the surface." That is to say, the author here uses the word "force" in two different senses. It means the "potential energy of a given mass," when its *value* is spoken of ; but it means the "weight of a given mass" when its *direction* is spoken of. For it is inconceivable that the author could have meant to state that the weight of a body is the same at all points of a level surface. We could give without practically any limit instances of a similar kind (not mere slips of the pen, from which no man's work can be free), but we will be merciful, and simply extract the following passages, putting a word or two in italics, and leaving the reader to exercise himself in finding what is erroneous :—

"An arrow shot upwards from a bow reaches to a certain height : show that if the weight of the arrow be doubled, *other circumstances remaining the same*, the height reached will be *one-fourth* of its former value."

"Gravity and distance together represent the *force* employed from the beginning in putting things where they are, and whenever they come together by attraction they develop a corresponding *force or heat*."

"When heat is continually applied to water it is found that if the water is in an open vessel its *heat cannot be raised* beyond a certain point."

"The *forces* of heat, electricity, magnetism and light are now considered to be all *species of motion*, discoverable and measurable only by the amount of *movement* they can produce or counteract."

". . . water is boiled by placing a lamp beneath the flask so that the upper part of the flask becomes full of steam, the air being expelled. The flask is now stopped with a cork, removed from the lamp, and allowed to cool. . . . By pouring cold water . . the water begins to boil again. *The experiment requires great care to prevent accidents*."

"Matter in motion is FORCE."

"Electrical attraction is *the name given to some cases* resembling magnetic attraction, in which electricity is the agent."

"Liquids possess a *small but very perfect* elasticity, which differs in amount in different liquids."

". . . gold, which *in the case of a sovereign* falls as fast as anything which we have commonly in view, may be beaten out to a thin leaf which *almost floats on* the air . . ."

"*It is sometimes stated incautiously*, that the weight of a body may always be supposed to be collected at the centre of gravity, but the present case shows that *such a statement is too wide*."

There is no justifying the existence of a new way of doing anything except by showing that it is better than the old one ; but, if that can be done, the new way is justified. And, as it is not our interest to become Encyclopædias, when we get a good new way, let us adopt it, and at once drop the old. And we are not without hope that Clerk-Maxwell's book may effect the complete abolition of the older methods, which are already sadly shattered. Perpetual "distinctions without a difference," like the three classes of levers above alluded to, can only confuse and irritate the student—often making him doubt whether he really understands the gist of an explanation or no. And to give rules for calculating results without explaining how to obtain these rules, or what they imply physically, is, in the vernacular, simply CRAM :—call it euphemistically what you will. To learn how to do this is not, in any sense, to acquire knowledge.

Clerk-Maxwell's book is not very easy reading. No genuine scientific book can be. But the peculiar characteristic of it is that (while any one with ordinary abilities can read, understand, and profit by it) it is the more suggestive the more one already knows. We may boldly say that there is no one now living who would not feel his conceptions of physical science at once enlarged, and rendered more definite by the perusal of it. Short and (on the whole) simple as it is, it is one of the most suggestive works we have ever met with. The following extract needs no comment of ours: let us see how the metaphysicians will digest it :—

"But, as there is nothing to distinguish one portion of time from another except the different events which occur in them, so there is nothing to distinguish one part of space from another except its relation to the place of material bodies. We cannot describe the time of an event except by reference to some other event, or the place of a body except by reference to some other body. All our knowledge, both of time and place, is essentially relative. When a man has acquired the habit of putting words together, without troubling himself to form the thoughts which ought to correspond to them, it is easy for him to frame an antithesis between this relative knowledge and a so-called absolute knowledge, and to point out our ignorance of the absolute position of a point as an instance of the limitation of our faculties. Any one, however, who will try to imagine the state of a mind conscious of knowing the absolute position of a point will ever after be content with our relative knowledge."

We can afford space for only one other quotation ; but, as it is very important, we quote *in extenso* :—

ARTICLE CXIV.—CENTRIFUGAL FORCE.

"This is the force which must act on the body M in order to keep it in the circle of radius v, in which it is moving with velocity V.

"The direction of this force is towards the centre of the circle.

"If this force is applied by means of a string fastened to the body, the string will be in a state of tension. To a person holding the other end of the string this tension will appear to be directed towards the body M, as if the body M had a tendency to move away from the centre of the circle which it is describing.

"Hence this latter force is often called Centrifugal Force.

"The force which really acts on the body, being directed towards the centre of the circle, is called Centripetal Force, and in some popular treatises the centripetal and centrifugal forces are described as opposing and balancing each other. But they are merely the different aspects of the same stress."

This is one of the few passages in the work to parts of which exception may fairly be taken. Of course, the *physical* statements are correct, and they are very clearly put. But it is hardly fair to a junior reader to begin by telling him that Centrifugal Force means *the force which must act* on a body in order to keep it moving in a circle, and then to say that *the force which really acts* is called Centripetal Force ! That force is required *to produce change of direction* of a body's motion, and that when this is applied by means of a string held in the hand the impression on our "muscular sense" is the same as if the body were *pulling* at the other end of the string, are facts : but they no more justify the use (however guarded) of the word "centrifugal" than the tension of the couplings, just before a train starts, proves that the carriages have a tendency to run *backwards*.

There is one very great blemish in Clerk-Maxwell's book, from which those of his rivals are comparatively free. Some of the woodcuts are simply atrocious. This must be looked to in future editions, for passages of great importance are at present rendered totally unintelligible to the beginner : and from this cause alone.

Clerk-Maxwell's work, then, is simply Nature itself, so far as we understand it. The peaks, precipices, and crevasses are all there in their native majesty and beauty. Whoso wishes to view them more closely is free to roam where he pleases. When he comes to what he may fear will prove a dangerous or impassable place, he will find the requisite steps cut, or the needful rope attached, sufficiently but not obtrusively, by the skilful hand of one who has made his own roads in all directions, and has thus established a claim to show others how to follow.

In the rival elementary works the precipice and the crevasse are not to be seen : there are, however, many pools and ditches ; for the most part shallow, but *very* dirty. You are confined to the more easily accessible portions of the region. In the better class of such books these are trimly levelled—the shrubs and trees are clipped into forms of geometrical (*i.e*, unnatural) symmetry like a Dutch hedge. Smooth straight walks are laid down leading to old well-known "points of view,"—and, as in Trinity of ormer days, undergraduates are warned against walking on the grass-plats.

These "royal roads" to knowledge have ever been the main cause of the stagnation of science in a country. He would be a bold man indeed who would venture to assert that the country which, in times all but within the memory of many of us, produced such mighty master-minds as Lagrange, Fourier, Ampère, and Laplace, does not now contain many who might well have rivalled the achievements even of men like these. But they have no chance of doing so ; they are taught, not by their own struggles against natural obstacles, with occasional slight assistance at a point of unexpected difficulty, but by being started off in groups, "eyes front" and in heavy marching order, at hours and at a pace determined for all alike by an Official of the Central Government, along those straight and level (though perhaps sometimes rough) roads which have been laid down for them ! Can we wonder that, whatever their natural fitness, they don't now become mountaineers ?

I still vividly remember the horror with which I watched the the struggles of a former class-fellow of my own, whose friends had just sent him to another school that he might learn geometry a little earlier than was the custom with us. For him there was no longer any play—

all his spare time was devoted to the committing of Euclid to memory! I shuddered as I thought of what was to be my own fate in a few short months, when I too must be subjected to this fearful imposition. But the first hour or two which Dr. Gloag (a name strange, perhaps, to southern ears, but very high indeed on the roll of successful teachers—Clerk-Maxwell, indeed, was one of his pupils) devoted to geometry showed those of us who had any taste for the subject that it was one to be learned by head, not "by heart" (the idiotic phrase in common use)—and that my friend's parents had simply taken him from a good teacher and sent him to an exceedingly bad one—for it came to be discovered after some time that he had really considerable aptitude for geometry.

But if he had been in fact quite unfit for the study, otherwise than in learning to repeat Euclid by rote, what object beyond mere torture would have been attained by forcing it upon him? This leads to another remark of great importance in connection with the mass of elementary text-books.

What sort of students are those who require to be told to *take the square of the velocity, divide it by the radius, and find the proportion of this quotient to* 32 . . . :—without farther explanation or proof? What the better are they of the information? Call you this "teaching science?" Has it improved their minds? Will they be able to make any use of it in after life? I do not see how these questions and many other connected ones can be answered except by a prompt negative. One of two things. The pupil who requires to be taught in this way is either as yet too young, or is one who will never become old enough, to learn even the rudiments of science.

To our metaphor once more. Grass-plats, moss, and flower-beds for the happy sports of children—the bare rock and rough moor for the stern work of men. Your gravel-walks and Macadamised roads are excellent things in their way, but keep them to their legitimate users, the carriage and the perambulator for the invalid and the infant who can neither work nor even play.

My reasons for writing on this subject are very serious ones. I have to consider each year how best to instruct some couple of hundred students in the elements of physics, and have to be constantly on the out-look for a really good text-book of an elementary character. In the higher branches of the subject there is, happily, little difficulty, but that a really good, short, and simple treatise on the merest elements has been (at least till very lately) wholly unprovided is, I think, clear from the ridiculous discussions about *Centrifugal Force*, and other connected ideas, which are even now constantly to be found in our more practical periodicals.

P. G. TAIT

LETTERS TO THE EDITOR

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

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Nectar-Secreting Glands

MR. FRANCIS DARWIN has made an interesting addition to his important discovery of nectar-bearing glands on the young fronds of *Pteris aquilina*, supplied from the ever-welcome experience of Mr. Fritz Müller. The latter gentleman finds that in Brazil the *Pteris aquilina* is protected from the leaf-cutting ants by those attracted to the nectar, and Mr. Darwin adds some speculations on the origin of the glands and their continued functional activity in Europe where they now appear to be useless. On this part of the question I should like to make the following remarks:—

Prof. Heer has shown that in the Miocene plant-beds at Œningen and Radoboj, ants are the most numerous amongst the fossil insects, and in 1849 as many as sixty-six species had been described from these two localities. In 1865 the number found at Œningen alone is recorded as forty-four. I do not know what the total number of species is that have been recorded from the two places up to the present time, but it probably does not fall short of eighty. Amongst the fossil ants from Radoboj there are species of the Tropical American genera *Atta* and *Ponera*. One of the fossil species of *Atta* resembles in general form and in the venation of the wings the curious *Atta cephalotes* of Tropical America.

As there are only about forty species of ants existing now in the whole of Europe it is evident that in the Miocene epoch they must have played a much more important part in Europe than they do now. Plants may then have been exposed to the attacks of enemies that have become extinct along with the general impoverishment of the fauna and flora of Europe that took place in Post-pliocene times; and the protection afforded by ants attracted to the nectar-bearing glands at the critical stage of the unfolding of the young and tender leaves may have been as important to some plants in Europe, then, as it is to many in Tropical America now.

With regard to the persistency of the nectar-producing glands up to the present time in Europe, it is to be remarked that many plants are identical with those living in the Miocene period and the world-wide distribution of *Pteris aquilina* seems to indicate that it is of very ancient origin. If a plant has not otherwise varied there is no reason apparent why it should do so in this respect so long as the secretion of nectar is not positively injurious to it. I have recently noticed in my garden that the ants that attend the glands at the bases of the leaves of the cherry, the plum, the peach, and the apricot, stroke with their antennæ some of the glands that are not excreting when they arrive at them, just as they do the bodies of the aphides. I have not actually noticed that this promotes a flow of nectar, but ever since I became a disciple of Darwin I have been convinced that the most trivial circumstance is worthy of notice; and it may be that the slight irritation of the glands kept up by the ants is sufficient to ensure the perpetuation of a function of the plant now useless to itself. It is, however, perhaps too soon to assume that the glands are entirely useless to the plants in Europe. Darwin states that there is good evidence that the absence of glands in the leaves of peaches, nectarines, and apricots leads to mildew ("Animals and Plants under Domestication," vol. ii. p. 231).

Darwin refers at the same place to the variation of the glands of the leaves in the above-mentioned fruit trees and I may add that they are extremely variable on the cherry, being sometimes absent, sometimes on the stalk and sometimes on the blade of the leaf. The young leaf in its earliest stage, before it expands, has a complete fringe of them, thus bearing out Mr. Francis Darwin's theory that they are homologous with the serration-glands of Reinke.

May I suggest to some of your correspondents that information as to how far north in Great Britain or in Europe the glands on the above fruit trees are attended by ants and especially if the wild cherry (which I have not had an opportunity of observing) is so attended, would be of great interest.

THOMAS BELT

Cornwall House, Ealing, June 8

On Time

"The fact is, that we have not yet quite cast off the tendency to so-called metaphysics."—Tait, "Rec. Adv. in Phys. Sc.," p. 11.

IN Thomson and Tait's "Natural Philosophy," of which I have only the German edition in my possession, I find, § 246: "Die Zeiten, während welcher irgend ein besonderer Körper, der durch keine Kraft angetrieben wird, die Geschwindigkeit seiner Bewegung zu ändern, gleiche Wege durchläuft, sind einander gleich." And § 247: "Dieser Satz drückt bloss die für die Messung der Zeit allgemein getroffene Uebereinkunft aus."

These quotations quite express what is generally understood.

Yet in the definition of the equality of two lapses of time there is a logical fault. It is not allowed implicitly to introduce in a definition what is to be defined. There is no body of which we know *à priori* that no force tries to alter its velocity; in order to ascertain this, we must find out in consistency with the usual definition of force, given in § 217, whether it moves through equal spaces in equal times.

The definition of § 246, therefore, really says: The times, in which a body that goes through equal spaces in equal times moves through equal spaces are equal. It is evident that we are reasoning in a circle.

I am very well aware of the objection which will be made. We have it in § 245: "Auch werden wir später sehen, dass ein vollkommen glatter sphärischer Körper, welcher aus concentrischen Schalen besteht, deren jede von gleichförmiger Material und überall von derselben Dichtigkeit ist, sich, wenn man ihn in eine Drehung um eine Axe versetzt hat, trotz hinzutretender einwirkender Kräfte mit gleichförmiger Winkelgeschwindigkeit dreht, und seine Rotationsaxe in einer absolut festen Richtung erhällt." Thereupon it is said in § 247 that the earth is a body which fulfils these conditions very nearly, and that therefore its rotation gives us the means to measure time. But this assertion is not at all proved.

I now request my readers to be so good as to follow the exposition of *my* view. I assume that we are able to decide whether two lapses of time are equal. How this is done I shall dwell on afterwards.

When the conception of time is combined with the conception of motion we arrive at the building up of kinematics, in which the ideas of velocity and of acceleration are introduced. In abstract dynamics the idea of force is first introduced, wholly separated from any definite physical sense. As soon as the state of motion of a body (which is determined by the magnitude and the direction of its velocity) undergoes a change, we think of a cause of this change, and call this cause a force. We ascribe to a force magnitude and direction. If a body, which primarily is in rest, acquire a rectilinear motion, the force has constant direction. The magnitude of a force of constant direction is judged by the increase of velocity, which it gives in a definite time to a body primarily in rest. If the increases of velocity in equal times be equal, the force has constant magnitude. Two forces of constant direction and magnitude are in the same proportion as the increases of velocity which they give in equal times to the same body. Unity of force is the force which in unity of time gives to a particular body unity of increase of velocity.

It is conceivable that equal forces acting on different bodies cause different accelerations. Therefore another idea is introduced—the idea of mass. It is settled by definition that the masses of two bodies are in inverse proportion to the accelerations which they receive from equal forces. To a particular body unity of mass is ascribed. Unity of force is the force giving to unity of mass unity of acceleration.

I need not dwell on other ideas which are introduced, *e.g.*, moment, work, energy, &c. The whole building can be constructed, and there is room for every investigation which belongs to so-called theoretical mechanics. So it is demonstrated that a centrobaric body, which has kinetical symmetry in respect to its centre of gravity, and which has been brought in rotation about an axis going through the centre of gravity, retains constant angular velocity, when no forces are acting on the surface, and on the component parts only central forces which are in the same proportion as the masses of the parts.

Before kinematics and abstract dynamics can be applied in interpreting phenomena, we must be enabled to measure time.

What is time? There are mental conceptions which cannot be described by words, and I reckon "time" amongst them. But I shall try to answer the question how the conception of time originates with us.

The formation of the conception of "time" is preceded by the formation of the idea of "lapse of time." The idea of "lapse of time" we arrive at by the simultaneous observation of two phenomena, in conjunction with the observation of two phenomena not occurring simultaneously, in such a manner that we receive the impression of the second phenomenon when the impression of the first one is not yet effaced from our memory.

A lapse of time, from the nature of the idea, is limited. If we abstract the *definite* limits, we have the conception of time.

It is clear that in speaking of the measuring of time we properly mean the measuring of lapses of time.

In order to measure lapses of time we must know when a lapse of time is twice as long as another. We easily come to this on its having been established which lapses of time are equal.

If we wish to compare the length of two bodies we place the one beside the other, or if circumstances prevent us from doing so, we successively place a third object beside each of them.

For the comparison of two lapses of time we lack such means and have to follow another way.

In nature phenomena present themselves that persistently return. *Now we simply settle by definition that the lapses of time between the first occurrence of a particular phenomenon and the second is equal to that between the second and third occurrence.* Which phenomenon is to be chosen? Flux and reflux? Earthquakes? For the application of kinematics and abstract dynamics in interpreting phenomena, the choice is no indifferent matter.

I confine myself to the phenomenon which is still the usual base of the measurement of time. It is settled by definition that the lapses of time between the successive culminations of a definite fixed star in a definite place are equal. To divide these lapses of time themselves into equal parts, it is settled that the apparent motion of the fixed star, and therefore of all fixed stars, is uniform.

The results arrived at in the attempts at interpreting phenomena show that a very good hit has been made. But it is not impossible that after greater development of science we may have to make the measurement of time independent of the rotation of the earth. The application of abstract dynamics to the theory of the motion of the earth round the sun and of the moon round the earth has furnished admirable results. But in comparing the results of calculation with the accounts of eclipses found in ancient chronicles, a difference is met with, and in the opinion of some it is too considerable to be accounted for by the imperfection which may adhere to ancient descriptions. Therefore the theory of the motion of the earth and of the moon is incomplete. But hitherto no omission can be pointed out. For this reason some men of science are inclined to *settle by definition* that the theory of the motion of the earth and of the moon is complete, and to make it the base of the measurement of time. Then, of course, the former definition must be abandoned, and two arbitrary intervals between successive culminations of a fixed star no longer are equal.

Prof. Clerk Maxwell says ("Theory of Heat," second edition, p. 81): "This shows that time, though we conceive it merely as the succession of our states of consciousness, is capable of measurement, independently, not only of our mental states, but of any particular phenomenon whatever." In my opinion this assertion is erroneous. If we reject the rotation of the earth as the base of the measurement of time, we must have recourse to the motion of the earth round the sun or to that of the moon round the earth, or to any other phenomenon. Thomson and Tait, in § 406, already allude to a metal spring oscillating *in vacuo*. It should then be *settled by definition*, for example, that such a spring has a harmonical motion. If we proclaimed the lapses of time between the successive arrivals of flux and reflux at a particular station to be equal, and if we admitted, in order to divide these lapses of time into equal parts, *e.g.*, that the water sinks and rises uniformly, *then the whole of kinematics and abstract dynamics would retain the same form*; even then a centrobaric body with kinetical symmetry in respect to its centre of gravity, would show the peculiarity already mentioned. But it would be seen that our kinematics and abstract dynamics were but a highly deficient aid for the interpretation of phenomena; and the earth would not at all be a body with the same motion round its axis, as if it were a centrobaric body with kinetical symmetry in respect to its centre of gravity.

Of course it is wise to maintain provisionally the definition by which the earth in equal times rotates through equal angles.

In applying abstract dynamics to the interpretation of phenomena, we are led to identify the idea of mass with the idea of quantity of matter, and this has furnished excellently satisfying results. From this, in conjunction with experiment, it follows that two bodies which have equal weight, possess equal quantities of matter; that no matter is annihilated or created, &c.

This article is already too long for me to dwell on other consequences which follow from my view. Only a few words on the conservation of energy. This law threatens to be considered an axiom. Yet I believe it desirable that we should always remember that it is the result of experiment. If the measuring of time were founded on a different basis, it would not hold. Still the experiments do not give perfectly satisfying results. Usually this is ascribed to the imperfection of our methods and instruments, which really may be the cause. But, probably, if in future times it be found by improved methods and instruments that the law does not hold, it would be advantageous to proclaim by definition the conservation of energy and to deduce from it the measurement of time. Then we should have the analogon of the absolute scale of temperature of Thomson.

If any one after the perusal of this article asserts that my views are at variance with the historical development of science, I answer that often in the reasoning of man there are gaps, which by contemporaries are not perceived; but that we must try to find them out and to fill them.

I hope my readers will not be too much annoyed by the defective manner in which I may have expressed myself in English; it is always difficult to make use of a foreign language.

Before closing I am bound to state that I have particularly mentioned the assertions of Sir W. Thomson, Prof. Tait, and Prof. Clerk Maxwell, because in their works I found most emphatically stated what in my opinion is erroneous. These eminent men stand so high that it is unnecessary for me to express my profound respect for them. V. A. JULIUS

Roermond, Holland

OUR ASTRONOMICAL COLUMN

THE D'ANGOS COMET OF 1784.—Encke's investigation relating to this reported comet appears in Zach's *Correspondance Astronomique*, as an "Imposture Astronomique grossière du Chevalier D'Angos, dévoilée par J. F. Encke, à Gotha." Olbers, in a letter addressed to him, had, as already stated, asked his attention to the subject, saying, "I would invite you to the examination of a doubtful comet of which the result will be either the knowledge of the yet unknown orbit of a very remarkable comet, or the discovery of a most shameful imposture," and adding particulars to which allusion is made in our previous note.

Encke remarks at the outset that, contrary to all general usage amongst astronomers, D'Angos had given the Malta observations with mean times for Paris, and the comet's positions expressed in longitudes and latitudes, which confirmed the suspicion that he had computed from the elements of an imaginary orbit and had not taken the trouble to convert the results into right ascensions and declinations, in which astronomers are accustomed to present them. If it is demonstrable that according to the observations (at least without supposing them erroneous to the amount of many minutes) the comet could only have moved in a very improbable orbit, in fact almost as a satellite of the earth and at a distance less than that of the moon, and if further it can be shown that by a very simple error of calculation D'Angos was misled in deducing the places of the comet from the imaginary elements, then, Encke urged, there remains no longer the smallest doubt that he had invented all these observations. Making use of the positions given for April 15, 22, and 29, Encke assuming arbitrarily a value of the comet's curtate distance from the earth at the first date, finds the corresponding value for the same at the third date in order to represent precisely the longitude on April 22, and compares with the corresponding latitude. Thus if the curtate distance on April 22 be taken as 0·42 (we somewhat contract Encke's figures) the third distance is 0·55, the error on the middle latitude, − 16′, and the resulting conic section is a hyperbola; the same form of orbit is deduced when the comet's distance on April 22 is diminished to 0·25. If this distance be further diminished to 0·146, the orbit becomes an ellipse, but the error on the middle latitude is still − 12′·8, and it was found necessary to reduce the curtate distance to 0·00126 in order to represent this latitude with no greater error than − 2′·5; the resulting orbit being also an ellipse. Taking the solar parallax at 8″·86, this distance corresponds to 116,000 miles, or about half the moon's distance from the earth, and under the condition named above, on April 29 it would still be less than 160,000 miles. Thus Encke found it was necessary to assume the comet's distance from the earth, almost incredibly small if the errors of calculation are to be brought within the limit assigned by D'Angos to the differences between the places computed from his elements and his observations, or about 1¼′; and, he continues, a celestial body under such circumstances remaining for so long a time in immediate proximity to the earth, would assuredly have been retained within its sphere of activity, and D'Angos if he were the first would certainly not have been the only observer of this second moon. Rejecting then as beyond probability the conclusions necessarily drawn from an investigation in the manner here briefly described, Encke proceeded to examine the calculation of geocentric longitudes and latitudes of the comet from the elements assigned by D'Angos.

Taking for example the observation of April 15, the logarithm of the radius-vector calculated from the orbit of D'Angos is found to be 9·8208333, and continuing the computation the resulting position differs from the observation 47° in longitude and 15° in latitude, but supposing that by an error of the pen D'Angos had used a log. radius-vector ten times greater, or 0·8208333, with the same heliocentric longitude and latitude, the errors are reduced to 56 seconds and 34 seconds respectively, and making the same change in the log. radii-vectores at the other dates of observation, Encke arrived at the extraordinary result that the whole of the reputedly-observed places were represented within about the limit of error mentioned by D'Angos, and he insisted that with such proof there could be no possible doubt that the observations and the orbit of the comet, "ne soient entièrement fausses et controuvées, et que par conséquent il faut les rayer de tous nos catalogues des comètes, comme un astre chimerique qui n'a jamais existé;" while at the same time he acknowledged himself ready to do justice to the accused and to make the most ample satisfaction if he could be opposed by arguments as strong and apparently conclusive as those upon which he had founded an adverse verdict.

THE TOTAL SOLAR ECLIPSE, 1889, DECEMBER 22.—In continuation of notices of future total eclipses of the sun which have appeared in this column, the elements of the eclipse of 1889, December 22, are subjoined:—

G.M.T. of Conjunction in R.A. Dec. 22, at 0h. 24m. 50s.

	° ′ ″
R.A.	271 0 10·4
Moon's hourly motion in R.A. ...	41 20·7
Sun's " " " ...	2 46·6
Moon's declination	23 14 1·8 S.
Sun's "	23 27 10·3 S.
Moon's hourly motion in decl. ...	3 4·2 S.
Sun's " " " ...	0 1·1 N.
Moon's horizontal parallax ...	61 17·1
Sun's " " ...	9·1
Moon's semi-diameter	16 42·0
Sun's "	16 16·1

The central and total eclipse commences in long. 78° 52′ W., lat. 15° 22′ N., and ends in long. 60° 55′ E. and lat. 6° 53′ N., and it occurs with the sun on the meridian in long. 6° 27′ W., lat. 11° 5′ S.

At a point on the coast of Africa in 10° 6′ S. lat., totality commences at 2h. 8m. 55s. local time, and continues 3m. 34s. At Bridgetown, Barbadoes, totality begins at 6h. 47m. 6s. A.M. local time, and continues 1m. 48s., but the sun's altitude is only 6°. The following are points upon the central line, which will show that with a fair duration where the sun is near the meridian, the course of the eclipse is not a favourable one for observation:—

Long. ° ′	Lat. ° ′	Long. ° ′	Lat. ° ′
59 22 W.	13 30 N.	5 5 E.	11 23 S.
47 39	7 37 N.	14 0	9 45
32 28 W.	0 23 S.	18 32 E.	8 27 S.

HOW TO DRAW A STRAIGHT LINE[1]

III.

BEFORE leaving the Peaucellier cell and its modifications, I must point out another important property they possess besides that of furnishing us with exact rectilinear motion. We have seen that our simplest linkwork enables us to describe a circle of any radius, and if we wished to describe one of ten miles radius the proper course would be to have a ten mile link, but as that would be, to say the least, cumbrous, it is satisfactory to know that we can effect our purpose with a much smaller apparatus. When the Peaucellier cell is mounted for the purpose of describing a straight line, as I told you, the distance between

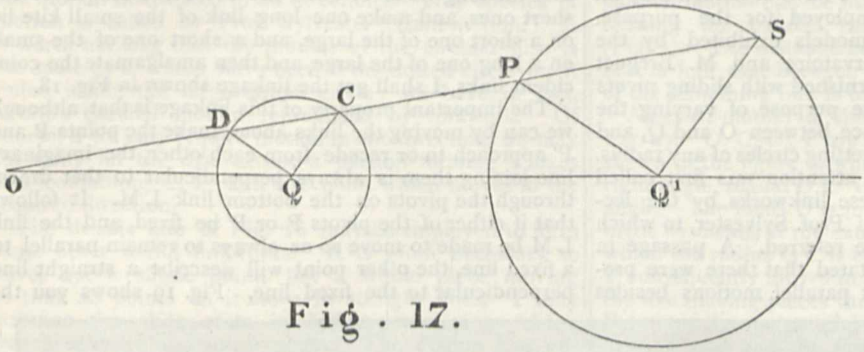

Fig. 17.

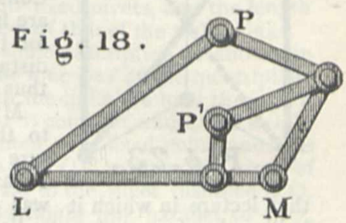

Fig. 18.

the fixed pivots must be the same as the length of the "extra" link. If this distance be not the same we shall not get straight lines described by the pencil, but circles. If the difference be slight the circles described will be of enormous magnitude, decreasing in size as the difference

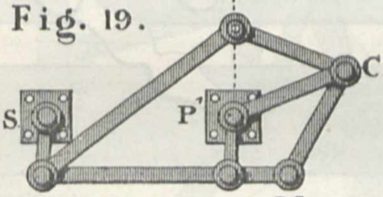

Fig. 19.

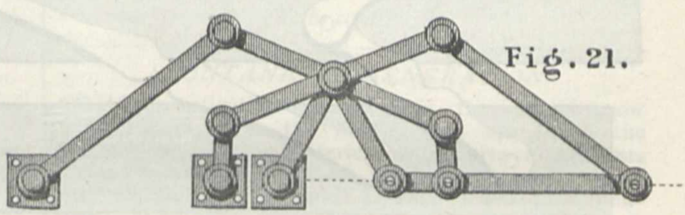
Fig. 21.

increases. If the distance Q O, Fig. 6, be made greater than Q C, the convexity of the portion of the circle described by the pencil (for if the circles are large it will of

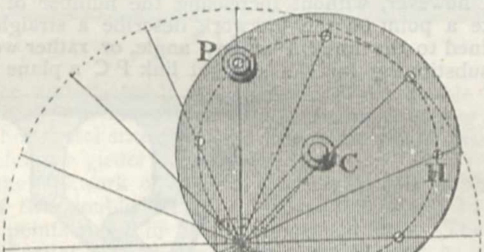

Fig. 20.

course be only a portion which is described) will be towards O, if less the concavity. To a mathematician, who knows that the inverse of a circle is a circle, this will be clear, but it may not be amiss to give here a short proof of the proposition.

In Fig. 17 let the centres Q, Q′ of the two circles be at distances from O proportional to the radii of the circles. If then O D C P S be any straight line through O, D Q will be parallel to P Q′, and C Q to S Q′, and O D will bear the same proportion to O P that O Q does to O Q′. Now considering the proof we gave in connection with Fig. 7, it will be clear that the product O D · O C is con-

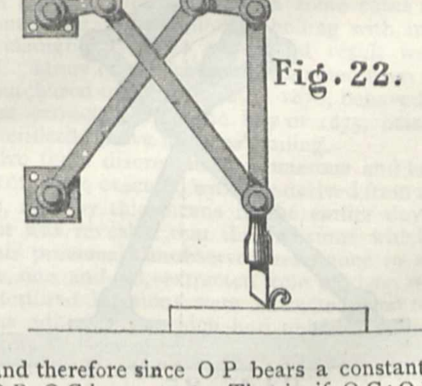
Fig. 22.

stant, and therefore since O P bears a constant ratio to O D, O P · O C is constant. That is, if O C · O P is constant and C describes a circle about Q, P will describe one about Q′. Taking then O, C, and P as the O, C, and

[1] Lecture at South Kensington in connection with the Loan Collection of Scientific Apparatus, by A. B. Kempe, B.A. Continued from p. 89.

P of the Peaucellier cell in Fig. 7, we see how P comes to describe a circle.

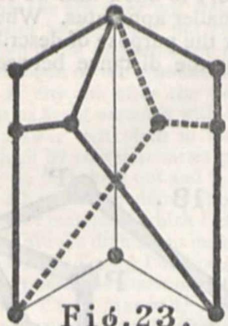

Fig. 23.

It is hardly necessary for me to state the importance of the Peaucellier compass in the mechanical arts for drawing circles of large radius. Of course the various modifications of the "cell" I have described may all be employed for the purpose. The models exhibited by the Conservatoire and M. Breguet are furnished with sliding pivots for the purpose of varying the distance between O and Q, and thus getting circles of any radius.

My attention was first called to these linkworks by the lecture of Prof. Sylvester, to which I have referred. A passage in that lecture in which it was stated that there were probably other forms of seven-link parallel motions besides M. Peaucellier's, then the only one known, led me to investigate the subject, and I succeeded in obtaining some new parallel motions of an entirely different character to that of M. Peaucellier. I shall bring two of these to your notice as the investigation of them will lead us to consider some other linkworks of importance.

If I take two kites, one twice as big as the other, such that the long links of each are twice the length of the short ones, and make one long link of the small kite lie on a short one of the large, and a short one of the small on a long one of the large, and then amalgamate the coincident links, I shall get the linkage shown in Fig. 18.

The important property of this linkage is that, although we can by moving the links about, make the points P and P′ approach to or recede from each other, the imaginary line joining them is always perpendicular to that drawn through the pivots on the bottom link L M. It follows that if either of the pivots P or P′ be fixed, and the link L M be made to move so as always to remain parallel to a fixed line, the other point will describe a straight line perpendicular to the fixed line. Fig. 19 shows you the

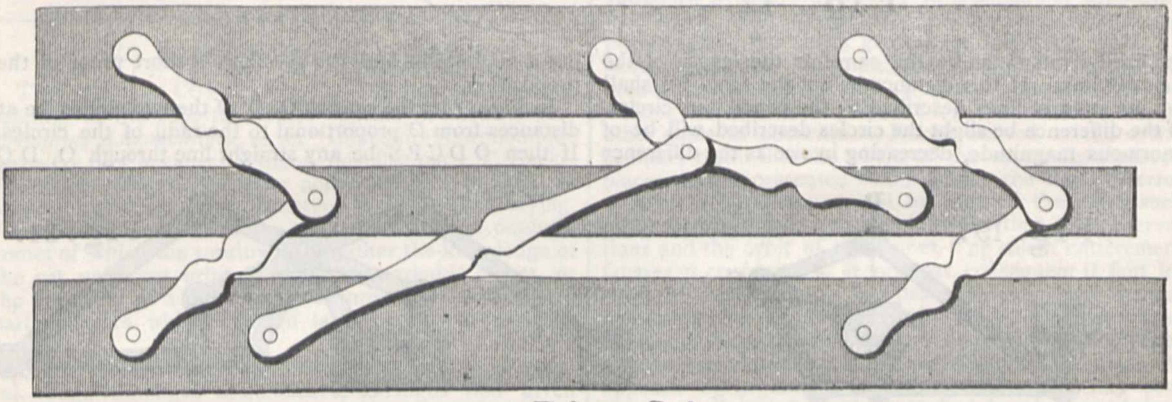

Fig. 24.

parallel motion made by fixing P′. It is unnecessary for me to point out how the parallelism of L M is preserved

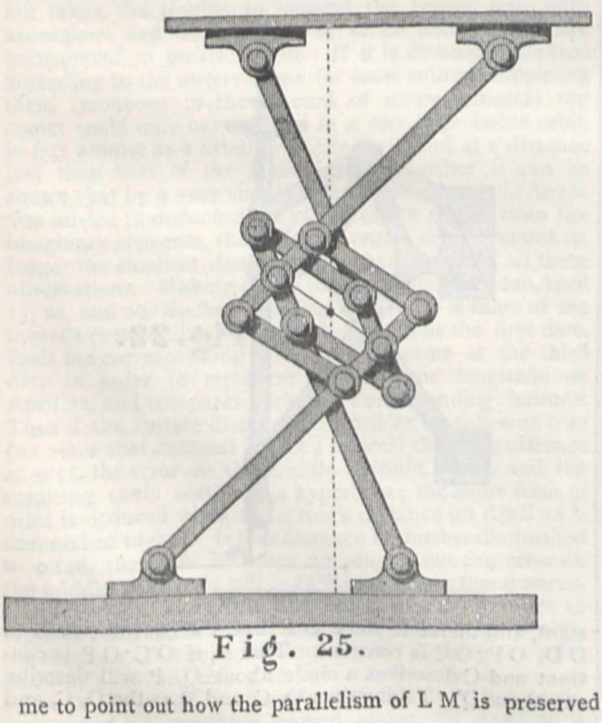

Fig. 25.

by adding the link S L, it is obvious from the figure. The straight line which is described by the point P is perpendicular to the line joining the two fixed pivots; we can, however, without increasing the number of links make a point on the linkwork describe a straight line inclined to the line S P at any angle, or rather we can, by substituting for the straight link P C a plane piece,

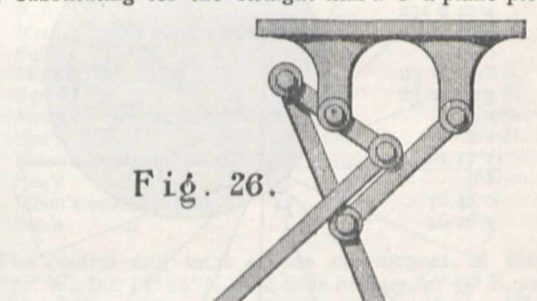

Fig. 26.

get a number of points on that piece moving in every direction.

In Fig. 20, for simplicity, only the link C P′ and the new piece substituted for the link P C are shown. The new piece is circular and has holes pierced in it all at the same distance—the same as the lengths P C and P′ C—from C. Now we have seen from Fig. 19 that P moves in a vertical straight line, the distance P C in Fig. 20 being the same as it was in Fig. 19; but from a well-known property of a circle, if H be any one of the holes pierced in the piece, the angle H P′ P is constant, thus the straight line H P′ is fixed in position, and H moves along it; similarly all the other holes move along in straight lines passing through the fixed pivot P′, and we get straight line motion distributed in all directions. This species of motion is called by Prof. Sylvester "tram-motion." It is worth noticing that the motion of the circular disc is the same as it would have been if the dotted circle on it rolled inside the large dotted circle; we have, in fact, White's parallel motion reproduced by linkwork. Of course, if we only require motion in one direction, we may cut away all the disc except a portion forming a bent arm containing C, P, and the point which moves in the required direction.

The double kite of Fig. 18 may be employed to form some other useful linkworks. It is often necessary to have, not a single point, but a whole piece moving so that all points on it move in straight lines. I may instance the slide rests in lathes, traversing tables, punches, drills, drawbridges, &c. The double kite enables us to produce linkworks having this property. In the linkwork of Fig. 21, the construction of which will be

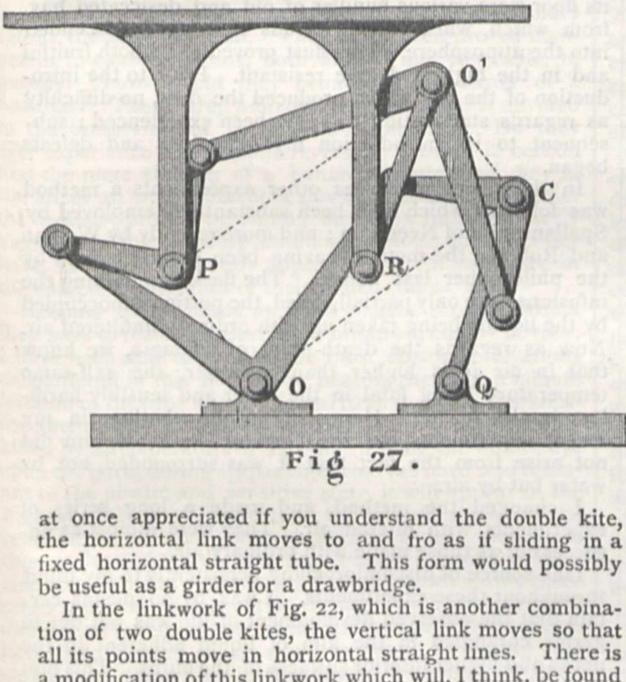

Fig 27.

at once appreciated if you understand the double kite, the horizontal link moves to and fro as if sliding in a fixed horizontal straight tube. This form would possibly be useful as a girder for a drawbridge.

In the linkwork of Fig. 22, which is another combination of two double kites, the vertical link moves so that all its points move in horizontal straight lines. There is a modification of this linkwork which will, I think, be found interesting. In the linkage in Fig. 23, which, if the thin links are removed, is a skeleton drawing of Fig. 22, let the dotted links be taken away and the thin ones be inserted; we then get a linkage which has the same property as that in Fig. 22, but it is seen in its new form to be the ordinary double parallel ruler with three added links. Fig. 24 is a figure of a double parallel rule made on this plan with a slight modification. If the bottom ruler be held horizontal the top moves vertically up and down the board, having no lateral movement.

While I am upon this sort of movement I may point out an apparatus exhibited in the Loan Collection by Prof. Tchebicheff which bears a strong likeness to a complicated camp-stool, the seat of which has horizontal motion. The motion is not strictly rectilinear; the apparatus being, as will be seen by observing that the thin line in the figure is of invariable length, and a link might therefore be put where it is, a combination of two of the parallel motions of Prof. Tchebicheff given in Fig. 4, with some links added to keep the seat parallel with the base. The variation of the upper plane, from a strictly horizontal movement is therefore double that of the tracer in the simple parallel motion.

Fig. 26 shows how a similar apparatus of much simpler construction employing the Tchebicheff approximate parallel motion can be made. The lengths of the links forming the parallel motion have been given before (Fig. 4). The distance between the pivots on the moving seat is half that between the fixed pivots, and the length of the remaining link is one-half that of the radial links.

An *exact* motion of the same description is shown in Fig. 27. O, C, O′, P are the four *foci* of the quadriplane shown in the figure in which the links are bent through a right angle, so that O C · O P is constant, and C O P a right angle. The focus O is pivoted to a fixed point, and C is made by means of the extra link Q C to move in a circle of which the radius Q C is equal to the pivot distance O Q. P consequently moves in a straight line parallel to O Q, the five moving pieces thus far described constituting the Sylvester-Kempe parallel motion. To this are added the moving seat and the remaining link R O′, the pivot distances of which, P R and R O′, are equal to O Q. The seat in consequence always remains parallel to Q O, and as P moves accurately in a horizontal straight line, every point on it will do so also. This apparatus might be used with advantage where a very smoothly-working traversing table is required.

(*To be continued.*)

SPONTANEOUS GENERATION[1]

THE investigation embodied in the memoir now submitted to the Society was opened in the summer of 1876 by a series of tentative experiments on turnip-infusions, to which were added varying quantities of bruised or pounded cheese. I was soon, however, drawn away from them to other experiments on infusions of hay. With this substance no difficulty was encountered in my first inquiry. Boiled for five minutes, and exposed to air purified spontaneously or freed from its floating matter by calcination or filtration, hay infusion, though employed in multiplied experiments at various times, never showed the least competence to kindle into life. After months of transparency, I have, in a great number of cases, inoculated this infusion with the smallest specks of animal and vegetable liquids containing *Bacteria*, and observed twenty-four hours afterwards, its colour lightened, and its mass rendered opaque by the multiplication of these organisms.

But in the autumn of 1876, the substance with which I had experimented so easily and successfully a year previously, appeared to have changed its nature. The infusions extracted from it bore in some cases not only five minutes' but fifteen minutes' boiling with impunity. But on changing the hay a different result was often obtained. Many of the infusions extracted from samples of hay purchased in the autumn of 1876, behaved exactly like those extracted from the hay of 1875, being completely sterilized by five minutes' boiling.

To solve these discrepancies, numerous and laborious experiments were executed with hay derived from different localities, and by this means in the earlier days of the inquiry, it was revealed that the infusions which manifested this previously unobserved resistance to sterilization were, one and all, extracted from old hay, while the readily sterilized infusions were extracted from new hay, the germs adhering to which had not been subjected to long-continued desiccation.

I then fell back upon infusions whose deportment had

[1] "Further Researches on the Deportment and Vital Resistance of Putrefactive and Infective Organisms, from a Physical Point of View." By John Tyndall, LL.D., F.R.S., Professor of Natural Philosophy in the Royal Institution.—Abstract.

been previously familiar to me, and in the sterilization of which I had never experienced any difficulty. Fish, flesh, and vegetables were re-subjected to trial. Though the precautions taken to avoid contamination were far more stringent than those observed in my first inquiry, and though the interval of boiling was sometimes tripled in duration, these infusions, in almost every instance, broke down. Spontaneously purified air, filtered air, and calcined air,—calcined, I may add, with far greater severity than was found necessary a year previously,—failed, in almost all cases, to protect the infusions from putrefaction.

I had the most implicit confidence in the correctness of my earlier experiments; indeed, incorrectness would have led to consequences exactly opposite to those arrived at. Errors of manipulation would have filled my tubes and flasks with organisms instead of leaving them transparent and void of life. By the unsuccessful experiments above referred to a clear issue was therefore raised: Either the infusions of fish, flesh, and vegetable had become endowed in 1876 with an inherent generative energy which they did not possess in 1875, or some new contagium external to the infusions, and of a far more obstinate character than that of 1875, had been brought to bear upon them. The scientific mind will not halt in its decision between these two alternatives.

For my own part the gradual but irresistible interaction of thought and experiment rendered it at first probable, and at last certain that the atmosphere in which I worked had become so virulently infective as to render utterly impotent precautions against contamination, and modes of sterilization, which had been found uniformly successful in a less contagious air. I therefore removed from the laboratory, first to the top, and afterwards to the basement of the Royal Institution, but found that even here, in a multitude of cases, failure was predominant, if not uniform. This hard discipline of defeat was needed to render me acquainted with all the possibilities of infection involved in the construction of my chambers and the treatment of my infusions.

I finally resolved to break away from the Royal Institution, and to seek at a distance from it a less infective atmosphere. In Kew Gardens, thanks to our President, the requisite conditions were found. I chose for exposure in the Jodrell laboratory the special infusions which had proved most intractable in the laboratory of the Royal Institution. The result was that liquids which in Albemarle Street resisted two hundred minutes boiling, becoming fruitful afterwards, were utterly sterilised by five minutes' boiling at Kew.

A second clear issue is thus placed before the Royal Society:—Either the infusions had lost in Kew Gardens an inherent generative energy which they possessed in our laboratory, or the remarkable instances of life development, after long-continued boiling, observed in the laboratory are to be referred to the contagium of its air.

With a view to making nearer home experiments similar to those executed at Kew, I had a shed erected on the roof of the Royal Institution. In this shed infusions were prepared and introduced into new chambers of burnished tin, which had never been permitted to enter our laboratory. After their introduction the liquids were boiled for five minutes in an oil-bath.

The first experiment in this shed resulted in complete failure, the air of the shed proving to be sensibly as infective as the air of the laboratory.

Either of two causes, or both of them combined, might, from my point of view, have produced this result. First, a flue from the laboratory was in free communication with the atmosphere not far from the shed; secondly, and this was the real cause of the infection, my assistants in preparing the infusions, had freely passed from the laboratory to the shed. They had thus carried the contagium by a mode of transfer known to every physician.

The infected shed was disinfected; the infusions were again prepared, and care was taken, by the use of proper clothes, to avoid the former causes of contamination. The result was similar to that obtained at Kew, viz., organic liquids which in the laboratory withstood two hundred minutes' boiling, were rendered permanently barren by five minutes' boiling in the shed.

A third clear issue is thus placed before us, which I should hardly think of formulating before the Royal Society, were it not for the incredible confusion which apparently besets this subject in the public mind. A rod thirty feet in length would stretch from the infusions in the shed to the same infusions in the laboratory. At one end of this rod the infusions were sterilized by five minutes' boiling, at the other end they withstood two hundred minutes' boiling. As before, the choice rests between two inferences:—Either we infer that at one end of the rod animal and vegetable infusions possess a generative power, which at the other end they do not possess; or we are driven to the conclusion that at the one end of the rod we have infected, and at the other end disinfected air.

The second inference is that which will be accepted by the scientific mind. To what, then, is the inferred difference at the two ends of the rod to be ascribed? In one obvious particular the laboratory this year differed from that in which my first experiments were made. On its floor were various bundles of old and desiccated hay, from which, when stirred, clouds of fine dust ascended into the atmosphere. This dust proved to be both fruitful and in the highest degree resistant. Prior to the introduction of the hay which produced the dust, no difficulty as regards sterilization had ever been experienced; subsequent to its introduction my difficulties and defeats began.

In these and numerous other experiments a method was followed which had been substantially employed by Spallanzani and Needham; and more recently by Wyman and Roberts, the method having been greatly refined by the philosopher last named. The flasks containing the infusions were only partially filled, the portions unoccupied by the liquids being taken up with ordinary unfiltered air. Now as regards the death-point of contagia, we know that in air it is higher than in water, the self-same temperature being fatal in the latter and sensibly harmless in the former. Hence my doubt whether, in my recent experiments, the resistance of the contagium did not arise from the fact that it was surrounded, not by water but by air.

I changed the method, and made a long series of experiments with filtered air. They were almost as unsuccessful as those made with ordinary air.

One source of discomfort clung persistently to my mind throughout these experiments. I was by no means certain that the observed development of life was not due to germs entangled in the film of liquid adherent to the necks and higher interior surfaces of the bulbs. This film might have dried, and its germs, surrounded by air and vapour, instead of by water, might on this account have been able to withstand an ordeal to which they would have succumbed if submerged.

A plan was, therefore, resorted to by which the infusions were driven by atmospheric pressure through lateral channels issuing from the centres of the bulbs. As before, each bulb was filled with one-third of an atmosphere of filtered air, and afterwards heated nearly to redness. When fully charged, the infusion rose higher than the central orifice, and no portion of the internal surface was wetted save that against which the liquid permanently rested. The lateral channel was then closed with a lamp without an instant's contact being permitted to occur between any part of the infusion and the external air. It was thus rendered absolutely certain that the contagia exposed subsequently to the action of heat were to be sought, neither in the superjacent air nor on the interior surfaces of the flasks, but in the body of the infusions themselves.

By this method I tested in the first place the substance which, at an early stage of the inquiry, had excited my suspicion—without reference to which the discrepancy between the behaviour of infusions examined in the winter of 1875-76 and those examined in the winter of 1876-77 is inexplicable, but by reference to which the explanation of the observed discrepancy is complete—I mean the old hay which cumbered our laboratory floor.

Four hours' continuous boiling failed to sterilise bulbs charged with infusions of this old hay. In special cases, moreover, germs were found so indurated and resistant, that five, six, and in one case even eight hours' boiling failed to deprive them of life. All the difficulties encountered in this long and laborious inquiry were traced to the germs which exhibited the extraordinary powers of resistance here described. They introduced a plague into our atmosphere—the other infusions, like a smitten population, becoming the victims of a contagium foreign to themselves.[1]

It is a question of obvious interest to the scientific surgeon whether those powerfully resistant germs are amenable to the ordinary processes of disinfection. It is perfectly certain that they resist to an extraordinary extent the action of heat. They have been proved competent to cause infusions, both animal and vegetable, to putrefy. How would they behave in the wards of a hospital? There are, moreover, establishments devoted to the preserving of meats and vegetables. Do they ever experience inexplicable reverses. I think it certain that the mere shaking of a bunch of desiccated hay in the air of an establishment of this character might render the ordinary process of boiling for a few minutes utterly nugatory, thus possibly entailing serious loss. They have, as will subsequently appear, one great safeguard in the complete purgation of their sealed tins of air.

Keeping these germs and the phases through which they pass to reach the developed organism clearly in view, I have been able to sterilise the most obstinate infusions encountered in this inquiry by heating them for a minute fraction of the time above referred to as *insufficient* to sterilise them. The fully developed Bacterium is demonstrably killed by a temperature of 140° F. Fixing the mind's eye upon the germ during its passage from the hard and resistant to the plastic and sensitive state, it will appear in the highest degree probable that the plastic stage will be reached by different germs in different times. Some are more indurated than others and require a longer immersion to soften and germinate. For all known germs there exists a period of incubation during which they prepare themselves for emergence as the finished organisms which have been proved so sensitive to heat. If during this period, and well within it, the infusion be boiled for even the fraction of a minute, the softened germs which are then approaching their phase of final development will be destroyed. Repeating the process of heating every ten or twelve hours, and before the least *sensible* change has occurred in the infusions, each successive heating will destroy the germs then softened and ready for destruction, until after a sufficient number of heatings the last living germ will disappear.

Guided by the principle here laid down, and applying the heat discontinuously, infusions have been sterilised by an aggregate period of heating, which, fifty times multiplied, would fail to sterilise them if applied continuously. Four minutes in the one case can accomplish what four hours fail to accomplish in the other.

If properly followed out the method of sterilisation here described is infallible. A temperature, moreover, far below the boiling point suffices for sterilisation.

Another mode of sterilisation equally certain, and perhaps still more remarkable, was forced upon me, so to speak, in the following way:—In a multitude of cases a thick and folded layer of fatty scum, made up of matted *Bacteria*, gathered upon the surfaces of the infusions, the liquid underneath becoming sometimes cloudy throughout, but frequently maintaining a transparency equal to that of distilled water. The living scum-layer, as Pasteur has shown in other cases, appeared to possess the power of completely intercepting the atmospheric oxygen, appropriating the gas and depriving the germs in the liquid underneath of an element necessary to their development. Above the scum, moreover, the interior surfaces of the bulbs used in my experiments were commonly moistened by the water of condensation. Into it the *Bacteria* sometimes rose, forming a kind of gauzy film to a height of an inch or more above the liquid. In fact, wherever air was to be found, the *Bacteria* followed it. It seemed a necessity of their existence. Hence the question, What will occur when the infusions are deprived of air?

I was by no means entitled to rest satisfied with an inference as an answer to this question; for Pasteur, in his masterly researches, has abundantly demonstrated that the process of alcoholic fermentation depends on the continuance of life without air—other organisms than *Torula* being also shown competent to live without oxygen. Experiment alone could determine the effect of exhaustion upon the particular organisms here under review. Air-pump vacua were first employed, and with a considerable measure of success. Life was demonstrably enfeebled in such vacua.

Sprengel pumps were afterwards used to remove more effectually both the air dissolved in the infusions and that diffused in the spaces above them. The periods of exhaustion varied from one to eight hours, and the results of the experiments may be thus summed up:—Could the air be completely removed from the infusions, there is every reason to believe that sterilisation *without boiling* would in most, if not in all cases, be the result. But, passing from probabilities to certainties, it is a proved fact, that in numerous cases unboiled infusions deprived of air by five or six hours' action of the Sprengel pump are reduced to permanent barrenness. In a great number of cases, moreover, where the unboiled infusion would have become cloudy, exposure to the boiling temperature for a single minute sufficed completely to destroy the life already on the point of being extinguished through defect of air. With a single exception, I am not sure that any infusion escaped sterilisation by five minutes' boiling after it had been deprived of air by the Sprengel pump. These five minutes accomplished what five hours often failed to accomplish in the presence of air.

The inertness of the germs in liquids deprived of air is not due to a mere *suspension* of their powers. They are *killed* by being deprived of oxygen. For when the air which has been removed by the Sprengel pump is, after some time, carefully restored to the infusion, unaccompanied by germs from without, there is no revival of life. By removing the air we stifle the life which the returning air is incompetent to restore.

[1] A hard and wiry hay from Guildford, which I have no reason to consider old, was found very difficult to sterilise.

AGRICULTURAL EXPERIMENTS AT WOBURN

IN the autumn of 1875 Mr. C. Randell proposed to the Council of the Royal Agricultural Society that it be referred to the Chemical Committee to consider the propriety, and the manner, of instituting a series of experiments, to test the accuracy of the estimated value of manure obtained by the consumption of different articles of food, as given in Mr. Lawes' paper, in the Spring Number of the Journal of the Society.

As it was decided that experiments by practical farmers in different districts could not be relied on, the Duke of

Bedford very kindly offered to afford facilities for making new experiments at his own cost.

Mr. Lawes and Dr. Voelcker were requested to draw up a scheme for carrying on, at Woburn, such experiments as they, in communication with the Chemical Committee, might determine on. His grace offered to give up for the purpose Crawley Mill Farm, comprising about ninety acres, with the house and buildings. But, on examination, it was found that there was no sufficient area on that farm so even in character, and in condition, of soil, as to render it available for a considerable series of comparative field experiments. Eventually, after inspection of many others, a large field of much more suitable land was selected, on Birchmoor Farm. Crawley Mill Farm is, however, also retained, as a means of providing a residence for the Superintendent of the experiments, the requisite buildings, and the opportunity of having at command the necessary horse and hand labour for the experiments. Mr. P. H. Cathcart, formerly at the Royal Agricultural College, Cirencester, has been appointed the Resident Superintendent of the experiments.

As experiments to determine the value of the manure obtained by the consumption of purchased foods obviously involved the necessity of feeding animals under conditions in which the manure could be collected with as little loss as possible, the Duke of Bedford has erected eight very complete feeding boxes, in which the manure for the experimental barley and root crops recently sown has been made.

The field devoted to the field experiments has an area of twenty-seven acres; the soil has been carefully tested all over, and an account taken of the history of the field since 1874.

It was considered important, especially with reference to valuations under the Agricultural Holdings' Act, to add, if possible, to our knowledge of the manure-value of both artificial manures and consumed feeding stuffs; and it was decided, therefore, both to compare the effects of the manure obtained by the consumption of selected purchased foods, with those obtained by artificial manures estimated to supply the same constituents, and also to determine the effects of dung and artificial manuring substances, applied year after year, on the Woburn soil, and to compare these with the results obtained for so many years, with the same manures, on the very different soil at Rothamsted. Accordingly, 2¾ of the six acres where wheat had been grown in 1876, after tares and turnips, each fed with cake, are devoted to the continuous growth of wheat, and 2¾ acres to the continuous growth of barley. In each case the area is divided into eleven plots, of a quarter of an acre each.

The description and quantities of the manures for these experiments have been so carefully selected that in the end valuable results must be obtained as to the comparative value of various kinds of artificial manure as compared with farmyard manure, the constituents of which are accurately known. Two of the plots are unmanured; seven are manured with artificial manure of more or less complicated composition, and two with farmyard manure estimated to contain different proportions of nitrogen. In connection with the farmyard manure an accurate record is kept of the kinds and quantities of food from which it is produced, as also of the increase in the live-weight of the stock thus fed.

Besides these continuous experiments a series of rotation experiments—seeds, wheat, roots, barley, in successive years from 1877 to 1881—are to be carried out. The stock which is to supply the farmyard manure for these experiments is to be fed with decorticated cotton-cake, which among purchased feeding stuffs has a very high manure value, and maize-meal, which has a very low manure value. The effects of the manures obtained by the consumption of these foods will be compared with those of artificial manures supplying, in one case the same amount of nitrogen, potass, phosphoric acid, &c., as is estimated to be contained in the manure from the cotton-cake consumed, and in another the same as in that from the maize-meal consumed. Accordingly, four feeding experiments have been conducted, in each of which the same amount of litter has been used, and the same amount of roots, and the same amount of wheat-straw chaff consumed. In Experiment 1, 1,000 lbs. decorticated cotton-cake were given in addition; and in Experiment 2, 1,000 lbs. maize-meal. In Experiments 3 and 4 no purchased food was given; but in Experiment 3 artificial manures estimated to contain the same amount of the chief constituents as the manure from 1,000 lbs. of cotton-cake, and in Experiment 4 the same as from 1,000 lbs. maize-meal, will be applied to the land, in addition to the root and chaff manure.

Four areas of four acres each have been devoted to these rotation experiments, eight of them coming into exact experiment this year, and the remaining eight in 1878. Each area of four acres is again divided into four plots, each of the latter sub-divisions bearing the same crop during the rotation of four years, but undergoing different treatment in the way of manure. For example, rotation No. 1, now under seeds, is treated as follows. Each plot is being separately fed by sheep. Plot 1 with cotton-cake; Plot 2 with maize-meal; and Plots 3 and 4 without purchased food. But, for the succeeding wheat, artificial manure estimated to contain nitrogen, and other constituents, in amounts equal to those in the manure from the consumed cotton-cake, will be applied to Plot 3, and artificial manure, equal to that from the consumed maize-meal will be applied to Plot 4. For the roots in 1879 (succeeding the wheat), the 4 acres will be manured as already described, and barley will complete the course in 1880. The other rotations are so treated as at the end of the four years to yield a collection of data that must be of the highest value in agricultural chemistry, and therefore to practical agriculture. In a "Statement" as to the objects and plan of the experiment which lies before us, full details are given on all points, and carefully constructed tables relating to every stage of the experiments, which show that all possible care has been taken to secure accuracy and practical utility in the results. The experiment will no doubt be anxiously watched by all interested in scientific agriculture.

NOTES

Prof. Frankland, D.C.L., F.R.S., has now in the press a volume containing his collected researches in Pure, Applied, and Physical Chemistry, dedicated to Prof. Bunsen, of Heidelberg. The section on Pure Chemistry treats, amongst other matters, of the Isolation of the Organic Radicals, and the Discovery of Organo-Metallic Bodies, and their Application to the Synthetical Production of Organic Compounds. In the section devoted to Applied Chemistry, the author describes his Investigations on the Manufacture of Gas for Illuminating Purposes; on the Qualities of Potable Waters; and on the Treatment of the Sewage of Towns. Physical Chemistry includes his Experiments upon Flames, and upon the Source of Muscular Power, together with those on the Spectra of Gases and Vapours. Each chapter is preceded by introductory remarks, having reference to the scope, object, and future development of the subject treated of. Mr. Van Voorst is the publisher.

A movement has been commenced in Spain for the formation of an association similar to the British Association. The Madrid Societies of Natural History, Anthropology, and Geography have appointed a joint commission to consider how best to organise an annual meeting in different parts of the kingdom for the purpose of investigating matters of scientific interest within the domain of these societies, and also to arrange for the publication of the results that may thus be obtained. A movement like this leads one to hope that a fair future is yet in store for Spain.

IT is probable that the Sixth Congress of Russian Naturalists will not be held this year, the Government not having granted a sum of money for the expenses of the Congress, and private help being unlikely to be forthcoming on account of the war.

THE fifth session of the International Congress of the Medical Sciences will be held at Geneva, from September 9 to 15. In connection with the Congress there will be an exhibition of new apparatus and instruments used in medicine, surgery, physiology, &c. Articles for exhibition should be sent free of all charges to the "Direction de l'Exposition du Congrès Médical; Dr. J. L. Reverdin, place du Lac, Geneva." Intending exhibitors should intimate before August 15 what space they are likely to require.

WE hope shortly to give an account of the proceedings which took place in connection with the recent Gauss centenary celebration. We may here state that the festival speech was delivered by Prof. Dr. Sommer, that a sketch for a monument by the Berlin sculptor, Schæfer, was exhibited in the Festival Hall, and that his Majesty, the Emperor of Germany, was a contributor to the Memorial Fund. The following pamphlets have appeared:—"Briefe zwischen A. von Humboldt und Gauss. Herausgegeben von Dr. K. Bruhns;" "Gauss. Ein Umriss seines Lebens und Wirkens. Von F. A. T. Winnecke;" "Über die Anzahl der Ideal-Classen in den verschiedenen Ordnungen eines endlichen Körpers. Von R. Dedekind." The committee also intend to publish an account of Gauss's relations with Brunswick.

FRANCE appears to be becoming more and more anxious to do honour to her science worthies by the erection of statues. A statue to Arago is being erected at Perpignan, in the department of Orientales Pyrénées. Another to Niepce de Saint-Victor, a name well known in connection with improvements in photography, will be erected at Chalons, his native place, by public subscription, at the instance of the Municipal Council of the city. It is also stated that a public subscription will be opened at Lyons on behalf of Ampère, the inventor of the electro-magnet, and the precursor of Faraday in the invention of the inductive electricity. Ampère was born in that city in 1775, and his father was guillotined there on the Place des Terraux for having been active in the great royalist rebellion against the Convention, which ended in the famous siege of Lyons and his capture by Dubois-Crancé.

AT the usual fortnightly meeting of the Royal Geographical Society on Monday, a paper on "Journeys up the Niger and Notes on the Neighbouring Countries," by Bishop Crowther, was read. The paper, which dealt with the journeys of Bishop Crowther in Western Africa, between 1841 and 1871, described the character of the river Niger, the villages of the natives upon its banks, the tribes scattered about the neighbouring countries, &c. It was remarked that the actual extent of the delta of the river was still uncertain, but the lecturer inclined to the opinion that the affluents of the river, and particularly the Bénuwé, on the south bank, if traced to their source would lead to a rich field of discovery. What might be called the delta of the river was a vast tract of marshy country extending along a coast line of 120 miles, and probably in parts some 150 miles in breadth. In the course of the journey of some 700 miles no less than thirteen tribes, speaking as many different languages, were met with. Ten of the tribes appeared to be of the same family, and might be classed as aboriginal. The Housas were a tribe spread in the widest direction, and the territory in which their language was spoken appeared to be more considerable than any in Africa. It was a beautiful language, and had become to Africa what French is to Europe. The other important language of that part of Africa was the Fulah. The Filanis were a remarkable people who had conquered extensive parts far to the south of the river Bénuwé. Dr. Barth stated that he had been told by natives of the interior that in bygone days an ancient kingdom called Ghanata had existed. The trade routes which meet the Egga on the Niger were important; the chief came from the north, from Tripoli, across the Sahara, with European produce on camels to the Nupe kingdom, where it was distributed in the neighbouring countries. It has been resolved by the Church Missionary Society to send out a small steamer, drawing only three feet of water, to push further into the interior, and afford assistance to Bishop Crowther to carry the missionary work more completely among the natives.

A MUSEUM of Science and Arts has been established at St. Louis, U.S.

WE are glad to learn that the experiments with Jablochkoff's Electric Light are to be repeated at the West India Dock tomorrow evening. We hope all will go well and fairly on this occasion so as to allow a real test to be made of the practical utility of the invention.

AN Italian optician established in Paris has constructed a very sensitive metallic thermometer on a new principle. The dilations of a small sheet of platinised silver are amplified by means of a system of levers, and the motion is communicated to a needle on a dial, on which degrees are marked. The motion of the needle is almost instantaneous. The apparatus has been tested in the "Ville de Paris," a new balloon sent up on June 3 at Paris.

THE St. Petersburg Society of Naturalists has intrusted Professors Fr. Schmidt and Inostrantseff with a geological exploration of the Valley of the Neva, from Schlüsselburg down to the Finnish Gulf. From the interest possessed by the glacial accumulations in this valley, as well as the qualifications of both professors for this special subject, we may expect much new light on the question of the glaciation of Northern Russia.

THE *Turkestan Gazette* gives the latest news from M. Prshevalsky, dated from Lob Nor, February 22. After having reached this lake by the valley of the Lower Tarim, M. Prshevalsky advanced 130 miles east of the lake. The survey and the astronomical measurements of latitudes and longitudes he has made give a totally new aspect to the map of the country. The population on the banks of the Tarim and around the Lob Nor is very sparse; the people speak almost the same language as that of Eastern Turkestan. The flora and fauna of the locality are very poor; some vegetation is found only in the Tarim valley, the neighbourhood being a true desert. During February and March M. Prshevalsky was to stay in the Lower Tarim, during May at Yuldus, and during June at Kunghes. About the beginning of July he proposes to return to Kuldsha to begin in August his journey to the Tibet.

THE *Gardener's Chronicle* learns that it is proposed to erect at Ootacamund, in the Neilgherry Hills, a statue of the late Mr. MacIvor, to whom the successful cultivation of Cinchona on those Indian slopes is so pre-eminently due.

WE see from the Report of the Auckland (New Zealand) Institute for 1876-7, that that society is in a flourishing condition, and that during the year thirteen papers on subjects of scientific importance in connection with the Colony were read. About a year ago a fine new museum was opened, the cost of building having been 4,000*l.*, half of which was raised by private subscription and half obtained as a grant from the New Zealand Government.

AT a recent meeting of the St. Petersburg Technical Society, M. Chikolef made an interesting communication as to the experiments recently made at St. Petersburg for determining the lighting power of the electrical light at great distances. The power of the light is notably increased by covering the carbon of the lamp with a thin sheet of copper (one-sixteenth of the diameter of the carbon at its upper part, and from one-forty-eighth to one-sixty-fourth in its lower part). It depends also upon the direction given to the carbon, the best being to turn the cup towards the object to be lighted. The great machine of Alteneck, with a carbon of 12 millim. of diameter, gave a *maximum* of light equal to 10,210 candles, and a mean of 5,739 candles; whilst with a carbon of 10 millim., but galvanised, it gave a maximum of 16,255 candles (20,275 when the cup is turned as above) and a mean of 14,039 candles. The light was sufficient to make objects visible (for military purposes) at a distance of 3,080 yards. Of many machines used, the most economical proved to be the great one of Alteneck.

THE Society for Improvement of Public Health in Utrecht, offers a prize of 100 gulden for the best work on the liquid manure of stables, giving (1) an accurate account of the literature of the subject; (2) a description of original experiments on the means of obtaining from horse urine diluted with water, products which, either as manure or as a chemical preparation, may be brought into commerce in comparatively large quantities; (3) full numerical tables on quantitative chemical analyses made; (4) a thorough treatment of the financial side of the question. The memoirs may be written in Dutch, German, French, or English, and are to be sent, with sealed envelope and motto, to Prof. Dr. Th. Mac Gillvory, Director of the Veterinary School in Utrecht, before September, 1878.

THE very interesting discoveries in prehistoric archæology made by M. Kibalchich at Kief, were the subject of his last communication at the Russian Archæological Society. The numerous caves in the limestone on the banks of the Dnieper seem to have been a favourite haunt of men, even during the first ten centuries of our era. Very important objects have been found in these caves dating from the time of the introduction of Christianity in Russia, showing a remarkable mixture of articles used in Pagan and in Christian worship, establishing a link between Christian and Indian religious symbols. We notice especially those caves at Kief which date from the earliest stone period. They are very long, sinuous, but narrow, and contain great quantities of the plainest stone weapons and stone pearls, together with burned bones of various animals. Some facts lead us to infer the existence of lake-dwellings in the vicinity. Close to these oldest dwelling-places there exists a profusion of conic mounds of boulders and *koorganes* (high mounds of earth), or burial-places, coming from times anterior to the introduction of Christianity in Russia. They contain skeletons, often without skulls, which are buried separately, and a variety of weapons and utensils. The number of such burial-places at Kief and in its neighbourhood is very large. One cemetery of that epoch occupies twenty-three acres in the Fundukley Street, without reckoning the numerous "kitchen-mounds." The objects excavated by M. Kibalchich will form, it is hoped, the nucleus of an archæological museum to be opened at Kief. The excavations are to be continued.

THE *Panama Star and Herald* of the 21st ult., states that the destructive tidal wave experienced at Callao and the ports to the north of that place extended as far south as the northern boundary of Chili, but how much further south was not known, as the telegraph communication had been interrupted. The almost complete destruction was reported of Antofagasta, Iquique, Arica, Tambo do Moro, Pabellon de Pica, and Ilo. Severe shocks of earthquake were felt, but they caused little damage. The destruction of life and property was caused by the frightful upheaval and ingress of the sea. At Arica the sea washed over the town to the hill at the back of the church and destroyed much valuable property. The wreck of the United States steamer *Wateree*, carried inland a couple of miles by the tidal wave of 1868, was again floated, and carried a mile or two further up the coast. The sea in some places rose over sixty feet, and the destruction of life and property is believed to have been enormous.

THE final report of the Sub-Wealden Exploration has just been issued by Mr. H. Willett. He reports that the depth attained on December 21 last year was 1,823 feet, and on April 12, 1,905 feet. On the last-mentioned date a letter was sent by the Diamond Boring Company, stating they used the best endeavours to reach a depth of 2,000 feet and had failed, owing to the want of lining permitting the hole to fall in on the rods and jambing them. Mr. Willett writes:—"The Sub-Wealden exploration is, therefore, brought to a close, and has proved conclusively that in the lowest part of the Wealden area no palæozoic rocks exist within 1,900 feet of the surface. That the search was justified, and that the scientific deductions of Prof. Prestwich, F.R.S., were entitled to the highest consideration may be found in the fact that palæozoic rocks of the Devonian period have been discovered (containing spiriferæ) in the boring made at the brewery of Sir Henry Meux and Co., at the corner of Tottenham-Court Road London." Mr. Willett adds: "The search should undoubtedly be further promoted in the valley of the Thames and at other points in the south-east of England."

M. FERDINANDO TOMMASI has recently constructed a "thermodynamic motor," in which work is done by the mere dilatation of a liquid (oil) without change of state.

MR. SAMUEL HIGHLEY writes us:—"In connection with Mr. Atkinson's letter as to the phenomena connected with Japanese mirrors, and the question as to their method of manufacture, a few years ago Prof. Pepper exhibited the reflected figure formed by these specula when illuminated by a beam of oxyhydrogen light upon the screen at the Polytechnic Institution. In his 'Cyclopædic Science' the question of the method of the production of such mirrors is fully discussed. During the time the Japanese mirrors were being exhibited at the Polytechnic, an English brass-worker tried to solve the problem and apparently discovered the secret of the Japanese makers. He found that taking ordinary brass and stamping upon its surface with any suitable die, not once, but three times in succession, upon exactly the same spot, grinding down and polishing between each act of stamping, a molecular difference was established between the stamped and unstamped parts, so that images of the pattern could be reflected from the finally-polished surface, just as with the Japanese specula, though no difference of surface could be detected by the eye. One operation did not produce this result. Mr. James Princeps published an account of his investigations on this subject in the *Journal* of the Asiatic Society, vol. i., p. 242. He gives as the result of his anylysis of the Japanese alloy, copper, 80 parts; tin, 20—100; with no traces of silver or arsenic, but a slight indication of zinc. He supposed that the phenomena resulted from difference of *density* produced by means of stamping, and that the thinnest parts, from being the hardest, should give the stronger reflection."

IN the last number of the *Transactions* of the Institution of Engineers and Shipbuilders in Scotland is a paper read April 24, by Sir Wm. Thomson, on Compass Adjustment on the Clyde, the aim of the paper being to show that the Clyde is pre-eminently suitable for the adjustment of the compasses of ships under way.

THE ninth annual report on the noxious, beneficial, and other insects of the State of Missouri, by Chas. V. Riley, the State Entomologist, contains descriptions (with woodcuts) of the following insects:—The gooseberry span-worm (*Eufitchia ribearia*, Fitch), the imported currant worm (*Nematus ventricosus*, Klug.), the native currant worm (*Pristiphora grossulariæ*, Walsh), the strawberry worm (*Emphytus maculatus*, Norton), Abbot's white pine worm (*Lophyrus abbotii*, Leach), and Le Conte's pine worm (*Lophyrus le contei*, Fitch). There is an account of the progress of the Colorado beetle, the army worm, the wheat-head army worm, and the Rocky Mountain locust.

MUSCULAR contraction, it is known, is always accompanied with electric phenomena; the difference of electric potential between two points of a muscle, undergoes a diminution, which, according to Bernstein, precedes by about $\frac{1}{100}$ of a second, the contraction of the muscle. This *electric variation* has been observed on various muscles, and in particular on the heart (by Du Bois Reymond and Kühne), and recently M. Marey has represented it graphically by photographing the indications of a Lippmann capillary electrometer. We learn from the *Journal de Physique*, that M. De la Roche has tried the experiment on the heart of a living man. Two points of the epidermis of the chest were connected with the poles of a capillary electrometer, by means of electrodes, formed each of a bar of amalgamated zinc, with a plug of muslin at its lower end saturated with sulphate of zinc. Held with insulating handles, the bars were applied, one with its plug opposite the point of the heart, under the left nipple, and the other to another point of the chest. The mercurial column was then seen to execute a series of very distinct periodical pulsations synchronous with the pulse; each pulsation even marked the double movement of the heart (of the auricles and ventricles). The amplitude corresponded to about $\frac{1}{1000}$ Daniell.

WE have received from Perthes of Gotha a special map of Eastern Turkey, by Dr. Petermann, so full of details that for the war operations on and beyond the Danube, should the Russians succeed in crossing, we know of no better.

A RUSSIAN work, by M. Bogolubsky, on Gold and Gold Mining in Russia, is worthy of notice. It contains very interesting information upon that industry in Russia and Siberia. We observe that the area of gold mines occupies in the Russian empire about 2,100,000 square miles, and now yields yearly about 80,000 lbs. of gold, in value upwards of 3,000,000*l.* sterling. The total amount of gold produced in Russia since 1752 has been upwards of 2,500,000 lbs.

A VERY thorough and exhaustive investigation of the Alaska region may now be expected, through the agency of Mr. E. W. Nelson, a well-known naturalist, who has lately proceeded to Norton Sound, by way of Alaska, to relieve Mr. Turner. He has been provided with the necessary outfit by the Smithsonian Institution, and will probably greatly increase the amount of our knowledge of that interesting country.

WE have received from Mr. Stanford "Botanical Tables for the Use of Junior Students," by Miss Arabella B. Buckley. There are two tables—one of some common terms used in describing plants, and the other a table of the chief natural orders of British plants, arranged according to Bentham and Oliver. Both tables are well arranged, and seem to us well calculated to serve the purpose for which they are intended.

M. MEGUIN has lately been making important researches on Acarians, and on that strange asexual form called Hypopes, a form which is not absolutely necessary for reproduction, but which seems to occur under certain biological conditions, for the indefinite conservation of the species. In the aërial reservoirs of birds, especially Gallinaceæ, there breeds an inoffensive species, which M. Meguin calls *Kytodites glaber*, which sends colonies even into the bronchial branches, and into the marrowless bones of the limbs in communication with the air vessels in birds. Another harmless acarian is found in the cellular tissue of birds living and dying there, and persisting after death, surrounded by a calcareous tubercle. A third species, which lives normally between the barbs of the feathers, produces at the time of moulting, and in the skin of the birds, especially domestic and wild pigeons, a hypopial vermiform nymph. Without this precaution of nature, the species would be annihilated, by reason of the fall of the feathers in the moulting season.

THE additions to the Zoological Society's Gardens during the past week include a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, presented by Mrs. Cleaver; a Common Buzzard (*Buteo vulgaris*), European, presented by Mr. F. Buckland; a Smooth Snake (*Coronella lævis*) from Hampshire, presented by Lord Lilford, F.Z.S.; three Crested Guinea Fowls (*Numida cristata*), two Vulturine Guinea Fowls (*Numida vulturina*) from East Africa, an Imperial Eagle (*Aquila imperialis*) from Turkey, deposited; four Summer Ducks (*Aix sponsa*), bred in the Gardens.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—In a Convocation held June 5, the decree authorising the expenditure of 7,000*l.* on the construction and fittings of new chemical laboratories at the University Museum, to which we referred p. 94, was introduced by Prof. H. Smith, and carried on a division by 64 against 42.

A second proposal to grant a sum of 2,400*l.* for additions to the University Observatory was carried on a division by 46 placets to 27 non-placets.

The Trustees of the Johnson Memorial Prize for the encouragement of the study of astronomy propose the following subject for an essay:—"The History of the Successive Stages of our Knowledge of Nebulæ, Nebulous Stars, and Star-Clusters from the Time of Sir Wm. Herschel." The prize is a gold medal of the value of ten guineas, with what remains of the dividends of four years on 338*l.*, reduced annuities, after deducting cost for medals, and other expenses. The essays must be sent to the Registrar of the University on or before March 31, 1879, under the usual conditions.

CAMBRIDGE.—A curatorship in the Department of Zoology at the Museum of the University of Cambridge has just been established by the Senate, to which Mr. J. F. Bullar, B.A., of Trinity College, has been appointed. Mr. Bullar graduated in the first class of the Natural Sciences Tripos of 1875, and has been twice nominated by the University to study at the Zoological Station at Naples, where he is at present working.

The various special examinations for the Ordinary B.A. Degree were held on Friday and Saturday week, when the total number of candidates was 204, while at the corresponding period of 1876 the number was 190. Candidates can select one of the following subjects for this final examination, viz., Theology, Law, Modern History, Natural Sciences, Moral Sciences, Mechanism, and Applied Science. The number in each branch of study is as follows:—Theology, 95, Law, 31, Political Economy, 29; Modern History, 24; Natural Sciences, 21—viz., 13 in Chemistry, 5 in Botany, 2 in Zoology, 1 in Geology. In Mechanism and Applied Science there are four candidates.

Mr. William Napier Shaw, B.A., has been elected a fellow of Emmanuel College. He graduated as 16th Wrangler in the Mathematical Tripos of 1876, and obtained a first-class in the Natural Sciences Tripos, 1877, being distinguished in physics.

LONDON.—The Council of University College have elected Mr. G. D. Thane Professor of Anatomy for two years.

DORPAT.—The Annual Report of the Dorpat University for 1876, gives the number of students at the University as 815, of whom 86 study theology, 173 jurisprudence, 121 history and philology, 363 medicine, and 72 physics and mathematics. The number of professors is 67. The library of the University numbers 138,924 volumes.

RUGBY SCHOOL NATURAL HISTORY SOCIETY.—The Report of this Society for 1876, shows that it is in a "fairly healthy condition," to use the words of the preface. A considerable proportion of the papers are by members of the Society, as are also several of the illustrations. The papers are on very varied subjects and all up to a creditable standard. The preface complains that so few members take an active part in the Society's proceedings, but, in this respect, the Society is no worse than others of much greater pretension. Still it would be to the advantage of the youthful members if the patrons and office-bearers made every effort to increase the number of actual workers. We regret that our space prevents us making special reference to any of the papers. The Botanical Section has issued a list of local plants, by H. W. Trott, the result of many years' observation; this last, we daresay, may be obtained by any one desiring it. The price is only 9*d.*

London School-Board Districts.—Mr. Stanford is preparing for the School-Board of London a series of maps of the various School-Board districts of the metropolis, which are likely to possess considerable interest. These maps are on the scale of six inches to a mile, show the various School-Board subdivisions, the positions of the schools which have been erected by the Board, and, in a different colour, of those which are under the Board's inspection. We have seen the sheet of the Hackney district, and no better evidence could be produced of the thoroughly good work done by the Board since its institution.

SCIENTIFIC SERIALS

Memorie della Società degli Spettroscopisti Italiani, January.—Note from Prof. Draper on photographing the spectra of Venus and α Lyræ; a 28-inch reflector and a 12-inch refractor are the instruments used, and an exposure of from ten to twenty minutes. In the photograph of the spectrum of α Lyræ bands or broad lines appear in the ultra-violet region totally different to anything in the solar spectrum.

February.—Letter on the comet Borelly, 1877, Brorsen-Bruhns, 1857, and the eclipse of the moon of February 27, 1877. The spectra of the first appears, according to him, to consist of some carbon compound.—Tables of statistics of protuberances and spots observed at Rome in the months of January and February, 1877.—List of positions on the solar limb in which the vapour of magnesium was observed from February 20, 1876, to July 4 of the same year.—In the appendix to this number appears an article explaining the construction of the several different forms of aneroid barometers.

March.—List of positions on the solar limb in which the vapour of magnesium was observed from July to November, 1876, by Prof. Tacchini, and a table for the year showing the frequency of visibility of the *b*-line and 1,474-line, from which it appears that the latter line is more frequently visible than the former. Table of positions and size of protuberances observed at Rome in 1876, by Father Secchi.—Some observations of the zodiacal light, by Prof. A. Serpieri.—Note by Prof. Tacchini on Mr. Le Verrier's researches on the intra-Mercurial planet.—Drawings of chromosphere for September and October, 1875, made at Rome and Palermo.

April.—Spots and facula observed spectroscopically and directly at Palermo in 1876. This paper consists of the daily notes of observations of the chromosphere for last year.—Table of spots and faculæ observed in February and March, 1877, by Prof. Tacchini.—Drawings of the chromosphere for October, November, and December, 1875, by Secchi, Ferrari, and Tacchini, observed at Rome and Palermo.

Journal de Physique, April.—On the cause of the motion in the radiometer, by M. Gaffie.—On the capillary theory of Gauss and its extension to the capillary properties of liquid lines, by M. Lippmann.—New electric lamp, by M. Jabloschkoff.—On the quadrant electrometer of Sir W. Thomson, by M. Benoit.—Complement to the theory of the microscope and the dark chamber, by M. Neyreneuf.—Experiments of static electricity, by M. Grisson.

May. — On the observation of the infra-red part of the solar spectrum by means of the effects of phosphorescence, by M. Edm. Becquerel.—Determination of the polar distance in magnets, by M. Benoit.—Electric variation produced by contraction of the heart in the living man, by M. De la Roche.—On a new industrial application of heat, called the thermodynamic motor, by M. Ferd. Tommasi.—On the absorbent power of moist air, by M. Hoorweg.—On refrigerating mixtures of snow and sulphuric acid, by M. Pfaundler.

Morphologisches Jahrbuch, vol. iii. Part 1.—Oscar Hertwig, contributions on the formation, fertilisation, and cleavage of the animal ovum, part second (*Hæmopis*, *Nephelis*, *Rana temporaria*, and *R. esculenta*), 86 pages, 5 plates.—A. Rauber, the fixation of long bones in joints, and the form of the bones.—W. Moldenhauer, the development of the middle and outer ear, 56 pages, 4 plates.

Reale Istituto Lombardo di Scienze e Lettere, Rendiconti, vol. x. Fasc. vii.—Two new mycetes parasitic on vines, by M. Cattaneo.—On a cause little estimated in the pathogenesis of some female diseases, by M. de Giovanni.—The molecular velocity of gas and the corresponding velocity of sound, by M. Brusotti.

SOCIETIES AND ACADEMIES

London

Chemical Society, June 7.—Dr. Gladstone in the chair.—The following papers were read:—On the gases inclosed in lignite coal and mineral resin from Bovey Heathfield, by J. W. Thomas. Four samples were examined, two of which contained much hydrated oxide of iron in the cleavages. The gases consisted chiefly of carbonic acid, carbonic oxide, nitrogen, and sulphuretted hydrogen. In one case sulphur sublimed off in yellow crystals; organo-sulphur compounds, mercaptan, sulphide of allyl, &c., were also present in the gases. The lignites resemble cannel coal more than any other of the true coals as regards the occluded gases, but are far less stable, decomposing, *in vacuo*, below 200° C., whilst the true coals resist a temperature of 300° C. It seems probable that the iron pyrites of true coal have derived their sulphur from that existing in organic combination in the plants from which coal is produced.—On apparatus for gas analysis, by Dr. Frankland. The author proposes to substitute for the india-rubber cork, which has several disadvantages, at the bottom of the water-cylinder, a cast-iron base through which the two glass tubes pass, and are firmly clamped by a wooden clamp; the latter is screwed to the cast-iron base. The most important improvement is, however, the removal of the steel clamps which connect the laboratory and measuring tubes. These are replaced by a glass cup at the top of the measuring tube into which fits the drawn-out end of the laboratory tube, covered with thin sheet-india-rubber; this flexible joint, when wetted and covered with mercury, is quite air-tight.—On narcotine, cotarnine, and hydrocotarnine, Part V., by Dr. Wright. The preparation of bromhydrocotarnine hydrobromide, bromocotarnine hydrobromide, and tribromhydrocotarnine hydrobromide is described; the second of these bodies, when heated to 200° splits into a new base, tarconine, and a large amount of an indigo-blue substance; the latter body is very insoluble, but dissolves in strong sulphuric acid, forming a magnificent intense purplish solution. Bromocotarnin crystallises in fine scarlet crystals. Noropianic acid and other substances were also prepared and their properties examined.—On otto of limes, by C. H. Piesse and Dr. Wright. A terpene-like body boiling at 176° C. was obtained which yielded but little cymene. The residue in the retort, after standing two to three months, formed a quantity of crystals. These crystals were investigated and their composition determined.—On primary normal heptyl alcohol and some of its derivatives, by C. F. Cross. Pure œnanthol was prepared with a specific gravity of 0·823 at 16° C. Pure heptyl alcohol is colourless, has an agreeable odour, sp. gr. at 0° 0·833, boils at 175°. Heptyl chloride, bromide, iodide, acetate, and œnanthylate were prepared and examined; their boiling-points closely agree with those calculated by Schorlemmer.—On the transformation of aurin into rosanilin, by Messrs. Dale and Schorlemmer. The authors find the spectra of the hydrochlorides of their new base, and rosaniline quite identical; they have also prepared from their base Hofmann's violet, aniline blue, and aniline green.

Geological Society, May 23.—Prof. P. Martin Duncan, F.R.S., president, in the chair.—Richard George Coke, Robert Slater, and William Swanston were elected fellows of the Society.—The president read a letter from Mr. C. J. Lambert, announcing that he had allotted the sum of 500*l.* to the Geological Society out of the 25,000*l.* left by his father for distribution. The president further announced that the sum of 500*l.* had already been paid to the Society, and would be invested for its benefit.—The following communications were read;—Remarks on the coal-bearing deposits near Erekli, the ancient Heraclea, Pontus Bithynia, by Rear-Admiral T. A. B. Spratt, C.B., F.R.S.

—On the structure and affinities of the genus *Siphonia*, by W. J. Sollas, F.G.S. This paper contained, first, a full account of the history of the genus *Siphonia*, including a complete list of its described species, and, next, a description of its general and minute structure. Its skeletal network was shown to consist of spicular elements belonging to the Lithistid type of sponges, and most closely allied in generic details to the recent form *Discodermia polydiscus*. Not only in this character but in every other, *Siphonia* was shown to approach *Discodermia* so closely as to be almost identical with it. The mineral replacements which have affected the siliceous skeleton of *Siphonia* were then considered. The paper concluded with a systematic description of the genus.—On the serpentine and associated rocks of the Lizard district, by Rev. T. G. Bonney, F.G.S., fellow and late tutor of St. John's College, Cambridge. The author stated that considerable doubt appeared still to exist as to the true relations of the lizard serpentine and the associated hornblende schists, and as to the origin of the serpentine. He had carefully examined all the junctions accessible on the Cornish coast (inland they are generally obscured). Some of them are concealed by *débris*, &c., but the majority prove beyond doubt that the serpentine is intrusive. Further, almost everywhere large fragments of hornblende schist are caught up and included in the serpentine. Besides the serpentine there is a large mass of gabbro at Crousa Down, and many dykes and veins along the east coast almost to the extremity of the serpentine region. At Coverack Cove, near the above mass, are gabbros of two ages, the older much resembling a kind of troktolite. On microscopic examination it proves to be chiefly plagioclase felspar, augitic minerals (including diallage), and olivine partially converted into serpentine. There is a red and a green variety. The newer, a coarser variety, appears to be of the same age as the other veins on the coast, and connected with the main mass. Some remarkable changes have taken place in this also. In certain places it exhibits a separation of its mineral constituents, causing it to resemble a foliated rock. This is proved to be due to pressure at right angles to the structure. The minerals also are often changed. The felspar is replaced by a white granular mineral resembling saussurite; the diallage (which occurs sometimes in very large crystals) is often partially, or even wholly, converted into rather minute crystalline hornblende. In these specimens there is no olivine to be distinguished. The great mass, however, is rich in olivine, yet a weathered specimen from it, resembling in aspect the gabbro of the veins, does not show olivine. Hence the author believes that in certain cases the olivine, instead of being converted into serpentine, aids in forming the hornblende. Further, there are dykes and veins over the same area of a dark trap. Some of these are augitic, others hornblendic. The author believes that at any rate in certain of these the hornblende is of secondary formation. On the west coast are veins of granite; those on the east coast, said to be granite, prove, on careful examination, to be altered rock, remarkably like granite veins, but not really such. In discussing the origin of the serpentine the author called attention to a structure commonly seen, which appeared to be a true "fluidal structure." He then described the result of microscopic examination of many specimens of the lizard and some other serpentines. Commencing with slightly altered lherzolite (from the Ariége), he traced the change through the older gabbro of Coverack to the serpentine rock of that place, which contains a large quantity of unaltered olivine; and so to other serpentines in which the olivine is quite replaced by the mineral serpentine. He described also the mode of the change. The other minerals found in the serpentine rock are enstatite, varieties of augite, and occasionally a fair quantity of picotite, with, of course, oxides of iron. Hence he concluded that, as had been already shown as regards some other serpentines, that of the lizard was the result of the hydrous alteration of an olivine rock, such as lherzolite.—On certain ancient devitrified pitchstones and perlites from the Lower Silurian district of Shropshire, by S. Allport, F.G.S.

Physical Society, June 9.—Prof. G. C. Foster, president, in the chair.—The following candidates were elected members of the Society:—Mr. W. H. Northcott and Mr. L. J. Whalley.—Mr. S. P. Thompson read a paper on interference fringes within the Nicol prism. After referring to the original paper by the inventor in 1828, in which this phenomenon was referred to, he gave a general description of it prior to explaining the cause. If the "field" of a Nicol be explored by the eye it will be seen to be bordered on one side by a margin of violet-blue light, and on the other, when the light passes obliquely through the prism, by an orange band within which lie a series of coloured fringes; these latter are very clearly seen with monochromatic light, when a second set, within the blue band, also appears. The author showed that these two sets are due to interference taking place within the film of balsam at the critical angle of total reflexion for ordinary and extraordinary rays respectively; they are therefore analogous to the interference bands in a thin film, placed beneath a prism of a more highly refracting substance and occurring just within the limit of total internal reflection, as first observed by Sir W. Herschel. At the conclusion of the scientific business of the Society, a special general meeting was held.

Royal Microscopical Society, June 6.—Dr. Robt. Braithwaite, vice-president, in the chair.—Six new fellows were elected, and M. L'Abbé Renard was elected an honorary fellow of the Society.—A paper by the Rev. J. Delsaulx on the thermodynamic origin of the Brownian motion was read by the secretary, and described the observations of the author with regard to the motion of fluid in rock cavities and molecular motion generally, with a view to establish the theory that it was due to the action of temperature. The observations had been suggested by the study of Crookes's radiometer.—A letter from Mr. H. C. Sorby on the subject was also read to the meeting, and Mr. Hartley described his experiments which led to the same conclusions. The meeting was then adjourned until October.

EDINBURGH

Royal Society, June 4.—Prof. Kelland in the chair.—Sir C. Wyville Thomson read a paper on the structure and relations of the genus Holypus.—Mr. Alexander Buchan, M.A., secretary to the Scottish Meteorological Society, communicated the second part of his investigations of the diurnal oscillations of the barometer. He stated that the summer months of the northern hemisphere as indicated by the barometer were May, June, and July, the winter months being November, December, and January, both corresponding with the sun's declination. He has now results of the daily barometric readings from upwards of 110 stations at different parts of the earth's surface. His investigations showed that a long-continued series of observations was absolutely necessary to show the peculiarities of the barometric curve. For instance, three years' observations gave in the case of Great Britain only the broadest characteristics. He found that no theory as yet propounded would explain the diurnal oscillations of the barometer, and that as more facts were obtained the difficulty of framing a satisfactory theory was greatly increased.—In his paper on the air dissolved in sea-water, Mr. J. Y. Buchanan stated that the result of the analysis he has as yet made of the specimens of the air dissolved in sea-water which were collected in the recent *Challenger* expedition, tends to show that as regards surface-water least air was dissolved where the temperature was highest, *e.g.*, near the equator, and most where the temperature was least, as in the polar sea. As regards the percentage of oxygen present at different depths it diminishes from the surface to a depth of 300 fathoms and increases from that point to lower depths. Prof. Tait communicated two laboratory notes; (1) Two plates either of the same or different metals were placed very close to one another but insulated and one of them raised in temperature: a difference of potential was produced, which was capable of producing a current measurable by a sensitive galvanometer. (2) He had seen in Dr. Blair's "Scientific Aphorisms" a hypothesis to account for gravitation very like that of Lesage's ultramundane corpuscles, which Blair stated was suggested to him by Newton's works, and Prof. Tait was anxious to ascertain if any part of it was due to Lesage or was entirely original. Prof. Tait laid on the table an algebraic identity which could be used to sum various series.

DUBLIN

Royal Society, May 21.—Prof. J. Emerson Reynolds, M.D., in the chair.—The following papers were read:—On some measurements of the polarisation of light coming from the moon and from the planet Venus, by Earl Rosse, F.R.S. Lord Rosse gave the results at which he had already arrived from a very large number of observations on the polarisation of light from particular parts of the moon's surface made in the years 1872, 1873, 1874, and 1875, and which are still in progress. The observations indicate that the polarisation of the light coming from the plains is greater than that of the light coming from the mountainous regions.—Notes on the crustacea of Ireland, by Mr. William Andrews. An account of the rarer species found on the Irish coasts.—On the substitution of an alkaline base in chlorimetry, by Mr. J. Smyth, F.C.S. The author treats bleaching lime with an alkaline carbonate, and thus obtains the chlorine in a more convenient form for tetration.—On a specimen of quartz with a pearly lustre, by Mr. R. J. Moss, F.C.S. The faces of the pyramidal crystals, of which the specimens consist, possess a laminated structure, which causes a very perfect reflection of light. The laminæ consist of quartz exclusively. Mr. Moss concludes that this novel variety of quartz must be regarded as a modification of cup-quartz, the laminated structure being the result of periodic interruptions in the growth of the crystals.—An account of recent attempts to obtain water by deep wells under London, by Prof. Hull, F.R.S. The author referred principally to the scientific results of the recent boring at Messrs. Meux's brewery.—On a remarkable action of light on certain organo-metallic bodies, by Prof. J. Emerson Reynolds, M.D. The author described a number of experiments with mercuric ethide and its homologues, and showed that the pure ethide when sealed up in a tube and exposed to light for some months is wholly decomposed into mercury and nearly pure liquid hydrocarbon.—On the penetration of heat across Crookes's layer, by Mr. G. Johnstone Stoney, F.R.S. The author described the way in which heat is transferred across the vacuous spaces in Crookes's radiometers. He traced the laws under which this transfer of heat takes place, and showed that they are different from the already-known laws of radiation, convection, conduction, and contact. Mr. Stoney suggests that the newly-discovered mode of conveying heat should be called penetration. He showed that a large body of observations were made more than thirty years ago by De La Provostaye and De Sains upon heat conveyed in this way, but without its theory being understood. Unexplained observations made by Dulong and Petit, Grove, and other physicists also admitted of interpretation by the newly-discovered laws. Mr. Geo. M. Fitzgerald was the first to observe that the important results obtained by De La Provostaye and De Sains were due to this cause, and it was by him that they were brought under Mr. Stoney's notice.

PARIS

Academy of Sciences, June 4.—M. Peligot in the chair.—The following papers were read:—Preliminary reply to observations by M. Mouchez on the "Nouvelle Navigation," by M. Villarceau.—On the densities of vapour, by M. H. Sainte Claire Deville.—Researches on the law of Avogadro, by M. Wurtz.—On the atomic notation; reply to M. Berthelot by M. Wurtz.—Atoms and equivalents, reply to M. Wurtz, by M. Berthelot.—Reply to M. Fizeau, by M. Berthelot.—On the parallel striæ frequently presented by the surface of fragments of diamonds of the carbonado variety, and on their imitation by means of artificial friction, by M. Daubrée. These striæ seem to show that fragments, now sparsely scattered, must have been at one time, before they were thrust up to the surface, in contact with one another and exerting mutual pressure.—On the regeneration of the red blood corpuscles in frogs after considerable hæmorrhage, by M. Vulpian. The corpuscles result from evolution of small colourless spheroidal nucleated cells, which become discoid, then oval, and a little before they reach the size of red corpuscles, they produce hæmoglobin.—M. Gervais announced the receipt of a fresh batch of natural history specimens (chiefly birds and insects) from the Japanese Government. He remarked on some skeletons of Cetaceans.—On the true number of elementary co-variants of a system of two binary biquadratic forms, by Mr. Sylvester.—On the spectrum of Winnecke's comet, by P. Secchi. His experiments lead him to think it similar to that of other comets, and to have for base carbonic oxide.—On some metallic seleniurets and tellurets, by M. Margottet.—Study on oxides of iron, by M. Moissan. He believes the pyrophorus of magnum is formed in great part of pyrophoric protoxide of iron.—On the preparation and composition of emetine, by MM. Lefort and Wurtz. Emetine does not form basic salts, at least in the conditions in which it is ordinarily obtained.—Reproduction of albite, by M. Hautefeuille. Albite may be easily had by heating to a dark red a mixture of tungstic acid and a very alkaline silico-aluminate of soda.—Strontian; its diffusion in mineral and organic nature at the present epoch and in the series of geological times; consequences relative to saliferous mineral waters, by M. Dienlafait. *Inter alia*, of eight hundred springs coming under the designation of saliferous waters, there are only forty-four in which strontian has not been found.—On the affinity of blood corpuscles for carbonic acid, by MM. Mathieu and Urbain. A reply to M. Fredericq, who holds that the CO_2 is expanded in the plasma, not fixed by the globules. The authors cite figures to show that the globules of horse blood can fix about twice as much CO_2 as the serum. Some substances (as ether) increase the absorbing power for O and CO_2; others (as alum) diminish it.—Experimental studies on regeneration of cartilaginous and osseous tissues. by M. Peyrand.—Historical remarks on the theory of movement of one or several bodies, of constant or variable forms, in an incompressible fluid (continued), by M. Bjerknes.—Comparative study of experiments by day and by night made by MM. Perrier and Bassot, by M. Perrier. The mean errors of an isolated observation for the day and the night, are of the same order of magnitude. The errors arising from the atmosphere are compensated better in night than in day observations.—Siphon barometers whose indications are not influenced by variations of temperature, by M. Gautier.—Observations on a note of M. Yvon on nitrates of bismuth, by M. Ditte.—Combinations of quercite with butyric and acetic acids, by M. Prunier.—Investigation of salicylic acid in wines and urine, by M. Robinet.—Observations on M. Bert's experiments on anthracic disease, by M. Davaine.—Experiments showing that there is not in toxical putrefied blood, liquid or solid virus without organic ferments, by M. Feltz.—On various specimens of Brazilian clay and coal, by M. Guignet.—Female flowers of Cordaites, by M. Renault.—Result of geological explorations in 1875–76 in connection with the channel railway, by MM. Potier and De Lapparent.—Researches on the vitality of spermatozoids of trout, by M. Heneguy. They resist the action of alcohol and anæsthetics in such quantity as would kill *e.g.* infusoria.—Determinations of ammonia in the air and the meteoric water of Montsouris, by M. Levy.

GENEVA

Physical and Natural History Society, April 5.—Prof. Forel gave an account of his observations on the transparency of the waters of the Lake of Geneva. This transparency, much greater in winter than in summer, is modified very suddenly at the two periods of its annual variation. The changes of temperature are insufficient to account for the phenomenon. On filtering the water of the lake, M. Forel found in suspension fine particles almost exclusively organic, and proved that their proportion increases with the number of strata at different densities.—M. Théod. Turrettini described the discovery which he had made of a freezing mixture of chloride of magnesium and snow, which gave a temperature of $-34°$ C.—Prof. Wartmann announced a series of researches undertaken by him which enabled him to understand the extra-polar-derivation of currents traversing mercurial conductors.—M. Duby spoke of the capture of certain insects by the *Petunia* and the part played by the sticky glands in this respect.

ROME

R. Accademia dei Lincei, April 8.—Influence of nicotine on the animal organism, by M. Corso. The increase of blood pressure may be obtained without preserving intact the vasomotor centre. It is not due to reflex action. The first effect of nicotine on the pupil is dilatation.—The president (M. Sella) read a necrological notice on Prof. Panceri.—Considerations on specific heat, by M. Cerruti.—The Roman Tuscia and the Tolfa (continued), by M. Ponza.—A memoir by M. Bagnis was presented, containing illustrations of a first hundred cryptogams from the Agro Romano, studied by him.—M. Volpicelli replied to M. Cantoni's recent objections to the electrostatic doctrine of Melloni.

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