THURSDAY, OCTOBER 10, 1878

THE ELECTRIC ARC AMONG THE GAS SHARES

IN Wednesday's *Daily News* we read as follows :—

"Gas shares have been subjected to considerable depreciation owing to the publication of some statements stated to have been made at a meeting in Birmingham. The folly which shareholders exhibit in sacrificing their holdings on the slightest alarm cannot be too strongly deprecated." Among the changes recorded in the official list we find Imperial Continental fell 7; Gas Light and Coke, H issue, 7½; ditto, Ordinary, 5, and so on, and so on.

There is little doubt that some excellent fooling (if fun were the writer's only object) on the part of the *New York Sun* is among the causes of this wonderful exhibition of alarm on the part of gas shareholders. But we doubt the fun. Any man quoting the remark that the sun shines on the evil and the good, would, if he combined candour with perfect knowledge, make an exception in favour of the *New York Sun*. It has a decided predilection for the evil. Nor is this all. Mr. Edison, besides being the most wonderful inventive genius of the age, is one of those rare beings, an American humourist. Indeed, if we are to believe the Western newspapers, those universities and stock raisers beyond Chicago who hastened, after the manner of our own Oxford, to enshrine his name on the rolls of their illustrious men and beasts, did so not because he had benefited mankind or lived laborious days in his laboratory at Menlo Park, but because a word dropped by him had given rise to the rumour that he had just put the finishing-touch to a swearing-machine.

Here is the *New York Sun's* story;—

"Mr. Edison says that he has discovered how to make electricity a cheap and practicable substitute for illuminating gas. Many scientific men have worked assiduously in that direction, but with little success. A powerful electric light was the result of these experiments, but the problem of its division into many small lights was a puzzler. Gramme, Siemens, Brush, Wallace, and others, produced at most ten lights from a single machine, but a single one of them was found to be impracticable for lighting aught save large foundries, mills, and workshops. It has been reserved for Mr. Edison to solve the difficult problem desired. This, he says, he has done within a few days. His experience with the telephone, however, has taught him to be cautious, and he is exerting himself to protect the new scientific marvel, which, he says, will make the use of gas for illumination a thing of the past.

"While on a visit to William Wallace, the electrical machine manufacturer in Ansonia, Connecticut, he was shown the lately-perfected dynamo-electric machine for transmitting power by electricity. When power is applied to this machine it will not only reproduce it but will turn it into light. Although said by Edison to be more powerful than any other machine of the kind known, it will divide the light of the electricity produced into but ten separate lights. These being equal in power to 4,000 candles, their impracticability for general purposes is apparent. Each of these lights is in a substantial metal frame, capable of holding in a horizontal position two carbon plates, each 12 in. long, 2½ in. wide, and ½ in. thick. The upper and lower parts of the frame are insulated from each other, and one of the conducting wires is connected with each carbon. In the centre and above the upper carbon, is an electro-magnet in the circuit with an armature, by means of which the upper carbon is separated from the lower as far as desired. Wires from the source of electricity are placed in the binding posts. The carbons being brought together the circuit is closed, the electro-magnet acts, raising and lowering the upper carbon enough to give a bright light. The light moves towards the opposite end from which it starts, then changes and goes back, always moving towards the place where the carbons are nearest together. If from any cause the light goes out the circuit is broken and the electric magnet ceases to act. Instantly the upper magnet falls the circuit is closed, it relights, and separates the carbon again.

"Edison on returning home after his visit to Ansonia studied and experimented with electric lights. On Friday last his efforts were crowned with success, and the project that has filled the minds of many scientific men for years was developed.

"'I have it now!' he said, on Saturday, while vigorously turning the handle of a Ritchie inductive coil in his laboratory at Menlo Park, 'and, singularly enough, I have obtained it through an entirely different process than that from which scientific men have ever sought to secure it. They have all been working in the same groove, and when it is known how I have accomplished my object, everybody will wonder why they have never thought of it, it is so simple. When ten lights have been produced by a single electric machine, it has been thought to be a great triumph of scientific skill. With the process I have just discovered I can produce 1,000—ay, 10,000—from one machine. Indeed, the number may be said to be infinite. When the brilliancy and cheapness of the lights are made known to the public—which will be in a few weeks, or just as soon as I can thoroughly protect the process—illumination by carburetted hydrogen gas will be discarded. With fifteen or twenty of these dynamo-electric machines recently perfected by Mr. Wallace, I can light the entire lower part of New York City, using a 500 horse-power engine. I purpose to establish one of these light centres in Nassau Street, whence wires can be run up town as far as the Cooper Institute, down to the Battery, and across to both rivers. These wires must be insulated, and laid in the ground in the same manner as gas-pipes. I also propose to utilise the gas-burners and chandeliers now in use. In each house I can place a light meter, whence these wires will pass through the house, tapping small metallic contrivances that may be placed over each burner. Then housekeepers may turn off their gas and send the meters back to the companies whence they came. Whenever it is desired to light a jet it will only be necessary to touch a little spring near it. No matches are required.

"'Again, the same wire that brings the light to you,' Mr. Edison continued, 'will also bring power and heat. With the power you can run an elevator, a sewing-machine, or any other mechanical contrivance that requires a motor, and by means of the heat you may cook your food. To utilise the heat it will only be necessary to have the ovens or stoves properly arranged for its reception. This can be done at trifling cost. The dynamo-electric machine, called a telemachon, and which has already been described, may be run by water or steam power at a distance. When used in a large city the machine would of necessity be run by steam power. I have computed the relative cost of the light, power, and heat generated by the electricity transmitted to the telemachon to be but a fraction of the cost where obtained in the ordinary way. By a battery or steam-power it is forty-six times cheaper, and by water-power probably 95 per cent. cheaper.'

"It has been computed that by Edison's process the same amount of light that is given by 1,000 cubic feet of the carburetted hydrogen gas now used in this way, and for which from $2.50 to $3 is paid, may be obtained for from 12 to 15 cents. Edison will soon give a public exhibition of his new invention."

So much for the *New York Sun*. Although a student of science will have little difficulty in associating the results promised with the discovery of perpetual motion, it is quite probable that Mr. Edison has actually succeeded in doing what he states he has done in his telegram: "I have just solved the problem of the sub-division of the electric light indefinitely." What we wish to point out is that it is one thing to do this and another thing to produce an electric light for ordinary house and street use. Once put the molecules of solid carbon in motion, and just because a solid is in question, the light must be excessive and the expenditure of energy must be considerable.

While it is easy to believe that the future may produce a means of illumination mid-way between the electric light and gas, it is equally easy to see that the thing is impossible without great waste, and therefore cost, with dynamo-electric machines and carbon poles. So long as carbon is employed we shall have much light which, perhaps, can be increased and steadied if various gases and pressures are tried. But streets and rooms full of such suns as these would be unbearable unless we sacrifice much of the light after we have got it. Split up the currrent in the manner so cheerfully described by the New York paper, and the carbon will refuse to flow altogether if an engine of 5,000,000 horse-power be employed instead of the modest one of 500 which is to light the south part of the island. If Mr. Edison has succeeded in replacing carbon he may have turned the flank of the difficulty to a certain extent.

Although, however, we may pity the ignorance of those who act upon such statements as those made by the imaginative *New York Sun*, gas companies may well begin to feel uneasy at the general attention which is being drawn to the electric light as a substitute for gas *if they are prepared to let things alone*. That in one form or other it is likely to be partially adopted in all large cities and at extensive public works seems most likely. It will be one of the lights of the future, but not to the excluding or superseding of gas-light.

Our own columns have repeatedly borne testimony to the success which has attended its introduction into Paris, where it is to be met with at almost every corner, and at one or more of the railway stations. The general testimony of those who are unprejudiced is that at least for wide streets, squares, and open places, its lighting effect is all that could be desired. Every Londoner is familiar with the effect of the display which the enterprising Mr. Hollingshead has placed in front of the Gaiety Theatre, and the glowing contrast presented to the miserable yellow flames of the neighbouring street-lamps; but this contrast exists because the gas is bad and dear. Mr. Hollingshead, in a letter to the *Daily News*, corrects the view of the gas companies, that the electric light must necessarily cost more to produce than gas. His own display, necessarily wasteful, costs four-fifths what gas would, and he is probably correct in saying that with proper management it need not cost more than one half. Moreover, in yesterday's *Times*, Mr. E. J. Reed refers to the case of M. E. Manchon, a large manufacturer at Rouen, who has gone to considerable expense to alter his premises to suit the electric light, and who, even with hired engine power, finds that there is an annual saving of 22·6 per cent. over gas, with infinitely better light and a wholesome atmosphere. Mr. Reed is of opinion that even if the electric light cost more than gas, its advantages are so great, that for the lighting of public places, museums, art galleries, manufactories, &c., he would advocate its general introduction. Even Madrid, one of the most backward cities in Europe, has introduced the light, one great benefit of which, especially in theatres and other much frequented places, is that the heat generated and the contamination of the air is greatly less than in the case of gas.

Let the directors of gas companies do all they can to improve their gas. They may be certain that it will never cease to be required; a considerable splitting up of the electric current is impossible, while the brilliant light that we shall always get when electricity is employed will gradually so raise the *pitch* of illumination that more gas than ever will be used.

THE MEDICAL FACULTIES

THE opening addresses of the various London medical schools always form an interesting episode in the scientific year, and this session they have been even more interesting and have attracted more attention than usual. This is especially the case with the vigorous and trenchant address (published in full in the *British Medical Journal* of October 5) of Prof. Ray Lankester. On another page we reprint a remarkable article from the *Lancet*, in which it is plainly stated that without endowment of research the progress of medicine must soon become impossible in this country; that the work of scientific investigation demands practically the whole energies of a competent man and is incompatible with the necessity of earning a living in any other direction. It is somewhat remarkable that such an article should be published simultaneously with the outspoken address of Prof. Lankester, who aimed to show that the great Universities of this country are faithless to their duty and to the end for which they were established, in not providing for the pursuit of scientific research, in so far at least as that bears on the healing art.

"The work of the medical profession—its function in the community"—he showed, "is to bring into practical use an immense mass of accurate knowledge with regard to the conditions affecting the healthy working of the human body. Accordingly, two distinct kinds of activity—one dependent on the other, one as important as the other—are to be recognised as essential to the business of the medical profession. The one consists in the accumulating of knowledge relating to the human body and to the conditions affecting its health, the sifting of false from true knowledge, the producing of new knowledge; the other consists in the application of this knowledge to particular cases of disease or danger in such a way that action may be taken, avoiding the disease or danger, or alleviating the suffering which results from them.

"There is a most mischievous notion current at the present time," Mr. Lankester went on, "that the first of these lines of work is 'theoretical,' and that the second is 'practical;' and it is not unusual to separate the 'theoretical' from the 'practical' man, and to speak of the "theoretical" and the "practical" portion of medical studies. "Such a division," Mr. Lankester truly said, "is a relic from the bad old times. Theory and practice in medicine, as elsewhere, go hand in hand. The results of scientific investigation cannot be applied in the treatment of disease by the man untrained in scientific method any more than the delicate tools of a lathe can be used by one who has not himself devised them, or a watch be mended by the aid of a treatise on watch-making."

Mr. Lankester pointed out that in the present unsatisfactory state of the multitude of medical faculties in England, dependent on the voluntary services of busy medical practitioners, medical education must necessarily be defective; and that so far as this is concerned, the wealthy Universities of Oxford and Cambridge have for long most shamefully neglected their duty. As a result of this neglect on the part of our universities, medical education in the last century was a thing almost unknown in England. Those who were desirous of qualifying themselves for carrying on the profession with anything like thoroughness, had to go to Scotland, or Paris, or Italy, where the idea of a university with its various "faculties," has all along been kept in view. The multitude, however, were content with a simple "apprenticeship" to a medical practitioner, while a few latterly took to following the hospital physician round the wards, to take note of the "great man's" receipts. "But as for instruction in physics, in chemistry, in comparative anatomy, in physiology, in the general properties and activities of living things, it had no existence in London, and was not in any way required on the part of the licensing bodies. The English universities meanwhile, which possessed rich endowments for carrying on these studies, allowed jobbery and indifference to convert their ancient medical officer into sinecures." It was from Scotland, where the torch of true university life was kept burning, that an impulse towards the establishment of better things in benighted England came and set men to work in London. The origin of University College was then referred to, it being pointed out that Government, in its caprice, denied the privilege of granting degrees to the vigorous young institution, and conferred it upon a shadowy body, to which it misapplied the title of London "University," a mere *nominis umbra*, and an utter misapplication of the venerable term. However well the London University may have carried out its anomalous duty, we share in Prof. Lankester's profound regret, that the grand old title of "University" should have been in this way completely divorced from the work of study and teaching. The result is that, in this country, not one man in a hundred, even amongst those possessing university degrees, knows what a university is. "The Universities of Oxford and Cambridge, on the one hand, have entirely departed from the old standard, and ought long since to have been checked in their career and reformed by the power which chartered and protected them in their early days; whilst the admirable body which we call the University of London has precisely the same claim to be called a university as has the Archbishop of Canterbury."

Prof. Lankester then went on to show on how wide a basis of scientific investigation and study the medical ar ought to be built; it is the outcome, the final result, of observation.

"This is the spirit," he said, "in which the great universities of Europe, with the exception of Oxford and Cambridge, have fostered the study of medicine. This is the explanation of the existence of chairs of Chemistry, of Physics, of Botany, and of Zoology, in all their Medical Faculties. Such is the nature of his work that the medical man needs instruction and training in all the great branches of physical science; and from time to time the methods of investigation, the modes of speculation and the generalisations with which he has become familiar in the course of these apparently remote studies, render him most efficient service in the attempt to ascertain and to deal with diseased states of the human body. It is thus that a thorough knowledge of the organisation of both plants and animals becomes part of the equipment required by a medical man, but it is even more directly that the progress of knowledge relative to other organisms affects knowledge relative to the human organism. The knowledge of diseased and of healthy conditions of all organisms, all knowledge of living things, including necessarily man himself, forms one compact interwoven body of science termed biology, and upon this directly the medical art is built, in it all medical practice has its foundation."

But in order that the results of scientific research may be applied to the alleviation of human suffering, there must be continued investigation in order to produce new knowledge. "The production of new knowledge," Mr. Lankester justly said, "is a most absorbing and arduous business. Men who have anything else to do except a small amount of teaching can do very little—only a bit here and there—in the production of new knowledge. Men who are earning their livelihood in the practice of a profession can do very little at it. Men who are preparing students for examination all day all the year round can do but little at it. Only men with fortunes, or men who are paid by the institutions especially founded and meant for the production of new knowledge, can be expected to do much in this way. The institutions especially founded and designed for this production of new knowledge, and richly supported by large annual grants of money in the form of salaries and stipends, are abundant on the Continent of Europe; they are the Universities. In London we have no such institution; there is no University of London in this sense of the word. The medical profession in England, though it has eleven "Faculties" in London and other "Faculties" in provincial towns, is almost totally devoid of those splendid opportunities for profound investigation—for the production of new knowledge bearing on medicine—which the appropriation of public money and ancient endowments to the payment of the Medical Faculties in Germany, for instance, provides."

It is certainly, as Mr. Lankester said, at first sight rather astonishing that we laborious, hard-headed Englishmen, the countrymen of Harvey and Darwin, should have to go to Germany for so much of our new knowledge, and that our text-books of science, instead of being provided by the richly-endowed Fellows of Oxford and Cambridge, should to a large extent bear on their title-pages the names of German professors. This surprise ceases when we are told that the German university system, consisting of twenty-one universities and 1,250 salaried professors, is carried on at an annual expense of more than 600,000*l*." At the least, half of that sum and half of the number of salaried workers are devoted to the branches of science connected with medicine. "How is it we have nothing of the kind in England? Is it impossible?" The answer is, in Mr. Lankester's words, that "the production of new knowledge cannot go on without the assistance of endowments or their equivalent. It is impossible to name a single case of a man who did not enjoy either a private fortune or an endowment, and yet has added greatly to scientific knowledge. Medicine and the sciences which she protects have most urgent need of endowment for the purpose of supporting men who shall chiefly occupy themselves in the production of new knowledge." Mr. Lankester's words will show how things are managed in Germany :—

"It is a disgrace to English civilisation that a true university — an endowed university, a university in which new knowledge is continually being produced, and in which men are trained for this work of production as the work of their lives—does not exist in London and in each one of our large cities. I can briefly tell you some of the circumstances which have prevented the foundation of such desirable institutions in this country; and I will further indicate to you what we may hope to see done in this direction in the future. But first let me give you a sketch of one of these German universities which we so much admire and envy. I advisedly select one situated in a small town—the University of Heidelberg. Heidelberg is one of the two universities of the Grand Duchy of Baden, Freiburg being the other; whilst at Carlsruhe, in the same state, is an important technical school. The town of Heidelberg numbers but 22,300 inhabitants. The university has 61 professors, and, by the last returns, 834 students. Of these, 23 professors and 106 students belong to the faculty of medicine. The government of Baden, by which the salaries of the professors are paid and their number determined, does not consider that this proportion of one professor to every five students is an excessive proportion on the side of the professorial staff. This university was founded nearly five hundred years ago (in 1386), and, like all the German universities, was remodelled and greatly improved at the beginning of this century, whilst since that time its wants have continually been provided for with ever-increasing liberality by the state government. There are now four faculties—that of Theology, that of Law, that of Medicine, and that of Philosophy. The professors are divided into the ordinary and the extraordinary. The ordinary professors receive a stipend of about 400*l*. yearly, besides their fees, which in some cases bring their incomes up to 1,000*l*. When a vacancy occurs in a professorship the state minister invites the members of the faculty in which the vacancy has occurred to name two or more individuals whom they would recommend for appointment. The faculty meets and the name of a professor in some other university is proposed. He is written to and asked whether he will come; he probably replies that he would require an increased salary and a new laboratory; very usually his terms are agreed to by the state minister on the recommendation of the faculty, and he is installed in the vacant chair. Sometimes, of course, a younger and less known man is appointed at a lower salary. As an example of the way in which these things are managed in Germany, let me give you an actual history of what recently occurred at Strassburg. I quote from an American journal. 'After the transfer of that city to Germany neither pains nor money was spared to make the university a success. For the chairs of the medical faculty rising men were selected, all of whom were known for original research, and had practically proved their ability as teachers and writers. The chair of pathological anatomy was given to von Recklinghausen, one of the most brilliant of Virchow's pupils, who vacated a similar position at Würzburg in order to accept this new position. When the chair of pathological anatomy at Vienna became vacant by the retirement of the veteran Rokitansky it was offered to von Recklinghausen, and the salary proposed was 25,000 *francs* (1,000*l*.), or about three times the usual salary of such a professorship. But the Prussian government was quite as anxious to retain Prof. von Recklinghausen as the Austrian government was to obtain him, and asked him to say what he wanted. His reply was to demand, as the condition on which he would remain, that there should be constructed a large pathological institute, in accordance with his plans, and in connection with the hospital—an institute which will cost something like 50,000*l*., and will require a change in the fortifications. His demand was acceded to, and he is hard at work now in Strassburg.'"

Prof. Lankester then describes the magnificent arrangement in Heidelberg for carrying on all kinds of research by men whose great business is to add to new knowledge, with the minimum of destruction of any kind. Here, among other well-known names, Bunsen, "the most eminent of living chemists," and Kühne, the physiologist, the author of the text-book as well known in England as in Germany, have their laboratories and class-rooms; and Gegenbaur is the head of the anatomical institute.

Other names equally great in original research in the various departments of science and other towns in Germany could be mentioned. "Berlin possesses laboratories and museums on a palatial scale, and a perfect army of investigators and students supported by State endowments. Leipzig, again, Strassburg, and Munich, are larger and more richly provided than Heidelberg. All the twenty-one German universities, the eleven Austrian, the four Swiss, and six or more Russian universities (I do not speak of those in Scandinavia, France, Holland, Belgium, and Italy), are fitted out in the same way. In all, medicine is being advanced and developed by the never-ceasing production of new knowledge."

"We, in England," as Mr. Lankester goes on to say, "benefit by this knowledge; we, in common with the rest of mankind, reap the rewards won by the activity of these noble corporations; and yet, it is neither more nor less than the fact that we Englishmen do not possess, throughout the length and breadth of our land, a single institution of our own where such work is done." In the three or four institutions where anything like original research is carried on in this country, the endowments are so inadequate as to seriously hinder anything like complete and satisfactory work. "To speak of these institutions as taking the place in this country of the vast machinery and resources of the Medical Faculties of Germany would be about as reasonable as to compare the pleasure-boats on the Lake of Geneva with the British navy."

Prof. Lankester then sketches the state of things which have come to exist at Oxford and Cambridge. These Universities were founded for the purpose of giving education in medicine as well as in theology, and endowment after endowment was made by men anxious that the Universities should fulfil their functions with efficiency;

but through intrigue these magnificent endowments have been almost entirely filched from the purposes for which they were meant, and the property which was thus consigned to the tender mercies of the Church, is now estimated to produce yearly in each University over 300,000*l*.

"For many years the faculties of law and medicine struggled on in Oxford, growing weaker and more neglected in each decade, until now, after 200 years of this usurpation, there is not a single medical student in the place. In Cambridge the story was very much the same, excepting that there the degradation of the medical faculty has never proceeded quite so far as it has at Oxford, and medical studies are now, we have some reason to hope, being resuscitated in that university by the strenuous efforts of Prof. Humphry and Dr. Michael Foster." We take a few instances from Mr. Lankester's address:—At Oxford, shortly before the destruction of its character as a university, the King, Henry the Eighth, had founded a Regius Professorship of Medicine. The office still exists, and is worth about 500*l*. a year, but the present tenant of the office gives no lectures and has no pupils. Linacre, the founder of the College of Physicians of London, Mr. Lankester tells us, "left to Merton College in Oxford (in the reign of Henry the Eighth) a piece of land, the rental of which was to pay a lecturer in medicine. Within 100 years the office was abolished, and the money converted to the private uses of the Fellows of the College. Confiding benefactors came forward last century and put down their money, in the hopes of promoting medical study in Oxford. But they did not know—and at the present day you cannot make people believe—how shameless and unprincipled were the bodies to whom they entrusted their money. Lord Lichfield bequeathed 200*l*. a year for the reading of clinical lectures in the Radcliffe Infirmary to the students in medicine of the University. The office is now held by the Regius Professor of Medicine, but no lectures are given. About the same time, Matthew Lee confided money to the care of the governing body of the cathedral house of Christ Church, for the payment of a teacher of human anatomy, and to buy subjects for his demonstrations, but no such teaching is given; the money is applied to other purposes. Dr. George Aldrich, in 1798, left 9,000*l*. for similar purposes, but, at the present day, the bequest bears no fruit for the benefit of medicine."

These are only a few instances of the scandals connected with the history and present condition of our great universities, mainly owing to their complete subjection to clerical influence. The colleges, instead of being lodging-houses for poor students, as they were intended to be, were converted into boarding-schools, into which the Fellows received the sons of the landed gentry and wealthy citizens as pupils, on condition of certain payments. To quote Prof. Lankester:—

"The fees demanded by the College-Fellows increased at last to such an extent, and the expense of residing in one of these boarding-houses became so great, that the universities entirely ceased to be popular or national institutions in function, though they were so in foundation. They became the exclusive possession of the clergy and the wealthy classes, and so they remain at the present day. Long ago, students ceased to seek the lecture-rooms of Oxford and Cambridge for the purpose of serious study or professional training. Whilst the Scotch farmer's lad can earn enough in the fields during summer to keep him during a winter's session in the University of Glasgow or Edinburgh, whilst all classes of the community contribute to form the student-world of the German Universities, Oxford and Cambridge, under the influence of clerical domination, have become simply 'finishing schools for young gentlemen' (I quote the words of Prof. Max Müller). Men of moderate means—that is to say, the majority of our fellow-countrymen—now only go to Oxford or to Cambridge with the view of sharing in the scholarships and fellowships, which are annually distributed there by competitive examination. In their whole tenour, purpose, and being, these places are as different as they possibly can be from their quondam sisters the universities of Germany."

What Prof. Lankester insists upon is the establishment of a fully-developed and amply-endowed Medical Faculty in both Cambridge and in Oxford, and, still further, the establishment of one or two such faculties in London. We are glad to think that there is an immediate prospect of a great development of the Medical Faculty at Cambridge, where already experimental physiology, human anotomy, and clinical medicine are taught and prosecuted with energy. There is indeed some prospect that in the course of years, when men with a better spirit have sway in both universities, they will be brought to fulfil all the functions for which they were originally established; and we trust that Prof. Lankester's address may act as the little leaven in the minds of all who heard or may read it, and that gradually not only professional men but the constituencies generally will wake up to a realisation of the immense benefits which are the nation's birthright, but from the enjoyment of which it has for so long been barred.

Prof. Lankester then urges that one, or at most two, medical faculties should be established under the University of London.

"In this way," he concludes, "we might have in London, each provided with ample laboratory, museum, and assistants, two professors of physiology, one of surgical anatomy, one of comparative anatomy, one of embryology, one of botany, one of pathological anatomy, one of pharmacology, one of hygiene, one of forensic medicine, two of chemistry, one of experimental physics, and others of the history and practice of medicine, of surgery, of midwifery, and of psychiatry. The maintenance of such a staff, with their laboratories and assistants, would require an endowment of 20,000*l*. a year, whilst 100,000*l*. would have to be sunk in providing the necessary buildings. This proposition appears Utopian, but all I have to say further in defence of it is this, that in Berlin, Vienna, Leipzig, and other continental cities the thing is done, and on a more costly scale than I have here suggested.

"When such medical faculties as I have sketched to you exist in Oxford and in London, England will have begun to do her duty by the great profession of medicine. Until then we are but hangers-on of foreign nations; until then we reap where we did not sow, we gather where we did not straw. Until that time I earnestly beg every man who enters on a medical career to remember that he is joining the cause of a profession deprived of its heritage, and to make it his business to reinstate medicine in her seat, and to secure the restitution of her possessions."

Prof. Lankester is not only Professor of Zoology at University College, but a Fellow of Exeter College, Oxford: so that he speaks with full knowledge, and not as an outsider, who might be accused of ignorance and of interested motives. The subject which he has thus brought prominently into notice concerns the highest welfare of this country and the place which she holds among the cultured nations of the world.

MILLER'S CHEMISTRY

Elements of Chemistry, Theoretical and Practical. By William Allen Miller, M.D., LL.D. Revised by Charles E. Groves. Part II. *Inorganic Chemistry.* (London: Longmans and Co., 1878.)

THE number of editions through which this part of the late Dr. Miller's work has passed and the high position it has attained as a Text-book of Inorganic Chemistry render the work of the reviewer almost superfluous, and we have now only to notice what improvements or additions may have been made in the present edition.

The revision of the volume just published has been entrusted to Mr. C. E. Groves, whose position as sub-editor of the Chemical Society's *Journal* renders him particularly fitted for this work, by reason of his being constantly brought into contact with communications upon the more recent discoveries in the science.

The order in which the various elements and their compounds are discussed, as we are told in the preface, remains the same as in the last edition, this order commencing with the least complex compounds and finishing with those of a more complicated nature. Hydrogen is first studied as the standard of atomic weights, densities of gases, &c., and as affording a good example of an electro-positive element; it is followed by the monatomic but electro-negative element chlorine, subsequently by oxygen as illustrating the diatomic condition, boron illustrating the triatomic, carbon the tetratomic, nitrogen the pentatomic, and sulphur the hexatomic. We have thus presented to us in the first chapters of the work the consideration of seven typical elements and their compounds, which tends to give the reader in a simple manner a general idea of the scientific arrangement of the other elements which follow them in their respective groups.

We rather regret that Mr. Groves has thought it necessary to retain boron in its position as the typical element of the triatomic group to the exclusion of nitrogen, taking that latter substance as the illustration of the pentatomic group. Our reasons for saying so are, that boron is not so well known to the student as nitrogen, nor does it possess a hydrogen compound like ammonia; its atomicity, therefore, must be shown by its chlorine compound, thus destroying the similarity with the other types, which in the first four groups are illustrated by their compounds with hydrogen. Had nitrogen been taken as the type of triatomic bodies, phosphorus would then of course have replaced it as the example of a pentatomic element. Farther, by taking nitrogen as the type of pentads, as is done in Chapter x., a little confusion, we think, is liable to be produced in the mind of the beginner; as immediately after the consideration of that body as a pentad, he proceeds to consider ammonia where nitrogen is not *pentatomic* but *triatomic.*

Many of the more recently-discovered non-metallic compounds have been added to the work, and to some parts a considerable amount of new matter has been contributed by Mr. Groves in the description of recent experiments, as in Dr. Frankland's work on the luminosity of flames, &c. The metallurgy of iron, also, which is very fully described, occupies a considerable portion of the part devoted to the consideration of the metals, but not, however, to the exclusion of other important matters connected with these bodies. Mention is also made of the two recently-discovered metals gallium and davium.

Competing as this work must necessarily do with other large and recently written text-books, we should have liked to see a little freshening-up given to some of the diagrams, the apparatus in some of the illustrations appearing of rather an antiquated form. As an illustration of this point we might give the diagram illustrating the manufacture of sulphuric acid on the small scale, as shown in Fig. 322. A considerable improvement in the letter-press has, however, been introduced by printing the headings of the paragraphs in larger type than has hitherto been used; this, combined with the references to the original papers from which the information contained in the work has been taken, will, we think, prove of great value to the more advanced student.

The work throughout bears evidence of thorough as well as careful selection in regard to the new material introduced, and we think the publishers are to be congratulated on obtaining the services of such a conscientious worker as Mr. Groves for the revision of this standard text-book.

OUR BOOK SHELF

A Glossary of Biological, Anatomical, and Physiological Forms. By Thomas Dunman. (London: Griffith and Farran, 1878.)

MR. DUNMAN'S glossary is the result of an attempt "to place before the student the pronunciation, derivation, and definition" of the terms "usually employed in that department of biological science which treats of animal life, as set forth in standard text-books of Huxley, Carpenter, Foster, Flower, and others," and will be a useful book, no doubt—the more so as there is no other work covering exactly the same ground. At the same time the derivations and definitions appended to the terms are not always quite correctly given, particularly as regards the zoological terms. The order of birds called "Dromæognathæ" was so named by Prof. Huxley because the Tinamous which compose it have the palate formed like that of the ostriches (*Dromæus*, an Emeu)—not from the Greek "dromaios" directly. "Holothuridea" is from Holothourion—a good Greek Aristotelian word—and has certainly nothing to do with "thuris," a little door, as Mr. Dunman would have us believe. A more probable derivation is *θοῦριος, furiosus,* because the Holothuria burst in pieces when touched. There are no such Greek verbs as "*πνέυσω*, I breathe" (given under Pharyngnopneusta), or "*πτόω*, I fall" (given under ptosis). The correct Greek derivations in these cases are *πνέω* and *πίπτω*. "Egesta" is not formed from "*egestio*—getting-rid-of," but is simply the participle of *egero*, meaning such things as are got rid of. Nor are Mr. Dunman's explanations of the purely anatomical terms always faultless, although there is less occasion for criticism here. The "*ligamentum nuchæ*" is formed of elastic not of "connective" tissue. The "sectorial" tooth of the dog is certainly not definable as the fourth premolar, for the dog has no fourth premolar. The "trifacial pair of nerves" are not so called because they arise by three pairs of roots, but because they send three main branches to different parts of the face. We doubt "amnion" having anything to do with "amnos—a lamb." It is an old classical word for one of the fœtal membranes, as may be seen by reference to a lexicon. Lastly, we may remark that "hernia" is very imperfectly, not to say incorrectly, described by Mr. Dunman.

It would not be difficult to pick more holes in Mr. Dunman's volume—which, however, in spite of some defects, will be serviceable to the persons for whose aid it is designed.

The Native Flowers and Ferns of the United States. By Thomas Meehan. Illustrated by Chromolithographs. Issued by Subscription. (Boston: L. Prang and Co., 1878.)

THIS book is intended to be "an anthology in the truest sense of the word," and aims at culling the most beautiful, interesting, and important from the vast number of plants found in the enormous region ruled by President Hayes. Further, it is not merely scientific; a familiar treatment is adopted so that the cultivator and mere lover of flowers may derive both profit and instruction. The first parts which we have received lead us to think that the editor has hit upon a good working way of carrying out his intentions. The text is very readable, the printing is most excellent, and the name of Prang as publisher tells those who know that the chromolithographic part cannot be excelled.

Magnetism and Electricity for Schools and Science Classes. By George Porter. (Belfast: William Mullan and Son, 1878.)

SO many school manuals of Electricity and Magnetism have appeared during the last ten years, particularly since the establishment of the South Kensington Science Examinations, that one is led to question the advisability of adding to their number. The existing manuals are, as a rule, as complete and comprehensive as works of their size and price can be, and until new facts are discovered, or new methods of treating old facts are in vogue, we do not see the necessity of multiplying such text-books. The work before us does not present any special feature. It is suitable for low Forms in a Public School, and for the elementary examination in Electricity at South Kensington. It is cheap and sufficiently illustrated, but occasionally insufficiently explicit for young boys. The chapter on Terrestrial Magnetism might with advantage be somewhat enlarged, and would be distinctly improved by the addition of one or two simple figures.

La Revue Magnétique, Organe du Cercle Electro-magnétique de Paris. Rédacteur-en-Chef, H. Durville. Nos. 1, 5, 6. (Paris, 1878.)

WE have received the above three numbers, the first of which was published on April 16. Acting upon the saying *ex pede Herculem*, we give an extract or two from the "A nos lecteurs." "Il ne faut pas considérer le magnétisme comme une panacée universelle; mais son emploi peut rendre d'immenses services à l'humanité. Sans remplacer complètement la médecine, il peut la seconder puissamment, dans les maladies même les plus désespérées. Quand il sera bien compris de tous, dans la plupart des cas le père de famille deviendra le meilleur médecin de ses enfants; le frère traitera son frère; l'ami, son ami; et tous travailleront alors au progrès de tous. En publiant la *Revue Magnétique*, nous avons l'intention de rallier tous les éléments qui divergent encore autour de la doctrine; d'affermir celle-ci sur une base inébranlable, et de découvrir les secrets les plus cachés de la nature pour les utiliser au profit de l'humanité. . . . Nous ne reculerons devant aucun sacrifice; nous répondrons à toutes les objections qui nous seront posées, et nous insérerons avec empressement les articles qui nous seront adressés. Flétrissant de toutes nos forces le charlatanisme partout où nous le rencontrerons; combattant les abus de toutes nos forces, nous enseignerons le MAGNÉTISME comme une œuvre de science et de charité." These are fine professions. There are articles on magnetism and somnambulism, an unpublished manuscript of Mesmer, and other short notes. A paper by the Editor, entitled, "Les Nombres considérés dans leurs rapports avec les Sciences occultes et les différents Cultes," runs through all the numbers, and contains much curious matter, which, from the style in places, reminds us of the *brochures* we have noticed in these columns from the pen of the Comte L. Hugo. To an outsider the journal appears strange; it carries on its face a good motto from Bossuet: "La Vérité est un bien commun; quiconque la possède la doit à ses frères."

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.]

The Zoological Record

DR. HOEK'S complaint in NATURE, vol. xviii. p. 569, that not much care had been taken in the record as to zoological papers published in the Netherlands in 1875 and 1876, doubtless appears well grounded to himself from his locally restricted view, but, when analysed, is scarcely of sufficient importance to call for more than an expression of regret that a certain percentage of omission and error must inevitably occur in any large undertaking. No one of the recorders fancies that his work is complete, and the omissions of any one year are constantly supplemented in the following volumes, our best thanks being given to those who will take the trouble to put us right.

The copy of Dr. Hoek's paper, "Die Zoologie in den Niederlanden," sent for our guidance, was duly received after the usual delays, and I beg to thank him publicly for his forethought and kindness in presenting it. It bore the date 1877, and, if it had contained matter to be recorded, would be included in the volume for that year now being compiled. But, being bibliographical only, its use is as an index to the different recorders. It contains notices of eighty-seven papers, on no particular branch of zoology, and referring to no particular locality, but merely published in the Netherlands in 1875 and 1876. Of these eighty-seven papers, I find only seventeen not given in the records for those years; and, as no less than ten of these were published in the latter year, the delay of one volume only is caused by the omission. One of the seventeen (by Benjamins) is purely histological, and not in our scope; four others (by De Man and Winkler) are palæontological, and two at least of the rest are of the most unimportant nature (by De Graaf and Everts). The remainder are one in Mammalia (but more correctly to be referred to Vermes), one in Pisces, two in Mollusca, one in Insecta (Lepidoptera), and five in Crustacea, so that the charge of want of care, when shared by the recorders of these divisions, appears founded upon a very slight basis.

A certain querulousness in Dr. Hoek's last sentence is probably to be attributed to the incompleteness in the record of Crustacea, the five omissa in which are unfortunately all his own papers. (I observe that four papers of Dr. Hoek's on Crustacea are included in the records for 1875 and 1876, though apparently not the same as these.) Dr. von Martens, the recorder of that division, would, however, be the first to deplore this, and to feel the loss to himself as a worker; and his reputation for accuracy and fidelity is too great to suffer from this *lapsus*.

It may be mentioned, also, that two of the omissa are contained in Academical Inaugural Dissertations (which it is needless to explain are practically never published); and that another of them is published in the papers of the Zeeuwsch Society of Sciences (Middelburg), which, to say the least, are not in the hands of every working naturalist in this country.

October 1 E. C. RYE

Intra-Mercurial Planets

THE places sent you of the objects which I designated by (*a*) and (*b*) in my observations during the total eclipse on July 29 were derived from the hurried readings of the circles made immediately upon my return from the Eclipse Expedition, in order to be able to answer numerous inquiries addressed to me for information in regard to these observations. Subsequently I made a careful determination, and the readings of the circles and all the data for a definitive reduction of the observations were communicated to astronomers in this country and in Europe. These have probably already come to your knowledge and need not be repeated here.

The only outstanding question in regard to the place of the star which I designated by (*b*), is whether any disturbance of the telescope by the wind is to be feared. The position was marked on the hour circle first, and but a moment was occupied in passing from the eye-piece to the place where this was done. The wind was blowing fresh from a direction south of west, but our telescopes were, as you know, well sheltered by the semi-circular ledge behind which we observed. My own instrument was near the ledge on the west, and was more completely protected than any of the others, and hence it became desirable to know whether any such disturbance of their instruments was noticed by Prof. Newcomb, Commander Sampson, and Lieut. Bowman, who observed near me. Accordingly, I addressed letters to these gentlemen for information upon this point. Prof. Newcomb read the circles of his instrument for a pointing made at about the same instant, which proved to be on a fixed star, and there was no disturbance whatever of the position of his telescope. Lieut. Bowman says that there was no disturbance of his instrument by the wind during the totality, and Commander Sampson says that his assistant, Dr. Dewitt, who pointed for him while using the spectroscope, did not notice any disturbance.

These reports might be regarded as conclusive upon the question which I raised, when I came to reduce the observations, since otherwise the star (*b*) could not be ζ Cancri. If the totality had only lasted a few seconds longer I might have moved out to ζ Cancri, and by observing it also there would have been no uncertainty whatever. But I hardly realised at the time the possibility of there being two planets near the sun, and being sure of one, I gave more attention to it. The record of (*b*) was made just before the sun reappeared. In fact, the sun came out just as I turned to go to the eye-piece again, and anxious to have Prof. Newcomb's telescope also directed on (*a*), I ran across to where he was observing, but his telescope being then directed toward a suspicious object, for which he was reading the circles, it could not be disturbed. Returning to my own instrument it was too late to re-observe (*b*) or to find ζ Cancri, and I could not then determine whether the object observed was a stranger or not. It was very much brighter than I expected to see ζ Cancri, judging from the appearance of δ Cancri, which I had seen in a preceding sweep.

In order to obtain further evidence as to the stability of the instrument, I have made careful experiments with it, clamped as it was then, and I find that the danger feared has no significance whatever. During the present week, also, there have been days when the wind was blowing very strong from the same direction as on the day of the eclipse, and I have placed the telescope in the position as to direction in which it then was, but fully exposed to the wind, and it has remained hours at a time thus exposed without the pointing being sensibly changed. I conclude, therefore, that the object which I designated by (*b*) is also a new star.

I have lately examined, on two mornings, the stars in that part of Cancer, and my recollection of the appearance of the stars (*a*) and (*b*) being still vivid, I have compared, with the same telescope and magnifying power, the stars which I then observed in the vicinity of the sun. The moon shining brightly in the west, and the bright twilight in the east, gave a sky-illumination in some respects similar to that at the totality of the eclipse. By observing when the approaching daylight had extinguished the light of two small stars which I saw on July 29 east of the sun, so that they were just visible in the telescope as they were on that day, I proceeded to compare the light of θ and ζ Cancri. As a result of this examination, I am convinced that I under-estimated the magnitudes at the time. I think that (*a*) must be classed as good fourth magnitude, and (*b*) as third magnitude, if not brighter. JAMES C. WATSON

P.S.—I have begun some calculations, but being pressed just now in the preparation of elements, perturbations, and ephemerides of ten or twelve of the minor planets for the *Berliner Astr. Jahrbuch*, I have not yet progressed very far. It is probable that M. Gaillot will have worked up all the material available for this. J. C. W.

Ann Arbor, September 21

Sun-spots and Weather

IN the last number of NATURE (p. 567) there is a very interesting communication from Mr. Fred. Chambers of Bombay. He shows that the barometric pressure at Bombay when graphically exhibited for a series of years, gives a curve which is very similar to the sun-spot curve, and he remarks that the barometric curve lags behind the sun-spot curve particularly in the years of maximum sun-spots. He argues that the sun is probably hottest at times of maximum sun-spots. I have grounds for thinking that I found traces of a somewhat similar relation in discussing the daily range of the thermometer at Kew Observatory, although the results obtained were not so definite as those of Mr. Fred. Chambers.

When, however, we go from the meteorological to the magnetical influences of the sun we find a very marked and well-known relation between the sun-spot areas and the magnitude of the diurnal range of declination—this diurnal range being unmistakably greater when there are most spots. Here also the lagging behind comes prominently out whatever may be its cause.

Mr. Frederick Chambers quotes the following remark made by me (NATURE, vol. xvii. p. 326):—

"It is nearly, if not absolutely, impossible from observations already made, to tell whether the sun be hotter or colder as a whole when there are most spots on his surface. The sooner we get to know this the better for our problem."

I ought here to mention that in these words I referred more particularly to *direct* observations of the heating effect of our luminary. I ought also to state that the fundamental importance of such observations was impressed upon me by the remarks of a very distinguished physicist, who considers that a persistent and well-organised attempt should be made to determine by means of actinometric observations whether our luminary is in reality of variable heating power.

We know a good deal about sun-spots, although not nearly so much as we ought, but we know next to nothing about the variations (if any) in the direct heating effect of the sun. I can only here repeat what I said before, that "the sooner we get to know this the better for our problem." BALFOUR STEWART

Manchester, September 27

Cyclones and the Winter Gales of Europe

THE following figures may interest some of your readers as a contribution to the theory put forward in NATURE, vol. xvi. p. 505, regarding the meteorological effects of variations in the intensity of solar radiation, and of the consequent changes in terrestrial temperature.

According to this theory, the high temperature which generally coincides with sun-spot minima should have the effect of increasing the steadiness and velocity of the prevailing winds of the globe, whilst, at the opposite epoch of the solar cycle, the weakness and unsteadiness of these currents ought to give rise to heavy rain on the coasts and islands of the tropics, and to facilitate the generation of cyclones, which (as has been shown by Messrs. Blanford and Eliot in the case of the Bay of Bengal), are most probably caused by the condensation of aqueous vapour over the place of its production. If this view of the action of variations in temperature upon the convection currents of the atmosphere be the true one, it follows that the south-westerly gales of Europe should be most frequent and powerful at times when the cyclones of the West Indies are least frequent. This is borne out by the accompanying table, which shows the number of hours in each year during which the wind-velocity in the British Isles exceeded thirty miles, as compared with the number of cyclones in the West Indies, according to Poëy. The figures in the second line are taken from the *Quarterly Weather Reports*, and represent the averages of the annual totals for Valencia, Armagh, Glasgow, Aberdeen, Sandwick, Falmouth, Stonyhurst, and Kew. These are the only stations which give a continuous register for the six years in the table.

	1869	1870	1871	1872	1873	1874
Hours of high wind (British Isles) ...	714	570	537	679	571	658
Cyclones (W. Indies)	0	7	3	0	1	?

The period covered by the table is too short to afford any ground for a definite induction; but, as far as they go, these figures afford *primâ facie* evidence in favour of the theory.

I may add that the probability of this relation between the gales of the temperate zones and the cyclones of the tropics has been pointed out on purely speculative grounds by Mr. E. D. Archibald, in a pamphlet on the rainfall of the world, recently published by him in India. S. A. HILL

Allahabad, September 11

Magnetic Storm, May 14, 1878

IN the *Bulletin Mensuel* of the observatory of Zi-ka-wei, near Chang-hai, China, the following interesting remark occurs in the number for May, 1878:—

"Durant le mois de Mai, une seule perturbation a été enregistrée par le magnétophotographe; elle commença le 14, à 2h. 20m. de l'après-midi, et se termina 24 heures après; ce jour-là la déclinaison, par extraordinaire, ne présenta pas d'oscillation diurne normale, mais de très-nombreuses petites oscillations comme le bifilaire."

On examining the photographic trace of the Stonyhurst magnetograph I find that the only magnetic storm in May last commenced on the 14th at 6h. 4m., and lasted rather more than twenty-four hours. The longitude of Zi-ka-wei being 8h. 15m. 38s. E. of Stonyhurst, the storm began at the same time at both stations. The character of the movement was also identical, for the only disturbance at Stonyhurst from 6 A.M. until 4 P.M. was a tremulous motion of the declination and horizontal force magnets.

The storm was at its height at midnight, when all the magnets were much disturbed, and the vertical force magnet was thrown completely off its balance.

It is impossible to obtain more than the roughest outline of a magnetic storm from hourly readings, but even this slight datum from China shows a general agreement in the declination curves during the storm at these two distant stations.

The *Monthly Record* of the Melbourne Observatory also mentions the same magnetic disturbance, which commenced there at 4 P.M. on the 14th, and lasted until 8 A.M. on the 17th. Melbourne lies 9h. 39m. 54·8s. E. of Greenwich, and therefore 1h. 34m. 10s. E. of Zi-ka-wei. We thus see that the Zi-ka-wei storm commenced at 3h. 54m. Melbourne time: it was therefore simultaneous at the three observatories.

Stonyhurst Observatory, September 27 S. J. PERRY

Winds and Currents in the Pacific

THE occasional prevalence of westerly winds and of currents setting east in the intertropical portion of the Pacific, has such an important bearing on the possible eastward migrations of the Polynesians that I think the following, which I take from the *Samoa Times* for April 20, is worth recording in NATURE.

The brigantine *Ryno* is reported as having arrived at Apia, Samoa, on April 16, from the Tokelau, or Union Group. She was among those islands from March 1 to April 11, and, while there, she "experienced a succession of strong westerly and north-west winds, with high sea and frequent squalls and much rain, which made sad havoc amongst the vessel's canvas. Capt. Bower states that, when off Tokelau, he found a strong current setting to the eastward at the rate of two miles per hour. The *Ryno* left Tokelau on April 11, had fine E.S.E. weather, and no current whatever." S. J. WHITMEE

Blackheath, September 27

Blackburn's Double Pendulum

I TRUST I may be pardoned for observing, in reference to Prof. A. M. Mayer's description of the curves produced by Blackburn's Double Pendulum (NATURE, vol. xviii. p. 594), that a typical series of those curves was placed before the readers of NATURE in the year 1871 (vol. iv. pp. 310, 370), in illustration of a paper of mine on "Pendulum Autographs."

Prof. Mayer adheres to the funnel and sand as the mode of laying the curve on paper. The sand-trail thus left is necessarily rather coarse, and cannot be conveniently preserved. A more delicate and more enduring trace, in ink, can be obtained by the use of a tubular glass pen, as described by me in the paper above-mentioned. Only the increased friction makes it necessary to use a heavier bob. HUBERT AIRY

Circulating Decimal Fractions

AS a supplement to the interesting properties of circulating decimal fractions which have been published in two recent numbers of NATURE, I give you the following, which I think is sufficiently curious to merit attention:—

If the decimal fraction equal to $1 \div n$ *recur in a cycle of* $n-1$ *digits, the average value of the digits is constant, viz.,* $4\frac{1}{2}$ *for all values of* n; *in other words, the sum of the digits of the cycle is* $4\frac{1}{2}(n-1)$.

For example,—

$$\tfrac{1}{7} = \dot{1}4285\dot{7},$$

and $(1+4+2+8+5+7) \div 6 = 27 \div 6 = 4\frac{1}{2}$.

Again,

$$\tfrac{1}{17} = \dot{0}58823529411764\dot{7},$$

and $(0+5+8+8+\&c.) \div 16 = 72 \div 16 = 4\frac{1}{2}$.

The theorem is easily established from the two facts (1) that $1 \div n$, $2 \div n$, $3 \div n$, ... have the same digits in their cycles, and (2) that the sum of $1, 2, 3, \ldots, n-1$ is $\frac{1}{2}n(n-1)$.

The properties of circulating decimal fractions have been often studied from the time of Wallis downwards, and very probably those lately and now brought forward have been noted before, but have lain entombed in some out-of-the-way corner since. However, until a full index to mathematical literature is prepared, or exhaustive monographs on special departments like this are written, such resurrections are very desirable.

If either of your two previous correspondents on the subject would care to undertake a full examination of it I should be glad to furnish him with my quota of references to the extent of ten or a dozen, and, I have little doubt, other readers of NATURE would give like help. THOMAS MUIR

High School of Glasgow, September 23

[Mr. Muir's theorem is practically contained in the result that the two halves of the period are complementary, whence the sum of the figures is equal to half as many nines as there are figures in the period.—ED.]

An Old Map of Africa

EARLY in the year 1870 I visited the vineyard of Mr. J. L. Cloete at Constantia, near Cape Town.

Among other things of interest Mr. Cloete showed me an old map of Africa done, I think, in Amsterdam. This map had been in the possession of his family from time immemorial. My acquaintance with the geography of Africa was too slight to enable me to pronounce upon its accuracy, but I was greatly surprised to see marked upon it several large lakes and many rivers in the region now so well known to us through recent explorations.

In the critical study of this subject I have thought that a knowledge of the existence of this map, if it be as I remember it, might prove of value. C. F. GOODRICH,

Torpedo Station, Lieut.-Commander, U.S. Navy

Newport, Rhode Island

[We would refer Commander Goodrich to our article on Old Maps of Africa, in NATURE, vol. xviii. p. 149.—ED.]

Earth Pillars

A LETTER in your issue for September 26 (p. 569) refers to miniature earth pillars seen in the Tyrol. But there is no need to travel out of the country to obtain these mimic representations, at any rate on a moderate scale. Twice I have found them, formed by the drops from railway bridges, upon bare clays; and once in a half-finished building, in a sand-heap containing numerous pebbles. Evidently the heavy drops and the protection from driving rain favoured the result. One of the bridges, between Shepton Mallett and Wells, Som., covered Lias clays, protected by cinders, &c. The other instance, the bridge on the new North Approach just outside York Station, is the more interesting in its mimicry of the original Botzen earth pillars, since small boulders and pebbles in the glacial clays form the caps and shoulders in the Lilliputian columns.

September 28 J. EDMUND CLARK

White Swallows

MR. HERBERT W. PAGE, in NATURE, vol. xviii. p. 540, refers to the rare occurrence of a white *Hirundo riparia.*

I may mention that two white swallows of that species were captured in a high bank of sand at Hungryside, on the Forth and Clyde Canal, by Mr. Martin, of that place, in August, 1876, which I exhibited at a meeting of the Natural History Society of Glasgow, August 26, 1878. Both birds were white all over, with the exception of a brown tinge on the back of one of them. Mr. Stevenson, in his "Birds of Norfolk," vol. i. p. 342, mentions a light cream-coloured variety got at Eaton in July, 1861, and another at Weasenham in the following September.

DAVID ROBERTSON, Jun.

Glasgow, September 27

Secondary Lunar Rainbow

DURING the very stormy and unsettled weather we had about a fortnight ago, I was one of a party of friends on a visit at a country-house near Huntly, about forty miles from this, who were witnesses, on the evening of Sunday, 15th inst., to a very striking, and, as a friend well versed in meteorology has since informed me, very uncommon phenomenon. It was that not only of a complete and brilliant primary lunar rainbow with colours, but also, a few minutes later, of a complete and well-defined, but, of course, less brilliant, *secondary* bow. No trace of colour could be observed on the secondary, but, inside the primary, the space seemed, in contrast with the faint moonlight, even more brilliantly and uniformly illuminated than I recollect ever to have seen it within a solar rainbow. The time was about eleven o'clock, and the centre of the bows, therefore, bore about W. or a little to the N. of W.

R. WALKER

Aberdeen, September 30

BONE CAVES IN STYRIA[1]

1. TWO prehistoric implements, found some years ago in the "Badel" Cave, near Peggau, in Styria, are preserved in the Museum of the Johanneum, at Gratz. Count G. Wurmbrand has lately conducted an exploration of this cave. The floor is formed for the most part of a layer of fragmentary stalagmite, about seven centimetres thick, resting on a loam, with bones and gravel, forty-three to seventy centimetres thick. A peculiar laminated and very compact loam, permeated by a blackish substance (*pyrolusite*) lies on the rocky bottom. In the loam have been found abundant bones and teeth of *Ursus spelæus,* undeterminable remains of rodents, a canine of a stag, bones gnawed by *hyæna,* &c., but no human remains or relics.

2. In a cave on the north slope of the Erzberg, east of Wildalpe, in Upper Styria, Dr. A. Redtenbacher has found abundance of bones and teeth of *Ursus spelæus.* All these bones, except the phalanges, were broken; and the long ones of the extremities were split longitudinally.

3. On July 1 of this year, Dr. R. Hoernes and Dr. R. von Fleischhaker visited the Drachenhöhle (cave of Dragons) near Mixniz, about 1,292 (Austrian) feet above the town, and extending 1,440 feet into the rock, with an entrance 90 feet high and 72 feet wide. The floor of the cave is mostly covered with reddish-yellow cave-loam; but rock-fragments abound within the entrance. The superficial loam had been searched long ago. The inner parts of the cave, accessible only by ladders, were nearly untouched. On the day mentioned, numerous remains of *Ursus spelæus,* both young and adult, fragments of jaws with teeth, single teeth, long bones, ribs, phalanges, &c., were soon found. Some of the teeth and bones in the terminal fissure of the cave could have been brought there only by moving water. No other animal but the cave-bear was represented by the remains met with.

In the portion of the cave next the entrance, a still untouched layer of fragmentary stalagmite, including angular pieces of rock (some very large) was found beneath an accumulation of rock-fragments. Underneath it is an irregular dark-brown and nearly black stratum, about 15 centimetres thick, containing charcoal and partially burnt bones; and in a brown loam, immediately beneath, similar objects were abundant. These bones, differing from those of the yellowish loam of the inner cave, by their greater consistence and dark colour, belong mostly to *Ursus spelæus,* and a few to an undetermined ungulate. The long bones are nearly all broken. Among these dark-coloured bones are phalanges, cervical vertebræ, and the lower portion of a right humerus measuring 16 centimetres between the condyles, which must have belonged to a colossal individual. The connection of the cervical vertebræ (the atlas bearing traces of fire), and the presence of several other bones of this individual (as well as those of smaller ones) lead to the conclusion that it was brought into the cave after being killed, and prepared there for the food of the cave-folk. No implements were found.

[1] Imperial Geological Institute of Vienna Report, July 31, 1878.

OUR ASTRONOMICAL COLUMN

THE SATELLITES OF MARS.—The results of the complete reduction and discussion of the Washington observations of the satellites of Mars, undertaken by their discoverer, Prof. Asaph Hall, have just been published by the United States Naval Observatory. The observations of the outer satellite *Deimos* extend from 1877, August 11, to October 31, and those of the inner one, *Phobos,* from August 17 to October 15. On November 7 and 12 the satellites were looked for, but could not be seen. In treating these observations with the view to the determination of the most probable orbital elements of the satellites, Prof. Hall assumes in the first place that their paths are circular, and by a graphical projection finds the angle between the orbit-plane and the plane perpendicular to the line of sight, as well as the angle of position of the major axis of the ellipse into which the circular path is projected. The resulting elements are then compared with the observations, and elliptical elements deduced from the resulting differences between calculation and observation, both for position and distance, by means of equations of condition. Thus the following values of the elliptical elements have been obtained from the Washington observations alone:—

Epoch 1877, August 28·0 Greenwich Mean Time.

	DEIMOS.	PHOBOS.
	h. m. s.	h. m. s.
Period of revolution	30 17 53·86	7 39 15·07
Semi-axis major at distance unity ...	32″·3541	12″·9531
Ascending node on equator	48° 5′·7	47° 13′·2
Inclination to equator	35° 38′·7	36° 47′·1
Angle between the lines of nodes and apsides	40° 53′·6	45° 30′·4
Distance from node at epoch	357° 30′·5	285° 20′·2
Eccentricity	0·005741	0·032079

It will be seen that the planes of the orbits of the satellites are nearly coincident with the equator of Mars, the ascending node of which for the above epoch is in 47° 56′, and the inclination 39° 45′. Prof. Hall considers the elements to be determined with tolerable accuracy, with the exception of the periodic times, which remain to be decided within closer limits from the observations of another opposition; nevertheless they are sufficiently exact to carry forward an ephemeris to 1879. The eccentricity in the case of *Deimos* is so small that for most purposes of calculation circular elements will suffice. In the case of *Phobos,* Prof. Hall thinks the eccentricity has a real existence, every comparison of distance with the circular orbit confirming it.

The mass of the planet deduced from the measures of *Deimos* is $\frac{1}{3095313}$, and from those of *Phobos* $\frac{1}{3078456}$, that of the sun being taken as unity; the adopted mean value is $\frac{1}{3093500}$. The latest value of the mass resulting from theory is that given by Leverrier (*Annales*, vol. xi. p. 3), viz., $\frac{1}{2812526}$.

Prof. Hall compares his elements with the observations made at other observatories, of which those of Cambridge, U.S., Glasgow, U.S., Pulkowa, and Mr. A. Common, of Ealing, were the most successful. The magnitudes of the satellites, free from the glare of the planet, were estimated at about 12 and 11½, *Phobos* being the brighter of the two. On October 15, when their distances from the centre of the planet were respectively 23″ and 57″·5, they were of nearly equal brightness. Prof. Hall further remarks: "The chief difficulty of observing these satellites is on account of the brilliancy of the planet. At their elongations at opposition in 1877, they appeared to me brighter than the outer satellites of Uranus and much brighter than Hyperion, and on October 31 Deimos resembled Umbriel, the second satellite of Uranus, which, at elongation, is fainter, I think, than any other satellite."

With the angular values of the mean distances given above, the real distances of the satellites from the centre of their primary will be for *Phobos*, 5,800, and for *Deimos*, 14,500 miles; the former, from the rapidity of its motion (it performs more than three revolutions in the Martian day) will appear to rise in the west, meeting and passing the outer moon, and setting in the east, and it will have a horizontal parallax amounting to 21°.

Prof. Hall has definitively adopted the names proposed by Mr. Madan, of Eton, in the columns of NATURE.

THE SATURNIAN SATELLITE, TITAN.—The following are the approximate times of conjunction of the great satellite of Saturn, with the perpendicular to the plane of the ring, during the next month, at which times occultations by the planet, or transits over its disc take place. The elements used are very nearly those of Bessel, but with a somewhat larger ellipticity of the planet.

	G.M.T. of conjunction.	Angle of position.	Distance from Saturn's centre.	Saturn's polar semi-diameter.	
	h.	°	″	″	
Oct. 8	13·9	4·2	8·17	8·69	On disc?
,, 16	5·7	184·3	7·32	8·64	Occulted.
,, 24	11·7	4·3	6·53	8·58	On disc.
Nov. 1	3·5	184·4	5·91	8·50	Occulted.
,, 9	9·6	4·4	5·42	8·40	On disc.

WINGLESS INSECTS OF THE FALKLAND ISLANDS

WHILST on an excursion to Port Darwin, in the Falkland Islands, during the visit of H.M.S. *Challenger* to that group, I found at Darwin Harbour, Choiseul Sound, some insects which are of considerable interest, since, as I believe, they are closely allied to those of Kerguelen's Land. Amongst them were a gnat, practically wingless (Tipulidæ), and a fly with rudimentary wings. The gnats were found crawling on the rocks on the sea-shore, in sheltered places, and also on the sunny, sheltered side of a fence composed of a peat bank, with furze growing on the top of it. They run quickly, and, when in danger, draw up their legs and drop in order to escape, and they are not by any means easy to catch amongst the grass.

The flies were found only on the sea-coast, in hollows under overhanging slabs of the sandstone rocks, sheltering themselves in crevices. They have short wings, which they seem to use in jumping, and they spring nimbly, like fleas or small grasshoppers, and are difficult to catch. On comparing specimens of these flies with specimens of *Amalopteryx maritima*, one of the flies of Kerguelen's Land, with rudimentary wings, described by Mr. Eaton (Rev. E. A. Eaton, the *Entomologist's Monthly Magazine*, August, 1875), I have little doubt that they are very closely allied to this species, and to be referred at least to the same genus. Dr. Kidder describes the habits of the Kerguelen *Amalopteryx* as closely similar (J. H. Kidder, M.D., *Bulletin United States National Museum*, No. 3, 1876, ii. p. 52). Von Willemoes Suhm found a species of the same genus in Marion Island, and we all observed the fly at Kerguelen's Land as well as the wingless gnat which Mr. Eaton has named *Halyritus amphibius*, and which lives on the Kerguelen sea-shore amongst sea-weed constantly wetted by the tide. It would be interesting if the Falkland Island gnat proved allied to the Kerguelen one on further examination.

I found one beetle with wings at the same locality in the Falklands, and one wingless species. All the Kerguelen beetles are wingless. Two genera and all the species of that island are endemic (Mr. C. O. Waterhouse, *Entomologist's Monthly Magazine*, August, 1875, p. 50). The close connection between the Fuegian flora and that of the far distant Kerguelen's Land is well known from the investigations of Sir Joseph Hooker. It is interesting to find a further connection in the insects. The four wingless flies of Kerguelen's Land are assigned by Mr. Eaton to four new genera. I believe, though I am no entomologist, that the Falkland Island and Marion Island fly will come under one of these, and possibly further search may prove the existence of representatives of some of the other genera in Fuegia or the Falklands. I see from the "Histoire Nat. des Insectes Diptères" of the Suitesa Buffon, the only authority immediately at hand, that a wingless gnat, *Chionea araneoides*, is found in Sweden in woods on the snow throughout the winter, whilst two flies with rudimentary wings, *Apterina pedestris* of France and Germany, and *Myrmemorpha brachyptera* of Spain, exist in Europe. These merely as examples. Prof. Westwood tells me many other such diptera are known to entomologists, and he has shown me a specimen of a wingless fly, *Borborus apterus*, which occurs in England.

Exeter College, Oxford H. N. MOSELEY

SUN AND EARTH

THE Meteorological Reporter of the Bombay Presidency, Mr. F. Chambers, at the end of his recently-published report for 1877-78 gives us the first results of some important researches which he has not yet been able to complete, owing to want of clerical assistance. In a brief sketch of the meteorology of the Bombay Presidency in 1876 prepared for the Bombay Administration Report for 1876-77, he showed that the abnormal meteorological conditions which produced the famine of 1877 were of the same type as those which produce the usual alternations of seasons, and therefore are attributable to similar causes.

A commencement was made some time ago with the discussion of the Kurrachee wind observations. One of the most important results already obtained is that the numerical relation existing between the abnormal wind and abnormal barometric movements is exactly similar to the relation between the *annual* variations of the wind and barometer leading to the same conclusion as above, viz., that most of the abnormal variations of weather in India are due to causes which are similar to, if not identical with, those which produce the normal variations. The comparison of the normal and abnormal barometric movements at different stations points decidedly in the same direction, and Mr. Chambers believes that further investigation will prove this to be a general law, affecting perhaps all abnormal meteorological variations which are not cyclonic. He adds:—

"The fact that a famine has been raging in China, at the same time that one has ravaged Southern India, does not appear to be without its significance, in pointing to other than local causes of the meteorological disturbances which produce famines. Indeed, the inference that most of the unusual variations of weather in tropical climates are induced by corresponding variations in the *absolute* heating power of the sun, in the *same manner* that the seasonal variations are induced by those changes of heating power which depend on the relative *motions* of that body, seems almost irresistible, if it may not be regarded as already partly proved. The importance of this conclusion, if true, will be readily admitted, for it will be at once apparent, that if the absolute variations of the sun's heat are fitful in their occurrence, and do not obey definite periodical laws, it will perhaps never be possible to predict by more than a few days in advance the unseasonal variations of weather induced by them, while if such laws can only be discovered, the possibility of our being able to predict their consequences is equally certain."

OCCURRENCE OF FOSSILIFEROUS TERTIARY ROCKS ON THE GRAND BANK AND GEORGE'S BANK

AMONG the most important results of the investigations made by the party connected with the United States Fish Commission, stationed at Gloucester, Mass., during the present season, is the discovery of fragments of a hitherto unknown geological formation, apparently of great extent, belonging probably to the miocene or later tertiary. The evidence consists of numerous large fragments of eroded, but hard, compact, calcareous sandstone and arenaceous limestone, usually perforated by the burrows of *Saxicava rugosa*, and containing in more or less abundance fossil shells, fragments of lignite, and in one case a spatangoid sea-urchin. Probably nearly one-half of the species are northern forms, still living on the New England coast, while many others are unknown upon our coasts, and are apparently, for the most part, extinct. From George's Bank about a dozen fossiliferous fragments have been obtained, containing more than twenty-five distinct species of shells. Among these one of the most abundant is a large thick bivalve (*Isocardia*) much resembling *Cyprina islandica* in form, but differing in the structure of the hinge. This is not known living. *Mya truncata*, *Ensatella Americana*, and the genuine *Cyprina* are also common, together with a large *Natica*, a *Cyclocardia* (or *Venericardia*) allied to *C. borealis* (Con.), but with smaller ribs, *Cardium islandicum*, and also various other less common forms. These fragments came from various parts of the bank, including the central part, in depths varying from 35 to 70 fathoms, or more.

From Banquereau, N. S., we received one specimen of similar rock, containing abundant fragments of a large bivalve, and about a dozen other species, among which are *Fusus* (*Chrysodomus*) *decemcostatus*, *Latirus albus*, Jeff. (?), unknown species of *Turritella*, &c. From the Grand Bank two similar specimens were received. One of these, from thirty-five fathoms, lat. 44° 30′, long. 50° 15′, contained numerous specimens of *Cyprina islandica* in good preservation.

In gathering these specimens from the fishermen and working out the specimens Mr. W. Upham has been very active. It will probably be possible hereafter, when these specimens shall have been more fully examined, and more obtained, to give a pretty long list of species, especially from George's Bank.

At present it appears probable that these fragments have been detached from a very extensive submerged tertiary formation, at least several hundreds of miles in length, extending along the outer banks, from off Newfoundland nearly to Cape Cod, and perhaps constituting, in large part, the solid foundations of these remarkable submarine elevations.

A. E. VERRILL

THE BALLOON EXPERIMENTS AT WOOLWICH

THE military balloon experiments at Woolwich have been so far successful, that last week an aëronaut was lifted some 700 feet, to a height, therefore, sufficient for reconnoitring purposes. There is nothing of novelty in this, as a matter of aërial navigation, although it is the first instance, we believe, of any one in this country being raised from the earth by the agency of pure hydrogen, but it is, nevertheless, something to have achieved in the circumstances under which Capt. Templar has been working. Everybody knows that hydrogen is gifted with extraordinary lifting power, just as every chemist is aware that the gas may be produced in the way Capt. Templar produced it, namely, by passing a jet of steam over iron turnings. But the problem under solution was not to send up a hydrogen balloon so much as to discover whether the thing could be done in a haphazard fashion, and with such simple means as an army in the field would be provided with. It is one thing to make hydrogen in the laboratory, and another to make a sufficient supply of it just whenever the commander of an army may order a balloon reconnaissance to be made.

Capt. Templar has practically proved that this can be done. He requires a supply of steam, an improvised furnace of some sort, and a tube filled with iron turnings; given these, he can provide hydrogen sufficient to lift a scout high into the air. The tube at present employed by Capt. Templar is six or eight inches in diameter, and some half-dozen feet long; it is filled loosely with iron turnings and placed in a furnace where it becomes red hot. Steam is now passed through the tube, and hydrogen issues forth, the oxygen from the decomposed steam going to form ferrous oxide. So completely do the iron turnings do their work under these circumstances, that not only is the surface of the metal acted upon, but it is oxidised well-nigh throughout.

Naturally enough, the hydrogen comes away with a good deal of vapour, and, if pure gas is desired, some desiccating arrangement will have to be employed; but so far Capt. Templar has used none. His balloon, which is of lawn, dressed with boiled oil and glue, will contain about 10,000 cubic feet, but last week not more than 9,000 feet of hydrogen was introduced. The gas was generated from the tube at the rate of something like 1,000 cubic feet per hour, and there can be little doubt that, during the long period of filling, a large quantity of the vapour that was mixed with the hydrogen condensed, and ran out of the balloon in the form of water. Pure hydrogen should have a lifting power of 70 lb. per 1,000 feet, or perhaps a little more, but it is hardly likely that gas produced in a rough-and-ready fashion in the field will possess this degree of buoyancy. Still, Capt. Templar was successful in lifting balloon, aëronaut, ballast, and 700 feet of rope—for the ascent was a captive one—by means of 9,000 cubic feet of hydrogen, prepared in the way we have mentioned.

Another point is worthy of note in connection with the experiment. The fabric of the balloon kept the hydrogen imprisoned for a much longer period than had been anticipated. A dozen hours scarcely impaired the buoyancy of the balloon, and by adding yet another waterproof coating it is anticipated that the balloon will remain inflated for four-and-twenty hours.

The next step will be to discover how far it is possible to compress hydrogen so manufactured into cylinders for conveyance in transport waggons, so that a supply of hydrogen may be at hand whenever an ascent is determined upon in the field. Capt. Templar is sanguine of compressing the gas to a fourth of its volume, and thus decreasing its bulk considerably, when the balloon-train is on the march. How far this is practicable experiment only can prove.

THE WEATHER CASE, OR FARMERS' WEATHER INDICATOR[1]

Description

STAND facing and look at the weather case. Now: The right of the case is at your right hand; the left of the case is at your left hand.

The pointer or index at the top of the case (No. 1) slides on the brass arc; it is known as the "Sunset Barometer Index," and indicates, when set by the figures to which it points on the "Main Barometer Scale," which is just below it, the reading of the barometer at the time of the sunset yesterday.

The "Main Barometer Scale" (No. 2) exhibits all the barometric readings likely to be used with this instrument.

The pointer (No. 3) just below the "main barometer scale" is called the "mean barometer index," and indicates, when set, the mean or average reading of the barometer at the place at which the instrument is set and for each separate month. When the barometer reads above or below this reading at any place, such reading is said to be "above the mean" or "below the mean" for that place in that month. This index is set once for each month in the year.

When the barometer pointers go toward the right from this mean or average reading, the barometer is said to be "rising." When the barometer pointers go toward the left from the mean or average reading, the barometer is said to be "falling."

The mean barometer reading for each district for each month is stated in the *Farmers' Bulletin*, or can be had by application to this office.

The long brass hand over the glass face of the barometer is known as the "long pointer," and indicates, by the figures of the "main barometer scale" to which it points when set, the reading of the barometer when last set.

The black pointer on the face of the barometer under the glass face is known as the "short pointer, and indicates the existing pressure of the atmosphere at any time the instrument may be examined.

To Read the Barometer

If the observer stands facing the barometer the "short pointer" (black) moves toward the right as the pressure of the atmosphere (or weight of the air) increases, and to the left as the pressure of the atmosphere (or weight of the air) diminishes. The "long pointer" (brass) should be moved by the turning screw so as to coincide with, or exactly cover, the "short pointer" (black). The barometer is now set for reading, and the "barometer reading" is found by reading from the left to right on the "main barometer scale" from the lowest figures (or readings) on that scale to that exact division or mark upon that scale to which the "long pointer" points or which it covers. The inches and hundredths of inches are marked on the scale. The inches and hundredths are counted from left to right, or in the same direction as the hands of a watch move, and they are counted in the same way as the hours and minutes on a watch-face are counted. The inches and hundredths are written down, if they are to be written, in the same manner as dollars and cents are written, thus: one dollar and seventy-five cents; that is, one dollar and seventy-five hundredths of a dollar would be written $1.75, or one and seventy-five hundredths; $29.35, twenty-nine dollars and thirty-five hundredths. The "long pointer" pointing on the "main barometer scale" to twenty-nine inches and thirty-five hundredths, the barometer-reading would be twenty-nine inches and thirty-five hundredths of an inch, and would be written "29.35 inches," and so for other readings.

[1] Circular issued by the Signal Service of the United States Army—communicated by General Myer, Chief Signal Officer.

Rain Winds and Dry Winds

There are for each place and for each month two kinds of winds:—

First—Winds which, blowing from certain directions, are at that place and in that month more likely than any other winds to be followed by rain. These are called "rain winds."

Second—Winds which, blowing from certain directions, are at that place and in that month less likely than other winds to be followed by rain. These are called "dry winds."

The "rain winds" and the "dry winds" for each district and for each month are stated in the *Farmers' Bulletin*, or can be had by application to this office.

The wind direction for any day or time must be seen and taken at each place or station by a vane as well located as practicable.

The "wind disc" (No. 8) consists of a brass circle, on which slide freely two arcs—a red arc, called the "dry-wind arc" (No. 9), and a blue arc, called the "rain-wind arc" (No. 11). In the centre of the disc is a pointer, turning with a turning-screw, and called the "wind-disc pointer" (No. 10). Around the disc are letters to show directions, as N for north, E for east, NE for north-east, &c.

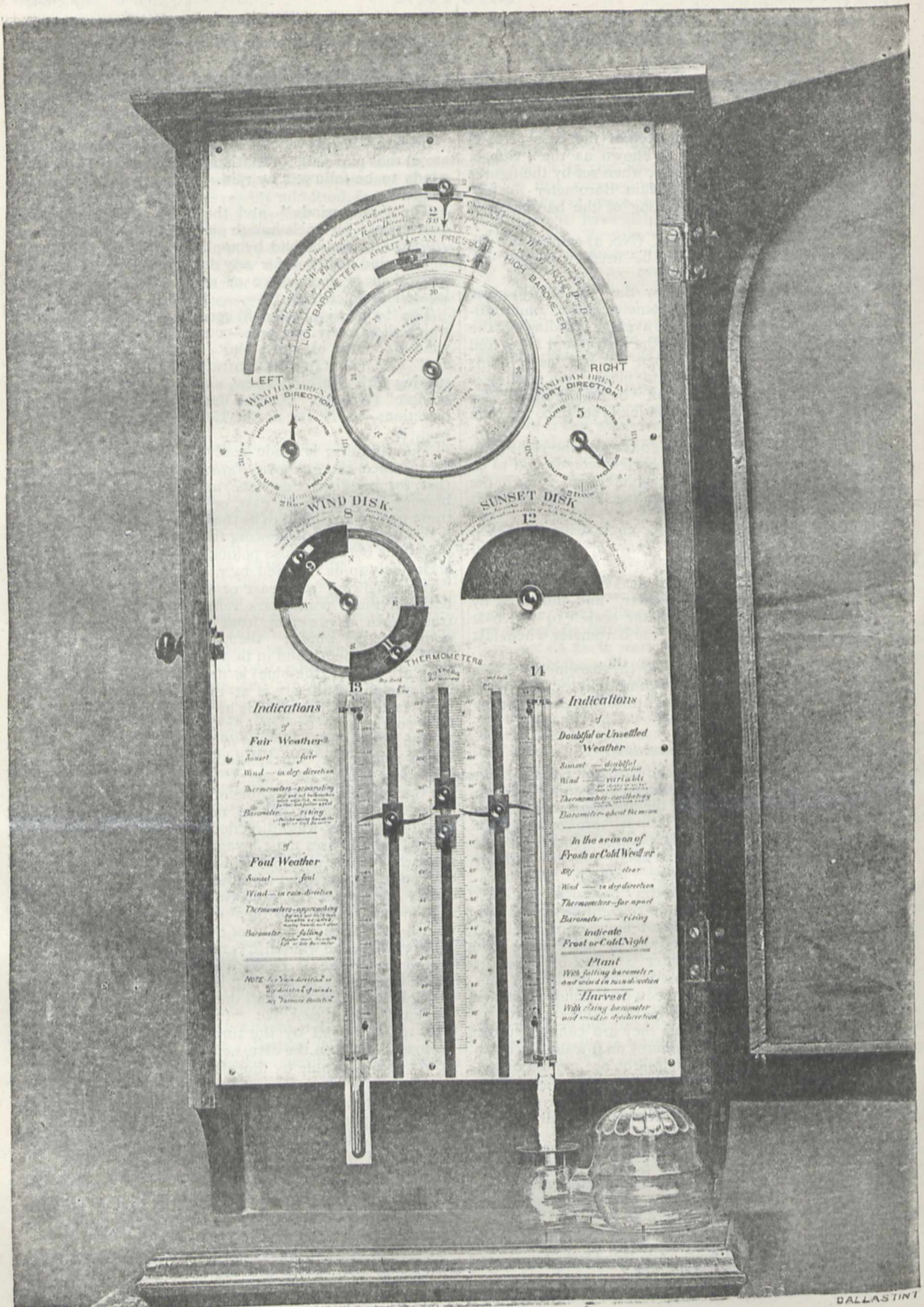
The wind disc is made ready for use as follows:—If, for instance, the *Farmers' Bulletin*, or other report, states that, for any district or place, and for any month, "winds blowing from south or east, or from directions between those points, are found to be the winds most likely to be followed by rain, winds blowing from north or west, or from directions between those points, are found to be the winds least likely to be followed by rain"—or, in other words, "winds blowing from east or south, or from directions between those points, are rain winds. Winds blowing from north or west, or from directions between those points, are dry winds;" then, if the instrument is to be used in that month and in that district or place, the rain-wind arc No. 11 (the blue) is moved on the brass circle until one end of the arc is at the letter E, which stands for "east," and the other is at the letter S, which stands for "south;" the dry-wind arc No. 9 (the red) is moved on the circle until one end of that arc is at the letter N, which stands for "north," and the other is at the letter W, which stands for "west." The arcs remain as they are thus placed for the whole of the month.

At the beginning of the next month the rain-wind direction and dry-wind direction must be located for that month, and the arcs must be again moved on the circle in the same manner until the rain-wind arc and the dry-wind arc touch, respectively, with their ends, the letters for the points named for the rain-winds and the dry-winds for that month. The arcs then remain so placed for that month—so for each month of the year.

Now, when the wind disc is thus ready, and on any day, the weather case is to be used. (1) The direction in which the wind is blowing is seen by a vane. It is noticed from what direction the wind is blowing, as from the north, south, east, &c. (2) The wind disc pointer (No. 10) is moved by the turning-screw to point to the compass-letters on the disc, or between them, showing as nearly as practicable by those letters the same direction, *i.e.*, that *from* which the wind is blowing when observed. If this pointer, so set, points at either of the ends of either arc, or any part of either arc, it shows, if it so points at the *red* arc, that the wind is at that time in the *dry*-wind direction, or is a *dry* wind; if it so points at the *blue* arc that the wind is at that time in the *rain*-wind direction, or is a *rain* wind.

The hour is noted by a clock or watch, and the time at which a rain wind or a dry wind commenced to blow, or was first noticed, is written down.

It must be also noticed if the wind shifts, as from blowing from a rain direction to blowing from a dry direction, or


LOW BAROMETER. ABOUT MEAN PRESSURE. HIGH BAROMETER.
LEFT
RIGHT
WIND HAS BEEN IN RAIN DIRECTION
WIND HAS BEEN IN DRY DIRECTION
HOURS
WIND DISK
SUNSET DISK
THERMOMETERS
Indications of Fair Weather
Sunset — fair
Wind — in dry direction
Thermometers — separating
Barometer — rising
of Foul Weather
Sunset — foul
Wind — in rain direction
Thermometers — approaching
Barometer — falling
Indications of Doubtful or Unsettled Weather
Sunset — doubtful
Wind — variable
Thermometers — oscillating
Barometer — about the mean
In the season of Frosts or Cold Weather
Sky — clear
Wind — in dry direction
Thermometers — far apart
Barometer — rising
indicate Frost or Cold Night
Plant
With falling barometer and wind in rain direction
Harvest
With rising barometer and wind in dry direction

DALLASTINT

is blowing from such direction as not to be within either arc.

The length of time for which the wind has blown continuously from a rain direction or a dry direction is of importance to be considered with other local signs and indications.

The pointer and scale (No. 5) on the right of and below the barometer are called the dry-wind time-record, and the pointer (No. 7) is called the "record-pointer," and indicates, when set, the length of time the wind has been blowing continuously from a "dry" direction, by the figures showing the number of hours on the scale to which it points.

The pointer and scale (No. 4) on the left of and below the barometer are called the rain-wind time-record, and the record-pointer (No. 6) indicates, when set, the length of time the wind has been blowing continuously from a "rain" direction, by the figures showing the number of hours on the scale to which it points.

The record pointer on the rain-wind time-record (No. 6) is always turned by the thumb-screw, and set pointing at the figure 0 on the scale when the wind is not blowing in the rain-wind direction. In the same way the "record-pointer" on the dry-wind time-record (No. 7) is always set pointing at the figure 0 when the wind is not blowing in the dry direction.

When in actual use, the hour at which the *wind-disc pointer* has been set is carefully noted. When the weather case is next examined, the wind-vane is again noticed, and the wind-disc pointer again *examined* or adjusted. If it still continues to point at any part of the same arc as before, the number of hours which have elapsed since the last setting and during which the wind-disc pointer has been so pointing is noticed, and the record-pointer on either the rain-wind time-record or dry-wind time-record is turned to show the *number of hours* the wind has been thus noticed as blowing from the *rain* direction or from the *dry* direction, as the case may be. This proceeding is repeated every time the instrument is set. It can thus be seen at a glance whether the wind is, or not, blowing in a rain-wind or a dry-wind direction, and for how long it has been so blowing. Whenever, on noticing the wind-vane, it is seen that the wind has shifted, the wind-disc pointer is set accordingly If it now points at neither arc, or points to the different arc from that at which it pointed at its last setting, the time-record pointer on the rain-wind time-record or dry-wind time-record (whichever may have been in use at the last setting), is turned to point at the figure 0 (zero).

The wind-disc pointer and the time-record pointer ought to be set thrice daily at least, early in the morning, at noon, and at sunset.

The sunset-disc (No. 12) consists of a circular disc, one half of which is coloured red and one half of which is coloured blue. The disc turns upon a central turning-screw in such manner that half of the disc shows through a semi-circular opening in the face of the weather-case. The sunset-disc is set as follows :—At the exact time of every sunset the western sky and the character of the sunset is carefully observed. The examination ought to be minute and careful, lasting for about fifteen minutes. If the sunset sky is clear or red, or markedly what is known as a "fair weather sunset"—a sunset such as is generally held to indicate a clear or fair day to follow on the next day, a day on which it will not rain—the sunset-disc is turned by the turning screw until the semi-circular opening shows all red. The sunset-disc, thus turned, is described as set for a "fair weather sunset."

If the sunset sky (the western) is cloudy or foul, or markedly what is known as a "foul weather sunset," a sunset such as is generally held to indicate foul weather to follow on the next day—a day on which it will rain—the sunset-disc is turned by the turning screw until the semi-circular opening shows all blue. The sunset-disc thus turned, is described as set for a "foul weather sunset." If the appearance of the western sky and the character of the sunset are neither markedly those of a "fair weather sunset" or of a "foul weather sunset," but such as to leave the observer in doubt how to style it, the sunset-disc is turned to show half red and half blue, or "doubtful." The sunset-disc, thus set, is described as set for a "doubtful weather sunset."

The term "fair weather sunset" expresses such condition of the sky, particularly the western, and such character of the sunset, as is considered to indicate a fair day, a day on which it does not rain, for the day ensuing.

The term "foul weather sunset" indicates that the appearances are such as to presage a rainy day, a day on which rain falls, for the ensuing day.

The term "doubtful weather sunset" indicates that the conditions are such as to leave the mind of the observer in doubt as to what the sunset presages for the following day. The indication is considered to be for the period of time from the "sunset" of the day on which the character of the sunset is examined until the "sunset" of the following day.

Dry-bulb and Wet-bulb Thermometers

In the lower part of the weather case there are two thermometers, a dry-bulb thermometer (No. 13) on the left-hand side of the case, and a wet-bulb thermometer (No. 14) on the right-hand side.

The dry-bulb thermometer is like any other thermometer, and shows by its readings the temperature of the air.

The wet-bulb thermometer is one, the bulb of which is kept constantly moist by the water passing up from the glass reservoir, through the wicking which covers the thermometer bulb.

The readings of the dry-bulb thermometer and those of the wet-bulb thermometer, are more and more unlike, or farther and farther "*apart*," as it is called, in proportion as the air contains less and less moisture; that is, is becoming *dryer*.

The readings of the dry-bulb thermometer and those of the wet-bulb thermometer become more and more *alike*—are nearer and nearer together—in proportion as the air contains more and more *moisture;* that is, is becoming saturated or *wet*.

By the side of the dry-bulb thermometer (No. 13) is the dry-bulb pointer which slides on the brass slide (No. 15). By the side of the wet-bulb thermometer is the wet-bulb pointer which slides on the brass slide (No. 16). In the centre of the case is the "dry- and wet-bulb scale," marked on the paper on which is the central brass slide-bar (No. 19), and on this slide move the dry-bulb keeper (No. 17) and the wet-bulb keeper (No. 18). To set the thermometers examine first the dry-bulb thermometer and move the "dry-bulb pointer" (No. 15) on the slide until the outside point is exactly level with the top of the mercury in the thermometer—as near to it as practicable. Examine next the wet-bulb thermometer, and move the wet-bulb pointer (No. 16) on the slide until the outside pointer is exactly level with the top of the mercury in the wet-bulb thermometer, or as near to it as practicable, then turn to the "dry- and wet-bulb scale," and on the "central brass slide-bar" (No. 19) move one of the keepers until it touches, as nearly as possible—is on an exact level with the inside pointer of the "dry-bulb pointer," then move the other keeper until it touches, as nearly as practicable—is on an exact level with the inside pointer of the "wet-bulb pointer." The thermometers are now set, and the difference between their readings can be known by counting on the "dry- and wet-bulb scale" the number of degrees between the keepers.

When the thermometers are examined and set again, following the same plan, it will be easily seen whether the "keepers" are, when set, *farther apart* than they were at the previous setting, or whether they are, when set, *nearer together* than at the previous setting.

If they are farther apart the thermometers are said to be "separating." If they are nearer together the thermometers are said to be "approaching." Other things being equal, the thermometers show, when they are "separating," that the air is becoming more dry—one sign of approaching fair weather. The thermometers show, when they are "approaching," that the air is becoming more moist or damp—one sign of approaching rain.

The reservoir at the bottom of the weather case ought to be kept half filled always with pure water. The wicking must be kept clean and changed occasionally—say, once in each month.

Uses of the Weather Case

The weather case is not intended to be used independently of the official weather reports. It is to be used always in connection with them. The weather case is for the purpose of supplementing the official reports by showing the local instrumental indications, and giving other information. It is intended especially for use at Farmers' Post Offices and places reached with difficulty by the printed reports. It will supplement often whatever knowledge there be of local signs, with the indications of the instruments. Its careful use taken either with the furnished reports or even without them (if they chance to fail), will often enable the character of the coming weather on the coming day to be so judged as to determine what kind of work or undertaking it is wise to plan for or to omit. The case gives the local instrumental indications, and will frequently aid in making fair forecasts for the next day.

It is well to limit the forecasting to the attempt to tell only whether it will or will not *rain* on the next day. Days on which it does not rain at all, are rated "fair," though the sky may be covered with clouds. Days on which there is rain enough to injure crops in the harvesting, are rated "foul."

It must never be forgotten that the weather case is only to aid, sometimes, in making up one's mind as to what the weather of the next day will be. While it will often be very useful, there will be many instances in which everything will be left in doubt.

Location

The weather case should be hung or stand in a fair light, where it will be always shaded, preferably on the northern side or part of the house, where it will not be exposed to artificial heat, and where there will be a free circulation of the air. It will be easier to find good locations in summer than in winter. As the readings of the instruments are examined for their general indications only, the great care as to the location will not be so needed for general use as if the readings were for exact record. For particular uses, the case may be particularly sheltered.

To Use the Weather Case at Sunset

(1) Read the description: See that the mean barometer index is set to the mean barometer reading for the place and for the month.

(2) Set the rain wind segment (or arc) and the dry wind segment (or arc) at the proper places on the wind-disc circle for the place and month.

At sunset turn the "long barometer pointer" until it covers the "short barometer pointer." Note whether on the "main barometer scale" the long pointer is then at a reading *above* or *below* (greater or less than) the mean barometer reading for the place and for the month. Move the "sunset barometer index" until it points to the same reading on the "main barometer scale" with the "long barometer pointer."

Note the direction of the wind and set the wind-disc pointer.

Set the sunset-disc.

Set the dry- and wet-bulb thermometers.

Endeavour to apply the "indications" printed on the face of the case, to determine what is to be the character of the next day. Read these "indications" carefully, and see how many of the instruments or discs *agree* in showing one or another kind of weather as to be expected.

Study the character of the clouds. The scud cloud is one of the prominent signs of coming rain. Learn to apply the local signs of weather changes, the more the better.

Examine the case frequently during the day.

It can always be found whether the "pointer" is moving toward the right or left—that is, whether the barometer is rising or falling—by turning the long pointer, so as to cover exactly the black barometer pointer; if then, on next examination, the black pointer is found to have moved toward the right, the barometer is rising. If the black pointer has moved toward the left, the barometer is falling.

At sunset of the next day set the case again; note whether the barometer has risen since the "Sunset Barometer Index" was set at the last sunset, whether the wind is in the rain or the dry direction, and for how many hours it has been in either, approximately. Study and record the character of the sunset and what it foretells. Set the wet- and dry-bulb thermometer, and note from the "keepers" whether the thermometers are approaching or separating.

Endeavour to make a forecast whether it WILL or NOT rain for the ensuing day, as well as a more general forecast.

For instance, if with a "fair weather sunset" there is a "high and rising barometer," "winds in dry direction," and "thermometers separating," the chances for a fair day for the next ensuing are increased; four indications of pleasant weather coincide. If, with a "foul weather sunset," the barometer is falling" (the pointer moving toward the left), the winds are in the rain-wind direction, and "thermometers approaching," four indications of rainy or unpleasant weather coincide. So three indications may coincide, as, with "thermometers approaching" there may be "fair weather sunset"—barometer pointer moving toward the right or high barometer—wind steady in the dry direction. The indications may be divided. In such cases weight must be given the different indications as experience may show their correctness. The purpose of the case is to be one aid only in making up a forecast.

The greater the number of the "indications," as stated on the face of the case, which are found when the instrument is set, to coincide in indicating dry or clear weather, or rainy or stormy weather, the more likely the success of the forecast.

Continue this practice at sunset from day to day. Skill will be found to increase with the knowledge and the use of the instrument and the habit of close observation of local signs which such use makes necessary.

In instances where the printed synopses and indications are had regularly, or where official weather charts, bulletins, &c., can be consulted, the weather case must be used with careful study of those papers. Locate the areas of high and low barometer on the map, and in reference to any location, as nearly as is practicable from the descriptions or data. Areas of cloudiness, rain-areas, areas of unusual temperature, &c., may be similarly located. When any such areas are found to the westward of and at all near any place, it is rare that the effects of them fail to be shown at such place by weather conditions similar to those within the areas noticed, and in a short time, as of hours or days. As the movements of the areas, easterly, have a certain regularity, it is soon learned to tell nearly *when* the effect of any area noticed as approaching will commence to be felt. The instrumental signs of the weather case will show the changes commencing, and it may be judged whether or when the weather conditions noticed in the approaching area are likely to prevail. Some elementary knowledge of meteorology is needed; but much can be done by a careful watching, solely. The instruments of the weather case will show the changes anticipated when they begin to be felt and before they have arrived in their full force.

If the area noticed has been large or small, with weather conditions decided or variable, it can, from these facts, be judged often if the "coming weather," of whatever kind it may be, will be steady for a day or for days, or more rapidly changing. The case makes practicable other and many studies.

ALBERT J. MYER,
Brig.-Gen. (Brevet Assg'd.), Chief Signal Officer, U.S.A.

WAR DEPARTMENT,
Office of the Chief Signal Officer,
Washington, D. C., July 21, 1878

ARE THE "ELEMENTS" ELEMENTARY?[1]

II.

IT would be a curious speculation were one to ask one's self what *is* the atomic weight of ozone? Is it 24? Is its atomic formula O1·5? or has oxygen the atomic weight of 32 and ozone of 48, and are the molecular weights 64 and 96 respectively? This can scarcely be, for the smallest amount of oxygen in two volumes of a gaseous compound of that element is certainly not 32, but 16 parts by weight. In fact *atoms* of allotropes scarcely appear to exist, the molecule appears to be the smallest amount of one of these substances that can exist either in, or out of combination. But can elements really exist in combination under various allotropic forms? We do not know. Weber thinks they can, Clarke thinks they cannot. The example of graphitic acid sometimes cited as proof of the existence in the combined form of allotropic carbon does not appear to me to *prove* either one view or the other. Graphite, may we not say, is an intermediate stage in the formation of graphitic acid from carbon? But it does not follow that the carbon in that acid is in a form different from that under which it exists, say in sugar. But it is exceedingly difficult, as yet, to attach a definite meaning to such a statement as "Carbon exists *under different forms* in this and in that compound."

The specific heats of allotropes vary. Weber has most carefully determined the specific heats of the modifications of carbon and boron. The numbers obtained at low temperatures are different, but when we come to those temperatures at which optical differences disappear, we find that differences in specific heats disappear also. At high temperatures there is but one specific heat for carbon and but one for boron; at low temperatures there are two or more specific heats for each. This seems to mean that at sufficiently high temperatures there is but one form of carbon and but one form of boron. As we do not know the atomic or molecular weights of the allotropic modifications of these elements, we can, it seems to me, draw no conclusions of any value concerning the atomic heats of these allotropes, and therefore the fact that the atomic heat of the elements is a constant number may be explained equally well on the hypothesis that the elements are all allotropes of one of themselves, or of an unknown substance, and on the hypothesis that the elements are essentially distinct forms of matter. It is well to bear in mind that, so far as our knowledge of allotropy goes—and it goes but a very little way—we have reason to believe that each allotrope has a different molecular, and therefore, probably, a different atomic weight, from every other allotrope of the same element; and, further, that we know that allotropes are at high temperatures resolved into one and the same form. Phosphorus has an abnormal vapour density: two volumes of the vapour of this body contain four (relative to hydrogen as two) atoms; red phosphorus (P_β) changes into common phosphorus (P_a) at comparatively low temperatures, therefore we do not know the vapour density of P_β. At a point not far from its boiling point, two volumes of the vapour of sulphur contain six atoms (if $S = 32$); at a higher temperature two volumes contain two atoms. We appear to have here a real gaseous allotrope. Is it analogous with ozone? Is it not therefore probable that the densities of P_a and P_β would be found to be different, supposing they were both obtained as vapours? But even P_a is abnormal. May it not be, then, that we have not as yet obtained normal phosphorus at all? That what we call phosphorus is an allotrope of the true phosphorus, viz., P_2? May it not be that at very high temperatures P_a splits up and yields true normal phosphorous vapour? Now let us briefly glance at isomers, or compounds having the same composition, but with different molecular weights. The mere fact that compounds of the same composition, but different molecular weights, exist, especially when taken in conjunction with the further fact that different compounds having the same composition and the *same* molecular weights also exist, renders the theory that the elements are really compound bodies not altogether improbable. So little has been done in the way of exact determinations of the specific heats, specific volumes, and other physical constants, of isomeric bodies, that I forbear from pressing the facts that are known into the argument, but content myself with saying that a more or less simple relation appears to exist between the physical and the chemical nature of the various isomers. The generally accepted theory of isomerism seeks to account for the facts by supposing that the atoms of isomers having equal molecular weights are differently arranged: another theory—to me it appears that the one theory is complementary of the other—supposes that the differences in the action of isomers are to be traced to differences in the amounts of "energy" possessed by these isomers (would it not be better to say, to differences in the relations between the potential and kinetic energy of isomers, and also perhaps to differences in total energy?); whichever theory is applied to isomers may be equally applied to the elements, on the assumption that these bodies are really compounds of one simple form of matter.

The positive evidence in favour of the theory of the non-elementary nature of the so-called elements is not very great. Yet, to say that the elements *are* truly elementary is, I am persuaded, a statement which is not justified by the facts which we possess. Either hypothesis may be adopted as a working hypothesis: the former, that the elements are *not* elementary, is, it seems to me, likely to lead to more discoveries, and to pave the way to more far-reaching generalisations than the latter.

But why should no one have succeeded in decomposing one of the so-called elements? In a sense we have succeeded. Ozone is an element, but it can be decomposed. Oxygen may, I think, be said to be a simpler form of matter than ozone. The introduction of the battery into chemistry led to the decomposition of potash and soda; the introduction of new engines of research may lead to the decomposition of some of those bodies, our conclusions regarding the elementary nature of which rests upon the same kind of evidence as did the conclusions regarding the elementary nature of potash and soda before the experiments of Sir Humphrey Davy. Analogy prompts us to ask, Is it not possible that what we cannot accomplish in our earthly laboratories may be actually brought about

[1] A paper read before the Owens College Chemical Society. Continued from p. 593.

in the great natural laboratories of the sun or stars? And facts recently observed by Lockyer give some countenance to those who would answer this question in the affirmative. The spectra of compounds are as a rule more complex than those of elements. The former bodies, speaking broadly, yield channelled or band spectra, the latter line spectra. The non-metallic elements, also, yield spectra which may be generally described as channelled or band spectra, while the spectra of the metals are more to be described as line spectra. Again, with increase of temperature band tend to change into line spectra; with increase of temperature compounds tend to be decomposed into their constituent elements. But the solar spectrum is a line spectrum, the spectra of certain stars—Sirius, &c.,—are line spectra, but simpler in their character than that of the sun; the hydrogen line is predominant in the Sirian spectrum. The spectra of certain other stars—*e.g.*, the red stars—are very complicated, and consist for the most part of bands and channelled spaces.

Putting these facts together, the hypothesis has suggested itself to Lockyer that the atmosphere of the red stars contains certain compounds and many non-metallic elements, that the atmosphere of the sun is characterised by the presence of metals, there the non-metals are decomposed into simpler forms, that those elementary bodies which are not found in the hotter part of the sun's atmosphere *are being* formed in the upper and cooler portions, but as they descend they are again dissociated, and lastly, that in the very hot stars our elements are for the most part resolved into simpler forms. Hydrogen, however, appears to exist there in the form in which it is known on this earth. This hypothesis is put forward tentatively by its author, and must only be accepted as a working hypothesis. It is most interesting to the chemist; in some of its bearings it also tends to throw light upon the physical conditions of the existence of stars and suns. Yet it is only an hypothesis, we must beware of accepting it as a dogma.

There is one point as bearing on Lockyer's hypothesis to which I should wish to direct attention. Non-metals show more variations in their spectra than are exhibited by the spectra of the metals. Metals yield, as a rule, only line spectra, non-metals channelled, then line spectra, as the temperature increases. Analogous with these changes in the spectra of non-metals is the well-known "plasticity" of these bodies. The instances of allotropy about which we have any accurate knowledge are instances among the non-metallic elements; probably, then, these bodies will be more readily decomposed than the metallic elements.

Putting together all that is known on the subject, the balance of probability appears to me to be in favour of the hypothesis that the elements are not really elementary. But if one is asked to put forward a positive hypothesis, not merely to favour a negative one, the task becomes much harder.

In a short, but exceedingly suggestive paper ("Speculative Ideas respecting the Constitution of Matter," *Phil. Mag.*, February, 1864), Graham tentatively put forward the hypothesis of original matter having a molecular or atomic structure, all the molecules being uniform in size and in shape, but not all possessed of the same amount of motion. In the differences in the motions of the parts of this original matter Graham sees the origin of all differences in the properties of our various elements. The gaseous molecules which we are accustomed to measure are not, says Graham, to be regarded as the ultimate molecules of the original matter, but as composed of a "group or system" of these. This hypothesis of Graham appears to me to be one of great merit. But if we start with one matter, whose molecules are of equal mass, may we not imagine these molecules originally possessed of equal amounts of motion? Having got these molecules, it is not, I think, beyond the powers of the scientific imagination to regard some of them as coming within the sphere of each others' action, and as coalescing to form new compound molecules, the mass of such new molecules being of course different from that of the original molecules. After such an encounter the new molecule will possess an amount of energy different from that possessed by the original molecules; hence it will exhibit new properties. The original matter has thus become differentiated; we have now more than a grained structure, the grains vary in mass, and in the amounts of energy which they possess. This process of evolution of higher and higher orders of molecules may proceed (may be now proceeding) until we arrive at those systems which are at present generally regarded by chemists as the molecules of distinct forms of matter, as the elementary bodies of to-day. These elementary bodies are again ready, under proper conditions, to form yet higher orders of molecules; these are our compounds, but these higher orders are less stable, under average conditions, than the lower (elementary) orders of molecules. If by any means a very large amount of energy be added to our elementary molecules they would tend to dissociate and to reform the simpler orders or groups from whence they have been derived. Such addition of energy appears to be given in the intensely hot atmosphere of the sun, where metallic bodies may seemingly remain in company with heated oxygen, yet unoxidised. If, however, a small amount of energy only be given to an element, then that element becomes ready to unite with another, or with others, to form a compound body. The bodies which we call elements would, on this hypothesis, be but intermediate stages in the evolution of complicated compounds from one original form of matter. At certain stages in this process points of comparative rest are reached; one of these points marks the existence of our so-called elements. Bodies which are elementary in our laboratories are compounds in the more energetic laboratories of the sun and hot stars. Many of our compounds, again, are elementary in the cold, listless atmosphere of the moon. Just as it is very difficult, if not impossible, to define a chemical compound, to say where the mere mechanical mixture or aggregation ends and the true chemical compound begins, so, in this view, would it be impossible to define a chemical element. Whether a substance is compound or elementary depends upon the point of time at which the investigation is made and upon the conditions of the environment. Graham has pointed out that the "colloidal state" seems to intervene between the liquid and crystalline states; the experiments of Faraday, of Caignard de la Tour, of Andrews, and more recently of Pictet and Cailletet, have taught us that between the gaseous and liquid states there is no hard line of demarcation; many facts in chemistry and in chemical physics appear to be explicable only on the supposition that the passage from mechanical mixture to chemical union is a gradual and continuous, not an abrupt and discontinuous one. Why, then, should not the passage from the one original element to the one final compound be also a gradual passage?

And as in animate nature we know that the (comparative) permanence of a species is in no way contradictory to the general law of gradual development, so in the history of molecular arrangements it may be that the present permanence of our so-called elements only marks a resting point in the slow but sure process of formation of more and more complicated compounds. The average conditions of our present surroundings may not allow of the existence of any less complicated molecular aggregations than those which we call elementary, just as they do not appear to allow of the existence of any extremely complicated aggregations of chemically united molecules.

There is another aspect of the question upon which one word may be said. I think that I am not in error in saying that the minds of most persons are imbued, more or less deeply, with the idea that nature is ultimately very simple; that could we but grasp the great laws of nature we should find them extremely simple, although the results of their actions are so wonderfully complex. This belief appears to be deep-rooted in most minds, yet if we are to study nature aright we must, I am persuaded, set it aside. We must be content to take nature as she is, *i.e.*, we must do our best to amass facts, and from these we must draw the conclusions warranted by the facts. Now as our knowledge of nature extends do we find that she becomes to us more and more simple? Yes, and No. It has been now and again given to a gifted few to pierce through the maze of tangled facts and to espy the great principle which binds them into an harmonious whole. But even in the case of these great generalisations, exact experiment and observation frequently show that little details have been overlooked—that the great simple law is too simple—that there are discrepancies, *very small*, it is true, but still there they are, demanding an explanation, telling us that our law does not express the whole of Nature's facts. Nature appears to be truly infinite; and it is well to remember that, *we can never get sensibly nearer a knowledge of an infinity*.

This idea of the simplicity of nature is very apt to lead us to adopt the hypothesis of the non-elementary nature of the elements without sufficient evidence. The idea that all the elements are really compounds of one primary form of matter is a most fascinating idea, it *seems* to be so much in keeping with the simplicity of nature; it is so symmetrical, it surely *must* be true. This is just how the old alchemists reasoned; we must absolutely forbid these *à priori* conclusions to influence us as students of nature. The hypothesis of the compound nature of the elements, of the existence of but one, or even of a few primary forms of nature, fits in with the nebular hypothesis of the formation of the worlds, but have we facts to support it? If one can only come back to facts we need not fear to start what may appear to be wild and romantic theories.

The outcome of the whole matter is this:—we want more knowledge, our facts are few and vague; there is room for almost unlimited work. Ask Nature; trust her: be sceptical of your own interpretations of her answers. M. M. PATTISON MUIR

THE LATE SIR RICHARD GRIFFITH, BART.

WE have just laid to rest all that is mortal of the "Father of Irish Geology" in Mount Jerome Cemetery, at the ripe age of ninety-four years. Few public men in Ireland have done so much for the material advancement of their country. If "the age makes the man" the late Sir Richard Griffith was the man whom the age called forth to indicate the road to material improvement at a time when roads, railroads, drainage works, and similar agents were urgently required in this country. Griffith's geological knowledge was the basis of his power; and while few understood, or cared to understand, the principles by which his judgment was guided, Government and the public were always ready to put faith in their application. Amongst the useful works carried out under his direction were the roads which he constructed or improved in the counties of Cork, Kerry, and Limerick, during the time when the Marquis Wellesley was Lord-Lieutenant. Some of these roads are striking examples of engineering skill. I have recently travelled on one of them, namely, that which crosses the wild and rocky range between Kenmare and Glengarriff. Before this road was made the country was inaccessible and the haunt of Whiteboys; now no district in the British Isles is safer for the traveller, and, I may add, more full of bold and beautiful scenery. With reference to Griffith's services to the cause of Irish geology, it is unnecessary for me to say a word here, except in so far as regards the public department with which I have the honour to be connected. It is to this subject that I wish especially to direct the attention of the readers of NATURE, as I am anxious to pay a tribute to the remarkable acumen which Griffith exhibited in determining the age of the various formations which are to be found in this country, as exemplified in at least one special instance.

It is well known that there is one point in the geological structure of the south-west of Ireland on which there has been a difference of opinion between the Government geological surveyors and Sir R. Griffith; I refer to the age of that great group of rocks which occupies the mountainous districts of the Dingle promontory, and those of Killarney, the Reeks, and Glengarriff. These were called by the late Prof. Jukes "The Dingle Beds," and they consist of a series of purple slates traversed by cleavage planes and massive green grits, and thrown into numerous grand flexures. They are of great but unknown thickness, as in the Dingle promontory they are overlaid unconformably by the beds of the Old Red Sandstone. In the Dingle promontory these beds are seen in contact with fossiliferous beds of recognised Upper Silurian age, and the whole series had been referred by Griffith to the "Silurian" formation, as may be seen by reference to his geological map of Ireland (edit. 1855). In a similar manner the mountainous regions abovenamed, and lying to the south of Dingle Bay, were mapped and coloured as "Silurian," and were separated off from the Old Red Sandstone throughout the counties of Cork, Kerry, and Waterford.

The views thus held and published by Griffith with regard to the geological age of the rocks forming the south-western highlands were not upheld by the officers of the Geological Survey, who, we may be sure, spared no pains to come to some clear decision on the question. On the maps of the Survey the mountains of Kerry and Cork are coloured "Old Red Sandstone," and "the Dingle Beds," with a distinct colouring, are placed in a position intermediate between the Old Red Sandstone and that of the Upper Silurian. Prof. Jukes, in the "Explanations" to accompany the maps of the Survey, has fully entered into the reasons which induced him and his able colleagues to arrive at this decision. Certain apparent obscurities in the sections of the Dingle district and those of the neighbouring regions prevented them accepting Griffiths' views, and the whole matter was left an open question, subject to further investigation.

Under these circumstances—the time being favourable—I received the sanction of the Director-General to make a preliminary examination of the sections at Dingle and in the districts of Killarney, Kenmare, and Glengariff—with a view (if possible) of coming to some decision on a question which has been confessedly left in an unsatisfactory position. In this tour I was accompanied by Mr. J. O'Kelly and Mr. A. McHenry, officers of the Survey—and we have returned from it fully satisfied in our own minds of the correctness of Sir R. Griffith's views regarding the age of the beds of the Dingle, Killarney, and Glengarriff Ranges. To our minds the evidence is clear and satisfactory that these beds are really of Upper Silurian age, as maintained by Griffith. Into this evidence I cannot enter here, but hope to do so at some length in another place. It was with great gratification that some days since I addressed a letter from Eccles' Hotel, Glengarriff, to Sir R. Griffith, announcing the result of our investigations. I little knew that at that moment the spirit of our venerable friend had passed away! Few men were less dogmatic in maintaining their conclusions than Griffith. If others differed from him he remitted the matter to the arbitrament of time, satisfied that if he was in the right time would show it. In *this* case it is only justice to his memory to bear testimony to the soundness of his judgment. EDWARD HULL

Geological Survey Office, Dublin, September 27

ROBERT HARKNESS, F.R.S.

ANOTHER of the captains in the phalanx of British geologists has dropped from the ranks. Robert Harkness died suddenly in Dublin on Saturday last. He had been ailing for some time, and the disease from which he suffered—an affection of the heart—had gained ground so much this year that he lately felt himself compelled to resign the chair of geology at Cork. It was the expectation of his friends that, released from duties which he had so long conscientiously performed, he might yet enjoy some years of comparative health in the quiet retirement of his Cumberland home, to which he used to return with such pleasure every summer. But this was not to be. He has fallen just as he had himself brought the public labours of his life to a close.

It is now some five-and-thirty years since the name of this able geologist first appeared as a writer on his favourite science. During this long period he had explored, on foot, the geology of large districts in the north of England, in Scotland, and in various parts of Ireland. The reports of the British Association and the *Quarterly Journal* of the Geological Society bear witness to his industry and to the painstaking minuteness of his method of investigation. To him we owe our earliest exact information regarding the correlatives of the reptiliferous sandstones of Dumfriesshire and Cumberland. It was his patient labours continued year after year over ground most difficult to unravel, that led the way to the working out of the structure of the silurian uplands of the south of Scotland. To his research, too, is due the identification of the metamorphic rocks of the north-west of Ireland with those of the west of Scotland. To the elucidation of every one of the palæozoic systems of deposits he contributed something of value.

But important as was his scientific work, it had not a wider and more hearty recognition among his brother geologists than his own admirable qualities of head and heart. Who that has been privileged with his friendship will not cherish the memory of his earnestness over even the driest of details, his quiet enthusiasm, his generous admiration for the work of others, his unfailing cheerfulness? Who will forget that beaming ruddy face, never absent from the platform of Section C at the British Association meetings, always ready to rise among the speakers there and to reappear at the festive gatherings in the evening? There have been men who have graven their names more deeply on the registers of scientific thought and progress, but there have been few whose sunny nature has more endeared them in the recollection of their friends than Robert Harkness. A. G.

MANGANESE NODULES IN LOCH FYNE

ON September 21, this year, I anchored the steam yacht *Mallard* near the mouth of Loch Fyne, in 104 fathoms, for the purpose of making physical and chemical observations on the water of this, the deepest part of the Firth of Clyde. When the anchor was got up a large mass of clay and shells was found sticking to one of the flukes. It was gently dried, and on examining it I observed a number of nodular concretions, which, on being freed from the surrounding clay, presented a finely mammillated black surface, were easily cut with a knife, giving a brownish-black powder, which liberated chlorine from strong hydrochloric acid, and possessed all the properties of peroxide of manganese; in short, they were identical with the manganese nodules which we found in the *Challenger* to form so important a constituent of the sea-bottom in the greatest depths.

One half of the dried mud was carefully broken up and searched through, the nodules being collected by themselves and also the shells. It was thus separated into three portions, which were weighed, with the following results:—

Manganese nodules ...	142·7 grammes. ...	30 per cent.
Shells	35·0 ,, ...	7·5 ,,
Sandy clay	289·0 ,, ...	62·5 ,,
Total	466·7	100·0

The manganese nodules, therefore, made up thirty per cent. of the weight of the mud. Compared with those frequently met with on board the *Challenger*, the nodules were small. In the sample examined there were eighty-three nodules weighing 142·7 grammes, hence the average weight was 1·7 grammes. Their volume was found to be 58 c.c., so that the average volume was 0·7 c.c., and the specific gravity 2·46. Their form was roughly spherical, the largest, which was somewhat elongated, measured 13 × 9 × 6 millimetres, the average diameter of them all being 11·4 millimetres.

Of the eighty-three nodules so obtained I have split twenty-two. When subjected to this treatment they are found to differ in constitution from the majority of those obtained on board the *Challenger*. Although they had not been exposed to any heat they were hard and sandy to the knife, and when treated with strong hydrochloric acid, they left a large amount of mineral (chiefly quartz) sand. This difference, however, is explained by the different kind of bottom from which they were obtained. In dissolving up nodules which had come from "red clay" in 2,500 or 3,000 fathoms I always found the same mineral sand left as on treating the clay in the same way. But the amount of sand was always quite insignificant, as compared with the clay; hence the nodules were easily cut with the knife. They, however, got harder on keeping. In Loch Fyne the bulk of the mud consists of quartz sand, giving the nodules the appearance of sandstone, whose binding material is made up to a great extent of peroxide of manganese, and hence the gritty feeling to the edge of the knife.

Where a hard nucleus has been found it has always been a piece of rock from the neighbouring shore, but in most instances (in sixteen out of twenty-two examined) the ordinary arrangement has been reversed, the nodule consisting of a soft rich nucleus of peroxide of manganese, surrounded by a black sandy rind, the whole enveloped in the characteristically mammillated black skin.

I hope very shortly to be able to report more fully on them; in the meantime, I have only been able to verify their nature by finding abundance of a higher oxide of manganese, easily recognisable quantities of cobalt, and the presence of water, which, on being expelled by heat, has an alkaline reaction and an empyreumatic odour, properties in which they agree with those which I had occasion to test on board the *Challenger*.

Their position in the mud, with dead shells above, below, and on all sides of them, will, when carefully studied, no doubt throw much light on their age and method of formation. I have observed two nodules firmly attached to the interior of shells, one having evidently been directed in its growth by the shape of the shell.

In endeavouring to procure a further supply I dropped anchor in about the same depth, but about a hundred yards further down the loch, and I obtained about the same amount of mud, but it contained very much more shell and no nodules. Also in Kilbrennan Sound, between Arran and Cantyre, in a depth of eighty-five fathoms, there was much shell and pebble, but no nodules. So far, therefore, this occurrence appears to be very local.

J. Y. BUCHANAN

STATE AID TO SCIENCE

UNDER this title the last number of the *Lancet* has the following:—

It was for a long time the fashion with zealous workers in the field of science to protest that the cause of discovery would not be advantaged by State patronage or State aid. The more thoughtful inclined to the belief that the patronage would be more mischievous than the pecuniary assistance was helpful. For some years past this persuasion has been losing ground, and, whether scientists are becoming more worldly or less exclusive in their views, it is abundantly evident that the reverse of a feeling of unwillingness to accept aid from the State prevails. It will doubtless be contended that the way in which help has been placed at the disposal of explorers and investigators so completely removes all difficulties, that it would be not less ungracious than impolitic to refuse the proffered assistance. The labourer chooses his own form of enterprise, or applies for aid in the course of an inquiry instituted at his own wish, that he has simply to satisfy a committee of fellow-workers as to the object of his pursuit or the nature of his researches, and, upon their recommendation, the necessary funds are forthcoming, without the least semblance of dictation or interference. The explanation is obviously satisfactory, but the fact remains, the objections which many of the older and more successful discoverers urged against seeking or accepting the assistance of the State in their investigations have been discarded, and the only grievance felt by contemporary inquirers relates to what they conceive to be the paucity of the grant and its wrong distribution, for which last fault, if fault there be, the governing bodies of the principal scientific societies are mainly responsible. We offer no present opinion as to the comparative merits of the old and new view of the State aid question, and we do not propose to discuss the complaints arising out of the system of administration extant; it is for the more pressing issue, whether a permanent provision should not be made for the support of men who live by investigation rather than teaching, we now ask a few minutes' attention.

It must be conceded by all who have any acquaintance with the subject, that not only the pioneers, but, in a practical sense, the advanced workers in science must necessarily be debarred from the ordinary rewards of their profession. If it be difficult to practise and preach, it is incomparably more embarrassing to study and teach. In short, the explorers and investigators in any department of work cannot live by communicating the knowledge they accumulate. The business of utilising the store of information amassed by the labourers in the advanced field must be performed by men who are not themselves engrossed with research. The enterprise of discovery cannot be delayed while the explorers strive to popularise their acquisitions, nor are the faculties which prove most useful in the field of inquiry especially well adapted for successfully imparting the knowledge obtained. The functions of the scholar and the schoolmaster, the collector and distributor, are essentially different; we might go further and affirm that they are scarcely compatible. It follows, therefore, that the two classes of workers must always exist. Some must make knowledge and live by that form of labour, while others distribute or apply it to general uses. It seems to follow, without the need of argument, that by some expedient the means of self-support must be placed within reach of those who are not in a position to render their produce marketable. This necessity has been long recognised, and with a view to meet the case, College fellowships and snug sinecures in the Church and at Law have been preserved for the shelter and support of those who required scholarly leisure for the pursuit of inquiries. The spirit of the age is, however, eminently utilitarian and strongly opposed to sinecures; and, as a matter of fact, the system was manifestly open to abuses. The medical profession has never largely enjoyed these advantages, although the discoveries made by its members are equal, if they do not surpass, those of any other branch of labour in science. The time has come when the whole question needs to be discussed from a new standpoint, and in a more practical fashion than hitherto. We venture to suggest that it should not be left in the hands of interested persons who cannot speak freely on the topic, but considered by the profession as a body. The points to be adjudicated are: first, has medicine its full and fair share of State aid? and secondly, has not the time arrived when some formal provision should be made for the support of men who devote their lives to inquiry and cannot reasonably or expediently be expected to practise or teach? In the old days much of the hardest work in science was done by Churchmen of the various orders who were supported by the ecclesiastical institutions of the country; now the labour of research is performed by laymen, and they must live.

THE TOURNAMENT OF INCUBATORS

IN a recent number we referred to the hydro-incubator invented by Mr. Christy. This incubator, along with a number of others, has been subjected to a comparative trial at the Hemel Hempstead Waterworks. An account of this competition appears in the *Live Stock Journal*. It commenced 6 A.M. September 5, and concluded on September 26, at 12 o'clock noon. The object of the committee who tested the incubation was—

(1) To ascertain whether incubators were of any practical value to the public generally; and

(2) If proved to be of value, to decide which was the best incubator for the ordinary purchaser to select.

That the person to whose management the incubators were entrusted should be unskilled and inexperienced in their use, was one of the conditions specially insisted on by the exhibitors. This was considered the best means of proving which incubator was of the simplest construction and could be worked most successfully without any previous apprenticeship. A large room in the Waterworks building was secured, where steam, hot water, or gas, could be employed, and where perfect privacy and quiet could be secured. Exhibitors were requested to send their incubators with full and clear instructions as to their method of working.

The eggs, which should be laid on Wednesday, September 4, had been bespoken some time before at several farmhouses in the neighbourhood, and these having been collected during the afternoon of that day, were brought to the Waterworks, and thoroughly intermixed by the members of the committee. Each egg was then marked with the word "*Couveuse*," by a stamp made at Brighton expressly for the occasion. By six o'clock on the morning of Thursday, September 5, the eggs had all been placed in the incubators by the members of the committee, the machines having been fully prepared for their reception. The incubators were then intrusted to the engineer, Mr. Twigg, with strict injunctions to follow implicitly the instructions of the exhibitors, and to admit no one without a written order from the committee to the room, which was to be kept locked, especially the exhibitors themselves or their agents. The following are the statistics of the competition itself, which were attached to the incubators as soon as possible after mid-day on September 26:—

No. 1.—VOITELLIER: Hydro-Incubator.

Of fifty eggs placed in incubator none were hatched.

This machine, from its simplicity and the ease with which the thermometer could be consulted, was quite a favourite with the engineer, who was most sanguine as to its results, and much disappointed at its failure. On subsequent examination no chickens were found in the eggs.

No. 2.—CHRISTY : Hydro-Incubator.

Eggs placed in incubator	50
Found fertile after testing	45
Unfertile	5
Broken during competition	0
Hatched by 12 noon, September 26 ...	34
Not hatched 11	11
Percentage of eggs hatched	75·55

Three more chickens were hatched alive after the competition had closed. The other eggs, on being examined, were all found to have living chickens in them. The prize of 25*l.* was awarded to this incubator.

No. 3.—CHRISTY : Hydro-Incubator.

Eggs placed in incubator	50
Found fertile	45
Unfertile	5
Broken during competition	1
Hatched by 12 noon, September 26 ...	20
Not hatched	24
Percentage of eggs hatched	44·44

Two chickens were hatched alive after the competition was ended. Of the remaining eggs ten were found to have living chickens in them.

No. 4.—BOYLE : Heated by lamp.

Eggs placed in incubator		48
Found fertile	40	
Unfertile	3	
Broken during competition	2	
Hatched by noon, September 26 ...	11	
Not hatched	27	
Percentage of eggs hatched	27·55	

On examining the eggs, Saturday, September 28, four chickens were found ready to break the shell. This incubator worked with great regularity, and deserves much commendation.

No. 5.—BOYLE : Heated by gas.

Eggs placed in incubator		52
Found fertile after testing	42	
Unfertile	10	
Broken during competition	26	
Hatched by 12 o'clock noon, Sept. 26...	0	
Not hatched	16	
Percentage of eggs hatched	0	

The egg rests in this machine are spiral wire springs The egg drawer did not fit well, and is certainly capable of improvement. Full and very clear instructions should always accompany this incubator. Its want of success must not be attributed altogether to the inexperience of the attendant.

No. 6.—PENMAN'S (worked by lamp) : Exhibited by Messrs. E. T. Brown and Son, Newcastle-on-Tyne.

By this no eggs were hatched, but twenty dead chickens were found in the eggs on September 28, having been dead apparently several days. The lamp in this incubator worked very irregularly, needing constant attention by day and night.

No. 7.—PENMAN'S (worked by gas).

By this also no eggs were hatched, but on examining the eggs at 6 P.M. on Saturday evening, Sept. 28, thirty-four chickens were found to be alive in them, two having been hatched out on the same morning alive. The source of heat—viz., gas—had been turned off at 8 P.M. on the Friday night previous, and the drawer had been opened and shut constantly after the exhibition was opened at noon on Thursday. The irregularity of heat from the gas was doubtless the cause of failure in this instance, the pressure being very unequal.

The committee subjoin to this report a register of the temperature maintained in the drawer of each incubator, together with that of the water drawn off from the boilers in the case of the hydro-incubators. The chickens hatched are doing well, some under them, some in artificial mothers.

Such are the facts of this interesting trial, and they seem to us to prove not only that artificial incubation is possible, but that by Mr. Christy's machine, if not with some of the others, it might become a remunerative business, and add materially to the sources of our food supplies.

NOTES

BY the kindness of Gen. Myer, the distinguished head of the U.S. Army Signal Service, we are enabled this week to give the official description of the weather case, the distribution of which among the 27,000 rural post-offices in the United States has just commenced. It is for use in those parts of the country where the daily weather indications cannot reach in time to facilitate agricultural operations, and its issue has been forced upon the Government because the American farmers are wise enough to see that for them, as well as for sailors, to be forewarned is to be forearmed. In a few centuries we may expect to have something of the same kind here.

M. BOUILLAUD, the once celebrated medical practitioner, who is a member of the Paris Academy of Sciences, assailed M. du Moncel in the sitting of September 30, and asserted that the phonograph and microphone experiments must be the work of ventriloquists. This fit of incredulity was occasioned by the recital of experiments made with the singing conductors. M. du Moncel asked for a commission of investigation to be appointed, although such accusations are not deserving of any notice, and have, indeed, raised universal ridicule. But the regulations of the Academy forbid any commission to be appointed to pronounce on the works or communications of members. Another curious scene took place at the sitting of last Monday. M. du Moncel presented to his colleagues, the "condensateur chantant," which had been exhibited on the previous Saturday. He retired to the room of the Académie Française, in company with M. Faye, closed the door and sang. His voice was heard coming from a number of sheets of paper, in which six sheets of tinfoil had been inserted, and connected with the wires of an induction coil. M. Bouillaud was obliged to retreat from the position he had taken at the sitting of September 30. He made no allusion to the accusation of ventriloquism, but read a long quotation from Descartes, to show that "even if a speaking machine had been constructed, it could by no means be considered as a thinking machine." He said that speaking was not only a mechanical action, but also an intellectual work, so that neither the phonograph nor the singing condenser could be regarded by any means as really speaking! The whole assembly, in spite of its usual gravity, burst into roars of laughter. M. Milne-Edwards, who spoke at the previous sititng, said with much propriety, he should not have answered M. Bouillaud if he had understood such was his issue. Unfortunately he had understood, as everybody in the assembly did, that M. Bouillaud questioned the honesty of the experimenter. At the end of the sitting M. du Moncel performed all the principal experiments of the phonograph.

SOME remarkable experiments in Electric Telephony were shown by Prof. Barrett in a lecture at the Midland Institute a few days ago. By means of Edison's carbon telephone, which promises to be the telephone of the future, the lecture was electrically transmitted to the Arts Club in an adjoining street, though the transmitting instrument was several feet from the speaker; conversely, an assistant, speaking near to the distant carbon transmitter, was heard in a dozen different receiving magneto-telephones distributed through the lecture Hall. Further, by employing a single "Phelps" telephone as receiver, and using a paper cone as the mouth-piece of the telephone, 300 or 400 people in the neighbourhood of the instrument were able to hear distinctly sentences and songs given to the distant carbon telephone. And finally, by the same means, the entire audience of some 1,500 people heard single words, such as Bravo! Halloa! &c., spoken to the far telephone. Other experiments, of a more crucial character, demonstrated that this new telephone of Edison's will probably place electric-telephony on an entirely new and more practical basis. Prof. Barrett also showed Mr. Edison's tasimeter, an adaptation of the principle of the carbon telephone, the instrument having been kindly sent over by Mr. Edison for this lecture. The extraordinary delicacy of the tasimeter to heat radiation was shown to the audience by the heat radiated from the face throwing the beam of light reflected from a galvanometer completely off a ten-foot scale. The megaphone and several other of Mr. Edison's recent inventions were also successfully shown in this lecture.

NEWS of Prof. Nordenskjöld's North-East Passage Expedition has reached Stockholm. It left the north coast of Norway on July 25, reached Jugor Straits on the 30th; steamed on August 1, and arrived at the mouth of the Yenissei on the 6th. It was intended to start afresh on August 10. The Kara Sea was nearly free of ice. A little scattered drift ice near White Island was the only ice met with during the whole voyage. The expedition has thus a good prospect of success.

THE *Times* correspondent writes from Naples, September 29, that for the past two or three days, according to Prof. Palmieri's report, the activity of the mountain had much diminished, and the seismic instruments had been quieter. At the time of the new moon there was an increase of activity, as is always the case, and it will be witnessed, no doubt, at the time of full moon; but the mountain pursues its regular course, except at these seasons, and some little time must elapse before what the world calls an eruption will occur. Of what character it will be it is impossible to say precisely, but appearances indicate that it will be a lava eruption—presenting, indeed, a most brilliant spectacle, but unaccompanied by those horrors which marked the eruptions of 1854, 1861, and 1872; but, as Prof. Palmieri observes, it is impossible to say how it will terminate. A *Daily News* correspondent sends an interesting account of a visit he has paid to the crater of Vesuvius, into which he descended, and tells what he saw:—The actual crater is placed almost in an amphitheatre, three-fourths of which are inclosed, while one-fourth is open. The inclosing walls rise above the bed of the crater from 250 or more feet in some parts, apparently composed of sulphur. The diameter, judging by the eye, from one side to the other, is about 300 yards, and the whole of this area is filled with lava on fire, but crusted on the surface with a skin some inches deep of lava that has been chilled. "Looking between the cracks or down the 'crevasses,' the glowing fires a few inches below our feet, and in the blocks whereon we were standing, were seen. The ten months' activity has enabled the volcano to raise a cone almost in the centre of the crater at least a hundred feet in height, very wide at the base, converging to the summit like a sugar-loaf, but with the summit of the loaf removed. With a pulsation as regular and as marked as that of the piston of a steam-engine in full motion, did the huge mountain carry on its work, so that now we were able clearly to understand what was meant by 'every pulsation of the volcano being duly registered at the observatory.' Clouds of smoke and fumes were issuing from the summit of the cone—now densely dark, as if a fresh supply of coal had been heaped on the fire; then intensely light, as if the engine were blowing off its steam; then most beautifully and delicately tinted with the tenderest rose-pink, as if an artist were testing how best to combine the loveliest tints of his art; then a pale salmon, a little while, and then as if five thousand torpedoes were simultaneously exploded. The huge mountain seemed to heave, and forth from its mouth issued immense quantities of molten lava, shot scores of feet high up into the air—apparently at the mouth all in one body, but there separating into millions of pieces, great and small, all glowing with the most intense red heat that can possibly be seen. Each piece as it ascended into the air was separate; no piece was partly red and partly black, but was on fire and at red-heat throughout; mostly the lava emitted fell back again into the bosom of the heaving mass, but with every emission quantities, large or small, fell on the outside of the mouth, and thus we saw readily how the cone had gradually but continuously increased in size and height. Every now and then a huge mass would drop outside, and then would be heard an immense crash, followed by vast quantities of lava rolling down the sides of the cone. As we stood watching, at intervals there seemed to be the firing of 10,000 guns of mightier calibre than Krupp's, and we soon found that this was the precursor of a grand display. Up rose, possibly 100 feet above the cone, an immense mass spreading in the shape of a lady's fan, and presenting one of the most magnificent sights the eye of man can ever see. And this upheaval was not a thing for which we had to wait till our patience was exhausted, and to wonder if it would be repeated or not, but was continuous and incessant, and almost seemed as if every renewed expulsion were grander than its precursor, or as indicating a trial of actual strength prior to the great event proposed to be completed."

THE foundation-stone of the proposed railway bridge across the Firth of Forth was laid last week.

LAST March a microscopical society was formed in Highbury under the presidency of Dr. Alabone. The numbers have steadily increased, and the society appears to have all the elements for a career of great prosperity. The opening *soirée* for the winter session will be held at Harecourt Hall, St. Paul's Road, commencing at 7.30 p.m. to-night.

MANY valuable papers were read at the meeting of the Sanitary Institute at Stafford last week, and if Government and the public are ignorant of the laws of public health and the best methods of carrying these into practice, it is from no want of enlightenment. The great want at present seems to be organisation and an efficient central authority, and we trust the practical and vigorous address of Sir Henry Cole on Sanitary Co-operation will meet with attention in the proper quarters, and lead to more systematic and efficient action than has hitherto existed. In an able paper Dr. Richardson advocated the appointment of a Minister of Health.

MESSRS. HARDWICKE AND BOGUE announce the following works for publication:—"A Manual of the Infusoria, comprising a Descriptive Account of all known Flagellate, Ciliate, and Pentaculiferous Protozoa," by W. Saville Kent, F.L.S.; "The Herefordshire Pomona," containing Coloured Figures and Descriptions of the most esteemed kinds of Apples and Pears, edited by Robert Hogg, LL.D., F.L.S., Part I., illustrated with coloured figures and woodcuts; "Clavis Synoptica Hymenomycetum Europæorum, conjunctis studiis scripserunt M. C. Cooke, A.L.S., et L. Quelet, M.D., O.A., Inst. et Sorb. laur.;

"The Sphagnaceæ, or Peat Mosses of Europe and North America," by R. Braithwaite, M.D., F.L.S., &c., illustrated with 29 plates; "Pollen," by M. P. Edgeworth, F.L.S. F.R.S., second edition, revised and corrected, illustrated with 438 figures; "The Ferns of North America," by Prof. D. C. Eaton, of Yale College, illustrated with numerous coloured plates by James H. Emerton, to be completed in 20 parts, published at intervals of about two months; "Flowers, their Origin, Shapes, Perfumes, and Colours," by J. E. Taylor, F.L.S., F.G.S., second edition; "Health Primers," edited by J. Langdon Down, M.D., F.R.C.P., Henry Power, M.B., F.R.C.S.; J. Mortimer-Granville, M.D., F.G.S., F.S.S.; and John Tweedy, F.R.C.S. Under this title will be issued a series of shilling primers on subjects connected with the preservation of health, written and edited by eminent medical authorities. The following volumes will be issued in October:—"Premature Death, its Promotion and Prevention;" "Alcohol, its Use and Abuse;" "Personal Appearances in Health and Disease (illustrated);" "Exercise and Training" (illustrated); "The House and its Surroundings;" "The Skin and its Troubles" (illustrated); "Baths and Bathing." Others will follow at short intervals.

THE national *fête* for the distribution of the Paris Exhibition awards will take place, as stated, on October 22. The whole of the Versailles Park will be lighted by electricity.

IN connection with the meeting of the Library Association last week at Oxford, we would recommend to our readers' attention a most interesting and really amusing little *brochure* by Mr. H. B. Wheatley, entitled "What is an Index? A few Notes on Indexes and Indexers." Mr. Wheatley gives many amusing instances of how not to do it, and his pamphlet will be found useful not only to indexers, but to all who in any way have to do with the arrangement of written or printed matter. It is published by Sotheran and Co. The Library Association has already assumed vigorous proportions, and in spite of its much talk seems likely to do real service to existing libraries and to the promotion of new ones.

THE great work of connecting the triangulation of Algeria with the geodetic net-work of Europe, through Spain, is progressing favourably. The Spanish staff officers under General Ibanez have established their post on Sierra Nevada and Mount Tetica, and the French near Nemours, and Ben Sabra, near Oran. M. Perrier, member of the Bureau des Longitudes, and director of the French Survey, will very shortly proceed to Algiers to take the last readings from the French side.

OF all the accidents to which submerged submarine cables are liable, one would suppose that that by fire would be the very last that would occur. Nevertheless, such an accident has happened to the Forth cable belonging to the Post Office. Lately all four wires were found earthy. The fault showed itself by test to be close to the shore. It was found below high-water mark at the foot of the cliff. Some boys during low water had been making a fire with the shavings and rubbish found on the beach, immediately over the cable, melting the compound and gutta percha of the core, and leaving the copper wires bare and in contact with the outside sheathing.

THE municipality of Prague, advised by the Hygienic Council of that city, have just issued an edict prohibiting ladies from wearing dresses with long trains in the public streets, on account of the dust which the appendices raise being detrimental to public health. The municipality of Leipzig published a similar edict some time ago. These measures are easily explained by the habit assumed by many representatives of the fair sex of letting their trains drag through dust (and worse) for the sake of producing an effect which we presume milliners consider important from the point of view of sexual selection.

THE Meteorological Central Office of Vienna reports upon an aurora borealis of immense extent on September 25. It appears that the phenomenon was visible for several nights in the whole of Scandinavia and Northern Russia. It covered the larger portion of the northern sky, and appeared in a yellowish red light, with frequent undulations of bright and intensely yellow rays.

WE are glad to see that Prof. Geikie has added to the usefulness of his "Elementary Lessons in Physical Geography" by preparing a series of questions. These have recently been issued by Macmillan and Co.

THE Paris *Temps* publishes daily the charts of the Central Bureau of French Meteorology. The experiment was tried by the *Opinion Nationale* two years ago, but was discontinued.

PROF. LEBOUR has prepared a convenient and complete catalogue of the Hutton collection of fossil plants, which are specially valuable as illustrating the carboniferous flora of some of the horizon in the Newcastle coal-field. The system followed is, with few exceptions, that of Schimper. The catalogue has been drawn up by order of the Council of the North of England Institute of Mining and Mechanical Engineers.

THE *Cologne Gazette* announces that on Saturday night two slight shocks of earthquakes were remarked at Buir and in the surrounding district. The first occurred about 10 o'clock, and the second about an hour later. There was a slight shock of earthquake at Parma during the night of the 2nd inst.

THE Annual Exhibition of the Photographic Society was opened yesterday.

THERE will be a meeting of anthropologists combined with an anthropological exhibition at Moscow during the summer of 1879.

THE appearance of phylloxera in some vineyards near Bonn, on the Rhine, has been officially announced.

"TABLES for Use in the Verification of Standards of Weight and Measure," by Dr. O. J. Broch, Standards Commission, Christiania, is the title of a valuable paper recently translated under the directions of the Standards Department, Board of Trade. It embraces tables of specific gravity, coefficients of expansion, elastic force of aqueous vapour, and the weight of water.

MR. L. S. BENSON, New York, of π notoriety, has submitted to English mathematicians his *demonstration* (this time in ink) of a *discrepancy* between the analytical and geometrical proofs of a property of the parabola, viz., that the area of any segment is exactly two-thirds of the rectangle on abscissa and ordinate.

THE Dutch Government, encouraged by the excellent results obtained, in a commercial point of view, through the construction of the Y-Muiden Canal, which connects Amsterdam directly with the German Ocean, has now the intention to construct a similar canal to connect Amsterdam with Gorinchem, and to render the Waal and the Rhine navigable for sea-going vessels, so that even larger vessels could in future sail as far as Arnheim, and s'Hertogenbosch. Of course Rotterdam, Dortrecht, Moordijk, and Flushing would also benefit by the completion of the intended new works.

THE New York *Daily Graphic* furnishes some particulars of interest respecting the Nez Percés, a tribe of Indians, the greater portion of which was captured by the United States' troops about a year ago and confined in an encampment near Fort Leavenworth, on the Missouri. Their chief, it appears, carries in his hand a looking-glass which "is used to direct military manœuvres in battle by means of reflected rays of light. Their various significations, however, have never yet been found out by the white man . . . The orders are apparently conveyed to distant parts of the field by a system somewhat similar to the dashes and dots of the Morse telegraphic code." The Nez Percés are described as a particularly fine race and well behaved. Their women are very industrious, and when not engaged in carrying wood and water, &c., "are generally hard at work in the manufacture of beaded mocassins, gauntlets, and Indian doll babies . . . The little boys, too, reap quite a harvest by displaying their skill with the bow and arrow." One of the most curious institutions of the tribe is a primitive description of Turkish bath. "The Missouri River runs close by their encampment, and on the bank of the river they have built what has the appearance of a gigantic ant-hill; in shape it is similar to a small Esquimaux hut, about six feet in diameter and two or three feet high. There is an aperture on one side just large enough for a man to get through, and in the interior there is just sufficient room for him to lie in a cramped position. They first build a fire outside in which they heat limestones until almost red-hot. They then shovel them into the hut and pour water on them so as to produce a dense vapour, after which they quickly rake the stones on, crawl in through the entrance hole which they cover up with a blanket, and lie there until the perspiration streams from every pore, when they come outside and plunge into the Missouri River and swim to the nearest sand-bank."

THE additions to the Zoological Society's Gardens during the past week include a Mona Monkey (*Cercopithecus mona*) from West Africa, presented by the Rev. W. N. Ripley; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by the Rev. E. L. Marrett; a Cape Zorilla (*Ictonyx zorilla*) from West Africa, presented by Mr. Calman; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. Delves L. Broughton; a Red-beaked Weaver-bird (*Quelea sanguinirostris*) from West Africa, presented by Mr. W. H. Simmonds; a Passerine Owl (*Glaucidium passerinum*), European, presented by Miss Turner; a Sumatran Rhinoceros (*Rhinoceros sumatrensis*) from Malacca, an Anubis Baboon (*Cynocephalus anubis*), a Macaque Monkey (*Macacus cynomolgus*), a Bonnet Monkey (*Macacus radiatus*) from India, a Kinkajou (*Cercoleptes caudivolvulus*) from South America, deposited; a Chinchilla (*Chinchilla lanigera*), born in the Gardens.

CYON'S RESEARCHES ON THE EAR

I.

THIS able and elaborate thesis, presented by Dr. de Cyon to the Medical Faculty of Paris, contains a further contribution of facts and speculations in reference to the function of the semicircular canals of the internal ear, a subject upon which, as Professor of Physiology in St. Petersburg, he had previously published an important and interesting paper.

As a knowledge of the form and position of these organs is absolutely necessary to enable the reader to follow a discussion of the theories as to their use, we shall preface this notice of Dr. de Cyon's thesis with a short anatomical statement.

The system of semicircular canals, which exists in the internal ear of all vertebrates, while differing greatly in size in different animals, is so nearly the same in general arrangement, that a description of it as found in man will be sufficient for our present purpose.

These organs are lodged in a bony cavity continuous with the cochlea which contains the organ of hearing. The vestibule is an irregular rounded chamber. In its walls are five openings leading to the semicircular canals. These are tunnels in the bone having an elliptical or circular section, and opening at each end into the vestibule. The central line or axis of each canal lies nearly in one plane (which we may call the plane of the canal), and is approximately an arc of a circle. At one end of each canal there is an enlargement called the ampulla. The diagram (Fig. 1) represents a section through the axis of one of the canals. The planes of the three canals are very nearly at right angles

[1] Recherches expérimentales sur les Fonctions des Canaux semi-circulaires et sur leur Rôle dans la Formation de la Notion de l'Espace. Par Elie de Cyon, M.D., &c., Lauréat de l'Institut de France.

to one another. The canals are named from their position—the horizontal, the superior, and the posterior; the two latter unite at their non-ampullary ends before joining the vestibule, so that

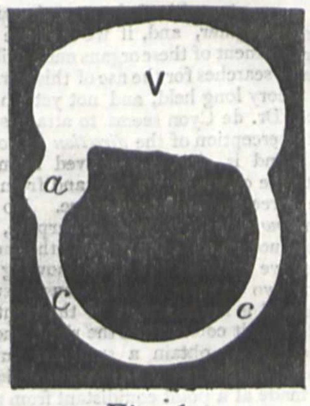


Fig. 1.

Section of bony labyrinth showing vestibule and one of the semi-circular canals. V, vestibule; *c c*, canal; *a*, ampulla.

there are five, and not six, openings into the vestibule—three ampullary, one for each canal, and two non-ampullary, one for the horizontal and one common to the superior and posterior canals.

The plane of the horizontal canal is nearly horizontal in the ordinary position of the head in all animals,[1] and is always at right angles to the mesial plane; the planes of the other two canals make nearly equal angles with the mesial plane. These relations are indicated diagrammatically in the accompanying sketch (Fig. 2), from which, to prevent confusion, the vestibule has been omitted.

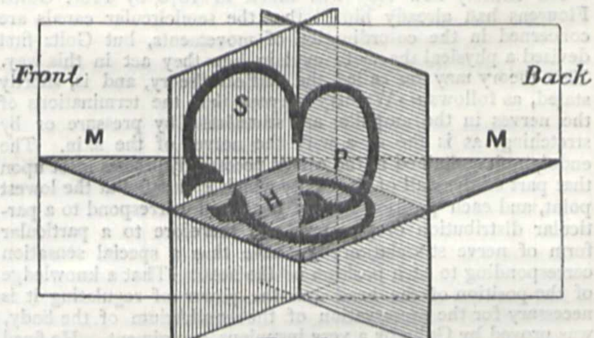


Fig. 2.

Diagram showing the relations of the planes of the three semi-circular canals of the left ear to each other and to the mesial plane. M M, mesial plane; S, P, H, the planes of the superior, posterior, and horizontal canals respectively.

In the bony labyrinth just described is inclosed a membranous labyrinth, similar in form, and consisting of the utricle, lodged in the vestibule, and of three membranous semicircular canals, each furnished with a membranous ampulla. The membranous labyrinth does not fit tight into its bony case—the utricle is much smaller than the vestibule (which contains, besides, other organs connected with the cochlea), and the diameter of the membranous canals is not more than one-fifth of that of the osseous canals. In the ampullæ the difference is not nearly so great; here there is only a narrow space between the bone and the membrane. The entire cavity is thus divided into two spaces, one within and one around the membranous labyrinth; each is filled with a liquid named the endolymph and the perilymph respectively. The external space contains, in addition to the perilymph, connective tissue attaching more or less firmly the membranous canals to the periosteum, and the blood-vessels and nerves which supply the membranous labyrinth. The nerves are distributed to a spot called the *macula acustica*, in the utricle,

[1] In man the plane of the horizontal canal slopes somewhat downwards and backwards, so that it becomes horizontal when the head is slightly bent forwards.

and to a crescent-shaped ridge, the *crista acustica*, near the middle of each ampulla. The nerves in each case end in hair cells, the hairs of which project into the endolymph. These nerves are derived from the vestibular branch of the so-called auditory nerve, or *portio mollis* of the seventh pair.

The remarkably regular, and, if we may use the expression, purpose-like arrangement of these organs must strike every one, and the mind naturally searches for the use of this curious geometrical apparatus. A theory long held, and not yet wholly abandoned, to which indeed Dr. de Cyon seems to attach some value, connects it with the perception of the *direction* of sound. The idea is not unnatural, and is obviously derived from the nearness of the apparatus to the organ of hearing and from the relation of its form to the three dimensions of space. No explanation has ever been given *how* it can serve this purpose, and a sufficient proof that it does not do so is supplied by the fact—easily tested by any one—that we have no means of knowing the direction of sound except by two or more simultaneous or successive observations. If a sound is heard louder in the right ear than in the left we conclude that it comes from the right, and by turning the head round we quickly obtain a sufficient number of observations to enable us to judge of the exact direction. If a sudden abrupt noise is made at a point equidistant from the two ears, we do not know its position unless we *see* what produces it. This theory may therefore be at once dismissed.

The first scientific inquiry into the function of the semicircular canals was made by Flourens in 1828. His experiments were conducted with an amount of care and the results described with a degree of accuracy and clearness not surpassed by any recent investigator. He observed that the section of a membranous canal was always followed by movements of the head, or even of the body of the animal *in the direction of the divided canal*; in other words, by oscillatory movements of rotation about an axis at right angles to the plane of the divided canal.

Flourens has been followed by various observers, Harless, Czermak, Brown-Sequard, Vulpian, and Löwenberg, who have confirmed the results above described, and referred them either to disturbances of hearing or to injury of the cerebellum.

An entirely new step was taken in 1870 by Prof. Goltz. Flourens had already hinted that the semicircular canals are concerned in the co-ordination of movements, but Goltz first devised a physical theory to explain how they act in this way. This theory may be called the statical theory, and is, shortly stated, as follows:—We may suppose that the terminations of the nerves in the ampullæ are stimulated by pressure or by stretching, as is the case with the nerves of the skin. The endolymph contained in the canals necessarily presses most upon that part of the wall of the cavity which is situated at the lowest point, and each position of the head will correspond to a particular distribution of pressure, and therefore to a particular form of nerve stimulation; we have thus a special sensation corresponding to each position of the head. That a knowledge of the position of the head and the power of regulating it is necessary for the preservation of the equilibrium of the body, was proved by Goltz by a very ingenious experiment. He fixed the head of a pigeon in an unnatural position by attaching its occiput to its breast, so that its beak was turned up and the vertex looked forward. He found that the animal so treated walked with great difficulty, and was quite unable to fly, and exhibited many of the phenomena observable in animals whose semicircular canals had been cut or destroyed.

The next investigations of importance as to the function of these organs are those of Prof. De Cyon in 1872. He repeated with great experimental skill, and with ingeniously-contrived modifications, the operations performed by Flourens and by Goltz, obtained the same results, and, guided by his medical observations on diseases accompanied by loss of equilibrium, formulated his theory as follows:—The semicircular canals give us a series of unconscious sensations as to the position of our heads in space; each canal has a strictly determined relation to one of the three dimensions of space; the loss of equilibrium and the other disturbances of locomotion caused by the section of the canals are solely due to the disorder of these sensations.

Before considering the next step in the development of the theory we must go back and examine the experiments on vertigo made by Purkinje about the time when Flourens first investigated the function of the semicircular canals.

Every one knows that if we stand up and turn round about a vertical axis and keep up this rotation for some time and then stop, we see, or think we see, surrounding objects moving round. Purkinje studied the conditions under which this apparent rotation occurs, and arrived at the following conclusions, which have been confirmed by all succeeding observers:—1. That the direction of the apparent motion of surrounding objects depends upon the direction of the preceding real motion of our body, and is always opposite to it. 2. That the axis about which the apparent motion takes place is always that line in the head which was the axis of the preceding real rotation. Thus, if we turn round with our head bent forward so as to look straight down at the floor, and then stop, keeping the head in this position, the apparent rotation takes place about a vertical axis; but if, when we stop, we lift up our head and look forwards, the apparent rotation takes place about a fore and aft axis, the fore and aft axis having been vertical while the real rotation occurred. A trick, illustrating this principle, is sometimes played on persons ignorant of this law of giddiness. They are asked to take a poker in their hand, plant it vertically on the floor in front of them, bow down so as to touch the end of the poker with their forehead, and walk quickly three or four times round the poker, then rise up and walk to the door. The apparent rotation takes place about a fore and aft axis, because the fore and aft line in the head was the axis of the real rotation; they see objects rise up on the one side and fall down on the other; the floor seems to incline itself to one side, and equilibrium becomes impossible. This experiment may be varied in many ways: thus, instead of the forehead, one ear may be placed on the end of the poker; the rotation then takes place about a right and left axis—the line from ear to ear. On rising, apparent rotation occurs about this line as an axis, and the floor in front seems to slope up or down according to the direction of the original real rotation.

Purkinje explained these phenomena thus:—"During the rotation of the body about its longitudinal axis, the brain, in virtue of its soft consistence, ought to have a tendency to remain a little behind the movement of the walls of the skull. This is the same phenomenon which we observe in a liquid when the vessel containing it is set in rotation. The particles of the liquid preserve their position relative to the external space, until their adhesion to the walls of the vessel forces them to take part in the motion of the latter. The cohesion of the brain is too great to allow of the reproduction of the same phenomenon exactly; but as the brain is soft and capable, to a certain extent, of internal displacement, it has some of the properties of liquids. We must therefore admit that a movement more or less intense must produce a displacement and relaxation of its parts, although an actual rupture of continuity cannot occur. Such distortions should produce the same disturbances as actual mechanical lesions, and differ from them only in degree."

This explanation is adhered to, in the main, by Dr. de Cyon, and although we do not admit that such tortional deformation of the brain is the chief or usual physical cause of the giddiness above described, it is probable that it has something to do with the phenomena when the rotation is very rapid and is suddenly stopped.

We now come to the modification of the theory of Goltz, which induced Dr. de Cyon to resume his experiments on the semicircular canals. This modification was made nearly at the same time by Prof. Mach, of Prague, Dr. Breuer, of Vienna, and Dr. Crum Brown, of Edinburgh. The papers in which they stated their views were written independently, and were published in the order above-mentioned within a period of about six weeks.

The views of the three observers are not quite identical, and it will be necessary to point out in what they differ; but they agree in so many details that a general description of their theory is possible. We have above called Goltz's theory *statical*; in opposition to it we may call the Mach-Breuer-Brown theory *kinetical*.

Goltz regarded the semicircular canals as organs which, by difference of pressure in different parts of the system, give us a notion of the *aspect* of the head in space. Mach, Breuer, and Brown regard them as organs which, by virtue of the *inertia* of the contained liquid and movable soft parts, give us a notion of the *change of aspect* of the head.

The simplest form of this theory is that in which it is stated by Dr. Breuer:—

"In a system of three ring-shaped tubes, approximately at right angles to one another, and filled with liquid, as is the case in the semicircular canals, there are produced by every rotational movement of the whole system (that is, of the head) currents of liquid in a direction opposite to that in which the head is turned. The amount of the flow in each canal depends upon the plane in which the head is turned and upon the rate of the rotation. There are perfectly fixed relations between the rotational movement of the head and the currents in the inclosed liquid; if these currents can be perceived they will give us an exact account of the rotational movement of the head. We may regard, as possible organs for the perception of the currents, the so-called 'auditory hairs' which project at right angles inwards from a widened and flattened part of the canal; they are thus placed so as to be most sensitive to currents in the canal, and are on the other hand connected to nerves, of which they form the end-organs.

"To turn these facts to account in the sense of Goltz's theory we must assume that every flow of the endolymph, perceived by the ampullary nerves, produces a sensation of rotation of the head in the plane of the canal in which the flow takes place, and in a direction opposite to it, but that the perceptions of the six ampullæ of the two labyrinths combine to form a joint sensation. . . . .

"Our assumption has a necessary consequence. If the rotation of the head (of course along with the body) is kept up, the initial backward flow of the endolymph will be destroyed by friction against the walls. If the head then suddenly stops, the endolymph must, in virtue of its inertia, flow on in the sense of the rotation of the head; a sensation will therefore be produced of rotation of the head and body in a direction opposed to that of the previous rotation."

In this view the endolymph is held to lag behind the rotational movement of the head when this movement begins;—when the movement has continued at a uniform rate for some time, the endolymph is constrained, by fluid friction, to take part in the movement of the head, and if then the rotation of the head stops, the endolymph moves on. We have, thus, two ways in which a relative motion can occur between the endolymph and the walls of the cavity containing it:—1. When the head begins to move—here the walls leave the fluid behind. 2. When the head stops—here the fluid flows on. In both cases the sensation of rotation is felt. In the first this sensation corresponds to a real rotation, in the second it does not, but in both it corresponds to a real acceleration (positive or negative) of rotation, using the word acceleration in its technical kinematical sense.

Mach's view differs from Breuer's in this, that while Breuer assumes an actual flow of endolymph through the canals, Mach believes that the very narrow bore of the canals will preclude such a flow—the friction being so great that the most abrupt rotational movement of the head will not produce sufficient difference of pressure to cause an actual current. Instead of a current there will be produced a change of pressure in the ampulla, which would produce a current were the canal wider, and this change of pressure may be sufficient to act on the hair-cells, and irritate the ends of the nerves.

In Brown's statement of the theory, not the endolymph only, but the whole liquid and soft contents of the bony canals are supposed to lag behind the movement of the head, and in his first paper he suggested that there might be a relative motion between the bony and the membranous canals. This view, founded on the statement to be found in various anatomical text-books, that the membranous canals float nearly loose in the bony canals, is scarcely tenable when we know that the former are somewhat firmly attached at one side to the periosteum.

Another important point in which Brown's statement of the theory differs from that of Mach and from that of Breuer, lies in his regarding the two labyrinths as forming one organ, all the six canals of which are required to form a true conception of the rotatory motion of the head.

The doctrine of specific nervous action, now we believe generally accepted by physiologists, implies that while greater or less stimulation of an end-organ produces difference of sensation, a variety in the *mode* of stimulation cannot be perceived. Flow through the ampulla from the utricle to the canal on the one one hand, and from the canal to the utricle on the other, must produce a precisely similar sensation if the hairs of the hair-cells are equally moved. We must therefore look further for an explanation of our power of distinguishing between rotation in the one sense and rotation in the other sense about the same axis.

Mach was at first inclined to suppose that in each ampulla there are two sets of nerves each sensible to rotation in one sense only. He now adopts the explanation proposed by Brown, who based it upon the fact established by careful measurements in a considerable number of animals, that the *six* canals are sensibly parallel two and two. Thus the two horizontal canals are in the same plane, while the superior canal of one side is in a plane nearly parallel to that of the posterior canal of the other side. Further, in each of these three pairs (right and left horizontal, right superior and left posterior, right posterior and left superior), the two canals are so placed that when rotation takes place about the axis to which they are perpendicular, one of the two canals moves with its ampulla preceding the canal, so that the flow, or tendency to flow, is from ampulla to canal, while in the other the ampulla follows the canal and the flow, or tendency to flow, is from canal to ampulla. If, then, we suppose that flow from ampulla to canal—or, adopting Mach's view as stated above, *increase* of pressure in the ampulla—alone stimulates the hair-cells, while no effect is produced by flow in the opposite direction—or by *diminution* of pressure in the ampulla—we have in the six canals a mechanical system capable of giving us an accurate notion of the axis about which rotation of the head takes place, and of the sense of the rotation. To this explanation Dr. de Cyon objects that it assumes two organs, the superior canal of one side, and the posterior canal of the other side, which are not anatomically fellows, to be physiologically fellows. To this it is sufficient to answer that the motions which these two organs are supposed to perceive are produced by altogether different muscles. Let us take the case of the right superior and left posterior canal—the former is sensitive to rotation in one sense about an axis approximately passing through the left eye and the right mastoid process, a motion produced by muscles on the right side of the front of the neck, while the latter canal is sensitive to exactly the contrary motion about the same axis, and this motion is produced by muscles on the left side of the back of the neck. It is surely unreasonable to expect anatomical relations to exist between the organs perceiving two motions which do not exist between those producing them.

ALEX. CRUM BROWN

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

WE have already referred to the mathematical courses for session 1878-9 in Johns Hopkins University, Baltimore. The following is the detailed programme:—I. Prof. Sylvester will lecture on (*a*) determinants, (*b*) modern algebra, (*c*) theory of numbers. II. Dr. Story will lecture on (*a*) higher plane curves, (*b*) solid analytic geometry, (*c*) quaternions, (*d*) elliptic functions. III. Mr. Craig will lecture on hydrodynamics. IV. Lectures will be given by appointed instructors on (*a*) differential equations, (*b*) analytic mechanics, (*c*) conic sections, (*d*) theory of equations, (*e*) differential and integral calculus. V. Mathematical Seminarium:—A Mathematical Seminarium is conducted under the guidance of the Professor and Dr. Story; it comprises all the instructors and students of mathematics in the university. At its monthly meetings, besides occasional papers, such topics as may from time to time suggest themselves in the course of reading to the students or instructors, or may otherwise be of general interest to persons pursuing mathematical studies, are made the subject of free oral discussion. VI. Scientific Association:—The Scientific Association of the Johns Hopkins University meets once a month for the discussion of subjects of general scientific interest. At these meetings an opportunity is afforded for communicating abstracts of recent mathematical progress, as well as the results of individual research. VII. Mathematical Journal:—"The American Journal of Mathematics" is published quarterly in the City of Baltimore, under the auspices of the Johns Hopkins University, and affords an efficient medium of intercourse between members of the university engaged in original investigation, and a wide circle of mathematicians in America and in Europe. VIII. All the mathematical journals published at home and abroad are taken in by the university. At the Peabody Library complete sets of "Crelle's Journal" and the most important scientific transactions are also accessible. The university library and reading rooms are open daily from 9 A.M. to 10 P.M.

THE Calendar of the Yorkshire College for its fifth session, 1878–9, forms a volume of 140 pages. The college has now day classes in the following subjects:—Mathematics, experimental physics, chemistry, geology and mining, coal mining, biology, engineering, classical literature and history, modern literature and history, modern languages, oriental languages, and textile industries, and evening classes in all the above except experimental physics.

IN reference to the question of help for lectures to the scientific societies of English public schools, a correspondent sends a Harrow list as a suggestion to other schools. He believes that all the hon. members who are masters shown in the list have delivered addresses to the society; the rule always was to invite the most eminent among the strangers who gave lectures to become hon. members. Hence several well-known names connected with literature or science are among the latter.

THE Working Men's College, which was founded by the late Frederick Maurice, in 1854 (and which naturally sustained a heavy loss by his lamented death in 1872), with a praiseworthy desire to extend its usefulness, has arranged for a series of general and popular lectures, which are intended to be perfectly free, not only to Students of the College, but also to the general public. With this view the Council has managed to secure the aid of such men as Professors Corfield and Lowne, Dr. Casson and Mr. Frederick Harrison, all of whom take part in these lectures between this and Christmas. This attempt to render the public uses of the College much more prominent than heretofore will not, as it appears, in any way interfere with its ordinary and recognised functions, and will not in any degree impede its class teaching, which has always been of the highest character. Various courses of scientific lectures by Mr. Dunman and Mr. Owen are announced.

SCIENTIFIC SERIALS

Journal of the Cincinnati Society of Natural History, July. Vol. i., No. 2.—This number gives earnest that good work is meant by the members. Its contents are chiefly interesting to palæontologists, who will find in it a list of lower silurian fossils of the Cincinnati group, by Messrs. J. Mickleborough and A. G. Wetherby, together with descriptions of many new forms found in these strata, by Messrs. Ulrich and Miller.

Reale Istituto Lombardo di Scienze è Lettere, Rendiconti, vol. xi. fasc. xiii.—We note the following papers in this number:—Causes and circumstances influencing hereditary transmission in animals (continued); Participation of the nervous system in the phenomenon of fecundation, by Signor Lemoigne.—Anæsthesia and anæsthetics in mediæval surgery, by Prof. Corradi.—Influence of water on the spinning of the cod of the silk-worm, and on the quantity and quality of the silk, by Prof. Gabba and S. Textor.—On some facts relating to saccharification of amides in the digestive process, by Dr. Solera.

Journal of the Franklin Institute, August.—This number opens with a discussion, by Mr. Isherwood, of some instructive experiments on the expansion of steam in the steam-engine.—A new method of grinding glass specula is described by Prof. Elihu Thomson, the principle of it being the fact that when two equal discs of glass or other material are ground together, one above the other, the under one always becomes convex, while the upper one becomes concave, and by making the strokes of the upper disc wide and sweeping, this change of form may be greatly accelerated.—Dr. Morton gives an account of the singing telephone as made at the Stevens Institute of Technology.—A new method of reduction for diffraction spectra observations is communicated by Dr. Rosenberg.—The problem of perforated pipes, as applied to "sprinklers" (a pipe system lately introduced into cotton-mills for preventing the spread of fires), is investigated by Mr. Frizell.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, September 30.—M. Fizeau in the chair.—The following papers were read:—Formation of an astronomical museum at the Observatory of Paris, by M. Mouchez. This is to include portraits of astronomers and *savants*, a collection of medals, drawings and photographs of celestial objects and phenomena, ancient instruments, &c.—Experimental facts showing that abundant sudoral secretions are not necessarily connected with excessive activity of cutaneous circulation, by M. Vulpian. In a dying cat, *e.g.*, when the heart's action is much weakened, and the digital parts are bloodless, the sweat exudes freely from these parts.—Remarks on the phonograph and the telephone, by M. Bouillaud.—Determination of the exact number of irreducible co-variants of the binary cubo-biquadratic system, by Prof. Sylvester.—Industrial utilisation of solar heat, by M. Mouchot. This describes experiments made during the Exhibition. *Inter alia*, he set in action, on September 2, a solar receiver with mirror having an aperture of about twenty square metres. It had, at the focus, an iron boiler weighing, with accessories, 200 kilogrammes, and having a capacity of 100 litres (30 for the steam chamber and 70 for the liquid). In half-an-hour the 70 litres were raised to boiling, and the manometer soon registered 6 atm. pressure. On September 22, with slightly veiled sun, he got 6·2 atm., and worked, under a pressure of 3 atm., a Tangye pump raising 1,500 to 1,800 litres of water hourly to the height of 2 m. With a clear sky on the 29th ult. 7 atm. was reached.—Discovery of a small planet at the observatory of Ann-Arbor, by Mr. Watson.—On intra-mercurial planets, by M. Gaillot.—On molecular attraction in its relations to the temperature of bodies, by M. Levy. To know all the isothermal and all the adiabatic lines of a body, and so to be able to study it completely, it is necessary and sufficient to know *two* of its isothermal lines and *only one* of its adiabatic lines.—On losses of charge produced in the outflow of a liquid when the section of the flow undergoes a sudden increase, by M. Boussinesq.—On the rotary power of quartz and its variation with the temperature, by M. Joubert. The angular coefficient of the curve of variation increases at first pretty rapidly up to 300°. From this to 840° (the boiling point of cadmium) it is nearly constant and the curve nearly a straight line with point of inflexion about 500°. Beyond 840° and up to 1,500°, the rotatory power increases only with extreme slowness. With a quartz of 46·172 mm., giving a rotation of 1,000° at zero, the increase from 300° to 900° is twelve minutes per degree. With a quartz of only 11 mm. the increase would still be three minutes per degree. Thus quartz makes an extremely sensitive thermometer, with the essential condition of comparability.—Phonic wheel for regularisation of the synchronism of motions, by M. Lacour. An iron-toothed wheel turns with its teeth very near an electro-magnet which is caused to exert periodic attraction by means of a vibrating diapason.—On the presence of isopropylic, normal butylic, and secondary amylic alcohols in the oils and alcohols of potatoes, by M. Rabuteau.

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