THURSDAY, FEBRUARY 17, 1881

ISLAND LIFE

Island Life; or, The Phenomena and Causes of Insular Faunas and Floras, including a Revision and Attempted Solution of the Problem of Geological Climates. By Alfred Russel Wallace. (London: Macmillan and Co., 1880.)

I.

MR. WALLACE is to be congratulated on his success in that most difficult part of book-writing—the choice of a good descriptive, yet short and euphonious, title. "Island Life!" What do not the words suggest! How many old associations do they not recall! A vacant and unsuspicious reader may indeed be lured by them to open what he may expect will prove a good novel, perhaps a story of the "Robinson-Crusoe" type. His hopes will be quenched by the first chapter; but if he possesses any capacity for an interest in the flowers, insects, birds, and beasts of his home, it will almost certainly be quickened by a perusal of that chapter. Like a skilful composer Mr. Wallace strikes at once with a firm touch the key-note of his volume. In a few pages he puts before us the problem he seeks to solve, and does this in so graphic and masterly a way that most readers will not only comprehend what he aims at, but will be persuaded into the belief that as they are familiar with some parts of the subject they have a personal interest in seeing what the author can make of it.

Hardly any problem in modern science is at once so complex and so fascinating as the geographical distribution of plants and animals. Strange to say, this complexity and fascination have steadily increased with the growth of knowledge. A generation ago, the grouping of floras and faunas found a ready explanation in differences of climate and special creations. But no such easy solution of the difficulties now avails. Ever since the classic essay of Edward Forbes on the history of the British flora there has been a growing conviction that the present arrangement of the life of the globe is the outcome of previous geological and biological changes. The doctrine of evolution has given to this conviction the strength of demonstrated truth. But while the theoretical aspect of the question may be clear enough, we are beset on all sides by what seem utterly insuperable obstacles when we try to work out the application of this theory to the history of any given flora or fauna. This is true even in those areas of Europe and North America where the living plants and animals are most fully known, and where some approach to a complete unravelling of the geological record has been made. But over most of the rest of the globe our knowledge of botanical and zoological distribution, and still more of geological history, is of the scantiest and most fragmentary kind. A few broad facts in the history of the mammalian life of the northern hemisphere are well established. The pedigree of some modern forms, such as the horse, can be traced back into early Tertiary times; the former wide spread of other forms, the lion for instance, and their gradual restriction in area, have been satisfactorily made out. But the kind of evidence available in these cases fails us in dealing with others. It seems as if all that we may hope to achieve is to establish by a few examples, capable of clear proof, the general laws by which variation in form and in geographical distribution appears to have been effected among the animal and vegetable populations of the globe.

By no living naturalist could these problems be more fittingly and exhaustively discussed than by the author of "The Malay Archipelago." Years of research in the East, followed by years of research and reflection at home, have enabled him to explore every highway and a vast number of byways in the wide realm of inquiry in which he has been so active and untiring a worker. Thoroughly conversant with all that has been done by others, he brings to his task a wealth of information and a breadth of view that stamp his works with the authority of a master.

The present volume may be regarded as an expansion of a part of the author's "Geographical Distribution of Animals." Further study of the problem of distribution has enabled him to treat it with greater fulness. He has devoted especial attention to geological operations that have affected the successive races of plants and animals, and has connected these operations with biological changes more closely and clearly than has hitherto been done. Of his new volume the first half is mainly occupied with a discussion of this subject. He there seeks to establish a number of fundamental propositions or laws, the confirmation of which leads in his opinion to a simpler and fuller solution of the problem than has before been possible. Two of these doctrines deserve the careful consideration of geologists and naturalists. They are (1) the permanence of continental and oceanic areas; and (2) the frequency of changes of climate during geological time and the combined influence of cosmical and geographical causes in the production of these changes.

The abundance of marine organisms in the rock-masses which constitute the bulk of the continents naturally led to a belief in the mutability of the land. Not once only but many times in succession the sites of some of our loftiest mountains were under the sea. And if it was discovered that the position of the land had been so variable, and that the sea-floor had been so continually upraised, the inference was easily drawn that land and sea must have been continually changing places. Tacitly or explicitly it was assumed that just as there appeared to be no area, even in the heart of the continents, which had not been submerged beneath the waves, so there was probably no tract even of mid-ocean where a continent might not have bloomed. It is probably a safe assertion to say that this is still the belief of most geologists. It finds formal expression in their most authoritative text-books, and can be traced everywhere in its influence upon the discussion of questions of geological history. From geological treatises it has passed out into the current literature of the time, as one of the accepted conclusions of science. Our Poet Laureate, who has embodied in musical language not a little of the scientific speculation of his day, has given terse expression to this universal belief in the often-quoted lines:—

> "There rolls the deep where grew the tree.
> O earth, what changes hast thou seen!
> There, where the long street roars, hath been
> The stillness of the central sea."

Inevitable as was this belief in the early days of geology, and firmly as it still maintains its hold, it is unquestionably based upon a partial view and erroneous interpretation of the facts. This has for some years been recognised by a few writers, and will before long be generally acknowledged. Instead of shifting their places on the earth's surface, continents, so far as the evidence of their history can be gleaned, have been wonderfully persistent.

This conclusion is reached by many different paths of inquiry. Of these it may suffice to notice here only two. (1) The rocks of which the greater part of the dry land consists, are upraised marine sediments. But their materials were derived from the waste of neighbouring dry land. They everywhere contain indications of the proximity of that land, and even reveal terrestrial surfaces, such as rippled-marked and rain-pitted shores, in the very midst of marine formations. Nowhere do they present indications of really deep water. (2) An examination of the floor of the present ocean proves that the sediment now removed from the surface of the continent is deposited in the shallower waters within 150 or 200 miles from land. Beyond this limit terrestrial sediment ceases to be transported and deposited, its place being taken by organic accumulations and by peculiar red and grey "clays" in which the inorganic material is mainly of volcanic origin, and must gather on the bottom with almost inconceivable slowness. This grouping of the detritus, derived from the degradation of the land, is evidently the only one possible, and it has now been abundantly demonstrated by recent deep-sea researches. We may be sure also that it must always have obtained in every geological period. The coarser and more lenticular sheets of sediment have accumulated nearest to the sources of supply, that is to the shores of the land; while the finer and more wide-spread silts have been spread over the farther and deeper tracts of that still comparatively narrow belt of sea to which sedimentation has always been mainly confined. To hasty readers it will seem an obvious and ridiculous paradox to maintain that the continents have been permanent throughout geological time, and yet to admit that probably no part of their surface has not been many times submerged beneath the ocean. Further reflection, however, and better acquaintance with the facts will convince every candid inquirer that the paradox is only in appearance. The continental ridges have been the great lines of terrestrial movement from the dawn of geological history. They have continually been undergoing disturbance; one portion has been equably upraised, another has been convulsed and corrugated, a third has been depressed. Every part of their surface has been subject to these changes. Moreover every portion of the crust which has risen above the sea-level has been exposed to the unremitting attacks of the subaërial agents of destruction. Again and again the solid bulk of the continents has been reduced to mere detritus and has been spread over the sea-bottom. And yet the continental ridges have never ceased to exist. Their disappearance would necessarily have been followed by the cessation of sedimentary accumulation. The character of their component rocks however teaches that, whether by the operation of underground movements or by the action of superficial causes, the land has been continually wandering, as it were, to and fro across the continental areas, disappearing beneath the sea in one region, reappearing from the sea in another. In one sense of course it may be said that land and sea have been continually changing places. But the submerged land has not become truly a part of the oceanic realm. The waters covering it have been mere prolongations of the upper layers of the ocean, like the Mediterranean, Black, and Caspian Seas of the present day. An elevation or depression of a few hundred feet, sufficed to turn wide tracts into land or into water. But such oscillations made no real change in the essential position of the grand aboriginal oceanic basins and continental ridges.

Mr. Wallace has thoroughly grasped the truth and significance of these averments, and has not been slow to perceive their fundamental importance in the history of terrestrial floras and faunas. He finds that they furnish new and unexpected assistance to the student of biological evolution, and indeed form a necessary part of the doctrine. "It is impossible," he says, "to exaggerate or even adequately to conceive the effect of these endless [terrestrial] mutations on the animal world. Slowly but surely the whole population of living things must have been driven backward and forward from east to west or from north to south, from one side of a continent or a hemisphere to the other. Owing to the remarkable continuity of all the land masses, animals and plants must have often been compelled to migrate into other continents, where in the struggle for existence under new conditions many would succumb; while such as were able to survive would constitute those widespread groups whose distribution often puzzles us. Owing to the repeated isolation of portions of continents for long periods, special forms of life would have time to be developed, which when again brought into competition with the fauna from which they had been separated, would cause fresh struggles of ever-increasing complexity, and thus lead to the development and preservation of every weapon, every habit, and every instinct which could in any way conduce to the safety and preservation of the several species."

Besides interchanges of sea and land Mr. Wallace lays great stress upon former vicissitudes of climate as agents in the modification of plant and animal life. He has discussed this subject with great detail and offers an original explanation of the causes of secular changes of climate. Adopting generally Dr. Croll's views as to the relation between the Glacial period and the excentricity of the earth's orbit, he introduces into them certain modifications and limitations. If, he argues, the effects of a high excentricity have always been shown in great Polar refrigeration and a general lowering of the temperature in the hemisphere whose winter occurred in *aphelion*, there ought to be geological evidence of the change. He confesses however that although indications of local ice-action have been noticed in different geological formations, even as far back as old Palæozoic deposits, there is certainly no trace of such general glaciations as the theory would lead us to expect. Not only so, but the testimony of organic remains is everywhere and unmistakably against the theory. He concludes, therefore, that while the astronomical influences must unquestionably be a *vera causa* in the production of terrestrial climate, and must always *tend* to produce alternate mild and severe conditions, there must be some counteracting cause whereby these influences are weakened or neutralised. This modifying effect he assigns to changes in the distribution of land and sea, especially in high latitudes. He contends that without lofty land there can be no permanent snow and ice. Consequently by the due elevation of Arctic land an area would be provided on which, when winter occurred in *aphelion* during a period of high excentricity, there would be so copious an accumulation of snow and ice, that even during *perihelion* the wintry conditions would continue, and perhaps even in an intensified form. Subsidence of this land, however, would admit the warm oceanic currents from lower latitudes, and so great would be the amount of heat thereby transferred that even winter occurring when the North Pole was turned from the sun and the earth's orbit was at a maximum of excentricity would be insufficient to cover the Polar regions with an ice-cap. The alternate phases of precession, which tend to bring warmer and colder conditions of climate every 10,500 years, would introduce a complete climatal change only where the land was partially snow-clad. The general conclusion is thus reached that, the climates of the globe being mainly dependent on geographical conditions, their mutations in former periods have been chiefly brought about by changes in physical geography. Mr. Wallace supports these views by much ingenious reasoning. He argues that during by far the greater part of geological time the distribution of land has been such that warm oceanic currents have been able to pass freely to the North Pole, giving a mild climate to the whole northern hemisphere. He would thus account for the palæontological evidence of long-continued glacial conditions within the Arctic circle from Palæozoic to late Tertiary times. It was only in very recent times, he thinks, that the great northern continents became so completely consolidated as to shut out the tropical currents and to render possible the wide-spread and intense glaciation which was actually brought about by the high excentricity that occurred about 200,000 years ago. According to this view geographical revolutions "have been the chief, if not the exclusive, causes of the long-continued mild climates of the Arctic regions, while the concurrence of astronomical influences has been essential to the production of glacial epochs in the temperate zones, as well as of local glaciations in low latitudes."

In a remarkable chapter, remarkable as the deliberate judgment of an accomplished naturalist, the author decides that the vast periods of time which used to be demanded for the changes of geological history are not required even for the evolution of the floras and faunas of the earth. He admits, with some geologists who have advanced the same view from physical data, that geological changes probably occurred more vigorously and rapidly in former times than they do at present, and as these changes have always been accompanied by relative alterations in the forms of the organic world, he believes that organic evolution has taken place far more rapidly than has been hitherto thought possible.

ARCH. GEIKIE

ALGÆ

Species, Genera, et Ordines Algarum, seu descriptiones succinctæ specierum, generum, et ordinum, quibus Algarum regnum constituitur, auctore Jacobo Georgio Agardh, Bot. in Acad. Lund. Prof. Emer. Vol. iii. pars ii. 8vo. pp. 301. (Lipsiæ: apud T. O. Weigel, 1880.)

THE appearance of Dr. J. G. Agardh's excellent work, "Florideernes Morphologi," published in the *Acta* of the Royal Scientific Academy of Stockholm, was duly noticed in the pages of NATURE (vol. xxi. p. 282), but, as the work was written in Swedish, a knowledge of its contents was accessible to a limited number of students only; the indefatigable author has therefore, with a view to render it more useful to those who take an interest in his subject, now issued an edition in Latin of the Morphology.

This new volume, which is in 8vo, forms the second part of the third volume of Dr. Agardh's "Species, Genera, et Ordines Algarum," and may be considered rather as a revised edition of the Swedish work than as an exact translation of it. The author has made some alterations both in the text and in the notes. These alterations include important remarks on the most recent algological publications, including M. Bornet's "Notes Algologiques," M. Sirodot's observations on the fecundation of the Batrachospermeæ, and those of M. Dodel-Port on the fertilisation of the spores of Algæ by Vorticellæ.

For a summary of the contents of the new work the reader is referred to the before-mentioned notice in NATURE; it may however be remarked that the present volume forms a valuable addition to the "Species, Genera, et Ordines Algarum," to which it is now appended, and its appearance will undoubtedly be welcomed by all who take an interest in the morphology of Algæ.

In addition to a table of contents and an *index rerum*, there is also an index of the species referred to. The latter is the more useful, because, in addition to the name of the species, there are special references to the descriptions of the structure, ramification, reproductive organs, and other particulars relating to the plants. This arrangement is especially convenient, inasmuch as these matters are treated separately in different parts of the work.

It is to be regretted that the beautiful illustrations appended to the Swedish edition do not accompany the present. The figures are referred to in the latter, and may be consulted by those who are fortunate enough to possess a copy of the former, or who have access to libraries which contain copies of the *Acta* of the before-mentioned Swedish Academy. It may be added that Dr. Agardh's descriptions of the parts of the plants are expressed with his usual precision and clearness, and can, therefore, be understood without the plates—though, undoubtedly, better with them.

It may be observed that the present volume treats solely of the morphology of the Florideæ, and the author does not allude to the classification of Algæ, except to express his opinion that certain Algæ of red or purple colours, such as Bangiæ and Porphyræ, included by many algologists among the Florideæ, do not really belong to that class (p. 9, *note*). MM. Thuret and Le Jolis excluded these plants from the old class of chlorosperms, to which they were formerly considered to belong, Dr. Agardh does not admit them among the Florideæ; and in Dr. A. W. Bennett's new scheme for the classification of the lower cryptogams,[1] they seem to be literally nowhere. Neither has Dr. Bennett assigned any place in his scheme to the rather extensive family Valonieæ. It is to be hoped that algologists will agree before long on the position which these forms are finally to occupy in the classification of Algæ.

His work on the Florideæ having been thus brought to a successful termination, it is to be hoped that Dr. Agardh will now turn his attention to the Melanosperms, and that he will, before long, give us a new edition of the first volume of his "Species Algarum"; a work rendered necessary by an increased knowledge of the structure and fructification of these plants, and by the discovery and accurate examination of many new species. The ex-professor has already revised and reconstructed the extensive genera *Laminaria*, *Zonaria*, *Fucus*, *Cystophora*, and *Sargassum*—the latter as far as relates to the Australian species of the sections *Pterocaulon* and *Arthrophycus* only. To these must be added descriptions of many new species of Melanosperms, all of which have been published in the *Proceedings* of the Swedish Academies, and are, therefore, not within reach of many who would gladly consult them. A new edition, in which these scattered papers shall be collected and classified, would be a boon to algologists, and, we trust, would not entail very great labour upon the learned and industrious author.

M. P. M.

[1] See the Report in NATURE (vol. xxii. p. 451) of a paper read before the British Association at Swansea last year.

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 New Cure for Smoke"

NATURE, vol. xxiii. p. 25, contains a letter from Dr. Siemens on a "new cure for smoke," and in that letter it is stated that, instead of using inert matter such as pumice-stone, he (Dr. Siemens) considers it far more economical and efficacious, in a gas-grate to transfer the heat of the gas-flames to gas-coke or anthracite, and that the result obtained shows that the "coke-gas fire" is not only warmer, but cheaper than its predecessor, the coal-fire, with the advantage in its favour that it is thoroughly smokeless.

Now having had considerable experience with gas-heating fires of various kinds, and being much interested in the success thereof, I determined to give the "gas-coke grate" a practical trial; and as the result of the trials which I have just completed, and which extended over a period of two months, may interest many of your readers, possibly you may be able to find space in your columns to record the following particulars:—

In the first set of trials a good modern fire-grate was arranged (as described by Dr. Siemens) with a solid iron dead-plate, and ½-inch gas-pipe, pierced with holes $\frac{1}{20}$th of an inch in diameter, placed in the front part of the grate, but behind the lowest bar, and all air excluded from below except that which was allowed to pass in between the hollow bottom and enter on the line of gas flames. The grate was then filled with coke broken into pieces about the size of a large walnut, the gas turned on and lighted. In a short time a good bright fire with a rich flame was obtained; the external temperature at starting was 32° F., and that of the room 45° F. This latter rose within two hours to 62° F., and was maintained as long as the fuel lasted, viz. fifteen hours, the gas and coke burning brightly the whole time.

The result of a number of these trials, with an expenditure of 28 lbs. of coke, was an average consumption of 325 cubic feet of gas, the expenditure of the latter being very accurately ascertained by passing the gas through a standard test meter.

A second set of trials was then made under conditions precisely similar to the above, with the exception that the gas was used only for lighting the coke at the commencement, and when a good fire was obtained, and the temperature had risen to 62° F., it was turned off, and only used again for a short time to reinvigorate the fire and maintain the temperature. This second series of trials—with an expenditure of 28 lbs. of coke—resulted in an average consumption of fifty cubic feet of gas; the fire, when the gas was turned off, was not so bright or rich as in the first series, and the fuel only lasted thirteen hours.

Upon the completion of the coke and gas trials as above, the grate was restored to its original condition for burning coal. The fire was lighted in the usual manner, and within two hours the temperature rose to 62° F. as in the previous trials, the external temperature at starting being 32° F., and that of the room 45° F., and with a consumption of 28 lbs. of coal 62° F. was maintained for fourteen hours.

The room in which the experiments were made was the same in each case, having a capacity of about 3000 cubic feet, a due north aspect, and situated about 150 yards from the river, to which it is entirely open.

The above facts having been ascertained after an extended and repeated series of trials, in which the fuel was most carefully weighed and the gas measured, it now becomes a simple matter to reduce the results to £ *s. d.*, and in doing so I have taken the present prices of gas, coke, and coal in this neighbourhood, which are as follows:—Coal, 26*s.* per ton; coke, 12*s.* per chaldron; and gas, 3*s.* 3*d.* per thousand cubic feet. From these we obtain the undermentioned results, viz.:—

First Trials. Coke and Gas continuously for fifteen hours

	d.
28 lbs. of coke at 12*s.* per chaldron	2·571
325 cubic feet of gas at 3*s.* 3*d.* per 1000 cubic feet	12·675
	15·246

Or 1·0164 pence per hour.

Second Trials. Coke, with Gas for lighting and use occasionally for assisting the Coke, for thirteen hours

	d.
28 lbs. of coke at 12*s.* per chaldron	2·571
50 cubic feet of gas at 3*s.* 3*d.* per 1000 cubic feet	1·950
	4·521

Or ·3477 of a penny per hour.

Third Trial. Coal only, for fourteen hours

	d.
28 lbs. of coal at 26*s.* per ton	3·9

Or ·2785 of a penny per hour.

It will, I think, be at once seen from the foregoing results that although by the use of gas and coke we get rid of the smoke nuisance, that desirable end is not obtained entirely without cost, and that, judging from these experiments, the "coke gas fire," while possessing many of the advantages claimed for it, has not proved in this instance to be warmer and cheaper than its predecessor the "coal fire."

Possibly some of your professional readers may find time to pursue the subject further and favour us with the result of their investigations. I have given a good deal of attention for years past to the employment of gaseous fuels, and have made many experiments, but I do not at the present time know of any fire-grate or stove (for ordinary household purposes) wherein gas is employed as the heating agent, either wholly or in part, which gives such good results as the raw material coal. At the same time there can be little doubt but that we shall yet discover the way to effect great economy in the use of fuel both for domestic and manufacturing purposes, and ultimately to solve the smoke nuisance question; but whether it will be by separating the raw material into its several constituents and bringing some of them together again under different conditions and proportions when being consumed in a gas furnace or grate, or by better and more perfect appliances for effectually burning the fuel in its raw state, has, I think, yet to be settled. The question however is one which concerns all alike, being a matter of both personal and national interest.

J. A. C. HAY

Royal Arsenal, Woolwich, January 29

I HAVE been much interested in reading the above, and I hope that Mr. Hay's example will be followed by other observers, in order to establish a fair average result of the relative cost of the coke-gas grate and the ordinary coal grate. Mr. Hay's results are not as favourable as those obtained by myself, owing probably to some imperfection in his arrangements, which are not described sufficiently to form any judgment upon them. I should like to know, for instance, whether the copper-back plate, which I presume he used, although it is not referred to, was backed by fire-clay, or whether it touched the ironwork of the fireplace, whereby its heat would be conducted away. This alone might account for the difference in result obtained by Mr. Hay and myself, and I think this opportunity is a favourable one to send you the figures resulting from my own observation of the original grate described in my article in NATURE, vol. xxiii. p. 25. This grate has now been in use from November 8 to January 31, during which period it has been alight sixty-six days, and the average time during which a bright fire has been kept up has been eight hours daily. During this period of 528 hours there has been consumed—

	£	s.	d.
1112 lbs. coke at 18*s*. a ton	0	9	0
581 lbs. smokeless coal at 20*s*. a ton	0	5	2
4100 cubic feet of grs at 3*s*. 6*d*. per 1000 ...	0	14	4
	1	8	6

Or an average of 0·518*d*. per hour, instead of 0·525*d*., as resulting from my first observation. The average consumption of solid fuel per hour has been 3·2 lbs., and of gas 7·7 cubic feet. The full supply of gas has generally been allowed during the first hour of lighting, after which it is turned down to about a third; this I find to be a convenient mode of working.

In comparing my results with Mr. Hay's, it must be borne in mind that my room has a capacity of 7200 cubic feet, with northern aspect, and his a capacity of 3000 cubic feet, also with northern aspect; his consumption should therefore be only $\frac{3000}{7200} \times 0{\cdot}518 = 0{\cdot}216d.$ (instead of 0·347*d*.), which figure would prove an economy in the employment of the coke-gas grate over the coal grate, which is 0·2785*d*. by his own showing, and would agree with the comparative results contained in my original communication.

C. W. SIEMENS

February 2

On the Spectrum of Carbon

IT is very desirable that, if possible, some definite conclusion should be arrived at concerning the chemical origin of the bands, which Prof. Liveing calls "hydrocarbon bands," and the importance of the point at issue must be my excuse for again addressing you on this subject.

In my previous communications I pointed out that if it can be shown experimentally that the electric spark, in an atmosphere of cyanogen *free from hydrogen*, gives the groups in question (the groups δ and γ, wave-lengths 5165 to 5082 and 5635 to 5478 respectively, are here the only ones considered), they must be due to carbon, and remarked that the hypothesis that they were due to traces of hydrogen present as impurity is "to adopt an extreme hypothesis which must be supported by cogent experimental evidence before it can be accepted." Prof. Liveing admits the justice of this demand, and then goes on to say that such "cogent experimental evidence, so far as the relations of carbon and *nitrogen* are concerned, will be found in our complete papers on the spectrum of carbon compounds in the *Proceedings* of the Royal Society." This appears to me to be equivalent to an admission that—as concerns carbon and *hydrogen*—no such experimental evidence has yet been given; which is also the conclusion to which I came after perusal of the papers of Profs. Liveing and Dewar referred to.

It would seem then that the burden of proof that cyanogen exists in which the spark will not give rise to the bands δ and γ rests with Prof. Liveing. Nevertheless I have repeated the experiment with cyanogen, described in this journal (vol. xxiii. p. 197), so as to set aside the objections raised by Prof. Liveing to the former experiment. The apparatus was in this case constructed of one piece of glass—a long piece of hard glass tubing. This was carefully cleaned, the tube was then contracted at two points, so as to separate a short portion of the tube, into which platinum wires were fused, so as to form a discharge tube. The whole tube was next heated to redness in a furnace, while a current of oxygen passed through it for some considerable time. The greater portion of the tube on each side of the part containing the wires was then filled with phosphoric anhydride, and a short length of the tube, separated from the discharge tube by as great a length of phosphoric anhydride as the length of the tube permitted, was employed as a retort, and filled with mercuric cyanide; the other end of the tube was drawn out and dipped beneath sulphuric acid. The mercuric cyanide employed, after being finely powdered, was dried for a long time in an air-bath, then transferred to a clean hard-glass tube, in which it was repeatedly heated, while a current of air dried by passing over calcium-chloride and phosphoric anhydride was drawn over it. From this tube it was transferred immediately to the retort-tube. In making the experiment the mercuric cyanide was heated so as to give as slow a current of cyanogen as possible, which was continued long enough to expel all the air from the tube. The tube was then sealed up, leaving the discharge-tube, with a phosphoric anhydride tube on each side of it, and put aside for a week. The spectrum was then examined, with the same result as before. The tube gave a brilliant carbon spectrum, of which γ and δ (the positions of which were measured) were the brightest groups. No trace of the hydrogen C-line was obtained. Prof. Liveing objects that this is not a sufficient proof that hydrogen is absent (in which I cannot agree with him), and suggests that "a real test would be to see whether, when the spark gives the line-spectrum of carbon, the hydrogen-lines do not also appear." This test is however not applicable, since (according to my experience) cyanogen cannot be made to give the line-spectrum of carbon. Further, in this particular case the spark could not be got through the tube when the condenser was put on.

Giggleswick, February 11 W. M. WATTS

"Prehistoric Europe"

As there was no space to allow of all the authorities being cited in my criticism of the above work I now give those which relate to the facts called in question by Dr. James Geikie in NATURE, vol. xxiii. p. 336.

1. Dr. James Geikie repudiates as absurd the view attributed to him, that the palæolithic gravels "which overlie the chalky boulder clay of East Anglia were covered by an upper and younger boulder clay," and denies that he ever wrote anything which would justify that opinion. In "The Great Ice Age," 2nd edition, p. 531, he writes: "The palæolithic beds dovetail into the glacial drifts, and are overlapped (as in Yorkshire) by the deposits thrown down during the final cold period. To the last interglacial period then we must refer the great bulk of the palæolithic river gravels of the south-east of England." If this be true, where are the glacial deposits in question to be seen? If they ever were above, or "overlapped," the palæolithic gravels, they have, so far as our present knowledge goes, been utterly destroyed. Of course this view *is* absurd.

2. The reindeer associated with the hippopotamus and hyæna in the same stratum in the Victoria Cave was discovered while the exploration was under my management, and was published in Brit. Assoc. Rep. 1872, *Trans.* p. 179, and again in Mr. Tiddeman's Report, *op. cit.* 1876, *Reports*, p. 118. The animal is omitted by the author where its presence would destroy his argument as to climate, but he does not forget to record its subsequent discovery at a higher level, where it falls in with his argument. It may be remarked that the association of reindeer with hippopotamus in this cave has no special theoretical value, because the two animals have been found together in several other hyæna dens.

3. The fossil mammalia of Mont Perrier are typical Upper Pleiocene, as may be seen from the works of Croizet and Jobert, Gaudry and Gervais, and as I can testify from their examination. The glacial origin of the overlying tuffs, which I have examined under the guidance of M. Julien, seems to me to be open to considerable doubt.

4. The mammalia of Leffé, and those of the Val d'Arno with which they are classified by Dr. James Geikie, characterise the Upper Pleiocenes of Italy, as may be seen from the careful essays published by Dr. Forsyth Major, and from an examination of the magnificent collection in the museum of the University of Florence.

5. If pages 309-318 of "Prehistoric Europe," dealing with "interglacial epochs," do not imply a belief that the Neolithic skull of Olmo is interglacial, I am unable to ascertain their meaning.

The questions whether a geological period is to be classified as hitherto it always has been classified, by an appeal to zoology or by an appeal to ice, and whether the naturalists who have devoted themselves to the study of mammalia have only "opinions," while Dr. James Geikie enjoys "the facts," may be left in silence to the judgment of geologists. In the review under discussion all reference to my own opinion and works has been carefully omitted. Here, so far as I am concerned, the discussion ends. W. BOYD DAWKINS

Owens College, February 11

Geological Climates

IN NATURE, vol. xxiii. p. 241, Dr. Haughton repeats his former statement that "it is imposible to suggest any rearrangement of land and water which shall sensibly depress the temperature of the east of North America." Now we must only look about us to see that the east of Asia is colder than the east of North America, parallel for parallel, and this especially in winter. The *mean temperature of January* is as follows in places situated as far as possible under the same latitude and at the same distance from the sea :—

Lat. N.	Eastern Asia.		Eastern North America.		Difference.
56½°	Ajan	− 4·2	Naïn, Labrador	− 3·8	... 0·4
53	Nikolayewsk, Amoor	− 12·1	Rigolet, Labrador	− 1·1	... 11·0
43¾	St. Olga Bay ...	12·9	Portland, Maine	21·2	... 9·3
43¼	Wladivostok ...	6·1	Portsmouth. N.H.	25·0	... 18·9
40¾	Newchwang ...	10·4	Paterson, N J. ...	26·6	... 16·2
40	Pekin	23·7	Philadelphia ...	31·3	... 7·6
31¼	Shanghaï	38·3	Mean of Savannah and Ft. Marion	53·6	... 15·3
22¼	Victoria, Hong Kong	59·5	Habana, Cuba [1]	71·4	... 11·9

This shows that (1) from lat. 20° to 55°, Eastern Asia is everywhere from 7½° to 19° F. colder in January than Eastern North America ; and that (2) those parts of the coast of Eastern Asia which are not separated by mountains from the interior lowlands are much colder than those which are sheltered, but even the latter parts, though relatively warmer, are yet much colder than the same latitudes in Eastern North America. These differences are explained by geographical position. Asia is the larger continent; its eastern interior is more secluded from the influences of warmer seas, and its eastern coast more subject to Continental influences, and thus colder in winter than North America. We thus see by the example of Asia that a colder temperature than in Eastern North America does really exist now in the same latitudes. The example of Eastern Asia shows us geographical conditions which tend to produce an exceedingly cold winter. We have but to look at the middle and higher latitudes of the southern hemisphere to see so cold summers that nothing of the kind is met with in the northern. I do not know on what authority Dr. Haughton states that the annual temperature of 32° F. is met in the southern hemisphere but on 62° 41′ S. We do not have observations during the winter in these latitudes, but the mean temperature of January (the warmest month) is found to be 35·2 on 60° S. and 32·4 on 63½° S. Or (by the observations of Sir J. Ross) the mean annual temperature can certainly not be less than 4½° below that of January, so that it would be not higher than 30·7 on the 60° S., and 27·9 on the 63½° S.

St. Petersburg, February 5 A. WOEIKOFF

[1] Nearly one degree to the North of Victoria.

Variable Stars

WITH reference to your remarks on variable stars in the Astronomical Column of NATURE, vol. xxiii. p. 206, I beg to send a few observations made by me (on some of the stars referred to) during the past few years :—

5. 35 Camelopardi. October 1875. I found this star about 6½ m. and fainter than *o* (27 Fl.).—October 6, 1879. 7 mag. ; about 1 mag. less than *o*.

6. Rümker's star. I have the following observations : March 27, 1875. About 7 m. ; fainter than 25 Monocerotis.—January 19, 1876. 6½ m. ; less than 25, but brighter than two 7 m. stars *s.f.* it.—March 18, 1877. Distinctly visible to the naked eye ; about 6 m., but less than 25 (5.6 m. Heis). The above observations were made in the Punjab.

7. 65 b^2 Geminorum. December 1, 1880. 65 so exactly equal to 64 Geminorum with opera glass that I could see no difference between them in magnitude.

8. 16 Leonis Minoris. March 27, 1875. About 7½ m.—January 19, 1876. 7·3 or 7·5 m.

10. Lalande 38405. August 31, 1877, I found this star fainter than Lalande 38388, which lies about 20′ north of it ; also less than a 6 m. Harding (Lalande 38214) *s.p.* it. Brighter than Lalande 38342 (7¼, 8), which lies *n.p.* it.

11. 33 Capricorni. August 1875 I estimated this star as 6½ m. ; August 1876, 6 m., and slightly brighter than 35 Capricorni.

12. ι (17) Andromedæ. From numerous observations, beginning in May 1875, I have detected a variation in the light of this star to the extent of about half a magnitude. It is sometimes distinctly brighter than κ Andromedæ, and sometimes decidedly fainter.

With reference to β and δ Scorpii I find the following observation in my note-book :—

"Punjab, August 10, 1876. β Scorpii (2 m. Heis) and δ Scorpii (2·3 m. Heis) almost exactly equal. Perhaps δ, if any thing, *very* slightly the brighter of the two. J. E. GORE

Ballisodare, Co. Sligo, Ireland, February 5

The Mode of Flight of the Albatross

THERE seems to be a prevailing idea that the albatross in his flight is in some way "assisted by the wind." I think this is a mistake ; the manner is well known. The method I believe admits of a very simple explanation. His secret consists in his power of acquiring great momentum together with the large superficial area of his extended wings ; with scarcely a motion of his wings he will fly straight against a strong wind with a velocity greater than that of any racehorse ; this is inconsistent with the idea of his being "assisted by the wind."

In attempting to rise from the water (I believe he is unable to rise from the land or from a ship's deck) he flaps his wings violently to get his body out of the water ; at the same time, paddling rapidly with his webbed feet, he acquires a moderate degree of momentum, sufficient, with outstretched wings, to carry him forward and upward upon an easy incline. The case is similar to that of a boy taking a run with his kite string in his hand to give his kite a start. During this first rise he will generally give a few heavy, lazy flaps, and then stretch his wings steadily to their full extent ; now as he gradually rises he must of course as gradually lose his acquired momentum till it suits him to acquire more, when he may be twenty, thirty, or fifty feet above the surface, but a much greater distance from the place where he left the water, measured on the surface ; by slightly altering his position, by a movement of his tail, he takes a shoot downwards at any angle that suits his convenience, still without his wings outstretched. This is precisely the case of a boy shooting down a coast on his sled ; the propelling force is the same. The bird directs his course mainly with his tail, the action of which upon the air is identical with the action of a ship's rudder upon the water. By this downward motion, his velocity rapidly increasing, he acquires a degree of momentum sufficient to carry him up again to a height equal to or greater than that from which he started. In this up and down long wave-like motion, with all its variations on either side, consists the whole of his flight day after day for hundreds of miles ; at long irregular intervals he may give a few lazy flaps with his immense wings. Other birds use the mode of flight of the albatross, but to a smaller extent, for the reason, in the case of smaller birds that, the ratio of feathers to bulk being greater, their specific gravity is less, consequently they are unable to acquire the degree of momentum necessary to carry them upward ; but on the other hand they have the power of sustained effort in moving their wings rapidly, which the albatross has not. Gravitation then, which prevents him from rising directly on the wing, is the motive power of the albatross when aloft. He must always take a run or paddle over the surface of the water in order to get a start, and on the land or the deck he is a prisoner, because he has no water in which to paddle himself along with his webbed feet, and he is unable to run. Instead of being assisted by the wind, his speed is lessened by just so much as the wind's velocity, when it happens that the direction of the wind and his intended course are opposed to each other, but with the wind his speed is just so much greater than it would be in a calm.

I do not advance this explanation as an imaginative theory. I claim more for it. I have had many opportunities of studying the movements of the albatross for consecutive days, and I feel confident that the above will be found to answer all required conditions. HOWARD SARGENT

Cambridge, U.S.

Auroral Phenomena

It is perhaps worth a note that my daughter saw at Folkestone a very unusual phenomenon on the evening of January 25, a little before 6.30. Some distance to the left of Orion (for the night was clear and starry) she observed a small cloud of a bright golden hue, from which streamers of great brilliancy darted in various directions, the cloud alternately paling and brightening. She describes the streamers as like small meteors, leaving trails of light behind them. C. M. Ingleby

Athenæum Club, February 12

Ozone

In reply to Mr. Capron (Nature, vol. xxiii. p. 219) the following explanation may perhaps serve :—

On a flat piece of brass two strips of paper are laid, one plain white, the other prepared. With a clean camel-hair brush they are moistened liberally with pure alcohol. This is then burnt off, firing it with a spirit flame; the plain paper remains *clear* and *white*, the prepared paper (beginning at the edges) gradually changes to a *purple brown*. On immersing both strips in clean water the plain paper still remains white, prepared paper changes to a deep purple (No. 8, Negretti's scale).

In about an hour this deep purple colour fades away *precisely in the same way as if the slip had been ozonized by exposure for a day or two to the air*. It may be added that if the prepared slip is not plunged into water the purple brown tint remains for several days.

The experiment suggested by Mr. Capron has been made, using a very delicate gold-leaf electrometer. When this is uncharged there is no apparent effect; when charged either directly or inductively with either positive or negative electricity the gold leaves collapse, the charge appearing to be dissipated with the flame.

I may add, when the leaves are *charged* the alcohol is lighted on the plate of the electrometer with a glass rod dipped in alcohol, care being taken to prevent the discharge by conduction. The above experiments have been performed in an ordinary study, but I cannot say they are very conclusive.

Mr. Capron states "ozone is very strong just now," and he obtains No. 10 (Negretti's scale) at an inland town. This is a very high number. I have repeatedly obtained this number at Hunstanton on the Wash (Norfolk), where I made experiments daily for a month. The ozone cage was kept in the shade, a fine cloudless day with cold north east wind blowing, and one day's exposure. I have been engaged for some years in testing for ozone on the coast to see if its abundance, or deficiency, is in any way dependent on the physical and geological conditions of the shore. My experiments are not sufficiently advanced to be published, but the three following conditions have always been found to be present where ozone is abundant.

1. A long sandy shore exposed for some hours to the sun's rays.
2. Cloudless sky, with cold north or east wind.
3. An abundance of phosphorescent light from the presence of *Noctiluca miliaris* with the evening flow of tide.

This town is singularly deficient in ozone. After numerous experiments I have as yet only obtained No. 1 (Negretti's scale). Whether this deficiency may not have some connection with our notoriously great infantile mortality of the autumn is a question for further consideration. J. P.

Leicester, January 27

Citania

I have not had an opportunity of reading Nature for some time, but I am told that in a late number there is some mention of a so-called "Pompeii" near Braga in Portugal.

I do not presume to write as a learned antiquarian; but having lived for some years within thirty miles of Citania, and having often visited the place and examined the ruins with a wish to gain some explanation of their mystery, I venture to write as an ordinary witness.

In no sense can Citania be described as a Celtic Pompeii; it is merely a collection of circular buildings erected so close to each other as almost to touch, and grouped on the top of a hill which runs out as a spur from the higher ground behind it, and overlooks the rich valley beneath it. The walls have fallen, and the stones which composed them remain *in situ*, generally visible, though more or less overgrown with grass. From the foundations it seems that these round houses must have been some ten feet in diameter internally, with walls eighteen inches thick. The original height of the walls may be inferred, from the quantity of stones fallen, to have been some twelve or fifteen feet.

My utmost examination discovered scarcely anything beyond some shattered bits of coarse pottery. But over the surface of the hill there are still lying about many well-shaped round stones about twenty inches in diameter, which I always thought to be hand millstones. These seemed to me to afford the most likely solution of any mystery connected with the place, and I inferred it was a place of security, to which the corn of the district round was carried. The apparent absence of water forbade the supposition that it was a place of permanent abode. I never could see any necessity for referring its origin to Celtic time. The buildings were probably used, and possibly only date from much later days. Remembering the condition of that district as being the debatable ground lying between the Asturian kingdom in the north and the moors in the south, and open to sudden and transient incursions from either side, the utility of such a place to the farmers of that district seems evident. A Portuguese gentleman, whose name I forget, has so far interested himself in the place as to rebuild one of the circular buildings in what he conceives its original condition, and inside he has collected any remains of antiquarian interest that he could scrape together. Unfortunately his enthusiasm for forming a kind of local museum has led him to carry to it what never belonged to the place. For outside his museum there is a large granite slab, which in character is utterly foreign to the place, and long mystified me. This "Pedra Formosa," as it is called by the neighbouring villagers, is about nine or ten feet long, six feet high, with an average thickness of one foot, and must weigh six or seven tons. It looks like a pretentious *façade* stone, which has survived the building to which it was once attached. It has some carving about it, and signs which may or may not have any meaning in them. But whatever the stone was, it has no right to be where it is; for one day, in a conversation with a local farmer at the inn in the valley, I learnt the fact that some years ago all the farmers of the neighbourhood combined, and yoking thirty-nine pair of bullocks together, dragged the said stone from the valley below, where from time immemorial it had been lying, and added it in triumph to the other objects of the museum.

I may add that during my stay in Portugal I corresponded with the late Senhor Herculano, the Portuguese historian, on the subject, and I believe I have stated his conclusions.

R. Burton Leach

Sutton Montis Rectory, Castle Cary, February 8

The Recent Severe Weather

Your correspondent H. W. C. in his communication on the above in Nature, vol. xxiii. p. 329, quotes Mr. Lowe's theory of an eleven-year cycle of "great frosts," and after giving the dates upon which that theory is apparently based, says: "There are some variations in the lengths of the intervening periods, *but there is a distinct recurrence of eleven-year epochs*."

With the first part of this sentence I quite agree, but I fail to see the very least ground for the latter part of it, the intervals taken in order being as follows:—9, 3, 6, 18, 3, 16, 4, and 10 years. Three intervals approximating to eleven years can be "screwed" in by manipulating the years between which you reckon, disregarding inconvenient ones and using others which suit better; but surely this cannot be held sufficient to justify the statement, such a method of dealing with the figures being, it is scarcely necessary to point out, quite unallowable.

I have noticed before that when the discovery of similar epochs for abnormal heat, cold, rain, &c., have been announced, a similar method of dealing with dates has been followed to that which seems to have been adopted in this case. F. M. S.

February 3

The epochs which show recurrence are obtained by "manipulating" the figures in the following manner:—

December 1801	to January 1814,	interval 12	years	2	months.
,, 1810	,, 1820,	,, 9	,,	2	,,
,, 1840	,, 1861,	,, 20	,,	2	,,

(It should here be remarked that a *long* but not "great" frost was experienced in the winter of 1849-50; as it was not severe enough to entitle it to the designation of great frost it was omitted from the table; if it had been inserted the 20 years and 2 months period would have counted as two 10 year periods.)

December 1860 to January 1841, interval 10 years 2 months.
" 1870 " 1881, " 10 " 2 "

Thus at least four periods (out of a possible seven) do not require much "screwing" to make them approximate to 11-year epochs; while if we were to add in the *long* frost of 1850 we should have no less than six periods, showing a distinct recurrence.

It may not be quite clear why the remaining dates are inserted; but if they are analysed in the following manner they are not uninstructive.

December 1813 to January 1838, interval 22 years 2 months.
" 1837 " 1857, " 19 " 2 "

These periods, like the one 1840 to 1861, tend to show that the intervals approximate nearer to 22 years. How does F. M. S. obtain the intervals he quotes? As regards the last paragraph of the letter of F. M. S. respecting the "abnormal heat, cold, rain, &c.," it is only necessary to say that he would have considerable difficulty to prove to Norman Lockyer, Meldrum, and others, that 11-year cycles do not exist, even if F. M. S. "screwed" his figures, as he seems to have done in his letter above. H. W. C.

Butterflies in Winter

A COUNTRYMAN has shown me to-day two fine specimens of *Vanessa urtica* in a lively condition caught on the 4th inst. in an empty room on the border of the New Forest, exposed to the severity of the late frost. THOMAS W. SHORE

Southampton, February 8

JOHN GOULD, F.R.S.

THE grave has recently closed over the remains of a very remarkable man, and although the annals of science, we are proud to think, afford many instances of indomitable energy and unceasing perseverance rewarded, they have no greater record of success than is to be found in the life of John Gould. No one can regard the series of works written and illustrated by him without acknowledging that they are a monument of human energy, and the story of his life makes the fulfilment of these large enterprises the more interesting. In the character of the man we must look for the secret of his success, because it is well known that he possessed neither the advantages of wealth nor education at the commencement of his career, and yet he has left behind him a series of works the like of which will probably never be seen again; and this because it is rare to find the qualities of a naturalist, an artist, and a man of business combined in one and the same person. John Gould was all these in an eminent degree; he knew the characters of birds as well as any man living, and although it has often been said that he made too many species—and latterly it has been the fashion with certain writers to sink a good many of them—yet the monographer, travelling over the ground again, generally finds that the critic, and not Gould himself, was at fault. As an artist he possessed talent combined with the greatest taste, and this, added to the knowledge of botany, acquired in his early days, enabled him to give to the world the most beautiful series of pictures of animal life which have yet been produced. Certain special works, where the pencils of Wolf or Keulemans have been employed, many vie with those of Gould, but taken in a collective sense, his splendid folios, full of coloured plates, are as yet without a rival. That he was a good man of business the fact that his writings were not only self-supporting, but further realised him a considerable fortune, is the best proof. Though in outward seeming he was stern and even somewhat brusque in manner, those who knew him well can vouch for the goodness of his heart, and can tell of many an act of kindness and charity, concealed from the world under a bluff exterior, and no one ever heard him speak unkindly of any of his contemporaries. Straightforwardness was one of his especial characteristics, as well as an exact manner of doing business, paying for everything the moment the work was done; and this probably accounts for the way in which his artists, lithographers and colourers, worked for him for long periods of years.

Mr. Gould at his death was in his seventy-seventh year, having been born in September, 1804. He was a native of Lyme in Dorsetshire, but when quite an infant his parents moved to the neighbourhood of Guildford When he was fourteen years of age his father was appointed a foreman in the Royal Gardens at Windsor, under Mr. J. T. Aiton, and here the lad had a grand opportunity of studying British birds in a state of nature; in his collection are still to be seen two magpies shot by himself and stuffed at the age of fourteen, which are even now most creditable specimens of taxidermy, and foreshadowed the excellence which he afterwards attained to in that art. Till the year 1827, when he came to London, he was still employed in active gardening, having left Windsor for a post at Sir William Ingleby's at Ripley Castle in Yorkshire. Immediately after coming to town he was appointed curator to the Zoological Society's Museum, at that time in its infancy, and he enjoyed the intimate friendship of Mr. N. A. Vigors, then one of the leading English naturalists, and through him John Gould received his first opportunity of appearing as an author. So rare were Himalayan birds in those days that a small collection was thought worthy of description by Mr. Vigors in the *Proceedings* of the Zoological Society, and the figuring of these specimens was commenced by Mr. Gould under the title of "A Century of Birds from the Himalayan Mountains." By this time however an event had taken place which had an influence on the whole of his later life, viz., his marriage with Miss Coxen, the daughter of Mr. Nicholas Coxen of Kent. Besides her other accomplishments Mrs. Gould was an admirable draughtswoman, and, from her husband's sketches, she transferred to stone the figures of the above-named work. Its success was so great that in 1832 the "Birds of Europe" was commenced, and finished in five large folio volumes in 1837, while simultaneously, in 1834, he issued a Monograph of the Rhamphastidæ or family of Toucans, and in 1838 a Monograph of the Trogonidæ or family of Trogons. To the last he maintained his love for these birds, and one of his most recently finished works was a second edition of the last-mentioned Monograph. It is a curious fact that when John Gould proposed to publish his first work, he applied to several of the leading firms in London, and not one of them would undertake to bring it out, so that it was only with reluctance that he began to issue the work on his own account. Besides these larger publications he had described the birds collected during the voyage of the *Beagle* by his friend Mr. Darwin, and had contributed papers on other subjects to the Zoological Society's publications.

We now come to what we consider the most striking incident in Mr. Gould's life, one unsurpassed in its effects in the annals of ornithology. Beyond a few scattered descriptions by some of the older authors and an account of the Australian birds in the museum of the Linnean Society, by Messrs. Vigors and Horsfield, the birds of Australasia were very little known at the date we speak of. Accompanied therefore by his devoted wife, Mr. Gould proceeded in 1838 to study Australian birds in their own home, and he personally explored Tasmania, the islands in Bass's Straits, South Australia, and New South Wales, travelling 400 miles into the interior of the latter country. This voyage, specially undertaken for the purpose of obtaining an exact knowledge of Australian birds, must ever be reckoned as a distinct scientific achievement, and the accounts of the habits of some of the more remarkable species, such as the mound-building Megapodes and the Bower birds were quite triumphs in the way of field ornithology. Nests and eggs were collected as well as an excellent series of skins, both of mammals and birds, and here Mr. Gould's beautiful method of preparation was especially noticeable; some of his specimens, skinned more than thirty years ago, are as neat in appearance and as fresh as the day they were prepared. Returning in 1840, after two years' absence, he commenced the great work on the "Birds of Australia," which makes seven folio volumes and occupied seven years in its production, being completed in 1848. One of the features of this work is the great increase in our knowledge of the range and habits of petrels and other seabirds, to which the author paid great attention during his travels.

Within a year of Mr. Gould's return from his adventurous voyage he had the misfortune to lose his wife, and for some time he was completely overwhelmed by his bereavement. His collectors in Australia too, about the same period, lost their lives; one of them, Mr. Gilbert, was killed during Dr. Leichhardt's expedition overland from Moreton Bay to Port Essington, and Mr. Drummond, while collecting in Western Australia, was also murdered by natives, and a third collector was killed by the explosion of a gun on one of the islands of Bass's Straits. It speaks volumes however for the zeal and energy with which Mr. Gould had prosecuted his researches in the Australian continent that very few birds, sufficient only to form a supplement in a single folio volume, have been discovered since he left the field of his labours in that quarter of the globe.

Another landmark in the career of this great ornithologist was the publication of his Monograph of the Trochilidæ, or Family of Humming Birds. These lovely little birds had been for a long time favourites with Mr. Gould, who gradually began to amass that fine collection which has been the admiration of naturalists for so many years. Taking advantage of the Great Exhibition of 1851, he obtained permission from the Zoological Society to erect at his own cost a large building in their gardens in the Regent's Park, where the collection was open to the public at a charge of sixpence per head. A considerable sum was realised by this exhibition, and a large number of subscribers to his monograph was obtained, including nearly all the royal families of Europe. Though sketched by Mr. Gould himself (for even to the last days of his life he executed the designs for all his plates), the majority of the humming-birds were placed on stone by Mr. Richter, who also did the same for Mr. Gould's next work, "The Birds of Asia." We cannot but regard this as one of the most valuable of all the works done by the author, for, notwithstanding the fact that it is left unfinished at his death, it contains a large number of plates of species not elsewhere figured. The "Mammals of Australia," produced simultaneously with the last-mentioned work, deserved, in Mr. Gould's own opinion, more credit for its issue than perhaps any work he had done, because it touched upon a branch of zoology of which he never pretended to have a very exact knowledge. So large however had been his collections of mammalia during his sojourn in Australia that some account of them seemed to be demanded, and he therefore published his large folio work; but the pecuniary results were less satisfactory than with any of his ornithological productions. His typical specimens of the Australian mammalia are in the national collection. No sooner were the humming-birds finished than his active brain conceived a new idea, to illustrate becomingly the birds of his native land, and he commenced the publication of the "Birds of Great Britain." Opinions may differ as to the merit of Mr. Gould's other works; volumes less ponderous than the folios which he adopted for the better figuring of the objects of the natural size, may take their place with the student; but no work of greater beauty will be produced than that on which John Gould, returning in his later life to his first love, bestowed the fulness of his energy and the acme of his artistic talent. The care bestowed on the plates of this work was remarkable, the aim of the author being to produce a picture of the birds as they appeared in their natural haunts, and especial pains were bestowed on the young, particularly those of the wading-birds and natatores. In this fine work most of the drawings were developed and placed on stone by Mr. W. Hart, who also executed all the plates of the later works.

In 1865 Mr. Gould republished his letterpress of the big work in an octavo form, under the title of "A Handbook to the Birds of Australia," but with all the additional species inserted in their proper families; these two volumes are therefore of great use to the student. After the completion of his work on "British Birds," Mr. Gould devoted himself to the continuation of the "Birds of Asia" and the Supplement to the "Birds of Australia," until in 1875 he commenced a work on the "Birds of New Guinea," which was to contain also descriptions of any new species to be discovered in Australia or any part of the Australian region. Of the last-named work eleven parts have appeared, and it was left unfinished at his death, as well as the following works:—a "Monograph of the Pittidæ or Ant-Thrushes of the Old World" (one part published), the Supplement to the "Monograph of the Humming Birds" (two parts published), and the "Birds of Asia."

The above list enumerates, we believe, nearly all the works published by Mr. Gould with the exception of the "Icones Avium," issued about 1838, and containing supplementary plates to his previous volumes, with descriptions of new species, and the "Monograph of the Odontophorhinæ or Partridges of America." In addition to the folio volumes he was also in the habit of publishing the introductions to his larger works in an octavo form.

Many of the above details of Mr. Gould's life are taken from "Men of Eminence," aided by the personal recollections of the writer, who was for many years an intimate friend of the deceased, and knew him first as a successful trout-catcher on the Thames, for his prowess in throwing the fly was scarcely second to his skill as an artist. Were he to write an epitaph of John Gould he would do so in the words which Mr. Gould himself was fond of quoting:—"Here lies John Gould, the Bird-Man." The latter words were used by an old and intimate friend in introducing Mr. Gould to another relative. We may hope that the Government, according to the well-known wishes of the deceased naturalist, will allow no false motives of economy to interfere with the purchase of Mr. Gould's collection of birds for the British Museum, and that the disgraceful spectacle of his Australian collection (unrivalled to this day, and offered to the nation for the small sum of 1000*l.*) being allowed to leave the country, may not be repeated.

THE BLACKHEATH HOLES

THE chalk forming the base of the escarpment between Woolwich and the entrance to the valley of the Ravensbourne, dips at a low angle to the south-south-east under Greenwich Park and Blackheath, where it is overlaid by the Thanet Sands, estimated by Mr. Whitaker of the Geological Survey at 40 to 50 feet, the Reading and Woolwich Beds, consisting of shelly clays, sometimes 40 feet thick, associated near Lewisham with fine laminated sands. These beds are overlain by the Oldhaven or Blackheath gravels, reaching a thickness of about 50 feet, which have been largely dug for gravel in various parts of the district.

In the centre of this tract at Blackheath, on the west side of the angle of the roads from Greenwich Park to Blackheath Station, and from the Park to the Paragon, appeared in the early morning of Thursday, April 12, 1878, a subsidence near the row known as Rotten Row, referred to in these columns at the time, the hole being 8 or 9 yards in circumference. In November, 1880, appeared another hole near the gravel pit below Eliot Place and Heath House, and about 550 yards south-west of the first hole; and still later in that month, on the 19th, a third subsidence made its appearance, this time about 100 yards to the south-east of the first subsidence, and nearer to All Saints' Church.

The Astronomer-Royal and other inhabitants of the district being anxious to know how far other subsidences were probable, asked the Metropolitan Board of Works, who have jurisdiction over the Heath, and who had fenced in the sinkings, to investigate their cause. This however they declined to do, though giving to the Astronomer-Royal permission to do so; this authority he handed over to a newly-formed society, called the Lewisham and Blackheath Scientific Association, who formed a committee of investigation, including members of the West Kent Natural History Society, for which end subscriptions are now being sought, and operations will shortly be commenced, as announced in our columns.

The surface of the chalk is estimated by one member of the committee, Mr. T. V. Holmes, as probably occurring at about 100 feet from the surface at or about the Ordnance datum line. The investigations so far made show the third sinking to consist of an oval vertical shaft 7 feet 8 inches diameter by 6 feet 9 inches, with a depth of 18 feet, opening into a cavity extending in both directions, and partly choked with fallen earth, giving a total diameter, as far as examined, of 14 feet. The upper part of the shaft is described by Mr. Holmes as consisting of sand and clay resting on sand, overlying pebbles, in which the cavity below is formed. The material carefully removed from the bottom of the pit is found by Mr. H. W. Jackson to be of the same material as the upper beds of the shaft, proving the sinking due to removal of material from below. The first sinking is filled up and cannot be investigated; the second is not fully examined for want of funds, but is wholly in gravel, and also extends underground in two directions.

Various theories have been suggested by different observers to account for their origin, some considering them artificial, Admiral Hamilton that they are caused by the abstraction of water caused by the main-drainage works, which tapped powerful springs in the Lower Woolwich Road; others connect their appearance with removal of chalk, and water in the chalk, by the Kent Waterworks, who lift daily about nine million gallons a day from their wells in the neighbourhood, whilst others connect them with excessive rainfalls, the first subsidence having taken place after the great floods in the Ravensbourne, caused by the rain of the night of the 11th and morning of the 12th of April, 1878.

The height of the chalk water-line (*Journal*, Society of Arts, 1877) at Woolwich Dockyard well is about 15 feet below the Ordnance datum line before pumping, at the Kent Waterworks, Plumpstead, 1 foot 4 inches below, but at the Kent Waterworks wells at Deptford it is pumped down to nearly 70 feet below, rising 50 feet after pumping, or about 20 feet below Ordnance datum. The surface of the chalk at Bromley, at the Shortlands pumping-station, has risen to 70 feet above the datum, the water rising after pumping to 122 feet above it. This district is on the south side of a synclinal axis ranging east-north-east through Eltham, described by Mr. Whitaker, which throws in a trough of London clay, that cuts off this supply, from the chalk water entering at the Greenwich Park escarpment.

The water-level under Blackheath is at, or about, Ordnance datum, trending south towards the London clay synclinal, corresponding, under the site of the subsidences, to the surface of the chalk beneath the Thanet sands, and if there is no great quantity of chalk above the water-level it appears improbable that the subsidences are due to pipes descending vertically into the chalk, but it is quite possible that the drainage works, removing the waters held by the pebble beds above, disturbed their stability, and caused their subsidence. On the other hand it is not impossible that drift levels may have been driven into the chalk from the ancient chalk-pits a mile distant, ceasing when they reached the outcrop of the chalk against the Thanet sand, and which is immediately under the site of the subsidences.

C. E. De Rance

MERCADIER'S RESEARCHES ON THE PHOTOPHONE

An elegant series of researches in photophony have lately been published by M. E. Mercadier of Paris, who has very carefully examined the phenomenon discovered by Graham Bell and Sumner Tainter, that an intermittent beam of light may generate a musical tone when it falls upon a thin disk. By way of distinguishing this phenomenon and its applications from the phenomenon of sensibility to light exhibited by annealed selenium, which constitutes the essential principle of the articulating photophone, M. Mercadier adopts the name of *radiophony* for the subject of his research: a name which appears moreover to have the advantage of not assuming *à priori* what kind of radiations, luminous, calorific, or actinic, are concerned in the production of the phenomenon. It is agreed by all who have experimented in this direction that the pitch of the note emitted by the disk corresponds precisely with the frequency of the intermittent flashes of light: but it has been disputed whether the effect is due to light or to heat. Prof. Bell found that the beam filtered through alum water to absorb the calorific ultra-red rays produced tones; and that even when a disk of thin ebonite rubber was interposed, the beam robbed of both heat-rays and light-rays could still generate tones. On the other hand, from the list of substances given by the original discoverers, it was evident that since dark and opaque substances with dull surfaces, and those which, like zinc and antimony, have high coefficients of thermal expansion, produce, *cæteris paribus*, the best results, the effects must probably arise from heating effects due to absorption of radiations of *some* kind and their degradation into heat of low temperature.

M. Mercadier has summarised his results in an article in the *Comptes rendus*, from which the substance of this article is translated freely. The chief conclusions are as follows:—

I. *Radiophony does not appear to be an effect due to the vibration of the receiving disk vibrating transversely in one mass as in an ordinary vibrating elastic plate.*— This conclusion appears to be justified by the following observations: that, given a thin plate of any kind, under the conditions necessary for the production of the phenomenon, it produces equally well tones of all different degrees of pitch from the lowest audible up to the highest that can be generated experimentally by optical intermissions, and which in M. Mercadier's apparatus attained to a frequency of 700 vibrations per second. Moreover it was found that these changes of pitch were accomplished without any defect in the continuity of the phenomenon; which would seem to indicate that it was not necessary for the plate to vibrate in any particular nodal or partial mode. Also the receiving disk will produce *chords* equally well in all possible tones from the highest to the lowest, the chord being complete no matter whether the fundamental pitch be raised or lowered by altering the speed of the rotating apparatus by which the intermittences are produced. M. Mercadier's apparatus consisted of a glass wheel carrying on its surface a paper disk pierced with four series of holes, numbering respectively 40, 50, 60, and 80. Through any one of these series of holes a small pencil of rays could be passed, and, by raising or depressing the axis of rotation of the wheel, could be sent successively through each of the four, thus producing, at any given rate of rotation, the separate tones of a common chord in succession: or by interposing a cylindrical lens to distribute the rays in a linear beam to the four series at once, the united tones of the chord could be produced simultaneously.

Further it was found that the thickness and the breadth of the receiving-disk makes no difference within certain limits in the loudness or quality of the resulting tone. And in the case of transparent substances such as mica and glass these limits may be wide: in the case of glass the loudness was the same with a disk of half a millimetre as with one of three centimetres thickness. In consequence rare substances may be used in disks as small as one square centimetre in area. Cracked or split disks of glass, copper, and aluminium produce sensibly the same effects as if they were whole.

II. *The molecular structure and state of aggregation of the receiving disk appear to exercise no important influence upon the nature of the tones emitted.*—Disks of similar thickness and surface emit sounds of the same pitch no matter of what material they be. Although there may be slight specific differences between the actual modes of production of the phenomenon from *very* thin disks of different materials, these differences are reduced to a vanishing quantity by rendering the receptive surface alike, as for example by covering them all alike with a film of lampblack. Moreover the effect produced by ordinary radiations is, *cæteris paribus*, the same practically for transparent substances as widely differing from one another as glass, mica, selenite, Iceland-spar, and quartz, whether cut parallel or perpendicular to the optic axis, and is the same in polarised light as in ordinary light.

III. *The radiophonic sounds result from a direct action of radiations upon the receiving substances.*—This proposition appears to be established by the following facts:—1. That the loudness of the sounds is directly proportional to the quantity of rays that fall upon the disk. 2. That by using a polarised beam and taking as a receiving-disk a thin slice of some substance which can itself polarise or analyse light, such as a slice of tourmaline, the resulting sounds exhibit variations of loudness corresponding to those of the rays themselves, when either polariser or analyser is turned; and the sound is loudest when the light transmitted by the analysing disk is a minimum.

IV. *The phenomenon appears to be chiefly due to an action on the surface of the receiver.*—The loudness of the emitted sound depends very greatly upon the nature of the surface. Everything that tends to diminish the reflecting power, and increase the absorbing power of the surface, assists the production of the phenomenon. Surfaces that are rough-ground or tarnished with a film of oxidation are therefore preferable. It is also advantageous to cover the receiving surface with black pulverulent deposits, bitumen black, platinum black, or best of all with lampblack; but the increase of sensitiveness under this treatment is only considerable in the case of very thin disks, as for instance from ·1 to ·2 of a millimetre. Very sensitive radiophonic receivers may be thus made with extremely thin disks of zinc, glass, or mica smoked at the surface. It may here be noted amongst M. Mercadier's results that for *opaque* disks, the thinner they are the louder is the sound, and that excellent results are given by metallic foil—copper, aluminium, platinum, and especially zinc—of but ·05 millim thickness. The employment of such sensitive receivers has enabled M. Mercadier to arrive at several other important conclusions

V. *Radiophonic effects are relatively very intense.*—They can be produced not merely with sunlight or electric light, but with the lime-light, and also with gas-light, and even with petroleum flames, and with a spiral of platinum wire heated in the Bunsen-flame.

VI. *Radiophonic effects appear to be produced principally by radiations of great wave-length, or those commonly regarded as calorific.*—In order to satisfy himself on this point M. Mercadier had recourse to the spectrum direct, without attempting to employ cells of absorbant material such as alum solution or iodine in dissolved bisulphide of carbon as ray-filters. A brilliant beam of light was produced by means of a battery of fifty Bunsen cells, and with this, by means of ordinary lenses and a prism of glass a spectrum was produced, the various regions of which could be explored with one of the sensitive receiving-disks mentioned above. The maximum effect was found to be produced by the red rays and by the invisible ultra-red rays. From yellow up to violet, and beyond, no perceptible results were obtained. The experiment was tried several times with receivers of smoked glass, platinised platinum, and plain bare zinc. The greatest effect appeared to be yielded at the limit of the visible red rays. The rays which affect the electric conductivity of selenium in the photophone are, as Prof. W. G. Adams has shown, not the red rays, but rays from the yellow and green-yellow regions of the spectrum. This fact alone would justify the distinction drawn between the phenomena of radiophony and those of the selenium photophone, though probably these are only two of several ways of arriving at a solution of the problem of the transmission of sonorous vibrations by radiation. Theoretically a telephone with a blackened disk inclosed in a high vacuum and connected with an external telephone should serve as a receiver; and the writer of these lines has already attempted to devise a thermo-electric receiver for reproducing sounds from invisible calorific rays.

S. P. T.

THE JOHN DUNCAN FUND

THE following subscriptions to this fund have been received during the past week:—

	£	s.	d.		£	s.	d.
Amount previously announced	48	6	0	Major Deedes ...	0	10	0
Charles F. Tomes, F.R.S.	1	0	0	Anon.	0	1	3
J. S.	2	0	0	Sir J. Fayrer	1	1	0
Dr. Vacher	1	1	0	T. C. Kent	1	0	0
R. R. Glover	1	1	0	Lawson Tait	1	1	0
Thomas Walker ...	5	0	0	Heinrich Simon ...	2	0	0
M. M. Pattison Muir	1	1	0		65	2	3

THE TIME OF DAY IN PARIS

THE importance of precise and uniform time throughout Paris becoming ever and continually more appreciated, the Municipality have taken the matter in hand, and have established a system of what they call "horary centres." These horary centres really consist of standard clocks, erected in different places, and controlled by electricity from the Paris Observatory. Moreover each standard clock is furnished with additional electrical work of its own, which enables it to send out an hourly current and control other clocks in its neighbourhood, placed in circuit with it. The advantage of this arrangement over any system of electrical dials is apparent, for with the latter any mischance or practical joke with the wires would cause the whole city to be misled or completely deprived of time. The problem, as put by Leverrier, and as it has been practically solved by M. Breguet, was this:—To keep correct the hour given by various regulators distributed in the city by means of an electric current sent from the Observatory. If the current, in consequence of any accident, fails, the regulators continue to work, with a very slight advance, without the electric correction. The wires have their centre at the Observatory, where there is an astronomical regulator on the first floor. This instrument is maintained at the exact time indicated by the astronomical observations, by means of an arrangement which obviates the stopping of the pendulum and changing its length. At the bottom of Fig. 1 is a box C, in which may be placed small weights. The weights are of such a shape that it is easy with suitable pincers to put them in or take them out without touching the clock or disturbing anything. The addition of a weight makes the regulator go faster; its withdrawal retards it. At the upper part of the pendulum is seen the apparatus by which the currents are transmitted; it is in duplicate, because the pendulum beats seconds, and it is desired to send the current every second. Each apparatus is composed of three identical pieces; three small levers are placed side by side, pivoted at their farthest ends. Their end *i* is raised by the arm V carried by the pendulum at each of its oscillations. During all the time which this contact lasts, the current of a battery passes by the suspension of the pendulum to the arm which carries the three screws and the three levers which conduct it to the line. With a single lever there would be danger of interruptions by a grain of dust; with three, contact and transmission of the current are absolutely assured. From the Observatory two wires set out; no use is made of the return earth current. The wires are entirely in the drains, like those of the Telephone Company. Fig. 3 shows these two circuits, each of

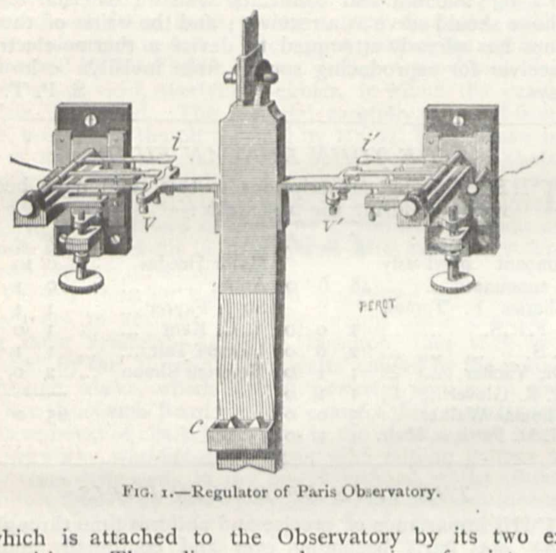

Fig. 1.—Regulator of Paris Observatory.

which is attached to the Observatory by its two extremities. These lines pass by a series of points and traverse the regulators, of which we shall now speak, and which are called horary centres. The pendulum of each regulator (Fig. 2) presents at its lower part a piece of soft iron, which in the oscillations of the pendulum is brought in front of the poles of two electro-magnets in succession. The transmission of the current into these electro-magnets tends to retard a little the movements of the pendulum, and causes each to be perfectly synchronous with that of the Observatory. The regulators of the horary centres show the second; they are placed in the street, and consequently in view of the passers-by, who may thus compare their watches. Watchmakers may also thus obtain the exact time without making a journey to the Observatory. They are placed in several prominent buildings in various convenient centres.

Why these regulators are called horary centres is explained thus: upon the circuit of horary centres spoken of above, and which the accompanying plan (Fig. 3) indicates by a black line, is grafted another accessory, called the transmission of the hour. Each regulator of the main circuit is itself the centre of a less extensive network of wires, which transmit the hour to the public clocks. For this second service no unique system has been adopted, and uniformity has not been aimed at. Several of the principal watchmakers of Paris, inventors each of a special method of transmitting the hour, are authorised to apply it to the clocks of which they have the care, by borrowing the hour and the current from the nearest horary centre. The most interesting horary centre is that installed at the Hôtel de Ville (at present the Tuileries), and which radiates to the twenty *mairies* of Paris. The city has a telegraphic communication which places the Prefecture of the Seine in connection with the twenty *mairies*. The wires of this system are interrupted about two minutes every hour to place the clock of each *mairie* into agreement with the regulator (horary centre) of the Hôtel de Ville as follows. Beside the regulator are placed twenty relays, into which it sends every hour a current, which cuts off the line from the telegraph; this commutation is made 100 seconds before the hour. The same regulator, about twelve seconds before the hour, sends the current from a second battery along

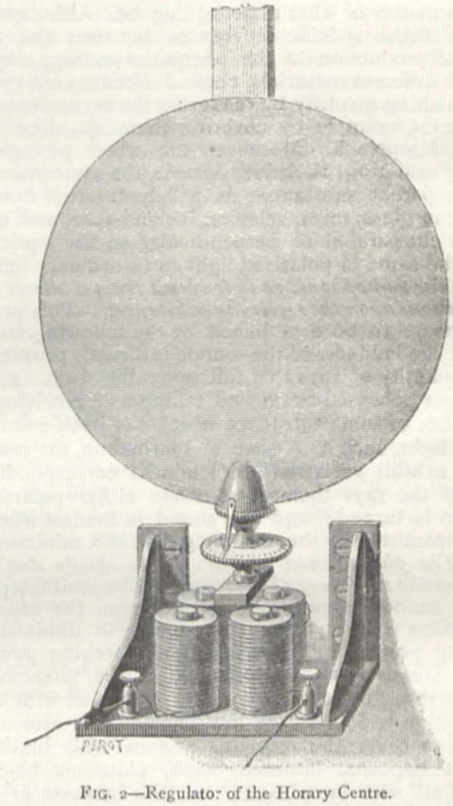

Fig. 2—Regulator of the Horary Centre.

the lines; it interrupts it at the hour precisely. Ten seconds after the hour the relays are restored to their normal position by the suppression of the first current; that is to say, the lines are restored to the telegraph.

On the other hand sixty-five seconds before the hour each *mairie* clock makes its commutation, *i.e.* cuts off the line from the telegraph and connects it with the electro-magnet of the clock. And five seconds after the hour it makes the inverse commutation and restores the line to the telegraph five seconds before the resumption of the line by the telegraph at the horary centre of the Tuilleries. As the clocks are thus regulated every hour their errors are extremely small. If however a clock gets suddenly out of order or stops, what happens? The current of the horary centre is sent into the telegraph of the *mairie* for thirty seconds continuously; this abnormal fact announces at once to the telegraphist that the clock is out of order, and he may give orders to have it set right.

In the other horary centres the organisation is less complicated; it is provided for only six lines, but on each of these may be placed several clocks. The lines are shorter, and radiate only in the quarter which surrounds the horary centre; but they are special to the service of

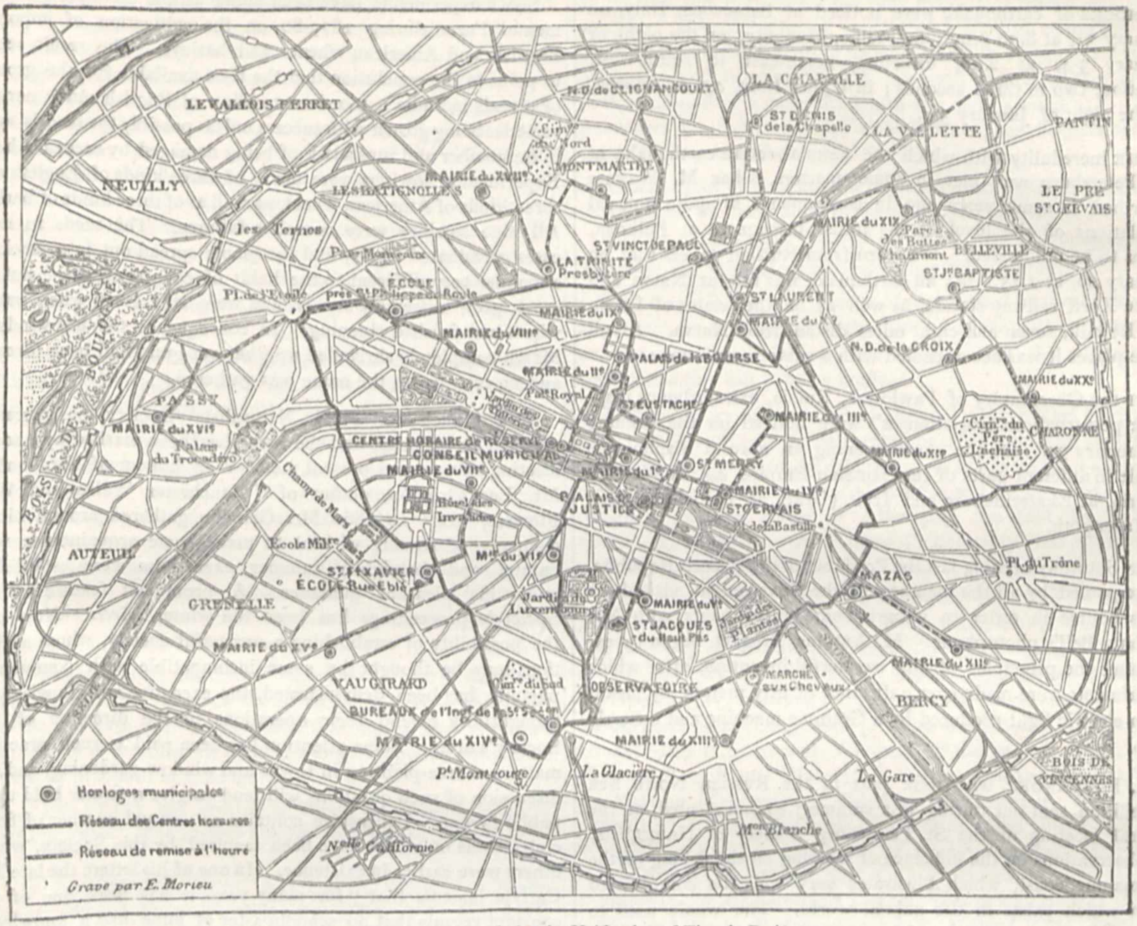

FIG. 3.—Telegraph for the Unification of Time in Paris.

clocks, and are not subject to the complicated operation which is necessary on the circuit of the *mairies*. This service has been organised under the direction of the City Engineers, and does them the highest credit; no doubt it will be gradually developed, so as to include the whole of the *mairies* of Paris.[1]

NOTES

THE honour of knighthood is to be conferred on Dr. James Risdon Bennett, F.R.S., President of the Royal College of Physicians.

M. MARCEL DESPREZ, the well-known electrician, has been created a Knight of the Order of the Legion of Honour.

THE first volume of the U.S. Geological Survey, issued under the headship of Mr. Clarence King, is a magnificent quarto by Prof. O. C. Marsh:—"Odontornithes: a Monograph on the Extinct Toothed Birds of North America"; with thirty-four beautifully executed plates and forty woodcuts. We hope to refer in detail to Prof. Marsh's work very soon.

IN the House of Commons on Tuesday Mr. Shaw-Lefevre said that both the botanical and mineralogical collections have been already removed from the British Museum to the new Museum of Natural History at South Kensington, and are now being arranged there. It is expected that these collections will be open to the public on the next bank holiday—namely, Easter Monday, April 18.

THE Paris Exhibition of Electricity will contain a number of curiosities. M. Salignac will present to the Director-General a plan for cooking by electricity in the grill-room of the restaurant. This plan should provide useful work during the day for the magneto-electric machinery, and test its warming power. M. Michels, an American residing in Paris, has patented a revolving carbon which can be rolled like an ordinary conductor.

WE have now more detailed information about the earthquake which was felt in the Swiss Jura on January 27 at 2h. 18m. p.m. There were two shocks at an interval of five seconds. They were felt especially at Berne, where several chimneys were thrown down, the bell of a church sounded, and the ceiling of a school fell down. At St. Imier the shocks were also rather strong. They were felt also at Neufchâtel, Corcelles, Fontaines, Colombier, Auvernier, and Chaux-de-Fonds to west; at Morges (but not at Lausanne) to south-west; at Solothurn, Basel, and Zürich to north and north-east; and at Signau, Hüttwyl, Berthoud, and Thoune to south and south-east. Two smaller shocks were felt: one on the same day at six o'clock in

[1] From an article in *La Nature*, by M. A. Niaudet.

the evening, and the other at three o'clock on the morning of the following day.

SHOCKS of earthquake were noticed at Baraccone, Italy, on January 31, at 8.30 p.m.; at Fiume, Hungary, on the night of January 3-4, at 2.26, direction, north-east to south-west, duration, two to three seconds; in Upper Italy, *e.g.* at Ancona, on the night of January 3-4.

THE incredulity with which the news about an earthquake at St. Petersburg was met in some quarters, when M. Wagner described it some years ago in consequence of quite unusual oscillations of the level of his transit-instrument at Pulkova, seems to be unfounded. We learn from Russian papers that on January 26, at 2.15 p.m., an earthquake was felt at Narva and at the Korff railway station, as well as on the estates of Lagen and Repnik, seven and eight miles distant from Narva. At all these places it was accompanied with a subterranean noise.

AT the Observatory of Pawlowsk, Russia, extraordinary magnetic perturbations (variations 2°) were noticed on the evening of January 31. On the same evening an auroral display was visible in different parts of the Russian Empire, *e.g.* in Western Siberia, at Ekaterineburg and Irbit (Ural), at Baltishport, and at Hasenpoth.

ON Saturday evening last the President of the French Republic, accompanied by all the members of the Government, visited the Paris Bourse in order to witness some experiments with Mr. Graham Bell's photophone. M. Antoine Breguet began by explaining the principles of the wonderful invention, after which experiments were made over a distance of fifty metres, by means of an electric light produced by a Gramme machine and a Serrin lamp.

M. BOGDANOFF, who took part in the Russian North Sea Expedition sent out during last summer, communicated, at the general meeting of the St. Petersburg Society of Naturalists, his observations on the influence of whaling on the fishing on the Normannic coast, which illustrates very well the complicated chain which exists in the animal world. The whale used to be very important to the fisheries, as during the spring it drove to the coast immense shoals of small fishes. Now, whaling being pursued by means of steamers which use a bullet instead of the old harpoon, and the annual number of whales killed being, during the last seventeen years, from 50 to 143, the amount of small fishes coming to the coast has much diminished. Besides, the great quantities of fat which are thrown into the sea at Varanger attract sharks, and these last destroy cod-fish, so that now the cod-fishing is nearly extinct in the western parts of the Varanger fjörd region.

MR. WALLACE in his "Island Life," pp. 495, 496, has discussed the apparent inability of Australian plants to become naturalised in the northern hemisphere. The gist of his explanation is the want of elasticity in their constitution, owing to their long-continued insular and uniform conditions of existence. The accompanying extract from the Report of the Government Gardens at Rangoon for 1880 points to the incapacity of even the vegetation of Tropical Australia to stand really humid climatic conditions:—"The Australian Eucalypti grew well during the dry weather, and some of them were four feet in height when the monsoon commenced; they then damped off one after another, as did also the Australian Acacias and the Queensland Ficus. The Moreton Bay Chestnut is not flourishing. From the above it will, I think, be evident that plants of Tropical Australia will not readily accommodate themselves to this very moist climate." It may be added that the result of attempting to grow species of *Eucalyptus* from all parts of Australia in the West African Settlements has uniformly failed, and apparently from the same cause.

SOME experiments have been made at the Cawnpore Experimental Farm during 1879-80 on the cultivation of imported English and American wheats and barleys. The result seems to point to the conclusion that the time available for the growth of cereals in India is too short to allow of English and American varieties being grown with success unless possibly the seed is sown in September and runs a risk of being damaged by excessive heat. Experimental sowings were made of three kinds of English and three kinds of American wheat, as well as of three kinds of barley. All nine sowings were complete failures. The seeds in most cases germinated freely, and the plants spread out into stools in a manner very different to the habit of country wheat. But all crops grew extremely slowly, and were still green when native wheat had finished ripening. In consequence the hot winds of March completely shrivelled up whatever grain had been formed, and no crop worth the name was gathered.

THE Manchester Field Naturalists' Society has recently attained its majority, and the event has been marked by a social meeting of past and present members in honour of the founder, Mr. Leo Grindon, author of "Manchester Walks and Wild Flowers," &c. In 1860 Mr. Grindon gathered around him a company of friends wishful to make some acquaintance with nature, and fortnightly summer excursions were established under his pleasant guidance. A prominent feature of the Society's proceedings has been the winter *soirées*. Now that the possibility of establishing a successful society, whose aspirations may be thought by some incompatible with commercial pursuits, has been demonstrated, the executive will do wisely to thoughtfully extend their operations in the direction of the Society's aims. Some attention has been paid to such practical matters as tree-planting in towns and window-gardening, and the discussion of such questions will tend to give a firmer hold upon public favour. Lancashire contains an unusual number of field-clubs, some of which have been inspired by this Society, whilst others were earlier in existence. In one of his letters the late Mr. Carlyle laments that "for many years it has been one of my constant regrets that no schoolmaster of mine had a knowledge of *natural history*, so far at least as to have taught me the grasses that grow by the wayside, and the little winged and wingless neighbours that are continually meeting me with a salutation which I cannot answer." Had he been a native of Lancashire he would have found many instructors willing to read him the lessons of the wayside.

ON the 9th inst. the Dundee Naturalists' Society held their sixth annual *conversazione*, which seems to have been quite successful. All sorts of scientific *matériel* were exhibited, and among other lectures given was one by Dr. McIntosh of Murthly on Sponges. This society is evidently in a flourishing condition, and is no doubt doing something to create an interest in science in the important seaport in which it is located.

MR. QUARITCH has just issued the second part of his new Catalogue of Works on Natural History. It seems to contain a large number of very scarce and valuable works.

A FEW months ago three large blocks of petrified wood were found in the Devonian bed at Döppersberg, Germany. They were recognised by Prof. Göppert of Breslau as belonging to a fossil Araucaria, named by him *Araucarites Elberfeldensis* (Döppers).

THE Baltic Centralverein für Thierzucht und Thierschutz will hold its third exhibition of domestic birds on March 11-13 at Greifswald. In addition the exhibition will include living and dead freshwater and marine fish, fish embryos, &c., and all apparatus pertaining to pisciculture and fishing.

AN important step has been attained in telephony by Dr. Cornelius Herz, by which the principle of magnetism has been entirely discarded and the magnetic receiver abolished. A long series of experiments have been successfully conducted under the patronage of the French Government on the telegraphic lines of the State; concluding trials were witnessed, among others, by M. Cochéry, Minister of Postal Telegraphy, M. Jules Ferry, Prime Minister, M. Leon Say, President of the Senate, M. Becquerel, and other Members of the Academy of Sciences, and other Members, Senators, Deputies, and a great number of engineers. One of the most extraordinary experiments was the transmission of speech on a single wire from Tours to Brest, on a wire passing through Paris, the length of which exceeded eight hundred miles. One single Leclanche's element was the sole battery in use.

SOME dredging work which is going on at Zürich in the bed of the Limmat has brought to light the shore pillars of a Roman bridge, as well as the skeleton of a prehistoric stag.

INTERESTING new discoveries have been made at Pompeii. In block 7 of the 9th district a house has been excavated which was in course of construction when the terrible catastrophe occurred, and which differs materially from all other Pompeiian houses in its plan. In another house a large square piece of black glass was found fixed into the wall, which when slightly moistened forms the most perfect mirror. In a third house various wall-paintings were discovered, which however are rather of artistic than scientific interest.

THE newly-elected Municipal Council of Paris has been summoned by the Prefect of the Seine for a session which will begin on the 11th inst. It is stated that one of the proposals made will be to establish in Paris a system of police telephonic stations, as practised in Chicago.

M. JULES FERRY has created a library for patients in every hospital in Paris. The system will be extended to the whole of France.

AT the meeting of the Royal Academy of Sciences at Berlin on January 27 last, the year's report (for 1880) for the Humboldt Institution for Natural Research and Travels was read. Prof. du Bois Reymond, in conjunction with Prof. G. Fritsch, is about to publish the observations and experiments made by the late Dr. Karl Sachs on *Gymnotus electricus* in South America during 1876 and 1877, by order of the Institution. The present traveller of the Institution, Dr. Otto Finsch, after staying for nearly a year upon Talint Island (one of the Marshall group) proceeded to Matupi (on the north coast of New Britain) at the end of last year. His last letter is dated October 27, 1880, and he announces that he has made rich zoological collections. He intended to visit New Ireland and New Guinea if possible, and then to return to Europe by way of Dutch East India. Four of Dr. Finsch's collections have arrived at Berlin; a fifth is announced by his first letter from Matupi. The funds of the Institution have been increased by small legacies. The sum which will be at the disposal of the Institution for 1881 is 12,750 marks (635*l.*)

THE Sydney correspondent of the *Colonies* writes:—"We have long had in Sydney splendid botanical gardens, containing the choicest plants in the world, but we have only recently started a 'Zoological Gardens,' though Melbourne has had one many years, which has been brought to a high degree of perfection. Last week a deputation waited on our Colonial Secretary, asking for funds to stock the Gardens. Sir Henry Parkes replied that if the members of the Zoological Society would undertake next year to put as many animals in the grounds of the Sydney Zoological Gardens as they have in Melbourne, he would guarantee them 10,000*l.* from the Government. The offer was not accepted."

THE *Chrysanthemum* is the title of a monthly magazine "for Japan and the Far East," the first number of which has been sent us. The contents are mostly of a literary character, the main object of the magazine being "to aid in bringing the pales of Eastern and Western thought into such contact as may result in the diffusion of a general warmth and light around us." The publishers are Kelly and Co. of Yokohama, the English agents being Trübner and Co.

A SKELETON of a mammoth has been discovered at Bendery, Government of Bessarabia, in the upper clay drift.

THE St. Petersburg Society of Naturalists has already 276 Fellows; the Mineralogical Society has 398 members.

THE Commission of Fisheries of the United States have sent a quarter of a million ova of the American whitefish to Bremen, *en route* for the Lake of Constance, where the attempt to acclimatise this fish is to be made.

THE centenary of the birth of the philosopher Karl Christian Friedrich Krause will be celebrated on May 6 next at his birthplace, Eisenberg (Saxe-Altenburg). At the same time a simple monument with a bronze bust of Krause will be unveiled. The design is by Herr Enger of Altenburg, the bust by Robert Henze of Dresden. A Krause Scholarship has also been established at the Gymnasium.

WE have on our table the following books:—"Practical Plane Geometry," John W. Pallister (Simpkin); "Introduction to Study of Indian Languages," J. W. Powell; "Journal of Iron and Steel Institute, 1880" (Spon); "Practical Botany," D. Houston (W. Stewart); "Popular Scientific Lectures," 2nd series, Helmholtz (Longmans); "The Evolutionist at Large," Grant Allen (Chatto and Windus); "Journal of Royal Society of New South Wales;" "Extinct British Animals," J. E. Harting (Trübner); "Calendar of University of Wales, 1880-81;" "The Silk Goods of America," 2nd edition, W. C. Wyckoff; "London Catalogue of British Mosses" (Bogue); "The Statistical Atlas," part 1, G. P. Bevan (W. and A. K. Johnston); "Kamelaroi and Kurnai," Fison and Howitt (Macmillan and Co.); "Meeresfauna," K. Möbius (Otto Enstin); "Annuaire pour l'an 1880" (Villars, Paris); "A Polar Reconnaissance," A. H. Markham (Kegan Paul); "Natural History of British Fishes," Frank Buckland (S.P.C.K.); "Ventilation and Heat," Frederick Edwards (Longmans); "Practical Physics," A. H. Worthington (Rivington); "Muscles and Nerves," Dr. T. Rosenthal (Kegan Paul); "Natural Philosophy Examination Papers," Rev. G. Molloy (Browne and Nolan); "On some Properties of the Earth," O. Reichenbach; "Evolution, Expression, and Sensation," John Cleland (Maclehose, Glasgow); "The Wild Coast of Nipon," Capt. H. C. St. John (Douglas).

OUR ASTRONOMICAL COLUMN

THE SO-CALLED NOVA OF 1600.—Referring to a note which recently appeared in this column on "Janson's Star of 1600," Prof. van de Sande Bakhuysen, Director of the Observatory of Leyden, writes us that "Janson or Gulielmus Jansonius is Willem Jansz Blaeu, who is well known as the maker of globes, which are now very rare, and as editor of a treatise on the use of globes, of different treatises on navigation, and of a great number of charts and different atlases. From 1598 till his death in 1638 he lived in Amsterdam. Janson signifies that he was the son of Jan (John), but his family name was Blaeu." This explanation will be acceptable to those who may have been perhaps somewhat in doubt as to the correct form of identifying the discoverer of the variable star of 1600; Kepler styled him Jansonius, without reference to what Prof. Bakhuysen states to have been his surname: and he is frequently called Jansen. Lalande refers to the globes constructed by Blaeu as the best of the period, and the fact of his remarking the star in question, of which there is no previous mention, proves that he was a careful observer of the heavens. In the *Bibliographie Astronomique* we find an astronomical work printed in 1625, attributed to him as Willem Jansz Blauw.

It will be seen from the works of Kepler and Cassini that Blaeu's star (34 Cygni of our present catalogues) at no time rose higher than the third magnitude, though even Mädler (*Populäre Astronomie*) has so far overlooked its history as to tell us "it reached the first magnitude"; and he attributes its discovery to Kepler.

THE "ASTRONOMISCHE NACHRICHTEN."—Contrary to what has been lately stated, it appears that this periodical will still be edited by Dr. C. F. W. Peters, who has for some time conducted it, and we are informed there is a probability that Prof. Krüger may set afloat a new astronomical journal under his own management. Whether the multiplication of high-class astronomical journals to the extent we are likely to witness is a practical advantage may perhaps be doubtful. For many years the *Astronomische Nachrichten* contained almost all that bore upon the progress of exact astronomy; *sed tempora mutantur, et nos mutamur in illis.*

THE COMET 1880 *e* (SWIFT, OCTOBER 10).—The completion of the mounting of the large Merz-Repsold refractor at the Imperial Observatory, Strassburg, enabled Prof. Winnecke to observe this interesting comet as late as January 26, when unfavourable weather interfered, and he was not without the hope that it would be within reach after the next period of absence of moonlight. Even if this should not prove to have been the case, there will be more than fifteen weeks' observations available for the determination of the actual orbit of the comet, affording every reason to expect that its track in the heavens nearly eleven years hence, or at its next visible return, may be pretty closely predicted. The following positions are deduced from MM. Schulhof and Bossert's last elements :—

At Greenwich midnight

1881.	R.A. h. m. s.	Decl. ° ′	Log. distance from Earth.	Sun.
Feb. 14	6 42 20	+21 21·4	9·9007	0·2147
16	6 44 54	21 10·4	9·9158	0·2192
18	6 47 29	20 59·8	9·9307	0·2236
20	6 50 5	20 49·7	9·9454	0·2280
22	6 52 41	20 39·9	9·9600	0·2323
24	6 55 18	+20 30·4	9·9745	0·2366

Prof. Winnecke reports that the Merz-Repsold refractor is a great success; *Mimas* is an easy object, and it may be hoped that the observation of the nebulæ, to which it is understood the instrument is to be chiefly directed, may not prevent attention being given to the closest of Saturn's satellites.

THE PERSEIDS IN AUGUST, 1880.—M. Baillaud, Director of the Observatory of Toulouse, has published the results of the watch for meteors, maintained by three observers on the nights of August 9, 10, and 11 in the past year: 1172 shooting-stars were observed, and 83 of the longest tracks were traced upon a chart; generally the tracks were very short, and their extremities pretty distant from the radiant. The meteors appeared to diverge from two points—the more numerous group from R.A. 42° 37′, Decl. 56° 39′; and a group of about one third the former, from R.A. 60° 39′, Decl. 62° 4′. The maximum occurred on August 10, between 14h. and 15h., in which interval 200 meteors were noted.

PHYSICAL NOTES

M. WIESNEGG has lately constructed for M. d'Arsonval a new steam-pressure regulator which deserves notice. It fulfils, according to the inventor, the following conditions:—(1) It maintains a perfectly constant pressure of steam in a boiler, whatever the actual output; (2) it maintains the consumption of fuel at a rate proportional to the output of steam; and (3) it is absolutely automatic, and therefore prevents all risks of explosion. This regulator is of very simple construction. A lead pipe from the boiler leads to a little apparatus somewhat resembling an ordinary lever safety-valve, but in which the valve-plug, instead of fitting into the usual conical seat, rests upon a thin disk of india-rubber. This disk rises when the pressure from below exceeds the downward pressure of the plug and the superincumbent lever, and of the weight which it carries. It cannot get hot, as it is far from the boiler, and the space below the disk is filled with water condensed from the steam. The upper surface of the valve-plug regulates by its movement the flow of gas, which comes in and goes out by two pipes leading to the upper part of the regulator. One of these comes from the gas mains, the other goes out to the burners under the boiler. By this arrangement, whenever the pressure in the boiler reaches any desired maximum, the apparatus itself reduces the supply and turns down the flame, thus maintaining the pressure constant and the consumption proportional to the output of vapour. It will be seen that the invention is only applicable to the case where the fuel employed is gas. The apparatus is also in itself an automatic safety-valve, putting out the fire when the pressure exceeds the limit. M. Wiesnegg has had practical experience during three years of the working of the new regulator, which appears to leave nothing to be desired in its performance. The same gentleman has constructed a constant-pressure air-blast on the same principle.

PROF. CASSANI invites attention in the *Rivista Sci. Ind.* (November 30) to some singular phenomena of geometrical optics, thus indicated:—The real images, presented by a concave mirror or by a convergent lens, of a plane or spherical mirror, a lens or a prism, may by a suitable arrangement be made to appear like a real mirror, lens, or prism respectively. An observer stands opposite a concave mirror supported (with slight slant) at a distance greater than the radius of curvature, and receiving no other light than that reflected from his face (illuminated by a dark lanterns). A small plane mirror is placed in a position nearer the concave mirror than the observer, and sloping in opposite direction (it is concealed from his eye). The effect is that, on looking obliquely upwards, the observer seems to see a plane mirror (which is of larger size than the other) with his direct image in it. The illusion is the more complete if the actual plane mirror have an ornamental frame, and this be illuminated by a special lamp. As the image in the ideal mirror is always rather small and too near the mirror, this may arouse suspicion, the more so when the image is seen to diminish on receding and increase on advancing; but a person not familiar with the phenomena of concave mirrors may easily be deceived, thinking he sees a real mirror.

IN the *Proc. R.S.E.* Sir W. Thomson describes a thermomagnetic thermoscope of an ingenious nature. It is well known that the "permanent" magnetism of steel magnets is not constant, but changes slightly with changes of temperature, the magnet becoming weaker when warmed, and recovering its strength as it is cooled. The magnetic thermoscope is intended to indicate differences of temperature by showing differences between the magnetic moments of steel magnets. Two thin wires of hard steel, each one centimetre long, are arranged so as to form a *nearly* astatic couple, being magnetised to equal strength and set in opposite directions, but not quite parallel, so that they set at right angles to the magnetic meridian. Two other magnets, about twice the size of the former pair, are placed one on each side of this astatic couple as "deflectors," being laid in one line nearly along the magnetic meridian, with their similar poles facing one another at about two centimetres apart. When properly adjusted the little astatic pair suspended between them will be found to be excessively sensitive to the least change in the strength of either of the deflectors, and if they are at different temperatures will turn through an angle which if small may be regarded as a measure of the temperature-difference. A small mirror suspended from the lower needle of the pair serves to reflect a spot of light on to a scale in the usual way.

IN 1870 and 1871 MM. Leverrier and Crova experimented with an optical telegraph between Nîmes and Redessan. Their system of signals were made by means of oil lamps or petroleum lamps fed by oxygen from a supply that could be turned on or off at will by an operator, who thus produced intermittent brilliant outbursts of flame according to a pre-arranged code. During December, 1880, a similar device was conceived by M. Mercadier, against whom M. Crova now reclaims the essential principles of his invention. He adds that two of the requisites of success lay in the use of oxygen under *very low pressure*, feeding the flame by an orifice in the midst of the flame, and in the employment of keys opening and shutting the gas-passages very suddenly by means of strong springs, without which the changes in the intensity of the flame go on too slowly to be comfortably observed. In the experiments of 1870-71 the lights at Nîmes were visible at Redessan and *vice versâ*, even in broad daylight. The oxygen supply was contained in ordinary gas bags of caoutchouc and prepared in the usual manner.

ONE of M. Mercadier's recent experiments in radiophony deserves a note. A disk of thin copper about 4 centims. in diameter, heated at its back by an oxyhydrogen blowpipe, was placed behind a rotating wheel with apertures, and the intermittent heat-rays were received upon one of his sensitive disks of thin metal blackened at the surface. With a bright red heat the customary note was well heard from the intermittent beams. On putting out the flame the sound gradually fell off in intensity, but was still audible after the copper disk had ceased to emit visible rays. All that this experiment proves, however, is that the dark rays, when they fall intermittently upon an absorbent surface, can cause it to undergo rapid expansions and contractions; while Graham Bell's earlier experiment showed that visible rays could produce this result.

M. CORNU discusses in the *Comptes rendus* the propositions of M. Gouy concerning the velocity of propagation of light proceeding from a source of variable amplitude, on which we lately published a note. He denies the truth of M. Gouy's fundamental assumptions, and concludes that since all our appliances can only change the amplitude of the waves by quantities which may be regarded as constant during a great many successive waves, the formula of waves of persistent type will still hold good, and the velocity of propagation of the amplitudes will be identical with that of the waves themselves.

M. CHAPPUIS thinks that the blue of the sky may be due to ozone present in the upper regions of the air. He argues that the electrical discharges constantly taking place will produce ozone; and the recent researches of himself and M. Hautefeuille have shown that ozone, at any rate when near its condensation point, is of a blue tint. He has examined the absorption-spectrum of ozone and finds nine dark bands in it, three at least of which correspond with known bands in the telluric spectrum.

To obtain enlarged impressions from the phonograph, MM. Roig and Torres (*Cronica cientifica*, No. 4) substitute for the metallic membrane which bears the indenting style a plate of mica, quite free at the border, and supported at the centre by an axis of caoutchouc fixed to a small spring. This axis carries, besides the short style for acting on the tin sheet, a small metallic piece in a plane perpendicular to the axis of the style, and this supports a second style, long and thin, the vibrations of which are inscribed on a cylinder blackened with smoke. The same angular velocity is imparted (by means of clockwork) to the cylinder of the phonograph and the blackened cylinder, and while the short style makes its usual marks on the tin, the long one produces a larger tracing on the cylinder, which the authors have tried to decipher. They have succeeded easily in recognising the different vowels, some consonants, and even some syllables, but they have not been able to read entire phrases. The curves are more characteristic if the voice be used with ordinary intensity; on forcing it they are deteriorated.

PROF. AVENARIUS, of Kiew, has taken out an Austrian patent for a new method of division of the electric light. The method is that of insertion of a polariser in a secondary circuit, connected with each electric lamp. The polariser, consists of several voltameters connected together. The current, supplied by an electrodynamic machine, divides before entering each lamp: one part goes through the lamp, while the second goes through the secondary circuit and the polariser and then back to the primary circuit. By insertion of a considerable resistance, *e.g.* increase of the voltameters, the light-intensity of the lamps may be varied. The individual lamps are independent of each other, and lamps of different systems may be simultaneously used.

WE notice in the minutes of meetings of the Russian Physical and Chemical Society (vol. xii. fasc. 9) the researches, by M. Glasenap, on refraction. The want of concentricity of sheets of air of equal density produces a certain variation in the normal refraction given in the tables; the surfaces of equal density being as a rule inclined to some degree instead of being horizontal, and the degree of inclination being submitted to a certain periodicity during a whole year, there necessarily arises from this cause a certain correction to be applied to the observed position of a star, much like to that of the annual parallax and aberration, and which might be described as "parallax of refraction." As this correction must obviously affect the values of the annual parallax and of aberration, it is easy to understand the necessity to determine its true value with much accuracy. The values deduced by M. Glasenap for the stars ι of the Ursæ Majoris, ι and O Draconis, are $-0''.04$, $-0''.11$, and $-0''.11$, which figures would explain to a certain extent the negative parallaxes received by M. Nyrén ("Nutation der Erdaxe"), and which respectively are $-''.03$, $-0''.05$, and $-0''.16$. The whole work of M. Glasenap on this subject will soon be published.

CHEMICAL NOTES

THE influence of time on processes of chemical change has not yet been thoroughly investigated. In a recent number of *Comptes rendus* Berthelot makes a contribution to this subject which is scarcely likely to be accepted by chemists without further investigation. From the results of many thermo-chemical measurements Berthelot states that the chemical change, which occurs when an acid soluble in water acts on a soluble base or salt, or *vice versâ*, or when two soluble salts mutually react, is completed in a space of time not appreciably greater than that required for completely mixing the two solutions.

FROM experiments on the evolution of carbon dioxide from the roots of plants, detailed in the *Bull. de la Soc. botanique de France*, M. Cauvet concludes that carbon dioxide is certainly evolved from plant-roots; that the quantity evolved is less during night than during day; and that the quantity evolved increases at sunrise, decreases towards midday, and again increases in the evening.

HERR SALLERON describes in *Naturforscher* an instance of the modifying influence of moderately heated liquids on glass. An aräometer used in a sugar-work lost about 0·5 grm. in weight after immersion for eight days in a sugar syrup at 95°. The syrup contained 115 grm. sugar and 91 grm. ash per litre. After a few more days the glass split off in splinters.

MR. A. A. NESBIT has recently patented a very ingenious process for preventing fraudulent alterations of bankers' cheques. Mr. Nesbit prints his cheques with a dye or dyes, the colour of which is differently changed by acids and by alkalies; the inscriptions on the cheques are apparent by virtue of the alkalinity or acidity of the dye. Immersion in dilute acid—for the purpose of dissolving out the written part of the cheque—causes the whole inscription to become acid tint; as subsequent treatment with alkali changes the whole inscription to alkaline tint, the original inscription cannot be restored. If the acid part of the inscription be printed with a dye which is more strongly acid than the alkaline part is alkaline, treatment of the cheque with a neutral solvent of writing ink suffices to blur the inscription, and this blurring cannot be removed. Various modifications of the invention, and details of the processes of printing, colours used, &c., are given in the specification.

M. ÉTARD thinks that boron shows certain analogies with vanadium; in endeavouring further to illustrate such analogies he has obtained indications, although not yet positive proof, of the existence of an acid containing more oxygen than boric acid. He has also obtained, by the action of a saturated solution of boric acid on hydrated barium dioxide, a salt to which he gives the formula $B_2O_4 . BaO . 3H_2O$, and the name *barium perborate*. This salt dissolves in acids with evolution of oxygen; it is very deliquescent (*Compt. rend.*).

IN continuation of his investigation into the compounds of sulphur and nitrogen M. Demarcay describes (*Compt. rend.*) various bodies which he regards as compounds of the radicle—$(S_4N_3)'$—called by him *thiotriazyl*. The more important of the new compounds are formulated as $(S_4N_3)Cl$, $(S_4N_3)NO_3$, and $(S_4N_3)HSO_4$.

LIEBEN describes (in *Wien. Akad. Ber.*) several compounds of calcium chloride with fatty acids, more especially the three compounds with butyric acid, viz.:—

$$CaCl_2 . C_4H_8O_2 ; CaCl_2 . 2C_4H_8O_2 . 2H_2O ; \text{and} \\ CaCl_2 . Ca(C_4H_7O_2)_2 . 4C_4H_8O_2.$$

M. BYASSON states (*Compt. rend.*) that if every trace of sulphurous acid be removed from chloral, the latter retains its liquid condition for an indefinite time, and that the change into solid metachloral, which soon takes place in chloral purified only by distillation, may be thus prevented. To remove the last traces of sulphurous acid M. Byasson agitates the chloral with $\frac{1}{10}$ of its weight of finely-powdered caustic baryta, decants the liquid, and distils.

BERTHELOT has recently succeeded in isolating several compounds of metallic chlorides with hydrochloric acid; in *Compt. rend.* he describes three such chlorhydrates of metallic chlorides, viz.:—

$CdCl_2 . 2HCl . 7H_2O$; $PbI_2 . HI . 5H_2O$; and $3AgI . HI . 7H_2O$;

and in another number of the same journal M. Ditté describes, among others, the salts $BiCl_3 . 3HCl$; $SbCl_3 . 3HCl$, &c. These hydrated salts are formed from their constituent compounds with a considerable evolution of heat, the amount varying from 11,000 to 15,000 units. The anhydrous salts readily undergo dissociation into their constituent compounds, and cannot therefore be readily obtained. Berthelot regards the formation and dissociation of these chlorhydrates as playing an important part in the mechanism of many chemical changes. Thus calomel is changed into corrosine sublimate and mercury by the action of hydrochloric acid: Berthelot would formulate this change as $Hg_2Cl_2 + xHCl = HgCl_2 . xHCl + Hg$ (attended with evolution of 9500 heat-units), with subsequent dissociation of the chlorhydrate of $HgCl_2$. Again in the reduction of metallic chlorides by hydrogen Berthelot supposes that chlorhydrates are produced, and that the heat thus developed aids in dissociating fresh quantities of the original metallic chloride; thus he would indicate the initial stage of the reduction of cadmium chloride by hydrogen, as:—

$$2CdCl_2 + H_2 = Cd + CdCl_2 . 2HCl.$$

M. WURTZ has recently been studying (*Compt. rend.*) the action of the ferment *Papaïn* on fibrin, whereby the fibrin is rendered soluble in water. The process appears to be analogous with many ordinary chemical changes in which the formation and decomposition of a compound are continually proceeding. Papaïn forms an insoluble compound with fibrin, which compound is then decomposed by the water present with formation of a soluble hydrated fibrin, and setting free of the ferment, which again acts on fresh quantities of fibrin.

IN the *American Chem. Journ.* Clarke and Stallo describe a series of experiments on the tartrates of antimony, wherein they are led to regard tartar emetic as the potassium salt of a new acid, to which they give the name *tartrantimonious*, viz. $Sb . C_4H_4O_6 . OH$. This acid they regard as derived from orthantimonious acid, $Sb(OH)_3$, which they have prepared in definite form. The behaviour of an aqueous solution of tartrantimonious acid towards heat is peculiar. Below 30° the solution remains nearly clear; at a few degrees above 30° a white curdy precipitate deposits; on evaporating in a water bath the curdy precipitate disappears and a transparent gummy mass remains, which is completely soluble in cold water, re-forming the original acid. These changes are shown to be expressible by the equations—

1. $C_4H_5SbO_7 + 2H_2O = SbH_3O_3 + C_4H_6O_6$; the curdy precipitate consisting of orthantimonious acid.

2. $C_4H_6O_6 + SbH_3O_3 = 2H_2O + C_4H_5SbO_7$; *i.e.* on heating, water is eliminated, and the original acid is reproduced.

IN a series of papers in the *Berliner Berichte* Th. Thomsen endeavours to show that the "molecular rotation" of many classes of compounds is, for each class, a simple multiple of a constant number. "Molecular rotation" he defines as rotatory power $\times \frac{\text{molecular weight}}{100} \left(\frac{M.(\alpha)_D}{100}\right)$. The constant for one group appears generally to bear a simple relation to that for other groups; in fact a constant may be found which belongs to many groups. Adopting a classification analogous to that of natural history, Thomsen shows that the constant 0·95 belongs to a large "class" of compounds; that this multiplied by 4 gives the constant (3·8) for the "family" of alcohols, and by 9 gives the constant (8·65) for the "family" of amides, &c. From a determination of the molecular rotation of compounds, aided by the use of these constants, he attempts also to deduce conclusions as to the chemical structure of the molecules of these compounds.

IN various papers noticed in this journal, Brühl attempted to show that the "molecular refraction" $\left[M.\left(\frac{\mu - 1}{d}\right)\right]$ of isomeric carbon compounds is constant when only "singly-linked" carbon atoms are present; and that variations in this quantity are to be traced to variations in the "linking" of carbon atoms. Janowsky (*Berliner Berichte*) maintains that slight differences are always noticeable between the molecular refractions of isomeric compound where isomerism is due not to "linking," but to "grouping" of carbon atoms: but he thinks that if the values of the refractive indices of such compounds are considered, better results are obtained than by calculating the molecular refractions. Brühl however had himself shown that the refraction indices of such isomers are not the same.

LANDOLT has gathered together in *Berliner Berichte* the more important data concerning the inversion of specific rotatory power of carbon compounds by the influence of heat or of inactive solvents: those data he supplements by further experiments of his own, and develops shortly the outlines of a mechanical theory analogous to that of Rammelsberg.

THE atomic weight of beryllium is still the subject of experiment. Emerson Reynolds has redetermined the specific heat of the pure metal (*Chem. News*) and obtained a number which points to 9·1 as the true atomic weight. The same value is assigned by Brauner, who (*Berliner Berichte*) criticises the arguments of Nilson and Pettersson, and attempts to show that the specific heat, specific volume, and general physical properties of beryllium oxide are more in keeping with the formula BeO (Be = 9·1) than with the formula Be_2O_3 (Be = 13·6) assigned to it by the Swedish observers.

IN a paper on bismuth compounds in *Chem. Soc. Journal*, by Muir, Hoffmeister, and Robbs, the new salts bismuth fluoride (BiF_3) and bismuth oxyfluoride (BiOF) are described. The former is the more stable of the halogen compounds of bismuth: it is not decomposed by water, and is scarcely changed at a red heat in air.

PROF. BEILSTEIN, who has recently studied the various substances used for disinfection, arrives, in a communication made to the St. Petersburg Technical Society, at the following conclusions:—Sulphuric acid would be the best disinfectant if it did not destroy the sides of the tanks; the use of lime and of salts of lime ought to be completely renounced, as they but temporarily destroy bacteria, and under some circumstances may contribute to their development; nor does sulphate of iron, even in a solution of 15 per cent., ultimately destroy bacteria, as they revive when put into a convenient medium. Therefore Prof. Beilstein recommends sulphate of aluminium, which is used in paper and printed-cotton manufactures. The best means for providing it is to make a mixture of red clay with 4 per cent. of sulphuric acid, and to add to this mixture some carbolic acid for destroying the smell of the matter which is to be disinfected.

ACTION OF AN INTERMITTENT BEAM OF RADIANT HEAT UPON GASEOUS MATTER[1]

THE Royal Society has already done me the honour of publishing a long series of memoirs on the interaction of radiant heat and gaseous matter. These memoirs did not escape criticism. Distinguished men, among whom the late Prof. Magnus and the late Prof. Buff may be more specially mentioned, examined my experiments, and arrived at results different from mine. Living workers of merit have also taken up the question, the latest of whom,[2] while justly recognising the extreme difficulty of the subject, and while verifying, so far as their experiments reach, what I had published regarding dry gases, find me to have fallen into what they consider grave errors in my treatment of vapours.

None of these investigators appear to me to have realised the true strength of my position in its relation to the objects I had in view. Occupied for the most part with details, they have failed to recognise the stringency of my work as a whole, and have not taken into account the independent support rendered by the various parts of the investigation to each other. They thus ignore verifications, both general and special, which are to me of conclusive force. Nevertheless, thinking it due to them and me to submit the questions at issue to a fresh examination, I resumed some time ago the threads of the inquiry. The results shall in due time be communicated to the Royal Society; but meanwhile I would ask permission to bring to the notice of the Fellows a novel mode of testing the relations of radiant heat to gaseous matter, whereby singularly instructive effects have been obtained.

After working for some time with the thermopile and galvano-

[1] Paper read at the Royal Society, January 13, by Prof. Tyndall, F.R.S

[2] Lecher and Pernter, *Philosophical Magazine*, January, 1881; *Sitzb. der k. Akad. der Wissensch. in Wien*, July, 1880.

meter, it occurred to me several weeks ago that the results thus obtained might be checked by a more direct and simple form of experiment. Placing the gases and vapours in diathermanous bulbs, and exposing the bulbs to the action of radiant heat, the heat absorbed by different gases and vapours ought, I considered, to be rendered evident by ordinary expansion. I devised an apparatus with a view of testing this idea. But at this point, and before my proposed gas-thermometer was constructed, I became acquainted with the ingenious and original experiments of Mr. Graham Bell, wherein musical sounds are obtained through the action of an intermittent beam of light upon solid bodies.

From the first I entertained the opinion that these singular sounds were caused by rapid changes of temperature, producing corresponding changes of shape and volume in the bodies impinged upon by the beam. But if this be the case, and if gases and vapours really absorb radiant heat, they ought to produce sounds more intense than those obtainable from solids. I pictured every stroke of the beam responded to by a sudden expansion of the absorbent gas, and concluded that when the pulses thus excited followed each other with sufficient rapidity, a musical note must be the result. It seemed plain, moreover, that by this new method many of my previous results might be brought to an independent test. Highly diathermanous bodies, I reasoned, would produce faint sounds, while highly athermanous bodies would produce loud sounds; the strength of the sound being, in a sense, a measure of the absorption. The first experiment made with a view of testing this idea, was executed in the presence of Mr. Graham Bell[1]; and the result was in exact accordance with what I had foreseen.

The inquiry has been recently extended so as to embrace most of the gases and vapours employed in my former researches. My first source of rays was a Siemens' lamp connected with a dynamo-machine, worked by a gas-engine. A glass lens was used to concentrate the rays, and afterwards two lenses. By the first the rays were rendered parallel, while the second caused them to converge to a point about seven inches distant from the lens. A circle of sheet zinc provided first with radial slits and afterwards with teeth and interspaces cut through it, was mounted vertically on a whirling table, and caused to rotate rapidly across the beam near the focus. The passage of the slits produced the desired intermittence,[2] while a flask containing the gas or vapour to be examined received the shocks of the beam immediately behind the rotating disk. From the flask a tube of india-rubber, ending in a tapering one of ivory or boxwood, led to the ear, which was thus rendered keenly sensitive to any sound generated within the flask. Compared with the beautiful apparatus of Mr. Graham Bell, the arrangement here described is rude; it is, however, very effective.

With this arrangement the number of sounding gases and vapours was rapidly increased. But I was soon made aware that the glass lenses withdrew from the beam its most effectual rays. The silvered mirrors employed in my previous researches were therefore invoked; and with them, acting sometimes singly and sometimes as conjugate mirrors, the curious and striking results which I have now the honour to submit to the Society were obtained.

Sulphuric ether, formic ether, and acetic ether being placed in bulbous flasks, their vapours were soon diffused in the air above the liquid. On placing these flasks, whose bottoms only were covered by the liquid, behind the rotating disk, so that the intermittent beam passed through the vapour, loud musical tones were in each case obtained. These are known to be the most highly absorbent vapours which my experiments revealed. Chloroform and bisulphide of carbon, on the other hand, are known to be the least absorbent, the latter standing near the head of diathermanous vapours. The sounds extracted from these two substances were usually weak and sometimes barely audible, being more feeble with the bisulphide than with the chloroform. With regard to the vapours of amylene, iodide of ethyl, iodide of methyl and benzol, other things being equal, their power to produce musical tones appeared to be accurately expressed by their ability to absorb radiant heat.

It is the vapour, and not the liquid, that is effective in producing the sounds. Taking, for example, the bottles in which my volatile substances are habitually kept, I permitted the intermittent beam to impinge upon the liquid in each of them. No sound was in any case produced, while the moment the vapour-laden space above an active liquid was traversed by the beam, musical tones made themselves audible.

A rock-salt cell filled entirely with a volatile liquid and subjected to the intermittent beam produced no sound. This cell was circular and closed at the top. Once, while operating with a highly athermanous substance, a distinct musical note was heard. On examining the cell however a small bubble was found at its top. The bubble was less than a quarter of an inch in diameter, but still sufficient to produce audible sounds. When the cell was completely filled the sounds disappeared.

It is hardly necessary to state that the pitch of the note obtained in each case is determined by the velocity of rotation. It is the same as that produced by blowing against the rotating disk and allowing its slits to act like the perforations of a syren.

Thus, as regards vapours, prevision has been justified by experiment. I now turn to gases. A small flask, after having been heated in the spirit-lamp so as to detach all moisture from its sides, was carefully filled with dried air. Placed in the intermittent beam it yielded a musical note, but so feeble as to be heard only with attention. Dry oxygen and hydrogen behaved like dry air. This agrees with my former experiments, which assigned a hardly sensible absorption to these gases. When the dry air was displaced by carbonic acid, the sound was far louder than that obtained from any of the elementary gases. When the carbonic acid was displaced by nitrous oxide the sound was much more forcible still, and when the nitrous oxide was displaced by olefiant gas it gave birth to a musical note which, when the beam was in good condition and the bulb well chosen, seemed as loud as that of an ordinary organ-pipe. We have here the exact order in which my former experiments proved these gases to stand as absorbers of radiant heat. The amount of the absorption and the intensity of the sound go hand in hand.

In 1859 I proved gaseous ammonia to be extremely impervious to radiant heat. My interest in its deportment when subjected to this novel test was therefore great. Placing a small quantity of liquid ammonia in one of the flasks, and warming the liquid slightly, the intermittent beam was sent through the space above the liquid. A loud musical note was immediately produced. By the proper application of heat to a liquid the sounds may be always intensified. The ordinary temperature however suffices in all the cases thus far referred to.

In this relation the vapour of water was that which interested me most, and as I could not hope that at ordinary temperatures it existed in sufficient amount to produce audible tones, I heated a small quantity of water in a flask almost up to its boiling-point. Placed in the intermittent beam, I heard—I avow with delight—a powerful musical sound produced by the aqueous vapour.

Small wreaths of haze, produced by the partial condensation of the vapour in the upper and cooler air of the flask, were however visible in this experiment; and it was necessary to prove that this haze was not the cause of the sound. The flask was therefore heated by a spirit-flame beyond the temperature of boiling water. The closest scrutiny by a condensed beam of light then revealed no trace of cloudiness above the liquid. From the perfectly invisible vapour however the musical sound issued, if anything, more forcible than before. I placed the flask in cold water until its temperature was reduced from about 90° to 10° C., fully expecting that the sound would vanish at this temperature; but notwithstanding the tenuity of the vapour, the sound extracted from it was not only distinct but loud.

Three empty flasks filled with ordinary air were placed in a freezing mixture for a quarter of an hour. On being rapidly transferred to the intermittent beam, sounds much louder than those obtainable from dry air were produced.

Warming these flasks in the flame of a spirit-lamp [until all visible humidity had been removed, and afterwards urging dried air through them, on being placed in the intermittent beam the sound in each case was found to have fallen almost to silence.

Sending, by means of a glass tube, a puff of breath from the lungs into a dried flask, the power of emitting sound was immediately restored.

[1] On November 29: see *Journal* of the Society of Telegraph Engineers, December 8, 1880.

[2] When the disk rotates the individual slits disappear, forming a hazy zone through which objects are visible. Throwing by the clean hand, or better still by white paper, the beam back upon the disk, it appears to stand still, the slits forming so many dark rectangles. The reason is obvious, but the experiment is a very beautiful one.

I may add that when I stand with open eyes in the flashing beam, at a definite velocity of recurrence, subjective colours of extraordinary gorgeousness are produced. With slower or quicker rates of rotation the colours disappear. The flashes also produce a giddiness sometimes intense enough to cause me to grasp the table to keep myself erect.

When, instead of breathing into a dry flask, the common air of the laboratory was urged through it, the sounds became immediately intensified. I was by no means prepared for the extraordinary delicacy of this new method of testing the athermancy and diathermancy of gases and vapours, and it cannot be otherwise than satisfactory to me to find that particular vapour, whose alleged deportment towards radiant heat has been most strenuously denied, affirming thus audibly its true character.

After what has been stated regarding aqueous vapour we are prepared for the fact that an exceedingly small percentage of any highly athermanous gas diffused in air suffices to exalt the sounds. An accidental observation will illustrate this point. A flask was filled with coal gas and held bottom upwards in the intermittent beam. The sounds produced were of a force corresponding to the known absorptive energy of coal-gas. The flask was then placed upright, with its mouth open upon a table, and permitted to remain there for nearly an hour. On being restored to the beam, the sounds produced were far louder than those which could be obtained from common air.[1]

Transferring a small flask or a test-tube from a cold place to the intermittent beam it is sometimes found to be practically silent for a moment, after which the sounds become distinctly audible. This I take to be due to the vaporisation by the calorific beam of the thin film of moisture adherent to the glass.

My previous experiments having satisfied me of the generality of the rule that volatile liquids and their vapours absorb the same rays, I thought it probable that the introduction of a thin layer of its liquid, even in the case of a most energetic vapour, would detach the effective rays, and thus quench the sounds. The experiment was made and the conclusion verified. A layer of water, formic ether, sulphuric ether, or acetic ether one-eighth of an inch in thickness rendered the transmitted beam powerless to produce any musical sound. These liquids being transparent to light, the efficient rays which they intercepted must have been those of obscure heat.

A layer of bisulphide of carbon about ten times the thickness of the transparent layers just referred to, and rendered opaque to light by dissolved iodine, was interposed in the path of the intermittent beam. It produced hardly any diminution of the sounds of the more active vapours—a further proof that it is the invisible heat rays, to which the solution of iodine is so eminently transparent, that are here effectual.

Converting one of the small flasks used in the foregoing experiments into a thermometer bulb, and filling it with various gases in succession, it was found that with those gases which yielded a feeble sound, the displacement of a thermometric column associated with the bulb was slow and feeble, while with those gases which yielded loud sounds the displacement was prompt and forcible.

Further Experiments.—Since the handing in of the foregoing note, on January 3, the experiments have been pushed forward; augmented acquaintance with the subject serving only to confirm my estimate of its interest and importance.

All the results described in my first note have been obtained in a very energetic form with a battery of sixty Grove's cells.

On January 4 I chose for my source of rays a powerful lime-light, which, when sufficient care is taken to prevent the pitting of the cylinder, works with admirable steadiness and without any noise. I also changed my mirror for one of shorter focus, which permitted a nearer approach to the source of rays. Tested with this new reflector the stronger vapours rose remarkably in sounding power.

Improved manipulation was, I considered, sure to extract sounds from rays of much more moderate intensity than those of the lime-light. For this light, therefore, a common candle flame was substituted. Received and thrown back by the mirror, the radiant heat of the candle produced audible tones in all the stronger vapours.

Abandoning the mirror and bringing the candle close to the rotating disk, its direct rays produced audible sounds.

A red-hot coal, taken from the fire and held close to the rotating disk, produced forcible sounds in a flask at the other side.

A red-hot poker, placed in the position previously occupied by the coal, produced strong sounds. Maintaining the flask in position behind the rotating disk, amusing alternations of sound and silence accompanied the alternate introduction and removal of the poker.

[1] The method here described is, I doubt not, applicable to the detection of extremely small quantities of fire-damp in mines.

The temperature of the iron was then lowered till its heat just ceased to be visible. The intermittent invisible rays produced audible sounds.

The temperature was gradually lowered, being accompanied by a gradual and continuous diminution of the sound. When it ceased to be audible the temperature of the poker was found to be below that of boiling water.

As might be expected from the foregoing experiments an incandescent platinum spiral, with or without the mirror, produced musical sounds. When the battery power was reduced from ten cells to three the sounds, though enfeebled, were still distinct.

My neglect of aqueous vapour had led me for a time astray in 1859, but before publishing my results I had discovered my error. On the present occasion this omnipresent substance had also to be reckoned with. Fourteen flasks of various sizes, with their bottoms covered with a little sulphuric acid, were closed with ordinary corks and permitted to remain in the laboratory from December 23 to January 4. Tested on the latter day with the intermittent beam, half of them emitted feeble sounds, but half were silent. The sounds were undoubtedly due, not to dry air, but to traces of aqueous vapour.

An ordinary bottle containing sulphuric acid for laboratory purposes, being connected with the ear and placed in the intermittent beam, emitted a faint, but distinct, musical sound. This bottle had been opened two or three times during the day, its dryness being thus vitiated by the mixture of a small quantity of common air. A second similar bottle, in which sulphuric acid had stood undisturbed for some days, was placed in the beam: the dry air above the liquid proved absolutely silent.

On the evening of January 7 Prof. Dewar handed me four flasks treated in the following manner:—Into one was poured a small quantity of strong sulphuric acid; into another a small quantity of Nordhausen sulphuric acid; in a third were placed some fragments of fused chloride of calcium; while the fourth contained a small quantity of phosphoric anhydride. They were closed with well-fitting india-rubber stoppers, and permitted to remain undisturbed throughout the night. Tested after twelve hours, each of them emitted a feeble sound, the flask last-mentioned being the strongest. Tested again six hours later, the sound had disappeared from three of the flasks, that containing the phosphoric anhydride alone remaining musical.

Breathing into a flask partially filled with sulphuric acid instantly restores the sounding power, which continues for a considerable time. The wetting of the interior surface of the flask with the sulphuric acid always enfeebles, and sometimes destroys, the sound.

A bulb less than a cubic inch in volume, and containing a little water lowered to the temperature of melting ice, produces very distinct sounds. Warming the water in the flame of a spirit-lamp, the sound becomes greatly augmented in strength. At the boiling temperature the sound emitted by this small bulb[1] is of extraordinary intensity.

These results are in accord with those obtained by me nearly nineteen years ago, both in reference to air and to aqueous vapour. They are in utter disaccord with those obtained by other experimenters, who have ascribed a high absorption to air and none to aqueous vapour.

The action of aqueous vapour being thus revealed, the necessity of thoroughly drying the flasks when testing other substances becomes obvious. The following plan has been found effective:—Each flask is first heated in the flame of a spirit-lamp till every visible trace of internal moisture has disappeared, and it is afterwards raised to a temperature of about 400° C. While the glass is still hot a glass tube is introduced into it, and air freed from carbonic acid by caustic potash, and from aqueous vapour by sulphuric acid, is urged through the flask until it is cool. Connected with the ear-tube, and exposed immediately to the intermittent beam, the attention of the ear, if I may use the term, is converged upon the flask. When the experiment is carefully made, dry air proves as incompetent to produce sound as to absorb radiant heat.

In 1868 I determined the absorptions of a great number of liquids whose vapours I did not examine. My experiments having amply proved the parallelism of liquid and vaporous absorption, I held undoubtingly twelve years ago that the vapour of cyanide of ethyl and of acetic acid would prove powerfully absorbent. This conclusion is now easily tested. A small

[1] In such bulbs even bisulphide of carbon vapour may be so nursed as to produce sounds of considerable strength.

quantity of either of these substances, placed in a bulb a cubic inch in volume, warmed, and exposed to the intermittent beam, emits a sound of extraordinary power.

I also tried to extract sounds from perfumes, which I had proved in 1861 to be absorbers of radiant heat. I limit myself here to the vapours of pachouli and cassia, the former exercising a measured absorption of 30, and the latter an absorption of 109. Placed in dried flasks, and slightly warmed, sounds were obtained from both these substances, but the sound of cassia was much louder than that of pachouli.

Many years ago I had proved tetrachloride of carbon to be highly diathermanous. Its sounding power is as feeble as its absorbent power.

In relation to colliery explosions, the deportment of marsh-gas was of special interest. Prof. Dewar was good enough to furnish me with a pure sample of this gas. The sounds produced by it, when exposed to the intermittent beam, were very powerful.

Chloride of methyl, a liquid which boils at the ordinary temperature of the air, was poured into a small flask, and permitted to displace the air within it. Exposed to the intermittent beam, its sound was similar in power to that of marsh-gas.

The specific gravity of marsh-gas being about half that of air, it might be expected that the flask containing it, when left open and erect, would soon get rid of its contents. This however is not the case. After a considerable interval the film of this gas clinging to the interior surface of the flask was able to produce sounds of great power.

A small quantity of liquid bromine being poured into a well-dried flask, the brown vapour rapidly diffused itself in the air above the liquid. Placed in the intermittent beam, a somewhat forcible sound was produced. This might seem to militate against my former experiments, which assigned a very low absorptive power to bromine vapour. But my former experiments on this vapour were conducted with obscure heat; whereas in the present instance I had to deal with the radiation from incandescent lime, whose heat is in part luminous. Now the colour of the bromine vapour proves it to be an energetic absorber of the luminous rays; and to them, when suddenly converted into thermometric heat in the body of the vapour, I thought the sounds might be due.

Between the flask containing the bromine and the rotating disk I therefore placed an empty glass cell: the sounds continued. I then filled the cell with transparent bisulphide of carbon: the sounds still continued. For the transparent bisulphide I then substituted the same liquid saturated with dissolved iodine. This solution cut off the light, while allowing the rays of heat free transmission: the sounds were immediately stilled.

Iodine vaporised by heat in a small flask yielded a forcible sound, which was not sensibly affected by the interposition of transparent bisulphide of carbon, but which was completely quelled by the iodine solution. It might indeed have been foreseen that the rays transmitted by the iodine as a liquid would also be transmitted by its vapour, and thus fail to be converted into sound.[1]

To complete the argument:—While the flask containing the bromine vapour was sounding in the intermittent beam, a strong solution of alum was interposed between it and the rotating disk. There was no sensible abatement of the sounds with either bromine or iodine vapour.

In these experiments the rays from the lime-light were converged to a point a little beyond the rotating disk. In the next experiment they were rendered parallel by the mirror, and afterwards rendered convergent by a lens of ice. At the focus of the ice-lens the sounds were extracted from both bromine and iodine vapour. Sounds were also produced after the beam had been sent through the alum solution and the ice-lens conjointly.

With a very rude arrangement I have been able to hear the sounds of the more active vapours at a distance of 100 feet from the source of rays.

Several vapours other than those mentioned in this abstract have been examined, and sounds obtained from all of them. The vapours of all compound liquids will, I doubt not, be found sonorous in the intermittent beam. And, as I question whether there is an absolutely diathermanous substance in nature, I think it probable that even the vapours of elementary bodies, including the elementary gases, when more strictly examined, will be found capable of producing sounds.

[1] I intentionally use this phraseology.

INTERESTING NEW CRINOIDS

In the *Memoirs* of the Swiss Palæontological Society for 1880 Prof. P. de Loriol has recently described a remarkable new Crinoid which he refers to the little known genus *Thiollericrinus*, Étallon, under the name of *T. ribeiroi*. It occurs in the Upper Jurassic beds of Engenheiro, in Portugal. The calyx, like that of most Jurassic *Comatulæ*, has five small prismatic basals attached to the under surface of the radials. But the centrodorsal piece on which the calyx rests is not entirely separated from the lower part of the stem, as is the case in the *Comatulæ*, though it resembles that of a *Comatula* in bearing cirrhi.

Thiollericrinus was a stalked Crinoid that never developed beyond the stage at which cirrhi appear on the enlarged uppermost stem-joint of the stalked larva of *Comatula*. The underface of the centrodorsal and the terminal faces of the other stem-joints resemble those of the *Comatula* larva and also of *Bourgueticrinus* and *Rhizocrinus* in their oval shape and in the presence of transverse ridges which are in different planes at the two ends of each joint. *Thiollericrinus* therefore is a permanent larval form, and furnishes an intermediate stage between the stalked *Bourgueticrinus* and the free *Comatula*. The top stem-joint of the former bears no cirrhi, as it does in *Thiollericrinus* and in *Comatula;* while in the latter it develops cirrhi, and unites closely with the calyx, separating from the rest of the larval stem on which it was previously fixed.

Another form of considerable morphological interest, from its occupying an intermediate position between two well-defined genera, has been lately described by Mr. P. H. Carpenter under the name of *Mesocrinus*. The stem-joints are of the type already mentioned as characteristic of *Bourgueticrinus*, having oval faces marked by transverse ridges in different planes. But the upper stem-joint is not enlarged as it is in *Bourgueticrinus* and in the *Apiocrinidæ* generally, while the form of the calyx recalls that of the *Pentacrinidæ*. It consists of five radials with well-developed articular faces, resting on five basals which form a complete ring as in the recent *Pentacrinus Wyville-Thomsoni*, from 800 fathoms in the Atlantic off the coast of Portugal.

Broadly speaking, therefore, *Mesocrinus* combines the stem of *Bourgueticrinus* with the calyx of *Pentacrinus*, or rather of *Cainocrinus*, as Prof. de Loriol prefers to call that section of the *Pentacrinus* type in which the basal ring is closed. *Mesocrinus* is an Upper Cretaceous genus, one species occurring in the "Plänerkalk" of Streben in Saxony, while another and larger one was found in the "Mucronaten Kreide" of Southern Sweden.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—In consequence of the unsatisfactory state of many of the lodging-houses in Oxford, in respect of their sanitary arrangements, a proposal will be brought before Congregation on March 1 "to make better provision for the supervision of lodging-houses." One of the delegates for licensing lodgings will be stipendiary, and it will be his duty to inspect every dwelling-house proposed for this use and to satisfy himself of its sanitary fitness. He shall have the assistance of a sanitary inspector, and shall have proctorial authority over members of the University in his character of inspector.

A special statute will also be proposed authorising the present delegates of lodging-houses to spend whatever sum they may think necessary on a general inspection of lodging-houses during the present year.

There will be holden at Christ Church on Saturday, March 12, an election to at least one Mathematical Junior Studentship, and at least one in Natural Science, tenable for five years from the day of election. They will be of the annual value either (1) of 100*l*. (including an allowance for room rent) if the Governing Body shall so determine; or (2) of 85*l*. (also including an allowance for room rent), which may be raised to the larger sum above named after the completion of one year's residence, if the Governing Body shall so determine. Candidates for the Mathematical Studentships and candidates for the Natural Science Studentships who offer mathematics will call upon the Dean on Monday, February 28, between 12.30 and 1.30 p.m.; candidates for the Natural Science Studentships who do not offer mathematics, on March 2, between 12.30 and 1.30 p.m. All must produce certificates both of the day of their birth and of good character. The examinations will follow in each case at

2 p.m. Candidates for either the Classical or the Mathematical Studentships must not have exceeded the age of nineteen on January 1, 1881; candidates for the Natural Science Studentships must not have exceeded the age of twenty on the same day.

CAMBRIDGE.—There was a meeting of the members of the Senate on February 11, for the purpose of discussing the report of the Syndicate appointed last June to consider certain memorials as to the higher education of women. The Syndicate recommend that, subject to certain conditions of residence at Girton and Newnham Colleges, female students may be admitted to the Tripos Examinations, and certificates issued to them as to the result of the examination.—The Master of Emmanuel, in opening the discussion, remarked that he had never sat on any Syndicate before where so little difficulty had been experienced in agreeing to a report. Personally he wished the Syndicate had arrived at a different conclusion, and had recommended the admission of women to all the University examinations. He claimed for the recommendations of the Syndicate, however, that they closely followed the views of an influential number of residents who had signed a memorial on the subject, and wished for an official sanction to that which had been done for ten years without authority. He contended that it was the imperative duty of the University to give all possible access to its educational advantages, and that the proposed scheme was only a step in that direction.—Dr. Campion contended that the public opinion of the University had been carefully excluded in the constitution of the Syndicate. He charged the report with being both illiberal and harsh. It was illiberal, because the Syndicate had restricted the examinations to inmates of particular colleges, and was not for the encouragement of the higher education of women all over the country. Why was the advantage given only to Newnham and Girton Colleges? The report was harsh, for when they admitted women to test their scientific powers, it was unfair to do so after the conclusion of a time race with the men. Why not let the women study as long as they liked? He did not object to their being compelled to pass the previous examination, but to compel them to go step by step with undergraduates was placing them, by reason of their defect of physical power, in a false position.—Prof. Kennedy said, it was proposed to limit the competition to those within their reach; if the experiment succeeded, it would be a matter for future consideration what extensions should be made. As to the harshness, that surely might be left to the better judgment of the friends, relations, and guardians of these women who asked for these concessions. Women were mentally men's equals, but physically not. To urge their want of physical power as an objection to their admission to the same intellectual pursuits and pleasures as men was more for the Brahmin than the believer in the Bible; it was a fitter argument for the Turk than the Saxon.—Prof. Liveing defended the Syndicate from the attack of Dr. Campion, and asserted that the matter was discussed fully and fairly, without any bias of previously formed opinions.—Prof. Westcott, who did not concur in the whole of the report, expressed his great regret that the Syndicate before reporting had not collected further information on a problem so difficult and obscure.—Mr. Prothero, King's, was of opinion that the same course of training which was good for male students was equally good for women.—Mr. Sidgwick, Trinity, draw attention to the remarkable fact that no objection had been raised to the main proposal of the Syndicate.—The discussion lasted upwards of two hours.

KIEFF.—The number of students at the University of Kieff was, on January 1, 1881, as much as 1041, with fifty-eight professors.

SCIENTIFIC SERIALS

THE *Quarterly Journal of Microscopical Science* for January contains notes on a peculiar form of Polyzoa closely allied to Bugula (Kinetoskias, Kor. and Dan.), by George Busk, F.R.S., with plates 1 and 2.—On the germination and histology of the seedling of *Welwitschia mirabilis*, by F. Orpen Bower, B.A., with plates 3 and 4.—Notes on some of the Reticularian Rhizopoda of the *Challenger*, by Henry B. Brady, F.R.S.—On the head-cavities and associated nerves of Elasmobranchs, by Prof. A. M. Marshall, M.A., with plates 5 and 6.—Contributions to the minute anatomy of the nasal mucous membrane, by Dr. E. Klein, F.R.S., with plate 7.—Histological notes, by Dr. E. Klein, F.R.S.—On the intra-cellular digestion and endoderm of Limnocodium, by E. R. Lankester, M.A., F.R.S., with plates 8 to 10.—On the micrometric numeration of the blood-corpuscles, and the estimation of their hæmoglobin, by Mrs. Ernest Hart.—Preliminary account of the development of the lampreys, by W. B. Scott, M.A.—On some appearances of the red blood-corpuscles of men and other vertebrata, by G. F. Dowdeswell, B.A.

THE *Journal of Anatomy and Physiology, Normal and Pathological*, vol. xv., part 2, January, 1881, contains—Dr. John Struthers, the bones, articulations, and muscles of the rudimentary hind-limb of the Greenland right-whale (*Balæna mysticetus*), (with four plates).—Dr. Creighton, on an infective form of tuberculosis in man identical with bovine tuberculosis.—Dr. W. Osler, medullary neuroma of the brain (plate 18).—A. Doran, case of fissure of the abdominal walls (plate 19).—Dr. D. Newman, description of a polygraph (with woodcut).—Dr. O. H. Jones, on the mechanism of the secretion of sweat.—Dr. P. S. Abraham, anomalous pilose growth in the pharynx of a woman (woodcut).—Dr. R. Saundby, histology of granular kidney (woodcut).—Dr. J. Oliver, two cases of cerebellar disease.—Prof. M'Kendrick, on the colouring-matter of jelly-fishes.—Dr. Cunningham, nerves of hind-limb of the Thylacine and Cuscus.—Dr. W. J. Fleming, pulse dicrotism.

THE *American Naturalist* for January, 1881, contains: Prof. A. Geikie, the ancient glaciers of the Rocky Mountains.—Fred. W. Simonds, the discovery of iron implements in an ancient mine in North Carolina.—William Trelease, on the fertilisation of *Calamintha nepeta* (woodcuts).—S. V. Clevenger, comparative neurology.—E. L. Greene, botanising on the Colorado desert.—W. J. Beal, on a method of distinguishing species of poplars and walnuts by their young leafless branches (woodcuts).—James L. Lippincott, an address to the fossil bones in a private museum.—The Editor's table: Recent Literature.—General Notes [this portion of the journal has been very considerably enlarged with this number. The Botanical, Zoological, Entomological, Anthropological, Geological, Geographical, and Microscopical Sections are each under the charge of a special editor as formerly].—Scientific News.—Proceedings of Scientific Societies.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 27.—"The Refraction Equivalents of Carbon, Hydrogen, Oxygen, and Nitrogen in Organic Compounds." By J. H. Gladstone, Ph.D., F.R.S.

Since the communication which I had the honour to read before this Society in 1869, "On the Refraction Equivalents of the Elements," very little has been done on the subject.

Of late however its importance in regard to theories of chemical structure has been recognised by Dr. Thorpe and other chemists in this country, and attention has been recalled to it in Germany by the papers of Brühl, who, following closely in the footsteps of Landolt, has endeavoured to explain the results in the language of modern organic chemistry.

At this juncture it may be of service to put on record my present views in regard to the refraction equivalents of the four principal constituents of organic bodies—carbon, hydrogen, oxygen, and nitrogen.

Carbon.—Carbon in its compounds has at least three equivalents of refraction, 5·0, 6·0, or 6·1, and about 8·8.

Whether its refraction should be one or other of these appears to depend on the way in which the atoms are combined.

When a single carbon atom has each of its four units of atomicity satisfied by some other element, it has a value not exceeding 5·0.

When a carbon atom has one of its units of atomicity satisfied by another carbon atom and the remainder by some other element, it has the value of 5·0. This is also the case if two of its units of atomicity are satisfied by carbon atoms.

When a carbon atom has three of its units of atomicity satisfied by other carbon atoms, its value is 6·0. The most striking instance is that of benzol, C_6H_6 (refraction equivalent 43·7).

There are other organic compounds in which only some of the atoms of carbon have the higher value. It has been especially the work of Brühl to point this out, and to show that where they occur (as in amylene or the allyl compounds) the carbon atom is in a condition similar to those in the phenyl nucleus, that condition in fact which is generally represented in our graphic formulæ by two carbon atoms linked by double bonds. The value assigned by Brühl in such cases is however 6·1. This somewhat higher figure is deduced from the aggregate value of the six carbon atoms in the nucleus of the aromatic series, which (except in benzol and its simpler substitution products) would appear to be nearer 37 than 36. The fact however is susceptible of another interpretation. The replacement of hydrogen by some nomad radicle is an important change; and if that radicle be CH_3 it is evident that according to present views the carbon atom must have all four of its units of atomicity satisfied with carbon, and by analogy we should expect it to have its refraction increased.

When a carbon atom has all four of its units of atomicity satisfied by other carbon atoms, each of which has the higher value of 6·0 or 6·1, its equivalent of refraction is greatly raised. There are compounds in which the atoms of carbon actually out-number the atoms of hydrogen or its substitute, such as naphthalene, $C_{10}H_8$ (ref. eq. 75·1), naphthol, $C_{10}H_8O$ (79·5), phenanthrene, $C_{14}H_{10}$ (108·3), and pyrene, $C_{16}H_{10}$ (126·1). That the refraction is greatly raised is evident from the fact that, if we were to reckon all the carbon atoms at 6·1, the refraction equivalent of the body would not be fully accounted for. It is evident that in pyrene only ten of the atoms of carbon can be in the same condition as they are in benzol or styrol, the other six must have all their units of atomicity satisfied by carbon alone. Provisionally I venture to assign 8·8 as the refraction equivalent of this highest carbon.

There are several other bodies, such as anthracene, anethol, furfurol, and hydride of cinnamyl, which from their abnormally high refraction appear to contain carbon in this last condition.

Hydrogen.—The general evidence with regard to hydrogen in organic compounds tends to show that it has only one refraction equivalent, that originally assigned to it by Landolt, 1·3.

Oxygen.—Brühl has been the first to point out that oxygen in organic compounds has two values, and he comes to the conclusion that it has the value 3·4 where the oxygen is attached to a carbon atom by a double linking, but 2·8 in hydroxyl and where the oxygen is united to two other atoms. This is deduced from experimental data. But there are other results which present difficulties, such as the various alcohols.

Nitrogen.—Nitrogen has two values, 4·1 and 5·1, or thereabouts.

The lower value, 4·1, is that originally deduced from cyanogen and metallic cyanides, and it seems to be generally confirmed by the observations on organic cyanides and nitriles. The higher value, 5·1, is deduced from observations on organic bases and amides.

I hope shortly to submit to the public the whole of the data for these conclusions.

February 3.—"On the Influence of Temperature on the Musical Pitch of Harmonium Reeds." By Alex. J. Ellis, F.R.S.

The writer gave a tabular account of the experiments on the harmonium reeds of Appunn's treble tonometer at South Kensington Museum, at temperatures differing by from 20° to 26° F., which rendered it probable that the pitch of such reeds was affected by temperature to twice the extent of tuning-forks and in the same direction, that is, that they flattened by heat and sharpened by cold about 1 in 10,000 vibrations in a second for each degree Fahrenheit.

"On an Improved Bimodular Method of computing Natural and Tabular Logarithms and Anti-Logarithms to Twelve or Sixteen Places, with very brief Tables." By Alex. J. Ellis, F.R.S.

A bimodular method is one founded on the familiar proposition, that if the bimodulus (that is, twice the modulus of any system of logarithms) be multiplied by the difference and divided by the sum of two numbers, the result would be approximatively the difference of their logarithms. The improvement consisted in a simple preparation of a given number to make it lie between two numbers in a given table of interpolation, consisting of 100 entries, and in then determining how many places might be trusted without correction, and in correcting the result by a short table so as to give twelve places at sight and sixteen places by means of ordinary table of seven figure logarithms. The antilogarithms were found by first depriving a logarithm of its correction, and then dividing the result added to the bimodulus by the result subtracted from the bimodulus—an entirely new rule. Complete tables and worked-out examples fully explained were added.

"On the Potential Radix as a Means of Calculating Logarithms to any Required Number of Decimal Places, with a Summary of all Preceding Methods Chronologically Arranged," by Alexander J. Ellis, F.R.S.

A positive numerical radix consists of the numbers r, $1 + \cdot 0_m r$, and their logarithms, where r varies from 1 to 9; 0_m means a series of m successive zeros, and m varies from 1 to any required number. The term "radix" is due to Robert Flower (1771) and is preserved *in memoriam*. It was shown that such a table would enable any logarithm to be calculated by the improved bimodular method and other methods. A negative numerical radix consists of the numbers $1 - \cdot 0_m r$, and their negative logarithms, and it was shown that such a table would serve the same purpose somewhat more easily. Hence the whole process is reduced to the construction of such radixes. A chronological summary was then given of all preceding methods, showing that most of them depended on having such radixes. The construction of a numerical radix is however a very long and troublesome process by the methods ordinarily used. For this purpose the potential radix for natural logarithms was first constructed, consisting of 10^r, 2^r, $(1 \cdot 1)^r$, and $(1 \cdot 0_m 1)^r$, negative $(1 - \cdot 0_m 1)^r$, from $r = 1$ to $r = 10$ (the latter terms being calculated by simple addition), and their natural logarithms, first to any number of places from the very simple series for nat. log. $(1 \pm \cdot 0_m r)$, and secondly, by simple addition. This gives a radix from which natural logarithms of all numbers can be calculated to any number of places by the improved bimodular method. But the main use of the potential radix is to calculate the nat. logs. of the numbers of the numerical radix. The radix for tabular logarithms is then found by multiplying by the modulus, already calculated from the potential radius. All this was fully explained by tables and examples.

Mathematical Society, February 10.—Mr. S. Roberts, F.R.S., president, in the chair.—Mr. W. Woodruff Benson, University of Michigan, was elected a member, and Prof. Rowe and Mr. J. Parker Smith were admitted into the Society.—The following communications were made:—On some integrals expressible in terms of the first complete elliptic integral and of gamma functions, by Mr. J. W. L. Glaisher, F.R.S.—Some theorems of kinematics on a sphere, by Mr. E. B. Elliott, M.A.—Supplement on binomial biordinates, by Sir J. Cockle, F.R.S.—An application of conjugate functions (to the case of membranes), by Mr. E. J. Routh, F.R.S.—Note on Abel's theorem, by Mr. T. Craig.

Linnean Society, February 3.—Robt. McLachlan, F.R.S., in the chair.—Lieut.-Col. A. A. Davidson was elected a Fellow.—Examples of Prof. C. Semper's method of preserving the soft tissues of animals as teaching-specimens were exhibited on behalf of Herr L. Würth of Würzburg.—Mr. G. Murray exhibited and made remarks on a Japanese book containing wood sections.—Mr. C. Craig-Christie exhibited, and a note was read on, the presence of what appeared to be deciduous stipules in *Ilex aquifolium*, thus contrary to the usually-accepted assertion that the order Ilicineæ is exstipulate.—The following paper by Mr. G. Bentham was read: "Notes on Cyperaceæ; with special reference to Lestiboudois' Essay on Beauvois' Genera." The essay in question was founded on Palisot and Beauvois' MS., which was originally intended to follow his "Agrostographia," and has been almost entirely lost sight of, and random guesses have been made at the species intended by the short characters given in Roemer and Schultze's "Systema."—Nees von Esenbeck, in the 7th, 8th, and 10th vols. of the "Linnæa," and Supplement 123, or Kunth in vol. ii. of his excellent "Enumeratio," appear to have correctly identified many of these. Eighteen so-called genera are now referred to various established genera. Steudel's Synopsis is marred by the author's hazy ideas of species. Boekeler has a thorough knowledge of species, but his diagnoses are often excessively long. Mr. Bentham proposes few changes in the order of genera as set forth by Kunth, and he considers that Boekeler's primary division of the order as to whether the fertile flower is hermaphrodite or female only, bears the test of detailed examination.—Hermaphrodite flowers:—(1) Scirpeæ, (2) Hypolyteæ, (3) Rhyncasporeæ. Unisexual flowers:—(1) Cryptangeæ, (2) Scheriæ, (3) Cariceæ.—A paper was read by Mr. A. D. Michael, observations on the life history of Gamasinæ. In this the author endeavours to decide some of the disputed and knotty points in reference to these humble parasites; M.

Megnin of Versailles and Dr. Kramer of Schleusingen, both good authorities on the subject, being at variance thereon. Mr. Michael, believing that detached observations on captured specimens may have produced unreliable results, has himself bred Gamasids, closely followed their changes and growth, and watched their manners, and thus has arrived at what he on good grounds assumes to be important results respecting their life-history. He states that the remarkable power of darting each mandible separately with speed and accuracy of aim far in advance of the body, the powerful retractile muscles attached to these mandibles, the organisation of the remainder of the mouth, the extreme swiftness of the creatures, the use of the front legs as tactile organs only, and not for the purpose of locomotion, and the ample supply of tactile hairs in front only, seem to fit the animals for a predatory life, and point to habits similar to those of *Cheyletus* and *Trombidium*, rather than to those of the true vegetable-feeders, such as the Orbatidæ and Tetramachi. He further concludes (1) that Megnin is correct in saying *Gamasus coleoptratorum* and other allied creatures, with the conspicuously-divided dorsal plates, are not species at all, but are immature stages of other species; (2) that the division of the dorsal plate is, in most cases at all events, a question of degree, and does not form a sound basis for classification, as applied by Koch, Kramer, and others; (3) that the dorsal plates do not grow gradually, but alter in size, shape, or development at the ecdysis; (4) that Megnin is right in saying that the characteristic of the so-called *G. marginatus* is simply a provision possessed by the females of a large number of species; (5) that the extent of the white margin depends upon the extent to which the abdomen is distended by eggs; (6) that Megnin is in error in saying that *Coleoptratorum* is the nymph of *Crassipes*. The nymph of *Crassipes* does not show any divided dorsal plates which can be seen on the living creature; (7) that in the species bred there has not been observed any inert stage before the transformations or ecdysis; (8) that in the same species copulation takes place with the adult female, and not with the immature one, as Megnin contends, and that it is by the vulva, not the anus.—Two papers were read on the coffee-leaf disease (see Science Notes, p. 354).

Institution of Civil Engineers, February 8.—Mr. Abernethy, F.R.S.E., president, in the chair.—A paper on the temporary works and plant at the Portsmouth Dockyard Extension, by Mr. C. H. Meÿer, Assoc. M. Inst. C.E., was read.

PARIS

Academy of Sciences, February 7.—M. Wurtz in the chair.—The following papers were read:—On photographs of nebulæ, by M. Janssen. It is comparatively easy to get a photographic image of the brightest parts of nebulæ, but very difficult to get complete images which may serve for future comparison. The optical and photographic conditions should be exactly defined. M. Janssen suggests taking for criteria images of stars, with plates a little out of focus, so as to give an opaque circle. Five or six of these stellar circles accompanying the photograph of a nebula would indicate what the conditions had been.—On the thermic formation of pyrogenic carburets, by M. Berthelot.—Some remarks on the characters of chloro-organic gases and vapours, by M. Berthelot. The formation of a white precipitate in neutral or slightly acid nitrate of silver, traversed by a gaseous current, is not a sufficient character of chlorine or hydrochloric acid.—Examination of materials from some vitrified forts of France; conclusions, by M. Daubrée. The methods of producing these forts seem to have been various. To soften a rock like granite (sometimes used), to fuse its mica, and even, at times, its felspar, in thicknesses of several metres, implies large use of fuel and prolonged skilful effort. The fire was probably applied *within* the walls, and a current of forced air may have been used, besides draught. The makers unconsciously produced some minerals that have only of late been reproduced in the laboratory.—On the Great Canal de l'Est and the machines set up to ensure its alimentation, by M. Lalanne. This canal (made in consequence of the change of frontier in 1871) runs from near Givet, on the Meuse, by Mezières, Sedan, Commercy, Toul, &c., to Port-sur-Saône (it includes, in a total length of 468 km., 20 km. of the canal from the Marne to the Rhine). There are two large pumps in the Moselle valley, worked by the water of that river, also steam-pumps at Vacon. Two large reservoirs are projected, one near Paroy, the other at Aouze.—Study of actions of the sun and the moon in some terrestrial phenomena, by M. Bouquet de la Grye.—Observations of Perseids at Toulouse Observatory in 1880, by M. Baillaud. 1172 falling stars were counted on August 9, 10, and 11; the maximum was on the 10th, between 14h. and 15h. The trajectories were generally very short, and their extremities pretty far from the radiant point. The meteors were divisible into two groups.—On modes of transformation which preserve lines of curvature, by M. Darboux.—On simultaneous linear differential equations, with rational coefficients, whose solution depends on the quadrature of a given irrational algebraic product, by M. Dillner.—On a property of the product of k integrals of k linear differential equations, with rational coefficients, the solution of which depends on the quadrature, respectively, of k rational functions of the independent variable, and of a given algebraic irrationality, by the same.—The problem of remainders in two Chinese works, by M. Matthiessen.—On a peculiar phenomenon of resonance, by M. Gripon. A tuning-fork, giving a simple sound, will set in resonance masses of air which produce a sound comprised in the harmonic series of the fork's sound. The form of the mass of air is unimportant. One grave fork set in vibration forks which gave harmonic sounds, but not others, the two forks being connected by very fine copper wire (stretched).—On elliptic double refraction, and the three systems of fringes, by M. Croullebois.—On a new apparatus for showing the dissociation of ammoniacal salts, by M. Tommasi. In a glass tube is hung with platinum wire a strip of blue litmus paper that has imbibed a concentrated solution of chlorhydrate of ammonia. On putting this dissocioscope in boiling water, the sal-ammoniac is dissociated and the paper turns red; if then put in cold water the dissociated ammonia combines again with the acid, and the paper turns violet again.—On derivatives of acroleine, by MM. Grimaux and Adam.—Action of hydrochloric acid on aldehyde, by M. Hauriot.—Inoculation of the dog for glanders, by M. Galtier. The dog may contract the disease (through inoculation) and recover many times; but its receptivity (comparatively small at first) gradually diminishes, and, there is reason to believe, may be quite effaced. The power of the virus is attenuated by successive cultivations in the dog; this appears in an ass, *e.g.* inoculated with the later virus of a dog inoculated several times.—Physiology of dyspepsia, by M. Sée. In grave dyspepsia the stomach pump may advantageously be used to clear the stomach of liquids unfavourable to digestion.—On the histology of the pedicellaria and muscles of the sea urchin (*Echinus sphæra*, Forbes), by MM. Geddes and Beddard.—Researches on the development of sterile sporangia, in *Isoetes lacustris*, by M. Mer.

VIENNA

Imperial Academy of Sciences, February 10.—V. Burg in the chair.—C. Heller, on the distribution of the fauna of the high mountains of Tyrol.—R. Maly and F. Hinseregger, studies on caffeine and theobromine (second paper).—V. Hochstetter, on the Kreutzberg Cave, near Laas, in Carniola, and *Ursus spelæus*.—R. Puluj (1) on radiant electrode-matter; (2) remarks relating to the priority claimed by Dr. Eugen Goldstein.

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