NATURE

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"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH.

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*"To the solid ground
Of Nature trusts the mind which builds for aye."*—WORDSWORTH

THURSDAY, MAY 1, 1879

COUES'S "BIRDS OF THE COLORADO"

Birds of the Colorado Valley. By Elliott Coues. Part I. *Passeres to Laniidæ.* Bibliographical Appendix. United States Geological Survey of the Territories. Miscellaneous Publications—No. 11. 8vo, pp. 807. (Washington, 1878.)

WHAT is commonly called a "popular" zoological work is nearly always one that is bad. The knowledge possessed by the writers of such books is seldom greater than that of the public for whose benefit the books are ostensibly published, and is far behind that of a moderately well-informed student of the particular branch concerned. We shall name no names, but our readers will doubtless be able to supply several instances in support of this assertion without inconveniently taxing their memories. Within a very short space of time they have seen the works of two English naturalists, whose writings have long attained a classical position, subjected to such treatment at the hands of "popular" editors as would "make the angels weep," if those celestial beings be actuated by human affections, while the number of books independently put forth by "popular" sciolists is past counting. These books have their day—and sometimes it unfortunately is a long day. Granting that they do some good by administering to or fostering the taste for natural history already so widely spread, the evil they perpetrate is far greater. This evil lies first in their instilling for the most part erroneous ideas into the innocent pupil, and secondly in their occupying and encumbering the ground to the exclusion of better books, which drop still-born from the press. The struggle for existence is admittedly slow in operation, and though we doubt not which way the triumph will eventually be, the end is far off, and ere it arrive dire mischief is done. The falsest notions are promulgated, the feeblest arguments are maintained, and the learner at last discovers to his sorrow that, instead of proceeding joyously on his course, he has to unlearn what he has acquired. Something may be said in favour of the mental discipline thus undergone, but on the other hand must be weighed the waste of time that attends the process, and the spirit of the age is against any discipline that is in the least doubtful of effect. As an epithet to a work on zoology, "popular" in nine cases out of ten really means debasing.

It is therefore with great pleasure that we can declare the volume before us—"The Birds of the Colorado Valley"—to be a popular book, not in the common sense but in the uncommon, highest and best meaning of the phrase. Dr. Coues has long since attained a scientific reputation that cannot be gainsaid. His numerous works are as well known and as highly esteemed on our side of the Atlantic as on his own, and one quality which is conspicuous in all of them is their thoroughness. When Dr. Coues writes a sentence he is in earnest, and there is no mistaking what he says. Whether the subject be the laboured description of an animal whose fur or plumage is mottled and diversified by the most delicate combination of tints—many a rodent, an owl, or goatsucker for example; the unravelling of an abstruse question of complicated synonymy; an account of the economy of a beast or bird to be given from his own wide experience or compiled from the observation of others—this quality is manifest. He has of course his faults. Some of them he has not been slow to acknowledge, but there is seldom a fault in his works that can be fairly called a blunder, and even such blunders were they twice as great and twice as numerous we could readily pardon, for there runs through all his writings, showing itself at times even in the driest spots, a humorous vein that can scarcely fail to excite a sympathetic flow even from the sternest of scientific breasts. In this volume Dr. Coues gives freer play to his lighter mood than, we think, in any of his former works, and at times (though he can be as serious when he pleases as the strictest man of science would wish) there is a boyish elasticity in his style which is exceedingly pleasant. He is always a readable author, whereby we mean that apart from the value of the information we derive from his statements, he clothes them in agreeable language, which far too many of his zoological brethren neglect to do. Nor is there any attempt at fine writing, which of course is a great mistake—*the* mistake in fact into which "popular" naturalists fall. Here is a passage which we extract, since it relates to a species now considered to be common to Europe and North America—the Tree-Creeper (*Certhia familiaris*)—and its accuracy will be recognised by all who have watched the bird in this country :—

"The leading trait of the Brown Creeper is its extraordinary industry—the 'incomparable assiduity,' as it has been well styled, with which it works for a living. Like all good workers, the creeper makes no fuss about it, but just sticks to it. So quietly, yet with such celerity, does it go about its business that it scarcely seems to be at work, but rather to be rambling in an aimless way about the trunks of trees, or at most only caring to see how fast it can scramble up to the top. During all this time, however, the bird is on the alert in the search for insects, which it extracts from their lurking-places with such dexterity that its progress is scarcely arrested for a moment; and the number of these minute creatures yearly destroyed is simply incalculable. The creeper is strongly attached to the trunks of large trees, being seldom seen foraging on even the larger branches; and it has a great fancy for travelling upward. These two traits combined result in its marked habit of beginning its curious search for insects near the bottom of a tree, and ascending with jerks in a straight or spiral line to the top. Then, if it likes the tree, and thinks it a good place to stay a while longer in, the bird launches itself into the air, and drops down on wing, to begin another ascent, in preference to scrambling down again, as a woodpecker or nuthatch would do. The easy, gliding motion with which it climbs has deceived one writer into stating that the creeper does not hop along like a woodpecker; but, in fact, the movement is exactly the same in both cases. One of the English writers (Barrington, Zool. 2nd ser. p. 3998) describes, however, something peculiar in the position of the feet during the act of climbing :—These, he says, are not held parallel with each other, and near together, under the belly, but widely straddled, and thrown so far forward as to form with the end of the tail a surprisingly broad-based isosceles triangle. So nimble is the bird, and such a sly way has it of eluding observation by turning in the opposite direction to that in which a person moves to look after it, thus continually interposing the trunk of the tree in the line of vision, that it is no wonder the way it holds its feet long remained unascertained. Many things conspire to screen the queer little bird from any but the most patient and closest scrutiny during its ordinary vocations; and so nearly do its colours correspond with the tints of the bark that it is likely to be overlooked altogether. But its habits are so methodical and undeviating that when one has learned them there is no difficulty. If we see a creeper alight at the base of a tree on the side away from us, we have only to stand still, and keep a sharp look-out for it higher up; in a few moments, its spiral twisting will bring it round to our side; the chief point is to look high enough up, for it is surprising how rapidly the bird ascends. It generally makes the whole journey before dropping on wing to the base of the tree again, or making off to another; sometimes, however, the tree seems to be not to its liking, when, as if actuated by a sudden impulse, it abandons an unprofitable search, and flies to a more promising feeding ground."

This is a very fair sample of the author's style in treating of birds' habits, but many extracts would be needed to show the enormous pains he has taken with the more scientific part of the book. The array of references prefixed to the account of each species is almost appalling, but when we come to look into them we find these citations are not printed merely for the sake of giving an exhaustive list, but that there is a sufficient purpose for the insertion of almost each of them. In like manner we can praise the care bestowed on the technical characters of the several species, so far as we have been able to test them, for in diagnosis, that touchstone of a descriptive biologist, Dr. Coues especially shines, as indeed one expect might from the author of the "Key to North American Birds."

There is, however, one thing in this volume that we must say has excited our wonder, and must, we suspect, have deeply disturbed the minds of more than one naturalist who has read it. Dr. Coues, fully conscious of the risk he is running, cannot bring himself to reject the notion of Swallows and other birds plunging into the water in autumn and passing the winter in deep slumber! He admits that "it is as much as a virtuous ornithologist's name is worth to whisper hibernation, torpidity, and mud!"; but he adds further on, "It is not permitted to us, in the present aspect of the case, to rule out the evidence" in favour of what, for our own part, we must unhesitatingly call an exploded fable. It is certainly as much as a virtuous reviewer can do to treat this matter calmly. Yet we hold ourselves a better judge of evidence than Dr. Coues, and in spite of this singular aberration we draw our conclusion from the rest of his work that his reputation for sanity need not be thereby impugned. But he certainly overstates his case when he says that "the testimony, so far from ceasing with the irresponsible infancy of science, is reiterated to-day with the full voice of science, in terms that have not been successfully refuted." Now what is a successful refutation to one man, we all know, is not necessarily so to another. Are there not virtuous gentlemen who still insist on having proved the flatness of the earth, the squaring of the circle, and various geometrical impossibilities, and does not their very existence show that their testimony has not been "successfully refuted"? Nothing short of a miracle will convince some people, and we say this in view of both believers and unbelievers in the torpidity of birds. From whom is "the full voice of science" to be heard if not from scientific men, and where is the scientific man of to-day (Dr. Coues himself excepted) whose testimony reiterates that of Achard, Dexter, Pollock, Kalm, Forster, and the rest of those named in our author's excellent bibliography of the subject? We may have persons of intelligence and veracity, of respectability and honour, but we find not of late years one scientific man who can vouch for any statement of the kind on his own authority. It would be idle, however, to pursue the subject further; we should like to know, nevertheless, whether Dr. Coues refuses to reject the testimony as to the existence of Were-wolves, which seems to be on a par with, or even stronger than that in regard to, the torpidity of birds, and we shall only add that we think he is indeed "greatly mistaken" in his view that the Chimney-Swift (*Chætura pelagica*) "is not recorded as occurring anywhere beyond the United States in winter." If he will refer to a certain "Nomenclator Avium Neotropicalium," published not long since, he will find this species entered as occurring in Mexico, and we think we "could give reasons for the supposition" that it winters regularly in that country and others lying further to the south, instead of "hibernating in hollow trees" in the United States, so that whatever our author builds upon his basis would seem to have an unstable foundation.

We have just mentioned the excellent bibliography of the swallow-question given by Dr. Coues, but this is by no means the only one contained in his work. By way of appendix we have a "List of Faunal Publications relating to North American Ornithology," with a most useful double index (of authors and localities) thereto, the whole extending over more than 200 pages. The like of this we know not elsewhere, and we cannot sufficiently thank him for it. It makes us forget and forgive the single *escapade* which we so much regret having had to notice. One remarkable merit it possesses is that except in specified cases—and these, it is easy to see, are very few in number—no title has been taken at second-hand. More than this, we are told that the present batch of titles is but an instalment of a Universal Bibliography of Ornithology which the author has in hand, and towards which he has already collected about 18,000 titles! We are sure our readers will agree with us in hoping that Dr. Coues will be able to complete his laborious task, as well as in considering that its completion will redound to the already great credit of the department over which Dr. Hayden presides, and also to the medical staff of the United States army, which numbers Dr. Coues among its members.

BRITISH BURMA

British Burma and its People. By Capt. C. J. F. S. Forbes, F.R.G.S., M.R.A.S., &c. (London: John Murray, 1878.)

THIS book is offered as the result of thirteen years' experience derived from close intercourse, both officially and privately, with the people of Burma during that period. Such works are frequently contributed by the pro-consuls of the British empire, and afford, apart from their scientific value, good material to judge of the men and methods of our colonial government. Their merits are naturally unequal. The volumes of Raffles and Tennent, become classical, supply the corner-stones of future compilations, and are the exciting causes of a more ephemeral literature. It is, however, seldom that we see combined with the administrative capacities of our governors and commissioners a thorough knowledge of the ethnology, biology, and physical characteristics of the regions over which they preside. When such a man appears, and further possesses the quality of observation, his work marks an epoch, and English rule receives a new significance. It is in no adverse spirit that we say thus early that Capt. Forbes' work will not rank in this category, and we desire rather to commend it for what it does possess than to criticise it for the information which it does not supply.

Omitting the long narrow strip of mountainous country and sea-coast which forms the Tenasserim province below Maulmain, British Burma may, roughly speaking, be said to consist of three broad mountain ranges, having outside them on the west the sea-board province of Arracan, embracing between them the two great valleys of the Irrawaddy and the Sittoung, which forms, south of Rangoon, one vast plain, the centre range of the three mountain chains being shorter than are the other two. Its physical geography is interesting and peculiar, and in its pluvial character most characteristic and remarkable. The wet season lasts from about May to October, and during these five months of almost constant rain the average rainfall amounts to 184 inches at Maulmain,—in one exceptional year to 228 inches. During this period the great Irrawaddy rises 40 feet above its summer level and floods the surrounding lowlands, whilst its main current travels with a velocity of five miles an hour. Many proposals have been made to found sanatoriums for Europeans on the high mountain ranges of Burma, but however pleasant in summer, they would, says Capt. Forbes, "have to be abandoned to the jungle beasts and the elements during the rains, for not even natives could remain to take care of the buildings; and so incredibly rapid and luxurious is vegetation there, that the very next year a forest would have to be cleared away to find the houses again." December, January, and February are the cold months, whilst the hot weather lasts from February till the rains commence again. The climate, however, is excellent; the registration returns show that the deaths of children under five years of age are in the proportion of 27·85 of the total death rate; the percentage of children under twelve years of age is 35·8 of the whole population.

The chapter on the physical geography of the region is evidently compiled from careful authorities. The author appears to have undertaken no original investigations, nor to have added any original information on the subject; the biological effects of these annual inundations, in such a region teeming with animal life, excite the profoundest interest, but await the chronicle of a qualified observer. The principal part of the volume is occupied with an account of the people of British Burma, which the sociologist may find a storehouse of useful facts, and which must prove of the greatest value as an introduction to the ethnology of the region to all such as are approaching that subject. The statistical tables of the Census Report for British Burma, 1872, "give eighteen divisions of the indigenous races of so-called Mongolian origin." According to Capt. Forbes four great races occupy the Burman peninsula—the Mōn, the Karen, the Burman, and the Taï, or Shan, of which the Mōns form the majority of the inhabitants of British Burma. As regards the author's endeavour to give "a probable account of the route and order by which they arrived in their present localities," we must refer the reader to his arguments, and, without expressing an opinion thereon, will merely remark that even in science, when the rigour of induction is at all relaxed, a sentence written by Mr. Leslie Stephen is very applicable—"one clever man's guess is as good as another, whatever the period at which he lived." The chapters devoted to "social life and manners," &c., are very valuable to the comparative ethnologist. Some of these facts have been related before, but collected thus in a compendious form, and enriched with the results of a long official experience, they form material to supply links in that chain of generalisations which during the last few years in the hands of Tylor and Lubbock have created a new branch of anthropology.

Among the hill tribes the Karens are now divided between "those who have permanently settled in the plains and betaken themselves to a regular system of agriculture and those who still remain in all their primitive freedom of the hills." This freedom, however, consists of a long and bitter struggle to raise their scanty crops on the hardly-wrought clearances of the virgin forest. Among the other enemies to their agricultural pursuits, Capt. Forbes mentions the visitations of vast hordes of "hill rats," which at long intervals of forty or fifty years settle on a tract of country for two or three years in succession, "till, like a swarm of locusts, they have reduced it to a desert." When on the move, in vast swarms, they cross the streams in shoals, so that the water is black with them, and from 1870 to 1874 they so devastated the hill country east of the Sittoung river that government was compelled to expend some 10,000*l.* in relieving the local Karen tribes.

The chapters upon Burman Buddhism must not pass without notice. Buddhism is not a subject quite suitable to the columns of NATURE, but there is exhibited in the short treatment of it such an intelligent appreciation of a vast system of philosophy, unaccompanied by narrow prejudice or preconceived ideas, as, if not perfect, proves the author to be capable of conducting investigations on thoroughly scientific principles.

W. L. D.

OUR BOOK SHELF

From Kulja, across the Tian Shan, to Lob-Nor. By Col. N. Prejevalsky. Translated by E. Delmar Morgan. With Introduction by Sir J. Douglas Forsyth, C.B. (London: Sampson Low and Co., 1879.)

COL. PREJEVALSKY has already proved himself one of the most scientific and determined of modern explorers, and has probably done more than any single man for an accurate knowledge of Central Asia. We have noticed in these pages his valuable work on his journey in Mongolia and Western China, and this narrative, short as it is, maintains the reputation he has already gained. The journey here described was made in 1876-7, and has been the means of clearing up several obscurities in the hydrography of the region visited. We have already, shortly after Prejevalsky's return, given the main results of the journey, from Kulja, south-east across the Tian Shan Mountains, by the Yulduz River, to the Tarim, and along that river to its termination in Lake Lob-nor, at the northern foot of the Altyn-tagh Range, on the 90th deg. of E. long., and just south of the 40th parallel N. Baron von Richthofen has endeavoured to prove that the present Lob-nor is not the Lob-nor of the old geographers, which he maintains was farther north. But to this Prejevalsky has an answer that it seems to us difficult to refute, notwithstanding that Richthofen probably knows more about the history of Central Asian geography than any one living. However the case may stand with regard to this, there can be no doubt about the value of Prejevalsky's observations on the present Lob-nor, which he states is fresh, shallow, almost overgrown with tall reeds, in the midst of which its strange mongrel inhabitants live, and of which they build their houses. The Altyn-tagh Mountains Richthofen considers the most surprising discovery of the Russian traveller, for it was generally supposed that there was an extensive tract of low country continuing through several degrees of latitude to the south of the lake. Prejevalsky's observations on the fauna of the Tarim and Lob-nor will be appreciated by zoologists, as will also his account of the wild camel. He has a special interest in ornithology, and above all in that department relating to the migrations of birds; and the part of his narrative which describes what he observed on this point during his stay at Lob-nor is one of exceptional value, and will, no doubt, be read with interest and profit by those who take an interest in the subject of migration. Mr. Delmar Morgan, who has made an excellent translation, has added to the brief narrative chapters on Lake Balkash, Lake Ala-Kul, and the Starovertsi, which, though somewhat irrelevant, are acceptable as being of real value. An excellent large map accompanies the volume, besides a smaller one, to illustrate the controversy between Prejevalsky and Richthofen.

A Manual of Practical Chemistry: The Analysis of Foods and the Detection of Poisons. By Alexander Wynter Blyth, M.R.C.S., F.C.S., &c. (London: Charles Griffin and Co., 1879.)

THIS work of 468 pages consists of two divisions, the first treating of the analysis of the principal articles of diet in daily use, the second of the detection and estimation of certain organic and inorganic poisons. The matter pertaining to the first division is further divided into seven parts, in which the different articles of diet are considered in their proper groups. These chapters are well and pleasantly written, bringing the information as much as possible up to date, and introducing where necessary modern methods of analysis. This may be seen in the chapter on sugars, where a full description of the optical method for the estimation of these bodies by the polariscope is given, with an accompanying diagram of the various parts, lenses, &c., of Soleil's saccharimeter. The remaining portions of the first division contain the matter concerning bread and flour; milk, butter, tea, coffee, cocoa, &c.; the chapter on tea and coffee containing a large number of analyses which no doubt will prove of great use. A considerable part of the book is devoted to the examination of alcohols, wines, and beers, in which instructions are laid down for the examination of such substances. In connection with this part the author gives a reprint of the tables introduced by M. A. Gautier for the systematic detection of colouring matters likely to be met with in wines, and gives an abstract of Gautier's paper containing the necessary instructions for the preparation of the sample, &c., to be examined.

The second division of the book contains the detection and estimation of the different poisons, the consideration of the organic preceding that of the inorganic. Although the information conveyed by the author is exact and well arranged with regard to the individual tests for each separate poison, it is to be regretted that he has not thought it necessary to develop more fully his remarks on a systematic course to be employed in the separation of the different poisons from each other. In many cases where doubtful evidence of poisoning exists a most exhaustive analysis is required, and we fear the general instructions laid down in the book for this purpose, or "method of procedure in analysis," as the author terms it; are somewhat insufficient.

The organic poisons and the detection of phosphorus are first taken into account in two divisions, first, those detected mainly by methods of distillation, and second, those separated for the most part by alcoholic solvents. The consideration of mineral poisons is placed last in the book, and contains the usual received tests for these substances, with in some cases a description of the body. With regard to this latter part we do not see why in a book published so recently as 1879 there are no remarks on the detection or separation of tin as a poison since it has been shown in letters to some of the journals that this metal may contaminate articles of food, more especially tinned fruits.

The work is clearly printed, but some of the diagrams are somewhat crudely cut, and if refinement in the arrangement of apparatus is intended in the illustrations, hardly carry out the intention; thus in Fig. 15 it is difficult to see from the drawing what is meant in the arrangement figured between the washing bottle and the French drying tube. There are some traces also of careless printing, which it would be well to rectify in future editions, as in the equation of the action of arseniuretted hydrogen on silver nitrate on p. 372. The title of the book is also somewhat presumptuous; it is styled "A Manual of Practical Chemistry:" the two last words being in large type; a colon is here introduced and then follows the exact title of the book in smaller type, "The Analysis of Foods and the Detection of Poisons." The work cannot be fairly described as a Manual of Practical Chemistry, and the title should therefore have been restricted to the matter actually contained in the book.

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.*]

On the Spectrum of Brorsen's Comet

WITH reference to Prof C. A. Young's Note on the Spectrum of Brorsen's Comet, in NATURE, vol. xix. p. 559, it may be of interest to mention that observations made at the Royal Observatory, Greenwich, confirm his conclusion as to the coincidence of the brightest band in the comet spectrum with the green band of carbon.

We were not able to examine the comet's spectrum till April 17, as the Great Equatoreal was in the workmen's hands till that date for alterations required to allow of the more convenient use of the spectroscope. On that evening, and again on April 19, the comet's spectrum was repeatedly compared by Mr. Maunder and myself, with the spectrum of alcohol taken in a vacuum tube. The less refrangible edge of the brightest comet-band coincided as exactly as could be determined with the corresponding edge of the green carbon-band at 5,200, but the comet-band was very much wider, extending two-thirds of the way towards F (*i.e.*, about 200 tenth-metres), and covering the carbon-band at 5,200 (about 30 tenth-metres broad) and the two following fainter bands at 5,100 and 5,020. The comparisons were made on April 17 by the help of an occulting bar, and on April 19 with Hilger's bright-line micrometer, illuminated by red light. With the latter, readings for the comet- and carbon-bands respectively, agreed within half a tenth-metre. The half prism spectroscope with a dispersion of 10° from A to H (equivalent to two prisms of 60°) was used on the 13-inch equatoreal. From spectroscopic observations of the carbon compound, printed in the volume of Greenwich Observations, 1875, it appears that the bands in the spectrum of alcohol are identical with those in the spectra of olefiant gas, and of carbon oxide and dioxide.

A second band was seen in the orange of the comet's spectrum approximately coincident with the carbon band at about 5,600. This band was of about one-fourth the brightness of the principal band.

The results on April 17 were obtained without a knowledge of Prof. Young's work, and thus afford an independent confirmation of his conclusion. W. H. M. CHRISTIE

Royal Observatory, Greenwich, April 21

Blue Flame from Common Salt

I AM perfectly aware that, as Dr. Gladstone points out in your last issue, I have not *proved* HCl to be the origin of the blue flame, but I will give some of my reasons for *thinking* so.

In the first place I conclude every one will admit that chlorine in some form must be present, since only chlorides produce the flame. At one time I thought it was due to dissociated or atomic chlorine; that view, however, I discarded in favour of the hydrochloric acid theory.

When AmCl is heated, dissociation occurs, as is well known, NH_3 and HCl being formed; the HCl then plays its part in producing the blue flame. If calomel be used, it is natural to imagine that the mercury and chlorine are separated, and if the colour is due to HCl, the addition of hydrogen will be necessary before the flame is produced. As a matter of fact I have found that no coloration occurs if the calomel is heated in what I may perhaps be allowed to call the *solid* part of the Bunsen flame, *i.e.* where complete combustion takes place, but it is necessary to allow some of the unburnt gas to mingle with its vapour. In practice I adjust the wire gauze over the burner so that a black spot is seen surrounded by a red hot ring, a little calomel placed on the dark spot volatilises and colours the gas that is burning above the gauze; if the gauze is raised so that the dark spot vanishes and all is red hot, the salt volatilises without any coloration ensuing.

Although I have not been able to see any violet bands when a spark has been taken in HCl, I do not consider that it negatives my theory, since there is a considerable difference between an electric spark and a Bunsen flame, and I now have reason to think that under the influence of the spark the HCl is split up into its components, which will fully account for the absence of violet bands. I have likewise failed to get them from a spark in AmCl.

A drop of liquid HCl, introduced into a Bunsen flame by the aid of a platinum wire, gives a flash of blue colour, and a lighted taper immersed in a bottle of HCl gas has its flame surrounded by a blue mantle just before it goes out. The colour, to the eye, is identical in both cases to that produced by the volatilisation of a chloride, the peculiar violet tinge showing that it must contain rays of high refrangibility.

Lastly, if a stream of HCl gas be slowly passed into a large Bunsen flame, the colour is produced most vividly, the spectrum showing all the characteristic lines or bands. Here we have the HCl under the same conditions as the chloride and with a similar result.

Dr. Gladstone appears not to have obtained the flame by this method, since he says: "Hydrochloric acid passed into a flame never gives the violet light."

This may probably be explained by the fact that if the HCl be passed too rapidly the violet coloration gives place to green, similar to that produced by chlorine alone if the stream of gas be allowed to slacken, the violet is reproduced, and this may be repeated indefinitely. A. PERCY SMITH

Temple Observatory, Rugby, April 26

Did Flowers Exist during the Carboniferous Epoch?

ACCORDING to the position Mr. Wallace has taken in the discussion as to the order of insects to which *Breyeria borinensis* presumably belongs, everything depends upon the existence or non-existence of transverse reticulation. I re-assert that a regular and thoroughly well-marked transverse reticulation exists over all the wing.

If Mr. Wallace prefers to believe in the evidence afforded by a photograph in preference to my statement based upon actual examination, and to M. de Borre's words in his description ("Entre toutes ces nervures s'étend un réseau extrêmement complet de très-fines nervules allant transversalement d'une grosse nervure à l'autre"), it is evident that anything I could say would not alter his opinion.

Further, I utterly fail to comprehend by what process of reasoning he arrives at the conclusion that the photograph "is so beautifully sharp that it brings out the minutest details," when confessedly he has not compared that photograph with the original.

That the main nervures may be compared with some forms in Lepidoptera and found to agree to a certain extent is very possible; it would be singular if it were otherwise, considering the extreme variation in the neuration of Lepidoptera, and the practical certainty that the system of neuration in all orders of insects can be homologised. The presence of dense *transverse* reticulation in a lepidopterous insect would decidedly be an anomaly; but its absence would not prove that any particular fossil *did not* belong to the Ephemeridæ, for in some recent genera of the latter, such as Oligoneuria, Lachlania, &c., the transverse reticulation is so far absent as to be reduced to a few nervules that might be counted on the fingers of one hand.

Supposing, for the sake of argument, that my assertion may be based upon false premises (and no one is infallible), *Breyeria* would probably be relegated to that heterogeneous assemblage of extinct forms of insects possessing densely reticulate wings, to accommodate which the order Palæodictyoptera has been formed. It is not for me to here enter into an examination of the materials included in this so-called order. It will suffice to say that not one of them could be suspected of being lepidopterous.

The point at issue is, did anthophilous insects (and therefore flowers also) exist during the carboniferous epoch? According to my views we are without evidence of their existence.

I decline any further discussion on this subject until Mr. Wallace has examined the fossil, or has obtained evidence of its peculiarities from some one in whose judgment he has more confidence than he apparently has in mine.

Lewisham, April 25 R. McLACHLAN

Captain Cook's Accuracy

IN NATURE, vol. xix. p. 408, there is an article entitled "Captain Cook's Accuracy," which I think reflects unjustly upon the late Admiral Wilkes, U.S.N. As a specimen of Wilkes's inaccuracy the writer of the article cites first the discrepancy in the position of Turtle Island, the south-easternmost of the Fiji group, Cook and Wilkes differing more than 30′ of longitude. The narrative of the U.S. Exploring Expedition was written on board ship during the progress of the work, and was placed by Wilkes in the hands of the printer immediately upon his return, in order that the general results might be known without delay. The astronomical positions were given as they were recorded at the time, and were not corrected for final chronometric errors and rates, which were carefully ascertained while the charts were being prepared for publication. A comparison of the narrative with the atlas, published subsequently, will exhibit differences of longitude almost throughout.

On the general chart of the Pacific, sheet III., which is on a very small scale, so that a slight inaccuracy of the draughtsman or engraver will cause a difference of several minutes, Turtle Island will be found to be in about 178° 22′ W. long., but the special plan of the island (vol. 2, p. 94, of the Atlas) places it in lat. 19° 47′ S., and long. 178° 16′ 18″ W., while Capt. Denham, R.N., in 1856, places it in 19° 49′ 11″ S., and 178° 14′ 42″ W., where it is at present shown on the British Admiralty Charts. The difference of latitude is about 1′ 45″; that of longitude, 1′ 36″; differences which might readily be accounted for by different points of observation having been used. The difference in the outline is not very material.

As Cook placed the Island in 178° W., he was fifteen minutes in error; while Wilkes differs from the latest surveys about a minute and a half. Capt. Worth, of H.M.S. *Calypso*, in 1848, placed the island in 178° 8′ W., differing seven miles from the subsequent survey by Capt. Denham, the position by the latter being now borne on the British Admiralty chart, yet the former authority is quoted to prove the inaccuracy of Wilkes's work.

Findlay, judging from what he says upon this subject, consulted Wilkes's book, instead of his chart, which was published subsequently. The second example of Wilkes's inaccuracy, cited by the writer, is that he found from a position which he occupied at Savaii, a trend of coast differing from that as shown by Wilkes's chart, but it is a question whether he was not mistaken in the identity of the point occupied by him. The waters of the Samoan group are, so far as we know, navigated safely and almost exclusively with Wilkes's charts.

The third and last example is concerning Quiros Island (Swain's Island). The facts in this case are that the boats of the exploring expedition did *not* effect a landing on the island at all; efforts were made to do so, but were unavailing on account of the surf, so that it is quite impossible that they could report the existence of a lagoon hid from their view by a wooded strip of land even if only a quarter of a mile in width.

In criticising the work of such explorers as Cook, Vancouver, and Wilkes, it should be borne in mind that the expeditions which they commanded were for exploring rather than surveying purposes, and it is rather a matter of surprise that they should have come so near the truth when we consider the crude materials with which they had to work. S. R. FRANKLIN

Hydrographic Office, U.S. Navy, Washington, D.C., April 11 — Captain U.S.N. and Hydrographer

Sense of Force and Sense of Temperature

"J. T. B.'s" "discovery" of the distinction between muscular sensations—or, as he styles them, the "sense of force," whatever that may mean—and the sensation of temperature, has been long anticipated by Alexander Bain in his work on "The Senses and the Intellect."

Again, your correspondent's illustrations of the distinction he draws between absolute and relative muscular sensations and sensations of temperature are wholly illusory. How can it be said that a letter-sorter enjoys and improves absolute sensations of weight? Surely his sensations enable him to determine not "absolute weight" (whatever that may be), but the weights of particular letters relative to certain standards, according to which relation the postage is charged. These sensations enable him to say that certain letters are over, and others under, an ounce in weight, and thus they are in fact relative, not absolute, as "J. T. B." seems to suppose.

The same remarks apply to "J. T. B.'s" assertion that "the sense of temperature may also be rendered absolute to a certain extent," and to his illustration of the plumber who judges whether the heat of the soldering-bolt is adequate for his purpose. Here again the sensations are, in truth, purely relative, any inference drawn from them being based upon a comparison of present and previous sensations and present and previous experience of their results. A. K. R.

Mark Lane, April 23

Mr. Preston on General Temperature-Equilibrium

MY attention has been arrested by Mr. S. Tolver Preston's paper on general temperature-equilibrium in NATURE, vol. xix. p. 460, and by a letter from him in a later number (p. 555), pointing out a trifling literary ambiguity in it. As this implies that the paper is otherwise correct, you will perhaps allow me to protest, and to state that it is full of confusion of reasoning and of unsoundness.

I do not know how many sins against dynamics could be discovered by careful examination, but at least two pervade it throughout, viz. (1), the assumption that the simple relationship which exists between the movements and the temperatures of molecules of matter exists also between the movements and the temperatures of masses of matter; (2) the assumption that gaseous molecules (simple or compound) whose bond is chemical affinity differ mechanically from masses of matter (stellar or otherwise) in size and weight only, whereas they really differ in a multitude of other ways, and notably in elasticity; and from this difference alone it would be easy to show that the analogy in the paper is fanciful, and its reasonings and conclusions invalid, but I respect your space.

In conclusion I would say that I am not writing against the hypothesis of temperature-equilibrium itself. It may or may not be true. All I assert is, that this paper gives no real information about it. WM. MUIR

133, Upper Thames Street, E.C., April 26

The Migration of Birds

IT was because Prof. Newton mentioned such distances as six, seven, and ten miles (*vide* NATURE, vol. xix. p. 434), in connection with the flight of migratory birds, that I brought forward the matter of temperature, and the latter still appears to me to have as much bearing on the question, as has the density of the atmosphere.

The intense frost on Christmas eve, 1861, was said to have killed a large number of thrushes, blackbirds, &c., in Scotland. Near Edinburgh, where the thermometer registered about − 4° F. during the night, many dead birds were found. These deaths resulted from cold, not from starvation, for the weather was open until within a few days of Christmas day. Now, if a frost of this severity has such an effect on bird-life, surely it must be conceded that temperatures from − 25° to − 100° F.—those that would reign between six and ten miles' elevation, with surface temperature of + 80° F.—would slay the hardiest migrant.

There is a great difference between the elevation required to view a distant sea horizon, and an equally distant mountain-top. For instance, to obtain a sea-horizon of 300 miles, you must mount nearly twelve miles; but from an altitude of four miles, the summit of a mountain 20,000 feet high (less than 4 miles) would be visible, though its base lay 300 miles off. Similarly, if an elevation of 5,000 feet only be granted to the haze that constitutes the loom of land, birds flying two miles high will have a circle of vision, for the land indication, of over 200 miles radius. Under such circumstances, if the journey is 1,000 miles in length, a deviation of some 12° on either side of the true direction of flight can be made by migrating birds, without leading them out of view of their destination. With shorter journeys it is evident the error of flight may be largely increased without endangering the safety of the migrants.

Migratory birds that are strictly nocturnal cannot cross any very great expanse of barren ocean, hence, unless their error of flight is large, and the land they wing their way to small, there is not much fear of their losing themselves. Moreover, if they do go wrong, dawn must assuredly bring back their powers of vision.

E. H. PRINGLE

Beckenham, April 27

An Observatory of Newton's?

THERE is a tradition associated with a domed building, now covered with ivy, situate on Stamford Hill, that it was once employed as an observatory by Sir Isaac Newton. Can any of your readers give any information upon the subject? Immediately beneath the revolving dome there is a well-shaped excavation (now partially filled with water) in which is an extinguisher-shaped stand, supposed to be of iron; this may have formed part of the base of a telescope, but no information upon the subject can be obtained from the local inhabitants.

CHARLES COPPOCK

Grosvenor Road, Highbury New Park, N., April 23

Waterton's Wanderings—Goat-suckers

ONE would like further information respecting the "nocturnal flies" which settle on the udders of cows or goats, and may be seen on moonlight nights. Many lepidoptera and coleoptera and a few hymenoptera are nocturnal, but are not known to adopt the habit described. Of the true flies, diptera, are any nocturnal?

HENRY H. HIGGINS

A STATUE TO CAPTAIN COOK

THE Australians have found a hero worthy of their worship, and Capt. Cook has at length found an English-speaking people eager to take occasion to honour the memory and the work of one of the greatest of Englishmen. The mystery of the reticence of our wealthy but unwieldy Geographical Society on the occurrence of the centenary of Cook's death, still remains unsolved; they did not even send a representative to Paris, to the amazement of the enthusiastic French geographers; was the weather too rough for the gallant admiral who we believe volunteered to the indifferent Council to go to the Paris meeting? We are glad for the credit of the nation that it has not been left entirely to the foreigner to recognise the greatness of one of England's greatest navigators and discoverers. Our readers may remember that some time since a statue of Cook adorned Waterloo Place, near the Athenæum Club. The statue was admitted to have been exceedingly happy in conception, and successful in execution; it is supposed to represent the great navigator coming within the loom of the east Australian coast, which he first saw near Cape Howe, to the south of Sydney. It was for this city that the statue was designed, and it was to inaugurate the work of Mr. Woolner, that on February 25 last one of the greatest demonstrations took place that has been witnessed in Australia since the first shipload of convicts was landed at Botany Bay. When we said that Australia had found a hero, perhaps we spoke too widely, for only New South Wales as represented by Sydney, seems to have joined in the demonstration to commemorate the centenary of Cook's tragic end and the unveiling of his statue. It seems to us a great thing for a people to have a worthy national hero, and since the days when Abraham begat Isaac, and probably long before, every nation of any note has had its hero or demigod in whom all the national virtues have been embodied. The Australians have the making of a great people among them, and while they have a right to count our gods as theirs, still no doubt they would like to have a Hengist of their own to mark a new starting-point in their history. Happily, as we have said, they have found a worthy one—one whose character is in every respect worthy of their admiration, and the principles of whose conduct, if adopted and acted upon, will help to make of them a really great people. However desirable we may think the federation of our Australian colonies to be, any advocacy of it in these pages would be out of place. Still we cannot but think that it would have been a good thing in many ways—a good thing for the colonies themselves, and conducive to cordiality among them—had they all united to do honour to one so worthy of honour in all respects, and to whom, in a sense, they are indebted for their very existence.

Nothing could have been more successful than the gathering in Sydney on February 25, to assist at the unveiling of the statue by Sir Hercules Robinson. It was a universal holiday. Probably there were not much less than 100,000 people gathered in and around Hyde Park at the time of the opening ceremony—people of all classes who had voluntarily given up their work or business for the day, apparently, to a large extent, from genuine enthusiasm towards the man who first landed near the site of what in a few years has become one of the finest cities in the world. The statue seems to have given universal satisfaction, and the enthusiasm reached its height when Sir Hercules Robinson unveiled it at the conclusion of a solid and suitable speech. In his address the Governor traced in a sympathetic manner the career of the hero whom they had gathered to honour, from his birth as a peasant's son, till his unfortunate murder at Hawaii. Sir Hercules does not, however, seem to be well up in the latest evidence with regard to Cook's death, and seems, as of old, to have attributed it to mere savagery, whereas it seems pretty clearly ascertained that it was a blunder on the part of the poor natives. We have so recently written on the character and work of Cook, that it is unnecessary again to go over the same ground. Sir Hercules very happily, we think, read the moral of Cook's life to the people of Sydney. He was a man who eagerly pursued knowledge as his scanty opportunities afforded: who valued science, and endeavoured to do all his work by its light and guidance; who treated those under his command with the greatest consideration, and exercised the utmost tenderness and humanity towards the natives of the various islands with which he had any dealings. "Such a statue is creditable to ourselves," Sir Hercules justly concluded, "as marking our admiration of the character and services of the man, and our gratitude for the benefits which his discoveries have conferred, not only on Australia, but also on the world at large. . . . There is scarcely a lad born in this country who has not within his reach educational advatages superior to those which were available to the poor Yorkshire peasant boy, and I hope that the history of his early life may not be thrown away upon the young, but that many a child in the future will learn at the foot of this statue how a faithful, patient, cheerful attention to the details of daily duty, however monotonous and commonplace, will bring its own reward, and may perchance, as in the case of James Cook, leave behind a noble and imperishable memory."

While we regard it as right and proper that this fine statue should have been erected in Sydney to Cook, we think, moreover, the people of New South Wales would only be carrying out the work of Cook if they took some step to obtain a more thorough knowledge of these Pacific islands and seas, for a knowledge of which Cook did so much. We recently referred to the lecture given them by Dr. Miclucho Maclay on the want of a zoological station at Sydney; and we would suggest that the people of Sydney, helped by the other Australian cities, should carry out the work they have so well begun, by founding an institution, that under proper guidance would add immensely to our knowledge of the life of these interesting waters. Meanwhile let us be thankful that they have done something to redeem the race to which Cook belonged from the charge of insensibility to his greatness.

THERMO-CHEMICAL INVESTIGATION

THE introduction of a new method of research, or the invention of a new instrument, has repeatedly marked an epoch in the development of more than one branch of natural science. The last few years have witnessed the introduction into chemical research of a new method of examining chemical changes, a method which is founded upon the study of those thermal reactions which accompany these changes.

The older methods of chemical investigation failed to throw any definite light upon many important problems, some at least of which have been brought a step nearer complete solution by the application of the newer method of thermo-chemical measurement.

When solutions of two salts are mixed, the products of the mutual action of which salts remain in solution under the experimental conditions, it is frequently found impossible to determine, by means of the ordinary analytical processes, the chemical distribution of the mass of reacting matter at the expiry of the experiment.

Again, there are certain acids which undoubtedly form two series of well-marked salts, but which appear to be capable, under certain ill-defined conditions, of forming a third series of unstable saline derivatives. How to determine the basicity of such acids has long been one of the unsolved problems of chemistry.

Once more, the ordinary methods of investigation have failed to supply us with any far-reaching generalisation concerning the stabilities of series of compounds. Certain relations have undoubtedly been traced between general chemical properties of compounds, the properties of their constituent elements, and the stability of these compounds, but, nevertheless, the shadowing forth of well-marked generalisations, connecting stability of compounds with chemical structure, from which generalisations exact deductions, capable of experimental investigation, might be made, dates from the introduction of the thermo-chemical method of investigation.

That system of notation which is now employed in chemistry, although of the greatest value, is nevertheless far from being perfect; it fails to tell anything concerning the changes in forms of energy involved in those changes of distribution of mass which it formulates. Previous to the introduction of the thermo-chemical method little or no exact knowledge regarding these changes of energy was in the possession of chemists.

Chemists were long aware that certain reactions were possible only under stated conditions of temperature, pressure, &c., but until measurements had been made of the amounts of heat evolved or absorbed in these reactions they were unable to generalise the connection between the conditions of the reactions and the possibility of their occurrence.

Such are some of the problems which have been at least partially solved by the new method.

The fundamental position of thermal chemistry may be thus stated: "Every chemical change taking place without the aid of extraneous forces tends to produce that body, or system, in the formation of which the greatest evolution of heat occurs."

As a deduction from this statement Berthelot formulates his law of maximum work as follows:—"That salt, the formation of which is attended with the greatest evolution of heat, is always produced when those salts, from whose mutual action it may be formed, exist in solution in a condition of partial decomposition."

Many special instances illustrative of these generalisations might be cited; let one or two suffice. Chlorine decomposes dry sulphuretted hydrogen with formation of hydrochloric acid and separation of sulphur; iodine does not decompose sulphuretted hydrogen under the same conditions. The formation of hydrochloric acid and sulphur in the first change is accompanied with the evolution of a considerable quantity of heat; the formation of hydriodic acid and sulphur, in the second case, would involve the absorption of much heat. If, however, the action of extraneous forces be allowed to supervene, a new condition of equilibrium is attained; add water to sulphuretted hydrogen and iodine, hydriodic acid and sulphur are produced. But the solution in water of hydriodic acid, which is the potential product of the reaction, involves the evolution of more heat than is absorbed in the reaction itself.

Iodine scarcely decomposes water, but if sulphurous acid be added to water, iodine is capable of bringing about decomposition, the products of the reaction being hydriodic and sulphuric acids

$$(H_2O + I_2 + H_2SO_3 = H_2SO_4 + 2HI).$$

Now it is found that the formation of sulphuric from sulphurous acid is accompanied with the evolution of a considerable amount of heat; if, then, the decomposition formulated $2H_2O + 2I_2 = 4HI + O_2$ be started, the combination of the oxygen thus produced with the sulphurous acid present causes the evolution of more heat than would be evolved in any other series of chemical changes which could occur among the bodies present.

The applications of the thermal method in general chemistry are many and important. I propose briefly to consider some of the results obtained by this method, as shown in the phenomena attending the neutralisation of acids; in the changes which occur on mixing solutions of two salts which are capable of undergoing decomposition with the production of salts themselves soluble under the conditions of experiment; in the measurements of (so-called) affinities between elementary bodies; and in one or two other reactions of general interest.

The neutralisation of an acid by an alkali is attended with the evolution of a constant amount of heat; in some cases it is noticed that the total amount of heat evolved is independent of the relative quantities of acid and alkali employed, while in other cases the total heat evolution may be divided into two equal portions, one half of the whole accompanying the addition of the first portion, and one-half accompanying the addition of the second portion of alkali. Those results evidently point to the exhaustion of the available energy of the acid (or alkali) as a phenomenon which takes place in regular stages. The thermal results of neutralisation phenomena are rendered more intelligible when we find that an acid, the neutralisation of which is accompanied with the evolution of but one quantity of heat, is also a monobasic acid; while in the case of a dibasic acid the total amount of heat evolved on neutralisation with alkali is divisible into two distinct portions. Further, a difference is traceable between the thermal phenomena which attend the neutralisation of an acid by caustic potash or soda, on the one hand, and by ammonia on the other.

The reaction formulated

$$2KHO + H_2SO_4 = K_2SO_4 + 2H_2O,$$

involves the expenditure of 31,000 thermal units; but the reaction2 $NH_3 + H_2SO_4 = (NH_4)_2SO_4$ is attended with the expenditure of but 28,150 thermal units

If, however, a compound more strictly comparable with caustic potash in its chemical structure be employed to neutralise sulphuric acid, we find that the heat evolved is equal in both cases; the reaction

$$2N(CH_3)_4OH + H_2SO_4 = (N(CH_3)_4)_2SO_4 + 2H_2O,$$

is attended with the evolution of 31,300 thermal units.

From the point of view of their thermal reactions, the alkalis (including thallium hydroxide) and the alkaline earths, are strictly equivalent, so far as the power of neutralising one and the same amount of sulphuric acid is concerned.

The effect of substituting various compound radicles for the hydrogen of ammonia, is well shown in the phenomena attending the neutralisation of acid by ammonia, and by those substituted products. The introduction of a C_nH_{2n+1} group (C_2H_5, CH_3, &c.) into the ammonia molecule produces a substituted ammonia, the heat of neutralisation of which is the same as that of the parent body; but if a negative radicle (such as C_6H_5) be substituted for hydrogen, then a compound is produced in the neutralisation of which less heat is evolved than in the neutralisation of the parent body. Thus the neutralisation of hydrochloric acid by ammonia is accompanied with the evolution of 24,540 units of heat, while the neutralisation of the same acid by aniline ($NH_2C_6H_5$) is accompanied with the evolution of only 15,000 to 16,000 thermal units.

When solutions of two salts are mixed under conditions such that the products of their mutual action remain in solution, thermal measurements throw very considerable light on the progress of the chemical change.

The problem presented by the phemonenon now under consideration is one of those which are peculiarly difficult of attack by the older methods. If a third body were introduced into the mixture of salts, which should combine with, or render insoluble, one or more of the possible products of the action, a new configuration would be initiated, new chemical changes would probably occur, and we should be unable to say whether the results obtained were really trustworthy representations of the action which had taken place between the members of the original system.

But measurement of thermal changes involves no disturbance of the equilibrium of the reacting chemical system, and at the same time it yields trustworthy information regarding the changes which have occurred in the distribution of the mass of matter comprising that system. To take an example :—On adding a solution of potassium chloride to dilute hydrochloric acid no thermal change is noticed; on adding a solution of potassium sulphate to dilute sulphuric acid heat is absorbed, the amount of heat so absorbed increasing with the amount of acid added, until a limiting point is reached. If the solution of potassium sulphate be made more and more dilute less and less heat is absorbed. Now these facts evidently point to the occurrence of two processes of chemical change in the above reaction, viz., the direct action, formulated $H_2SO_4 + K_2SO_4 = 2KHSO_4$; and the inverse action, formulated

$$2KHSO_4 + H_2O = K_2SO_4 + H_2SO_4 + xH_2O$$

We are thus taught to regard this chemical change as dependent on the conditions of the experiment, and further we obtain a glimpse of the decompositions and recompositions which are continuously occurring among the molecules of our seemingly stable compounds.

If solutions of zinc acetate and sodium sulphate be mixed no thermal change is noticeable, but if solutions of zinc sulphate and sodium acetate be mixed, an evolution of heat occurs, that is to say, a chemical change (or a series of chemical changes) proceeds. Such an experiment as this, besides throwing light upon the special chemical change under consideration, leads to a clearer conception of those phrases "strong acid," "weak base," than were generally to be found before the introduction of the thermal method into chemistry. A strong acid is evidently an acid in the formation of the salts of which much heat is evolved, and a weak acid is one in the formation of whose salts little heat is evolved, or heat is absorbed. If therefore the heats of neutralisation of two acids by given bases be known, it may become possible to predict what chemical changes will occur when given salts of those acids are mixed.

Attempts have been made from time to time to measure the so-called affinities of the elementary atoms. These attempts have been considerably advanced, and the whole problem of affinity has been much defined by applying the results of thermal measurements to chemical reactions.

If chlorine be mixed with hydrogen, and the mixture be exposed to daylight, hydrochloric acid is produced with evolution of a large amount of heat; the formation of hydrobromic acid from its elements is accompanied with the development of less heat, while heat is absorbed in the formation of hydriodic acid from its elements. These thermal reactions show that more energy changes form in the first than in the second, and more in the second than in the third of these reactions. The amount of energy of motion which is convertible into thermal energy, under fixed conditions, seems, therefore, to measure the mutual affinities of chemical elements.

But we do not know what is the amount of energy spent in decomposing the molecules of hydrogen and chlorine; the heat developed in the reaction

$$2H + 2Cl = 2HCl$$

is therefore the sum of the plus and minus thermal changes during the cycle of chemical changes, the initial and final stages of which are chlorine and hydrogen molecules and hydrochloric acid molecules respectively. Therefore it is evident that thermal measurements do not give data which suffice for determining the absolute affinities of the elements.

If the elements comprised in a natural group be converted into similar compounds—say into oxides—and if that element in the formation of whose oxide the greatest amount of heat is developed be said to have the greatest affinity for oxygen, many remarkable relations may be shown to exist between the affinities and the atomic weights of the elements in such a series. Thus Thomsen has shown that in the group comprising magnesium, calcium, strontium, barium, the affinity for chlorine, bromine and iodine increases with increase of atomic weight, while the affinity of the haloid compounds of these elements for water decreases as the atomic weight of the elements increases. Many more exceedingly interesting results are brought out by Thomsen in the same paper.

The results of thermo-chemical investigation—a few of which I have endeavoured to sketch in thinnest outline—suggest one or two considerations regarding chemical action in general, and regarding some of those problems which yet remain to be solved by chemical science.

The older theory of chemical action is based upon the idea that the reacting bodies exert force upon one another; the word affinity has thus a positive meaning.

Recently the view has gained ground, with some chemists, that a chemical change is but the outward representation of a loss of energy occurring within the reacting system; that no positive force is exerted between the reacting molecules, but that the system, as it were, falls to pieces because the conditions are realised under which a loss of energy is possible.

The latter view, I think, fails to account for the facts; there is no doubt that it expresses a truth, but surely only a partial truth.

General considerations, no less than those derived from thermal measurements, compel us to regard the first action between two elementary molecules as consisting in a decomposition of those molecules with the production of their constituent atoms, which afterwards combine with the formation of new molecules. But the decomposition of elementary molecules involves the expenditure of energy; in other words, there is a mutual action and reaction between these molecules. If this stress be regarded from the point of view of one set of the reacting molecules only, we certainly have positive force exerted.

It is not a mere negative loss of energy, but a positive action of one kind of molecules upon another kind of molecules; and the amount of force exerted is different for different elementary molecules. Hence chemical affinity is a positive force. The mutual action and reaction between the molecular systems involves the loss (or gain) of energy, but this loss of energy does not furnish a complete account of the action.

Thermal measurements enable us to determine the quantity of energy entering or leaving a given chemical system during its passage from one state to another. These measurements, therefore, give us most valuable information concerning the phenomena exhibited by those chemical systems.

The results obtained by these measurements show how great is our ignorance with regard to the progress of chemical reactions in general; and they suggest many exceedingly interesting problems which will doubtless ere long meet with satisfactory solution. The great problem of chemistry is to determine the connection between the structure and the properties of molecules. To take a special case, it may be asked, why is the hydrogen of acids replaceable by metals under definite conditions? Many facts are known which enable us to give partial answers to this question; doubtless, thermal investigation, taken in conjunction with other methods of research, will some day furnish the complete answer.

Thermal measurements have already shown us that allotropic changes in elementary molecules are accompanied with changes in the energy of these molecules and that the same generalisation holds good with regard to isomeric changes among compound molecules. But the whole question of allotropy is yet in its infancy.

The thermal method promises to throw light upon those phenomena which are classed together under the name of valency, and perhaps to furnish an answer to the query, *why* does the valency of elementary atoms vary? The new method is full of hopeful anticipations.

M. M. PATTISON MUIR

ARE THERE NO EOCENE FLORAS IN THE ARCTIC REGIONS?

IN NATURE (vol. xix. p. 124) I expressed doubt whether the beds containing fossil plants in or near the Arctic circle, said by Heer to be miocene, are really of that age. It seemed to me then very probable, but now I may say certain, that at least all those said to be *lower* miocene are truly eocene. The article was translated in *Das Ausland*, No. 2, 1879, and replied to by Heer in No. 8 (February 24) of the same journal. In this reply he, as I expected, combats my views, and, although affecting to believe that I had written without thought or previous study, he devotes eight columns to contradicting me, yet without bringing forward any fresh evidence whatever, or indicating any sources of information which I had not already consulted.

Heer contends that all the known fossil floras containing dicotyledons, from all lands within at least 2,000 geographical miles of the Pole, are either cretaceous or miocene. I think, on the contrary, many of them are eocene.

The leading facts for and against the hypothesis of a miocene age for so large a proportion of them may be briefly summarised.

1. The great similarity of the floras (miocene of Heer) of latitude 70° to those of 47° and 46°, 98 species out of 363, or more than 25 per cent. being common to both, even in the present state of our knowledge. This, according to existing plant-distribution, precludes their being of the same age, unless the more southern ones grew in Alpine or even hilly regions; but no one has ever contended that they did do so. No floras so much alike, and assimilating so closely to those of the present day, could have grown simultaneously at the same level in such widely different latitudes.

Against this Heer states that a number of trees extend from the borders of Italy to the 70th parallel, as the firs, birches, aspens, bird-cherry, and mountain-ash. This fact has little bearing on the subject, since the trees are Alpine, or, at least, not in any way characteristic of the lowland flora of North Italy or of that latitude in Europe. Secondly, he says that of the fifty-nine phanerogams found by Feilden in Grinnell Land between 81° 44′ and 83°, forty-five are European, and six of these are not only found in Swiss valleys, but also in Italy. This should not have been advanced, being quite beside the question, unless he wishes to make believe that the present floras of Grinnell Land and Italy resemble each other. They are, in fact, all Alpine herbaceous plants, and have nothing to do with the fossil *forest* floras in question; besides which, the level of the Swiss valleys in which these six grow is not stated, and there is nothing curious in Alpines ranging into Italy. Thirdly, of 559 species of phanerogams of the Isle of Saghalien, 188 are found in Switzerland. Such occasional examples of wide lateral distribution among plants are well known, and might often be adduced, without affecting the question in the remotest degree. The present distribution of the *same* types of plants, trees, &c., as those which are found fossil, have alone any bearing on the subject. Heer, to sustain his theory, must prove that forest floras extend in some other parts of the world with a much less degree of change than we have experience of in our continent, over not less than 30° of latitude, and in about the same longitude.

2. The extreme improbability that the plant remains of the eocene, a far more important formation than the miocene, should have been alone overlooked in a series of deposits abounding in plants of immense extent and thickness, and continuous, it is supposed, from the middle cretaceous to the upper miocene. The absence of any intelligent explanation of the complete break in the sequence, which Heer's nomenclature implies, and of which there is not the least stratigraphical evidence. The vastness and immense extent of the formations which are ascribed to miocene. The universally admitted fact that continuous land existed in the north between Europe and America from early eocene times, as proved by the palæontological records of both continents, and supported by other considerations, and which must have left records at least in proportion to those of the miocene, since volcanic, the preserving agency, was active throughout the whole time.

Heer characteristically meets these important objections by stating that at Eisfiord, in Spitzbergen, there are 1,000 feet of strata between the cretaceous and miocene, which he thinks doubtless represent the eocene. It is strange to find any one with the least knowledge of stratigraphical geology simple enough to advance such evidence as the presence of 1,000 feet of beds at a single spot, in dealing with so colossal an interval as that between the cretaceous and miocene, especially when the latter alone, over the area, is several thousands of feet in thickness. Besides Nordenskjöld,[1] from whom Heer derives his information, says that the miocene (of Heer) habitually rests upon the cretaceous.

Heer further says that there is a deposit with lower miocene mollusca under a miocene deposit. This is exactly what I should expect; for the same reasons that make it improbable that the flora is miocene apply equally to these mollusca.

3. The much higher temperatures which prevailed in the eocene than in the miocene, and which could only have permitted the growth of such temperate floras in such high latitudes in the eocene period, according to existing laws of plant distribution.

Although I showed *seriatim* that a mean temperature higher by 20° F. in the northern hemisphere would inevitably have produced approximately just the series of

[1] Excursion to Greenland, *Geol. Mag.*, vol ix.

eocene floras that are met with in England, Iceland, Greenland, Spitzbergen, and Grinnell Land, and that from Heer's miocene standpoint no uniform increase could do so, his eight columns of reply do not embrace this question.

4. The total absence of any characters among the plants themselves, which would preclude their being considered eocene.

To this I must also await an answer until eocene floras are better understood. Heer's reply contains none.

It is obvious that if he has no more to say than this, the balance of the evidence, even as it stands, is already actually against him. But it is far more conclusive than I have represented it to be in the above summary.

We are told to believe that enormous deposits, many thousand feet in thickness, vast in extent, and resting everywhere conformably on the latest cretaceous beds, and indeed stratigraphically indistinguishable from them, are not as we should expect, in greater part at least—the next succeeding older tertiaries, but *the miocene*. We are not to question the reality of the marvellous gap thus created; not to point out that climatic considerations are entirely against the miocene age of the beds; not even to suggest that the plant evidence relied upon quite fails to support it; for Heer, like an infallible Pontiff, has, *on plant evidence*, pronounced them miocene.

He has tried to excommunicate me in his concluding paragraph, of which the following is but a feeble translation:—"The incorrect assertions and conclusions of Mr. Gardner proceed from want of knowledge or disregard of well-ascertained and solid facts, and it is much to be desired that those who occupy themselves with such difficult questions should first acquaint themselves with the facts before they express upon them such positive opinions."

I, however, to use a quotation, do not feel "one penny the worse."

The miocene hypothesis, which is not a scientific one, and would have been gladly overturned by Belt, rests entirely upon Heer's interpretation of the plants. I have therefore, I presume, but to show how completely unreliable in this case Heer's interpretation is, to break the spell of infallibility attaching to his work and to reopen the question for solution by scientific thought—"the application of past experience to new circumstances, by means of an observed order of events," as Clifford put it. In the first place, what are the "well-ascertained and solid facts" of Heer? I have looked at the Bovey Tracey beds formerly described, and erroneously, as miocene by Heer. Taking the ferns, with which I am just now most familiar, I find a form described as *Pecopteris lignitum*, and this species was at the time no doubt a "solid fact;" but I subsequently find Heer describes this *same* fern as *Aspidium lignitum*[1] and, extraordinary to relate, as *Dryandra rigida*.[2] Are these solid facts? Because he how speaks of the species as an Osmunda. I might analyse Heer's "solid facts" to a considerable extent, but refrain from doing so until the proper time arrives, in the pages of the Palæontographical Society. In the meantime I cannot but consider that his caution might more justly be applied to himself; for whilst I, at least, have had access to all Heer's published facts, I expressly stated that those I chiefly relied upon were unpublished.[3] I therefore marvel that he should have written so positively on so difficult a question without first, at least, endeavouring to acquaint himself with the latest facts.

Heer either does not possess, it appears, the knowledge requisite to separate stages of the eocene from the miocene, or he misapplies it. Of all the floras he has described but one is for him, eocene, and about this he expresses the greatest doubt. This single "great work"[1] on the eocene, as he calls it, was no larger than could be amply illustrated in ten not over-crowded plates, for I find the same species doing duty on more than one under different names. Beyond this he only claims to have studied the flora of Monte-Bolca, although he has published nothing upon it, and to have seen "many plants of the English eocene." Of the Monte-Bolca flora I can say little, as when I have been to Verona, where, I believe, large collections exist, the curator has been absent; but of the latter I can say that Heer's "many" must be used in a limited sense, for when he visited England, before either Mitchell or myself had commenced collecting, the collections open to him were meagre indeed.

Although, however, Heer modestly claims to have described but one eocene flora, I believe credit is due to him for describing several. Among these the most familiar to us is that of Bovey Tracey, lithologically and palæontologically precisely resembling some of the middle eocene beds of Bournemouth, only eighty miles distant from it.[2] Heer may, of course, deny their eocene age, and I cannot convince him by letting him see the specimens, as I did Ettingshausen, who, after being shown leaves, fruits, seeds, and spines, said the matter must be considered doubtful unless I could produce *Sequoia Couttsiæ* from Bournemouth. This, on looking through the cabinet of conifers, we found in abundance, not only from Bournemouth, but also from Alum Bay. This is but one instance selected from near home. If we look at Heer's tables in the third volume of his "Flora Tertiaria," we see that all the floras of France, Germany, Austria, Italy, and Switzerland are called miocene. The floras of Sotzka, Häring, Monte Promina, &c., although eocene to those who described them, are not so to Heer. He, in fact, persistently misrepresents the relative importance of the eocene and miocene formations, which he has always reversed, almost ignoring, indeed, the existence of the far more important of the two. Fortunately, accident has given to me what it has denied to Heer after a life of study, that is, access to large series of undoubted middle eocene plants; for my own collection, from Bournemouth alone, cannot number less than 10,000 selected specimens. These plants reveal how closely many of Heer's so-called lower miocene floras assimilate to the eocene, to which age they doubtless belong, and that forms thought to be characteristic of the former are really only met with in the latter, and that other species, ranging through both, are misleading and negative, so far as affording evidence upon this question. Of course Heer could not be acquainted with the unpublished English floras, and unfortunately their publication must be a work of time; but why, for example, in opposition to Unger and Ettingshausen, did he maintain the Sotzka, Häring, and Monte Promina floras to be miocene.

"When next you view,
Think others see as well as you."

is the moral of a fable with which Heer seems unacquainted.

I know that in very many cases what is lower miocene to Heer, is lower or middle eocene to me, and that therefore his *lower miocene* floras are practically and truly my *middle* or at latest *upper eocene* floras. There is thus a great difference of opinion between us, for the one nomenclature often implies immense gaps, which the other fills up.

While Heer's opinions of the ages of his localised floras are mostly based upon the evidence of the plants themselves, and the beds in which they are found contain little or no internal evidence, apart from this, of the formations to which they belong—those upon which I am

[1] "Sächsisch-thüringischen Braunkohlenflora," 1861, pl. ix. f. 2.
[2] *L.c.*, pl. x. f. 15.
[3] In course of publication by the Palæontographical Society.

[1] "Der sächsisch-thüringischen Braunkohlenflora," Berlin, 1861.
[2] *Geol. Mag.*, April, 1879.

at work are upon stratigraphical evidence *certainly* of the ages to which they are ascribed.

We have a limited Thanet sand flora; a considerable insight into the Woolwich and Reading Beds flora, obtained from Dulwich, Reading, Newhaven; an Oldhaven flora from Bromley; an extensive London clay flora from Sheppey; a Lower Bagshot flora from Alum Bay, Studland, and Corfe; a Middle Bagshot flora from Bournemouth and Bovey Tracey; upper eocene floras from Hordwell, Gurnet Bay, &c. All these will be embraced in the monograph now in course of publication by Ettingshausen and myself.

The nearly unbroken sequence seen in the eocene floras extends into the miocene. There is no great break in passing from one to the other when we compare them over many latitudes, and but little change, beyond that brought about by altered temperature or migration. If tertiary floras of different ages are met with in one area, great changes on the contrary are seen, and these are mainly due to progressive changes in climate. From middle eocene to miocene the heat imperceptibly diminished. Very gradually the tropical members of the flora disappeared; that is to say, they migrated, for most of their types, I think, actually survive at the present day, many but very slightly altered. Then the sub-tropical members decreased, and the temperate forms, never quite absent even in the middle eocenes, preponderated. As decreasing temperature drove the tropical forms south, the more northern must have pressed closely upon them. The northern eocene, or the temperate floras of that period, must have pushed, from their home in the far north, more and more south as climates chilled, and at last in the miocene time, occupied our latitudes. The relative preponderance of these elements, I believe, will assist in determining the age of tertiary deposits in Europe, more than any minute comparisons of species. Thus it is useless to seek in the Arctic regions for eocene floras, as we know them in our latitudes, for during the tertiary period, the climatic conditions of the earth did not permit their growth there. Arctic fossil floras of temperate and therefore to Heer miocene aspect, are in all probability of eocene age, and what has been recognised in them as a newer or miocene facies, is due to their having been first studied in Europe, in latitudes which only became fitted for them in miocene times.

When stratigraphical evidence is silent or inconclusive, this unexpected persistence and migration of plant-types or species throughout the tertiaries, should be remembered, and the degrees of latitude in which they are found should be well considered before conclusions are published respecting their age.

I need not here point out how completely this theory accords with that of the dispersion or migration of species from a northerly centre, so ably treated of by Asa Gray, Dawson, Dyer, Saporta, Hooker, and in fact by all who have pondered upon the subject, excepting Heer, for I hope to write a few words upon this at a future time. Before quitting it, for the present, Heer may as well learn that I am not alone in my opinions, for Prof. J. W. Dawson, of Montreal, considers with me that the reference of the beds in Greenland to miocene is not warranted by comparison with the tertiary plants of America.

"Immediately above these upper cretaceous beds we have the great lignite tertiary of the west—the Laramie group of recent American reports—abounding in fossil plants, at one time regarded as miocene, but now known to be lower eocene, though extending upward toward the miocene age. These beds, with their characteristic plants, have been traced into the British territory north of the 49th parallel, and it has been shown that their fossils are identical with those of the McKenzie River Valley, described by Heer as miocene, and probably also with those of Alaska, referred to the same age. Now this truly eocene flora of the temperate and northern parts of America has so many species in common with that called miocene in Greenland, that its identity can scarcely be doubted. These facts have led to scepticism as to the miocene age of the upper plant-bearing beds of Greenland, and more especially Mr. J. Starkie Gardner has ably argued, from comparison with the eocene flora of England and other considerations that they are really of that earlier date." [1]

Private correspondence has already informed me that others now share in these views.

Not content with withering my theories as to the eocene age of part of his miocene Arctic floras, Heer tilts against my explanation of the former higher temperatures which are known to have prevailed in our own and more northern latitudes. My explanation is, however, justified by our experience of what we conceive to be natural laws, and does not contradict that experience, and Heer has no theory to set up in its place.

The differences in the temperatures of the seas washing Arctic lands in the same latitudes are seen to alter the isothermal temperatures of their coasts to the extent of 27°; that is to say, the coasts which are refrigerated by the descending ice-laden currents are 27° colder than the shores of the North Cape, which are washed by an ascending current. With this fact and its causes palpably before us, we are justified in inferring that if the cold currents were shut off from these coasts, their temperature would rise by some 27°. The cold currents *were* shut off in the eocene time, for plants and animals passed freely between Europe and America, and therefore the temperature of the northern eocene lands may have been *from this cause* some 27° higher. But the Arctic eocene floras only required about 20° higher temperature, and the cause invoked is therefore more than sufficient.

Heer agrees with me that the higher temperature at the North Cape is due to warmer sea, and that continents extending far south also have their influence. He objects that Spitzbergen, being within the influence of the Gulf Stream, has a temperature of only 7° above the mean of its latitude. But then Spitzbergen is not shut in by the Gulf Stream, but only washed along one shore by it, and that after its current had been enfeebled and refrigerated to the last degree by the icy water it has to press through. Yet slight as the cause then is, it raises the isotherm of Spitzbergen 7°. He again objects that the closing of these outlets would stop the flow of the Gulf Stream. This, however, would not be the case completely. As long as any difference existed between the temperature to the north, and that under the tropics, a circulation would continue and would only cease when the whole Atlantic, north of the Equator, had reached a uniform heat. Not streams only, but the whole Atlantic from the Equator northwards, would be enormously warmed, and even parts of continents most remote from seas, would feel the influence.

This theory if true, Heer says, is at all events not original. In that case, so much the more likely to be true, but it is original to me.[2] It is true that very many theories have been put forward to account for former temperatures, and some of these have been based upon altered distributions of land and sea. But while some required change in the level of the sea, and others involved entirely novel continental areas, none have been supported by any kind of proof, either that the supposed changes had actually taken place, or were even competent to account for the former temperatures. The theory I have ventured to put forward is only absurd in its simplicity. The Atlantic may be likened to a great bath heated by the sun, from which we may shut off the cold taps either partially or entirely, from one or from both ends, thereby producing

[1] "The Genesis and Migrations of Plants," by J. W. Dawson. The *Princeton Review*, 1879, p. 282.
[2] NATURE, vol. xix. p. 123.

any known gradation of sea temperature. It not only accounts for the eocene heat when the land in the 70th and 80th parallels was upheaved by enormous volcanic action; the cooler miocene brought about when this action subsided, and permitted Arctic seas to again find egress; and the cold glacial period when both shores of the Atlantic were frozen by icy currents, as one shore is now; but by shutting off Antarctic currents it might have produced the hottest cretaceous times. Even the latter supposition is rendered likely by the past and present distribution of life, and such conditions doubtless did exist in remote times.

I am, however, speculating beyond the scope of my present paper, for, however the eocene climate was produced, the Arctic floras, I believe, flourished in it. Again I will close my paragraph with an extract from Dawson:[1] "But overlying this plant-bearing formation we have an oceanic limestone (the Niobrara) . . . . indicating that the land of the lower cretaceous was replaced by a vast Mediterranean Sea, filled with warm water from the equatorial currents, and not invaded by cold waters from the north. This is succeeded by thick upper cretaceous deposits. . . . these show that further subsidence or denudation in the north had opened a way for the Arctic currents, killing out the warm-water animals of the Niobrara group, and filling up the Mediterranean of that period." J. STARKIE GARDNER

[1] The *Princeton Review*, 1879, p. 282

AN ENGLISH MICROSCOPE FOR THE USE OF STUDENTS OF MINERALOGY AND PETROLOGY

IT may interest those who are studying petrology to know that a new microscope, specially suited for mineralogical and petrological research, has recently been constructed by Mr. T. W. Watson, of Pall Mall.

For several years past students have frequently asked me to recommend some microscope to them which would answer their requirements, and, finding that none of the cheaper instruments manufactured in this country were supplied with concentrically-rotating stages, bearing divided circles, and that even the high-class instruments failed to fulfil all the requirements, it appeared that this want might be supplied at a moderate cost, if one of our instrument-makers could be induced to make a few trials.

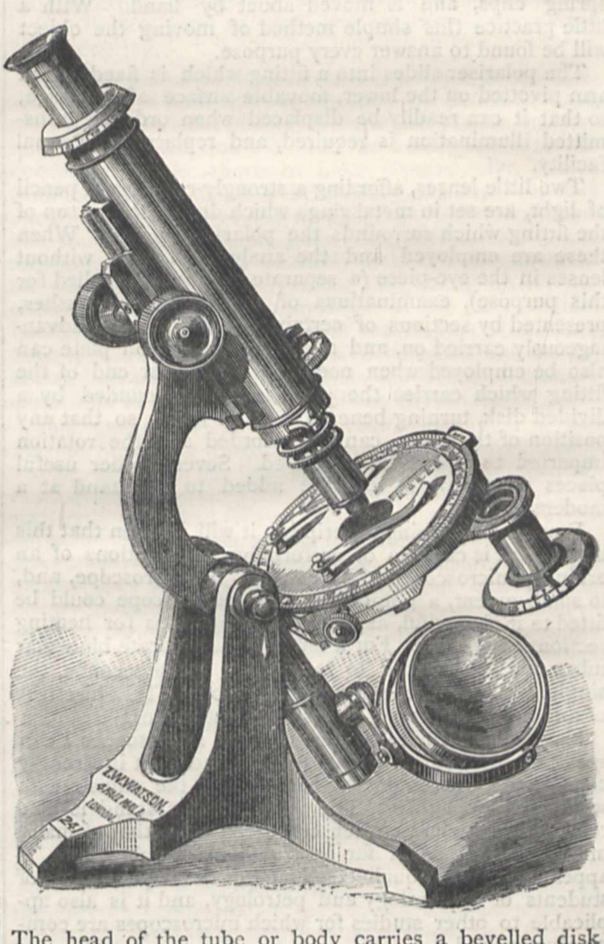
An examination of one of the microscopes devised by Prof. Rosenbusch and manufactured by Fuess, of Berlin, showed me that, although that instrument possessed many features of great merit, it also had certain defects which could be best overcome by adopting and modifying a good English model.

The great defects in most of the microscopes built on the continental patterns consist in their fixed vertical position, the smallness of their stages, and, very commonly, in the absence of any means of coarse adjustment, except by a sliding movement of the body or tube, which, if working stiffly, is very inconvenient, while, if sliding easily, is apt to be shifted by a very slight touch.

The microscope of Prof. Rosenbusch, apart from one or two of these defects, is a very admirable instrument, and presents various advantages over all other microscopes hitherto made.

The instrument, now manufactured by Mr. Watson, is in most respects quite equal in performance to Rosenbusch's microscope, so far as the mechanical appliances and adjustments are concerned, and is, I think, in point of convenience, decidedly superior to the latter instrument.

The foot is a brass casting of a pattern somewhat similas to that of Ross and other well-known makers. Upon this a gun-metal limb is supported on trunnions, which constitute the axis upon which the limb turns, so that the instrument can be inclined at any angle, or placed in a horizontal position for drawing. The right trunnion carries a clamp to fix the instrument at any angle. The lower portion of the limb bears the mirror, attached to a jointed arm. The upper part of the limb is bowed, or goose-necked, which renders it convenient as a handle, by which to lift the stand, without entailing any strain upon the working parts of the instrument. Above the curve it is ploughed out to receive the rack of the body or tube (on the pattern known as the "Jackson Model"), and the coarse adjustment is effected by a pinion turned by milled heads. The fine adjustment is of the usual kind, and is situated near the lower extremity of the tube. In the stand first made the milled head of the fine adjustment was divided for the measurement of the thickness of sections, but in future it is proposed to effect this object in a different manner by divisions engraved upon the limb and the sliding portion of the coarse adjustment.



The head of the tube or body carries a bevelled disk which is divided to 10° spaces. A corresponding disk with an index is attached to the bottom of the analyser-fitting, and rests directly upon the fixed divided disk; so that the analyser can be set in any required position, and any amount of revolution imparted to it can also be registered. The eye-piece, when inserted, is kept in a fixed position by a stud, which falls into a small slot. Crossed cobwebs are fixed within the eye-piece for the purpose of centring the instrument. A small plate of calc-spar, cut at right angles to the optical axis, is mounted in a little metal ring, which can be placed between the eye-glass and the analyser for stauroscopic examinations.

At the lower end of the microscope-tube a slot is cut to receive a Klein's quartz plate or a quarter-undulation plate, both of which are set in small brass mounts. When these are not in use the aperture can be closed by means of a revolving collar.

The thread which receives the objectives is of the gauge commonly used in this country, but an adapter can also be supplied which will carry the objectives of Hartnack and other continental opticians.

The stage is circular and capable of rotation, and it is divided on the margin to 360°. A vernier is attached to the front of the stage, giving readings to one minute. The edge of the stage is milled, and rotation is imparted by hand.

To insure concentric rotation with any powers used, two screws, carrying milled heads, are connected with the back of the stage. By the employment of these adjusting screws and the cobwebs in the eye-piece, a small object may readily be centred, so that it will revolve about a point central to the field afforded by any objective.

The object is held either by sliding clamps or by spring clips, and is moved about by hand. With a little practice this simple method of moving the object will be found to answer every purpose.

The polariser slides into a fitting which is fixed to an arm pivotted on the lower, movable surface of the stage, so that it can readily be displaced when ordinary transmitted illumination is required, and replaced with equal facility.

Two little lenses, affording a strongly-convergent pencil of light, are set in metal rings which drop into the top of the fitting which surrounds the polarising prism. When these are employed and the analyser is used, without lenses in the eye-piece (a separate fitting is supplied for this purpose), examinations of the rings and brushes, presented by sections of certain crystals, can be advantageously carried on, and a quarter-undulation plate can also be employed when needful. The lower end of the fitting which carries the polariser is surrounded by a divided disk, turning beneath a fixed index, so that any position of the prism can be recorded and the rotation imparted to it can be measured. Several other useful pieces of apparatus can be added to the stand at a moderate cost.

From the foregoing description it will be seen that this instrument is capable of performing the functions of an ordinary microscope, a polariscope, a stauroscope, and, to some extent, a goniometer. A spectroscope could be fitted to it if needful, as well as an apparatus for heating sections of crystals. For a few pounds separate binocular tubes can be supplied, to replace, in a few seconds, the single, but more generally useful, tube. The objectives of any maker can be used with the instrument.

Having carefully tested one of these microscopes I can speak most favourably of its performance. It is strongly constructed, convenient to handle, and the adjustments work very smoothly. The price of this stand is also remarkably moderate when compared with that of many microscope-stands of far less universal application. It appears to me well qualified to answer the requirements of students of mineralogy and petrology, and it is also applicable to other studies for which microscopes are commonly required.

Mr. Watson has taken especial pains to turn out a sound and serviceable instrument, and, after long experience of microscopes, I can confidently say that I have never seen one better suited for the work for which it is designed.

FRANK RUTLEY

STELLAR MAGNITUDES

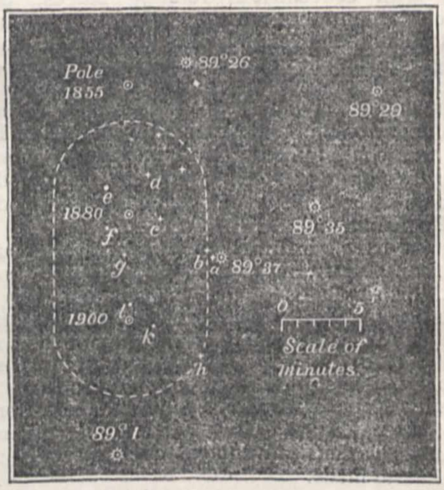
A REQUEST TO ASTRONOMERS

THE scales adopted by different observers in their estimates of stellar magnitudes differ considerably from each other, as is well known. As regards the brighter stars, these differences, indeed, are comparatively unimportant; but they become larger and more perplexing when the objects observed are faint. Variations of three or four magnitudes may be expected between the estimates made of the brightness of minute companions seen near a brilliant star. It is needless to point out the inconvenience of this state of affairs, which at times nearly deprives the estimated magnitudes, found in catalogues, of their meaning, and consequently of their value.

In the hope of providing a partial remedy for this defect, a series of photometric observations of stars of various magnitudes, situated near the north pole, has been undertaken at the Harvard College Observatory. The region has been selected as one which may always be conveniently observed in the northern hemisphere, so that the brightness of a star observed in another part of the sky can readily be compared by estimate with any standard polar stars, the relative brightness of which may have been determined by photometric measurements.

The table and chart given below are designed to serve as guides in finding the stars which are, as has been said, in course of phonometric measurement at the Harvard College Observatory. The stars given in the table are arranged approximately in the order of their brightness, the first being *α* Ursæ Minoris, which is taken in all cases as the standard of comparison, and the next three, *δ* Ursæ Minoris, 51 Cephei, and *λ* Ursæ Minoris. The chart is a copy of a sketch showing the approximate relative position of ten faint stars very near the pole, which are denoted by the italic letters *a, b, c, d, e, f, g, h, k, l.* The places

DM.		α 1880.		δ 1880.	
°	′	h.	m.	°	′
88	8	1	14	88	40
86	269	18	11	86	37
87	51	6	44	87	14
88	112	19	44	88	57
88	4	0	51	88	23
88	9	2	3	88	36
89	3	2	28	89	36
89	35	17	50	89	48
89	37	19	28	89	54
89	1	0	19	89	45
89	26	13	23	89	49



of the pole for 1855, 1880, and 1900, and of five stars from the Durchmusterung, four of which occur in the table, are also indicated upon the chart, to facilitate the identification of the faint stars. The objects called *c* and *e* are nearly in the prolongation of the line through DM. 89° 37′ and *b*. Between these last, and more nearly in the same line than it appears to be in the chart, lies the star *a*.

The value and interest of the photometric results to be obtained at the Harvard College Observatory may be greatly increased by the co-operation of astronomers elsewhere. All who are desirous of improving the present system of comparing the brightness of stars, are therefore requested to make estimates of the magnitude of as many as may be convenient of the stars above mentioned. It is desirable that the estimate should be made, for each star which may be observed, on five different nights, and that each estimate should be, if possible, entirely independent of those previously made. It will add to the value of the work if, on every occasion when the fainter stars are looked for, a record is made of such of them as can then be seen, even if no estimate of their magnitude is attempted.

Observers are also requested to note the approximate places of any stars not represented upon the chart, but within five minutes of the place of the pole at any time between 1880 and 1900. The boundary of this region is represented on the chart by a dotted line. The stars not shown within it have been omitted as unnecessary for the purpose of finding the others, and several of these omitted stars are inconveniently faint for photometric observations; but records of their visibility at any time and place will be valuable as evidence of the state of the atmosphere and character of the instrument employed in the observations.

All astronomers who may be induced by this request to make any observations of the kind just described will confer a favour upon the Harvard College Observatory by sending to it a copy of their records, accompanied by a statement of any modification of the proposed method of observation which they may have adopted, as well as any additional details which may appear desirable, with regard to the instruments employed, &c. Unless the contrary is requested, the results will be published with the photometric measurements obtained at the Harvard College Observatory; and a copy of the publication will be sent to each observer who has co-operated in the work.

It is hoped that a large number of those astronomers whose experience has been sufficient to establish a definite scale for their estimates of stellar magnitude will consent to take part in the proposed observations, in order that the published series of observations may be complete enough to be of general utility.

EDWARD C. PICKERING
Director of the Harvard College Observatory

GEOGRAPHICAL NOTES

AT the meeting of the Royal Geographical Society on Monday evening it was announced that the gold medals had that day been awarded to Col. Nicholas Prejevalsky for the great additions he has made to our knowledge of Central and Eastern High Asia by his successive expeditions into the unexplored parts of the great plateau of Mongolia and the lofty deserts of Western Thibet, and for the admirable way in which he has described the regions traversed by him in the published narratives of his journeys; and to Capt. W. J. Gill, R.E., for excellent geographical work performed during two journeys of exploration, voluntarily undertaken, along the northern frontier of Persia in 1873, and over previously untravelled ground in China and Thibet, in 1877; also for the elaborate memoir and route maps contributed to the forthcoming volume of the Society's *Journal*. A paper was afterwards read by the Rev. James McCarthy, of the China Inland Mission, descriptive of the journey which he made, mostly on foot, in 1877, across China, from Chinkiang, on the Yangtsze-Kiang, to Bhamô, in Burmah. The leading features of this journey have been fully described in NATURE. The most noteworthy incident of the evening was a speech, delivered in his native language by the Marquis Tsêng, Chinese Minister to England and France, expressive of the pleasure which he felt at Mr. McCarthy's acknowledgment of the uniformly courteous treatment he experienced during his long journey.

AT the next meeting of the Geographical Society on May 12, the second of the course of scientific lectures of the present session will be delivered by Prof. G. Rolleston, of Oxford, on the "Modifications of the External Aspects of Organic Nature produced by Man's Interference."

NEWS has arrived by the last mail from Zanzibar that Mr. H. M. Stanley is busily occupied in engaging porters for a journey into the interior of Africa, but that he preserves the utmost secrecy as to his intended movements. A rumour is current amongst the porters that their journey is to commence from the west coast; if this be the case, Mr. Stanley must have introduced a radical change into the original plans of the Belgian section of the International African Association, for whom he is believed to be acting. That, accidents apart, he will be more successful than the unfortunate leaders of the first Belgian expedition few will be so rash as to doubt, and he is sure to have good and sufficient reasons for the course he is adopting.

DURING the past few days there has been a considerable exodus of missionary explorers. Dr. James Stewart, the well-known head of the Livingstonia station, has returned to his post, and will soon be adding more to our knowledge of the shores of Lake Nyassa. Dr. Joseph Mullens, of the London Missionary Society, who has already done good service to geography in Madagascar, has started for Ujiji, on Lake Tanganyika, and before returning home he will probably make his way down to the north end of Lake Nyassa, thus filling up an important blank in our knowledge of the lake region. Lastly, the Rev. T. J. Comber, of the Baptist Missionary Society, has returned to Western Africa to found a station at San Salvador, and eventually to conduct a missionary expedition to the upper waters of the River Congo.

THE general report of last year's operations of the Marine Survey of India, under Commander A. Dundas Taylor, late I.N., has just reached this country. During that period two parties carried out the following surveys: Ratuagiri, including Mirya and Kalhadevi Bays; Viziadurg, including Rajapur and Ambol Ghur Bays; Paumben Pass (between Ceylon and the mainland) and its approaches; Beypore, Calicut, and Cochin. The natural history investigations of the season have been confined to an examination of the fauna inhabiting the shores in the vicinities of Ratnagiri and Viziadurg, and to the collection and preservation of the various ornithological specimens procured. The area examined includes the tract of country lying between the above places from the sea to the chain of hills known as the Western Ghâts. The examination of the sea-bottom with the dredge was impracticable, owing to the want of a vessel; this want, however, has since been supplied, as was recorded in NATURE, vol. xix. p. 298, and no doubt interesting results will be obtained during the present season. Captain Taylor's report is accompanied by a useful map showing the surveys completed by his officers, together with the sheets published or in course of publication, 1877-8.

THE second session of the Congress of Commercial Geography, inaugurated at Paris last year, will be held at Brussels in September, under the presidency of M. Bamps, and arrangements for the meeting have already been commenced.

NEWS has been received from Queensland that the remains of the two Prouts, well-known explorers, have at length been discovered, so that the question of their fate is now finally set at rest.

AT the last meeting of the Société de Géographie Commerciale at Paris Dr. Harmand gave some account of his observations in the Laos country of the Indo-Chinese peninsula. He stated that though elephants were common there, ivory was dearer than in Paris, and that the same remark applied to rhinoceros-horn. There are mines of lead, iron, and copper in the country, and probably gold will be found. The chief productions are indigo, lacquer, saffron, rice, cotton, &c., but the industry of the country is in a very undeveloped state.

A NEW project for the creation of an inland sea has been advanced and advocated by General Fremont, at present Governor of Arizona. The removal of a barrier ridge, he affirms, would admit the waters of the Gulf of California into an ancient basin, and would create a navigable inland sea 200 miles long, 50 miles broad, and 300 feet deep. This piece of engineering, which is very like Roudaire's Algerian inland sea project, he claims, would convert what is now a desert region into a commercial highway, and would greatly improve the climate of Southern Arizona and California.

AT last week's meeting of the Paris Academy M. de Lesseps announced that in a letter of April 15, Capt. Roudaire states that the sounding operations were being pursued with vigour and success, and that so far they justified the expectation of being able to create an interior Algerian Sea.

AN exploring expedition to New Guinea is being organised at Wellington, New Zealand, on a large scale.

L'Exploration states that a new African expedition is being organised at Lisbon, under the direction of Capt. Paiva d'Andrada. Its object is the exploration of the Zambezi and the foundation of commercial and agricultural colonies in the territories of Fete and Zoumbo.

IN the *Verhandlungen* of the Berlin Geographical Society, Nos. 2 and 3, is a suggestive paper by Dr. Güssfeldt on the Ice-Conditions of High Mountains. No. 80 of the *Zeitschrift* contains a paper of much originality, on the causes which have conduced to the formation of the surface of Norway, by Prof. Kjerulf.

Globus is publishing a valuable series of articles on the Red River of the North, from the French of M. de Lamothe, and the Hundu Kush Alps by Herr Emil Schlagentweit.

NOTES

THE Annual London Meeting of the Iron and Steel Institute will be held on Wednesday, Thursday, and Friday, May 7, 8, and 9, at the Institution of Civil Engineers, 25, Great George Street, Westminster. The following programme of proceedings has been arranged:—On Wednesday the retiring President (Dr. C. W. Siemens, F.R.S.), will take the chair at 10.30 A.M., and the President-Elect (Mr. Edward Williams) will deliver his inaugural address. The Bessemer Medal for 1879 will be presented to Mr. Peter Cooper, of New York, "the father of the American iron trade." The adjourned discussion on the paper read at Paris by Mr. Daniel Adamson, C.E., of Manchester, on "The Mechanical Properties of Iron and Mild Steel," will be resumed, and Mr. Adamson will present a supplementary paper. On the following days the following papers will be read and discussed:—"On the Use of Steel in Naval Construction," by Mr. Nathaniel Barnaby, C.B., H.M.'s Chief Constructor. "On the Use of Steel in the Construction of Bridges," by Mr. H. N. Maynard. "On the Elimination of Phosphorus in the Bessemer Converter," by Mr. Sydney G. Thomas, F.C.S., and Mr. Percy C. Gilchrist, A.R.S.M., F.C.S. "On the Removal of Phosphorus and Sulphur during the Bessemer and Siemens-Martin Processes of Steel Manufacture," by Mr. G. J. Snelus, F.C.S., &c. "On a New Volumetric Method of Determining Manganese in Manganiferous Iron Ores, Spiegeleisen, Steel, &c.," by Mr. John Pattinson, F.I.C., Newcastle-on-Tyne. "On a Ready Means of Moulding Lime, and making Lime or Basic Bricks and Linings for Furnace Converters, &c.," by Mr. Edward Riley, F.C.S., F.I.C., &c. "On a Practical Combination of the Bessemer and Puddling Processes," by Mr. Edwin Pettitt, Cheltenham. "On the Results of Working the Godfrey-Howson Furnaces at the Works of Tamaris, Gard, France," by M. Escalle. "On the Chemistry of Puddling," by Mr. H. Louis, A.R.S.M., Londonderry, Nova Scotia. "On a New Process for Protecting Iron and Steel against Rust," by Prof. Barff.

THE Rev. W. H. Dallinger, F.R.S., has been appointed Rede Lecturer at Cambridge this year.

AMONG those on whom the degree of LL.D. has been conferred by the Glasgow University is Dr. C. W. Siemens, Prof. Hull, director of the Irish Geological Survey, and Prof. Dickson, newly elected to the Edinburgh Chair of Botany.

THE death is announced of Dr. Charles Murchison, F.R.S.

MADEMOISELLE ADELAIDE MONTGOLFIER, a daughter of the inventor of balloons, is still alive, aged eighty-nine years. She is possessed of a large fortune, and presented the Museum of the Aëronautical Academy with a copy of the large medal executed by Houdon, and representing her father and uncle, who was associated with him in the invention of balloons. This medal was executed to commemorate that event. A movement will be got up in France for celebrating the centenary of that memorable event, which took place in June, 1783, in the vicinity of Lyons.

THE annual conference on National Water Supply, Sewage, and Health, will be held in the rooms of the Society of Arts, on Thursday and Friday, May 15 and 16, 1879. There will be an Exhibition of Mechanical and Chemical Apparatus in connection with Water Supply, Treatment of Sewage, and Health. Papers on any of above heads are requested. The object of the conference is to discuss existing information in connection with the results of any systems already adopted in various localities, referring to the subjects of National Water Supply, Sewage, and Health; to elicit further information thereon; and gather and publish, for the benefit of the public generally, the experience gained. The introduction and discussion of untried schemes will, therefore, not be permitted. The papers accepted for the conference will be printed and circulated at the meetings.

PROF. TYNDALL has been instructing the Select Committee appointed to inquire into the subject of electric lighting. He gave a brief sketch of the history of electricity and of its application to lighting purposes, illustrating his evidence by several interesting experiments. Seeing what had been done by Mr. Edison, he believed that many of the existing difficulties would be removed; for public illumination he was afraid platinum would be too expensive. Dr. Siemens has also been giving important evidence on the subject.

WE are glad to see that Dr. Brehm, the well-known naturalist, accompanies the Crown Prince of Austria in his tour through Spain.

WE learn from *Science News* that the Brazilian Government has appointed Mr. Orville A. Derby as geologist to the National Museum at Rio de Janeiro, to succeed the late Prof. Hartt, whose assistant Mr. Derby had been for a number of years. Next to Prof. Hartt, Mr. Derby was probably best acquainted with the geological structure of Brazil, and he is, therefore, the one most fitted to carry on the work. He accompanied Prof. Hartt, as an assistant, on both of his Amazonian trips, in 1870 and 1871, and largely shared in the honours arising from the discoveries made during those years, by which a firm foundation was laid for the complete geological exploration of the great valley.

THE following arrangements have been made for the meetings of the Society of Arts after Easter:—At the ordinary meetings on Wednesday evenings, at eight o'clock: May 14—"The Automatic Hydraulic Brake," by E. D. Barker; May 21—"Edison's New Telephone," by Conrad W. Cooke. In the African Section, on Tuesday evenings at eight o'clock: May 27—"The Contact of Civilisation and Barbarism in Africa, Past and Present," by Edward Hutchinson. In the Chemical Section, on Thursday evenings, at eight o'clock: May 8 and 15—"The History of Alizarine and Allied Colouring Matters, and their Production from Coal Tar," by W. H. Perkin, F.R.S. In the Indian Section, on Friday evenings, at eight o'clock: May 2—"The Wild Silks of India, especially Tussah," by Thomas Wardle; May 23—"The Harbour of Kurrachee," by W. J. Price.

A SPECIMEN of the electro-magnetic engine invented by M. Marcel Deprez is employed by the Academy of Aëronautical Ascensions, 50, rue Rodier, Paris, for working a sewing-machine which is used for the construction of a balloon called *L'Électricité*. The weight of the motor is only 4 kilogrammes, and four Bunsen elements of ordinary size are sufficient to give to the needle the required velocity.

FROM to-day postal cards will be sold in Paris at the price of 50 centimes each, for the transmission of messages by the pneumatic tube which connects the several telegraphic stations in the French metropolis.

IN a memoir presented to the Academy of Sciences and Literature of Lyons, we learn from the *British Medical Journal*, Dr. Henry H. Dor, a well-known oculist, contests the view held by Mr. Gladstone, and by Geiger and Magnus of Boston, that our ancestors were colour-blind, a view deduced from their writings and from the different names which they have given to colours. Dr. Dor endeavours to demonstrate that now, as in the time of Homer, poets insist too little upon the indications of the colours, but much more upon their luminous intensity. Moreover, Dr. Dor says that persons who do not possess any knowledge of physics find much difficulty in distinguishing the colours of the rainbow, and only see in it three or four colours, in place of the seven classical colours of its composition. Further, it results even from the very study of the Assyrian and Egyptian monuments, that those nations had not only perceived, but imitated, the greater part of the colours of which we are at present cognisant.

THE second annual meeting of the Midland Union of Natural History Societies will be held in the council chamber of the Town Hall, Leicester, on Tuesday, May 20, at half-past three o'clock. The business of the meeting will be to receive the report of the Council and the treasurer's accounts; to fix the place of the next annual meeting in 1880; to consider any suggestions that members may offer; to discuss the work of the Union during the coming year; and to transact all necessary business. The President will open the business with an address. A *conversazione* will be held in the Leicester Town Museum (entrance in Hastings Street) on Tuesday evening, May 20, the arrangements for which are under the direction of the Leicester Literary and Philosophical Society. There will be an exhibition of objects of general scientific interest, microscopy, the various departments of natural history, archæology, and art. On Wednesday, May 21, there will be an excursion to Charnwood Forest.

FROM the *Gardeners' Chronicle* we learn that an Agricultural and Horticultural Society has been founded at Mentone, many of the members being English residents.

THE *Electrician* of April 26 contains a long letter from Prof. Clerk Maxwell on the correct definition of "Potential."

AMONG Mr. Murray's list of announcements is "The River of Golden Sands," a narrative of a journey through China to Burmah, by Capt. William Gill, R.E., and "A History of Ancient Geography," by E. H. Bunbury.

THE *American Naturalist* for April contains a curious paper, by Mr. Xenos Clarke, on "Animal Music, its Nature and Origin." Mr. W. O. Crosby has a paper on "Native Bitumens and the Pitch Lake of Trinidad," and Mr. W. H. Holmes on a Deposit of Obsidian in the Yellowstone Park.

GRAVITATION experiments in liquids have recently been made by Herr Schröttner in Vienna, with a view to determining viscosity (as previously proposed by Pisati and De Heen). He took as basis a formula of Stokes for the resistance of a ball moved in a straight line in a liquid, and sought to determine the coefficients of friction in absolute measure. The practicability of the method was proved in a very viscous mixture of black pitch and beech-tar, and in concentrated glycerine. For the latter, higher values were obtained than by the transpiration-experiments carried on at the same time. From the author's experiments with glycerine, as also from Schieck's gravitation experiments with water, it appeared that the coefficients of friction were considerably greater whenever the velocities of fall exceeded a certain amount. For liquids with little viscosity, as water, small velocities of fall, such as met the conditions of experiment, could only be obtained by giving the balls a surplus weight of a few hundredths of a milligramme over the displaced mass of liquid, in case experiments were not made with very large balls and very considerable quantities of liquid.

ROMAN remains have just been discovered at Oberbreisig, a village near the Rhine, a few miles to the south of Bonn. A rectangular building of unquestionably Roman origin has been laid bare, the purpose of which, however, is very doubtful. The excavations leading to this discovery are in connection with others of greater extent which are being made in the neighbourhood, and which are principally directed to the investigation of a Roman villa near Waldorf and a Roman road leading to Sinzig.

IN a recent memoir communicated to the Belgian Academy, M. Lagrange offers some novel views on the formation of bodies in the universe. He supposes that before any expenditure of work the quantity of heat of the universe was *nil*, and that the temperature was gradually raised above absolute zero at the expense of work done by attraction. Hence the formation of solid bodies must have preceded that of liquids and gases. Through the gradual condensation of matter and consequent enormous development of heat, the earth would attain, at least in the parts near the surface, the state of fluidity necessary to explanation of its form and geological characters. As the temperature gradually rose with gradual agglomeration of matter, a very dense atmosphere would form, with pressure diminishing outwards, and in a more advanced phase, the temperature of this, after reaching a maximum, would gradually diminish, causing liquefaction or solidification of certain matters at first vaporous, while other solid bodies might remain suspended in the atmosphere. M. van der Mensbrugghe commends the author's views as original and worthy of the attention of *savants*, but, with M. Folie, he regards the initial absolute zero as inadmissible. In reply to objections by M. Folie, the author promises shortly to defend this hypothesis:—Space is occupied by two substances; one, attractive, which is matter properly so-called, or material atoms; the other, repulsive, which occupies the inter-atomic space, and from which results, between any two atoms, a variable repulsion exercised at the surface of the latter.

WE have received No. 11 (March, 1879) of the *Bulletin* of the Brooklyn Entomological Society, of the existence of which publication we were not previously aware. It consists of a half-sheet 8vo, with one plate, illustrating a paper by C. F. Gissler on Coleopterous larvæ of the family *Tenebrionidæ*, which appears to be carefully worked out and likely to prove of value, and the figures (chiefly concerning the pygidia and antennæ) seem to be well drawn. The other papers are on the genus *Colias*, *Samia cynthia*, and on some species of *Thecla*. The number of

American serials exclusively devoted to entomology is constantly increasing.

A SLIGHT earthquake was felt between 9.15 and 9.30 P.M., April 24, at Sigmaringen. The direction was not observed. The following particulars have reached the *Times* of the earthquake which occurred in Persia on March 22, at 3.42 A.M. (London time, 12.37 A.M.). It lasted 12s., was felt at Tauris and east as far as Zendjan; no damage was caused in Tauris, but in the vicinity of Mianeh, where the shocks continued with more or less vigour up to April 2, great damage and loss of life have occurred. An official report, prepared for the Persian Government by the Persian Telegraph authorities at Mianeh gives the damage, as far as is at present known, as follows:—21 villages totally destroyed, 54 greatly damaged, 922 persons killed, together with 2,660 sheep, 1,125 oxen, 124 horses, and 55 camels. The centre of the disturbances was the mountain of Bousgouche.

WE learn from the *Colonies and India* that an American explorer has recently discovered in the little-known district of Yucatan, bordering on British Honduras, a valuable insect, possessing properties which ought to make it a rival of the cochineal and shellac-producing insects. This is the *neen*, or *niin*, a species of *Coccus*, which feeds on the mango tree and similar plants, and exists in enormous quantities in Central America. It is of considerable size, of a yellowish brown colour, and emits a peculiar oily odour, containing as it does, a large quantity of fatty oil, or rather grease. This grease is used by the natives for various purposes, being highly prized as a medicinal oil for external application, and it is also employed for mixing paints. It can be made to change its condition very considerably by different processes. When exposed to great heat, the lighter oils evaporate, leaving a tough flexible mass, resembling half-softened wax, but unaffected by heat or cold, which may be used as a lacquer or varnish. When burnt, this material produces a thick semi-fluid mass, somewhat resembling a solution of india-rubber, which after a few days becomes hard and solid. As a cement this substance will be invaluable, and it might also be used for waterproofing purposes.

WITHIN a few days the scientific committee for the organisation of the Paris Exhibition of Applied Sciences will hold an important meeting. The exhibition will be open from July to November.

SEVERAL of the Conseils Généraux of the surrounding departments have voted funds for the erection of an observatory on the top of Mont Ventoux, in Vaucluse. It will be the third high meteorological station in France, and very likely not the last. M. Ferry, the Minister of Public Instruction, is favourable to the erection.

"THE Silk-Worm, being a brief Manual of Instruction for the Production of Silk," is the title of a pamphlet by Mr. C. V. Riley, professor of the U.S. Department of Agriculture. Silk-worm rearing seems likely to become an important industry in some parts of the United States.

THE March number of the *Journal* of the Statistical Society contains the concluding part, upwards of 180 pages, of Mr. C. Walford's elaborate and valuable paper on the Famines of the World. The whole paper, we believe, will be published separately.

MESSRS. HEYWOOD, of Manchester, have just published the tenth series of Science Lectures for the People delivered in that city. The volume, which can be had for a few pence, contains nine lectures by some of the most eminent men of science of the day. Huxley lectures on William Harvey, Roscoe on the Sun, Flower on the Tasmanians, Williamson on Insectivorous Plants, Barrett on Edison and his Inventions, Dawkins on Our Earliest Ancestors in Britain, Abel on the Modern History of Gunpowder, Dallinger on the Minutest Forms of Life, and Romanes on Animal Intelligence. Several of the lectures are illustrated.

MR. RICHARD RATHBUN has reprinted from the *Proceedings* of the Boston Society of Natural History, a pamphlet of 25 pages on the Devonian brachiopoda of the Province of Para, Brazil. The list is a long one, and many species are described for the first time.

IT will be difficult to surpass or even equal our American friends in the illustrated scientific works which they have begun to publish in such quantity. We have had occasion to mention more than one work of this class recently, and now we receive the first part of "Characeæ Americanæ," illustrated, described and published by Dr. T. F. Allen, of New York. The particular specimen described, and illustrated by an exquisitely coloured plate, is *Chara gymnopus*, A. Br., var. *elegans*, A. Br.

WE have on our table the following works:—"Our New Protectorate," 2 vols., J. C. McCoan (Chapman and Hall); Karl von Gebler's "Galileo Galilei," translated by M. Sturge (Kegan Paul and Co.); "The Encyclopædia Britannica," vol. ix. (A. and C. Black); "Geography" (School Books for South Africa, No. 1), Dr. John Shaw (W. Collins); "Elements of Natural Philosophy," Part i., Second Edition, Thomson and Tait (Printed at Cambridge University Press); "Chemistry of Common Life," J. F. W. Johnson and A. H. Church (Blackwood and Co.); "Shadows of the Coming Truth" (Elliot Stock); "Caves of South Devon and their Teachings," J. E. Howard (Hardwicke and Bogue); "Scientific Results of the Second Yarkand Mission;" 6 Plates, from the Notes of Ferdinand Stoliczka (Quaritch); "End-on Illumination in Private Spectroscopy," Piazzi Smyth (Neil and Son, Edinburgh); C. Peschel's "Geschichte der Erdkunde," Parts i. and ii., Edited by Prof. Dr. Sophus Ruge (R. Oldenburgh, München); "Pre-Historic Times," fourth edition, Sir John Lubbock, Bart. (F. Norgate); "Dictionary for Architects," No. 1, W. J. Christy (Griffith and Farren); "Reduction of Greenwich Meteorological Observations" (Spottiswoode); "The Flowers of the Sky," R. A. Proctor (Strahan); "On Certain Effects of Starvation on Vegetable and Animal Tissues" (D. D. Cunningham, Government Printer, Calcutta); "The Microscopic Organisms found in the Blood of Man and Animals" (Government Printer, Calcutta); "Rambles in North-Western America," J. M. Murphy (Chapman and Hall); "Atlas of Histology, Parts 1 and 2, E. Klein and N. Smith (Smith Hall); "How to learn Danish," E. C. Otté (Trübner and Co.); "Key to How to Learn Danish," E. C Otté (Trübner and Co.); "Anatomy and Physiology of Man," G. G. P. Bale (Remington and Co.); "On Artificial Manures," by M. Georges Ville, translated and edited by William Crookes (Longmans); "Agricultural Ants of Texas," H. C. McCook (Trübner); "De la Ligue Contre les Vivisections, ou la Nouvelle Croisade," Par un Anglais (Ernest Leroux); "L'Eclairage Electrique," Le Comte Th. du Moncel (Hachette); "Contributions to the Anatomy of the Central Nervous System of Vertebrate Animals," Alfred Saunders; "Infection—Diseases in the Army," Prof. R. Virchow (H. K. Lewis); "Recherches sur l'Électricité," Gaston Planté (Paris, A. Fournan); "On the Daily Inequality of the Barometer" (W. W. Rundell); "Freedom in Science and Teaching," Ernst Haeckel (Kegan Paul and Co.).

THE additions to the Zoological Society's Gardens during the past week include a Black-handed Spider Monkey (*Ateles melanochir*) from Central America, presented by Mr. D. R. Comyn; two Prairie Marmots (*Cynomys ludovicianus*) from North America, presented by Mr. W. G. Marshall; a Guilding's Amazon (*Chrysotis guildingi*) from St. Vincent, West Indies, presented by Mr. G. Dundas, C.M.Z.S.; a Cuvier's Podargus (*Podargus cuvieri*) from Australia, presented by Mr. R. S. C. Baber; a Lesser Long-eared Bat (*Plecotus brevimanus*), British Isles, presented by Mr. J. Ward; a three-toed Amphiuma (*Amphiuma means*) from North America, presented by Mr. A. C. Cole; a Bonnet Monkey (*Macacus radiatus*) from India, an Egyptian Cat (*Felis chaus*) from North Africa, a Common Ass (*Asinus vulgaris*) from Persia, a Grey-headed Porphyris (*Porphyris poliocephalus*) from South Asia, a Puff Adder (*Vipera arietans*) from the Cape of Good Hope, deposited.

RECENT CONTRIBUTIONS TO THE HISTORY OF DETONATING AGENTS[1]

AMONG the many explosive preparations which have during the last thirty years been proposed as substitutes for gunpowder, on account of greater violence and other special merits claimed for them, not one has yet competed with it successfully as a propelling agent, nor even as a safe and sufficiently reliable explosive agent for use in shells; for industrial applications and for very important military or naval uses, dependent upon the destructive effects of explosives, it has had, however, to give place, to a very important extent, and in some instances altogether, to preparations of gun-cotton and nitro-glycerine.

But there appeared little prospect that either gun-cotton or nitro-glycerine, whether used in their most simple condition or in the forms of various preparations, would assume positions of practical importance as explosive agents of reliable, and therefore uniformly efficient, character, until the system of developing their explosive force through the agency of a detonation, instead of through the simple agency of heat, was elaborated.

Before the first step in this important advance in the application of explosive agents was made by Alfred Nobel, about twelve years ago, the very variable behaviour of such substances as gun-cotton and nitro-glycerine, when exposed to the heat necessary for their ignition under comparatively slight modifications of attendant conditions (*e.g.* as regards the completeness and strength of confinement or the position of the source of heat with reference to the main mass of the material to be exploded) rendered them uncertain in their action, and at any rate, only applicable under circumstances which confined their usefulness within narrow limits. The employment by Nobel of an initiative detonation, produced by the ignition of small quantities of mercuric fulminate or other powerful detonating substances, strongly confined, for developing the violent explosion, or detonation, of nitro-glycerine, opened a new field for the study of explosive substances, and the first practical fruit was the successful application of plastic preparations of nitro-glycerine and of compact forms of compressed gun-cotton, with simplicity and certainty, to the production of destructive effects much more considerable than could be accomplished through the agency of much larger amounts of gunpowder, applied under the most favourable conditions. Whereas very strong confinement has been essential for the complete explosion of these substances, so long as the only known means of bringing about their explosion consisted simply of the application of fire or sufficient heat, no confinement whatever is needed for the development, with certainty, of a decidedly more violent explosive action than they are capable of exerting when thus applied, if they are detonated by submitting some portion of the mass to the blow or concussion developed by a sharp detonation, such as is produced by the ignition of a small quantity of strongly confined mercuric fulminate.

The conditions essential to the development of detonation in masses of nitro-glycerine and gun-cotton, or preparations of them, and the relations to and behaviour towards each other of these and other explosive bodies, in their character or functions as detonating agents, have been made the subject of study by the lecturer during the last ten years, and some of the earlier results published by him in connection with this subject also led to the pursuit of experimental inquiries of analogous character by Champion and Pellet and others.

Some of the chief results attained by Mr. Abel's experiments may be briefly summarized.

It was found that the susceptibility to detonation, as distinguished from explosion, through the agency of an initiative detonation, is not confined to gun-cotton, nitro-glycerine, and preparations containing those substances, but that it is shared, though in very different degrees, by all explosive compounds and mixtures.

It was demonstrated that the detonation of nitro-glycerine and other bodies, through the agency of an initiative detonation, is not ascribable simply to the direct operation of the heat developed by the chemical changes of the charge of detonating material, and that the remarkable property possessed by the sudden explosion of small quantities of certain bodies (the mercuric and silver fulminates) to accomplish the detonation of nitro-glycerine and gun-cotton, is accounted for satisfactorily by the mechanical force thus suddenly brought to bear upon some part of the mass operated upon. Most generally, therefore, the degree of facility with which the detonation of a substance will develop similar change in a neighbouring explosive substance, may be regarded as proportionate to the amount of force developed *within the shortest period of time* by that detonation, the latter being in fact analogous in its operation to that of a blow from a hammer or of the impact of a projectile.

Thus, explosive substances which are inferior to mercuric fulminate in the suddenness, and the consequent momentary violence of their detonation, cannot be relied upon to effect the detonation of gun-cotton, even when used in comparatively considerable quantities. Percussion cap composition, for example, which is a mixture of fulminate with potassium chlorate, and is therefore much less rapid in its action than the pure fulminate, must be used in comparatively large quantities to accomplish the detonation of gun-cotton.

The essential difference between an explosion and what we now distinguish as a detonation lies in the comparative suddenness of the transformation of the solid or liquid explosive substance into gas and vapour.

The gradual nature of the explosion of gunpowder is illustrated, in its extreme, by burning a train of powder in open air; the rapidity and consequent violence of the explosion is increased in proportion to the degree of confinement of the exploding charge, or to the resistance opposed to the escape or expansion of the gases generated upon the first ignition of the confined substance.

In the case of a very much more sensitive and rapidly explosive substance than gunpowder, such as mercuric fulminate, the increase in the rapidity of its transformation, by strong confinement, is so great that the explosion assumes the character of a detonation in regard to suddenness and consequent destructive effect. A still more sensitive and rapidly explosive material (such as the silver fulminate and iodide of nitrogen) produces when exploded in open air effects akin to those of detonation; yet even with these bodies, confinement operates in increasing the rapidity of the explosive to suddenness, and consequently in developing a more purely detonative action.

Detonation, developed in some portion of a mass, is transmitted with a velocity approaching instantaneousness throughout any quantity, and even if the material is laid out in the open air in long trains composed of small masses. The velocity with which detonation travels along trains thirty or forty feet in length, composed of distinct masses of gun-cotton and of dynamite, has been found to range from 17,000 to 24,000 feet per second. Even when trains of these explosive agents were laid out with intervening spaces of half an inch between the individual masses composing the trains, detonation was still transmitted along the separated masses with great though diminished velocity.

The suddenness with which detonation takes place has been applied as a very simple means of breaking up shells into small fragments and scattering these with considerable violence, with employment of very small charges of explosive agent. Thus by filling a 16-pr. common shell completely with water and inserting a charge of ½ oz. of gun-cotton fitted to a detonating fuze, the shell being thoroughly closed by means of a screw plug, the force developed by the detonation of the small charge of gun-

[1] Weekly Evening Lecture at the Royal Institution, Friday, March 21, 1879. By Professor Abel, C.B., F.R.S. Revised by the Author.

cotton is transmitted instantaneously in all directions by the water, and the shell is thus broken up into a number of fragments averaging fourteen times the number produced by bursting a shell of the same size by means of the full amount of powder which it will contain (13 oz.). Employing 1 oz. of powder, in place of ½ oz. of gun-cotton, in the shell filled with water, the comparatively very gradual explosion of the powder charge is rendered evident by the result; the shell being broken up into less than twenty fragments by the shock produced by the first ignition of the charge, transmitted by the water. In this case the shell is broken up by the minimum amount of force necessary for the purpose, before the explosive force of the powder charge is properly developed. Extensive comparative experiments carried on not long since by the Royal Artillery at Okehampton, demonstrated that this simple expedient of filling common shells with water and attaching a small charge of gun-cotton with its detonator to the fuse usually employed, allowed of their application as efficient substitutes for the comparatively complicated and costly shrapnel and segment shells.

Another illustration of the sharpness of action developed by detonation as compared with explosion, consequent upon the almost instantaneous character of the metamorphosis which the explosive agent undergoes in the case of detonation, is afforded by a method which the lecturer applied some years since for comparing the violence of action of charges of gun-cotton and of dynamite arranged in different ways. The charges (5 lb.) to be detonated were freely suspended over the centres of plates of very soft steel of the best quality, which rested upon the flat face of a massive block, or anvil, of iron, having a large central circular cavity. The distance between the upper surface of the plate and the charge suspended over it, was 4 feet. The sharp blow delivered upon the plate by the air suddenly projected against it by the force of the detonation when the charge was fired, forced the metal down into the cavity of the anvil, producing cup-shaped indentations, the dimensions of which afforded means of comparing the violence of the detonation. A much larger charge of powder exploded in actual contact with the plate, would produce no alteration of form in the metal, and the same negative result would be furnished by the explosion over the plate of a heap of loose gun-cotton of the same or greater weight than the charges detonated. The above method of experiment was devised, in the first instance by Mr. Abel, in July 1875, for comparing the quality of some specimens of Llandore steel proposed to be used by the Admiralty for ship-building purposes, with samples of malleable iron, and it has since been employed by Mr. Adamson in carrying out a very useful series of experiments, recently communicated to the Iron and Steel Institute.

It has been stated that detonation can be transmitted from one mass of gun-cotton or dynamite to another through intervening air-spaces. The extent to which such spaces can be introduced without checking detonation is obviously regulated by the size of the masses of explosive detonated; but the distances of air-space through which the detonation of a moderate quantity of the explosive agent will communicate to similar masses, are very limited, a space of 2 inches being sufficient to prevent the detonation produced by a mass of 8 oz. of gun cotton, freely exposed, from communicating to contiguous ones. If the dispersion of force is prevented in part, and direction is given to the gases violently projected from the centre of detonation, the power of transmitting detonation to separated masses of explosive is increased to a remarkable degree. This is readily accomplished through the agency of tubes, the charge first detonated being just inserted into one extremity, while that to which the detonation is to be transmitted is inserted into the other; or separate charges may be placed at different distances inside a long tube, with long intervening spaces, the initiative charge being inserted at one end. Thus, the detonation of a 1-oz. disk of gun-cotton in open air will not transmit detonation with *certainty* to other disks placed at a greater distance than half an inch from it; but if it be just inserted into one end of an iron tube 2 feet long and 1·25 inch in diameter, a similar disk, inserted into the other extremity of the tube, will invariably be detonated. In tubes of the same kind, of very considerable length, 2-oz. disks of gun-cotton placed at intervals of 2 feet, were detonated through the initiative detonation of one such disk inserted into one extremity of the tube. The results obtained with equal quantities of gun-cotton varied with the diameter, strength, and nature of the material of the tubes used. Dynamite and mercuric fulminate, applied to their own detonation, furnished results quite analogous to those obtained with gun-cotton; but in applying fulminate to the detonation of gun-cotton through the agency of tubes, some singularly exceptional results were obtained.

Silver fulminate was employed for the purpose of instituting more precise experiments than could be made in operating on a larger scale, with gun-cotton, on the influence of the material composing the tubes, of the condition of their inner surfaces, and of other variable circumstances, upon the transmission of detonation. Half a grain of silver fulminate freely exposed and ignited by a heated body, will transmit detonation to some of the compound placed at a distance of 3 inches from it, but does not do so with certainty through a distance of 4 inches. But when the quantity of the fulminate is just inserted into one end of a stout glass tube 0·5 inch in diameter, and 3 feet long, its detonation is invariably induced by that of a similar quantity of the fulminate placed just inside the other extremity of the tube. Glass tubes were found to transmit the detonation of silver fulminate much more rapidly than tubes of several other materials of the same diameter and thickness of substance. Thus, with the employment of double the quantity of fulminate required to transmit the detonation with certainty through a glass tube of the kind described, 3 feet in length, it was only possible to obtain a similar result through a pewter tube 31·5 inches long, a brass tube 23·7 inches long, an indiarubber tube 15·8 inches long, and a paper tube 11·8 inches long. The difference in the results obtained was not ascribable to a difference in the escape of force on the instant of detonation, in consequence of the fracture of the tube, nor to the expenditure of force in work done upon the tube at the seat of detonation. The transmission of detonation appeared also not to be favoured by the sonorosity or the pitch of the tube employed, as the sonorous brass tube was not found to favour the transmission to the same extent as the pewter tube. These differences appeared on further investigation not to be ascribable, to any important extent, if at all, to the difference in the nature of the material composing the tubes, but to be simply, or at any rate almost entirely, due to differences in the condition of the inner surfaces of the tubes. Thus, brass tubes, the inner surfaces of which were highly polished, and paper tubes, when coated inside with highly glazed paper, transmitted the detonation of the silver fulminate to about the same distance as the glass tubes; on the other hand, when the inner surfaces of the latter were slightly roughened by coating them with a film of fine powder, such as French chalk, they no longer transmitted detonation to anything like the distance which they did when the inner surfaces were in the normally smooth condition. Other very slight obstacles to the unimpeded passage of the gas wave through the tubes were found greatly to reduce the facility with which detonation could be transmitted by means of tubes; thus, when a diaphragm of thin bibulous paper was inserted into the glass tube about half-way between the two extremities, detonation was not transmitted, even with the employment of about six times the quantity of fulminate that gave the result with certainty under ordinary conditions.

Among several other interesting results furnished by an examination into the conditions governing and results attending the transmission of detonation by tubes, a remarkable want of reciprocity was found to exist between mercuric fulminate and gun-cotton. The latter substance is more susceptible to the detonative power of mercuric fulminate than of any other substance. The quantity of fulminate required to detonate gun-cotton is regulated by the degree to which the sharpness of its own detonation is increased by the amount of resistance to rupture offered by the envelope in which the fulminate is confined. From 20 to 30 grains are required if the detonative agent is confined in a thin case of wood, or in several wrappings of paper; but as small a quantity as 2 grains of the fulminate suffices to effect the detonation of compressed gun-cotton, provided the fulminate be confined in a case of stout metal (sheet tin) and be closely surrounded by being tightly imbedded in the mass of gun-cotton. If there be no close contact between the two, the quantity of fulminate must be very considerably increased to ensure the detonation of the gun-cotton, and, in attempting to transmit detonation from mercuric fulminate to gun-cotton by means of tubes, it was found necessary to employ comparatively very large quantities of fulminate in order to accomplish this, even through short lengths of tubes. But when the quantity of fulminate used reaches certain limits, the detonation may be transmitted from it to gun-cotton through very long lengths of tube. In applying gun-cotton, on the other hand, to accomplish the detonation of mercuric fulminate, it was found that this result could be attained, and through considerable lengths of tube (7 feet and upwards) by means of very much smaller quantities of gun-cotton than is needed of fulminate to induce the detonation of gun-cotton through the corresponding distances.

This want of reciprocity between two detonating agents corresponds to one even more remarkable, which was observed by the lecturer in his earlier investigations on this subject. In the first place it was found that the detonation of ¼ oz. of gun-cotton (the smallest quantity that can be thus applied) induced the simultaneous detonation of nitro-glycerine, inclosed in a vessel of sheet tin and placed at a distance of 1 inch from the gun-cotton; while with ½ oz. of the latter, the same effect was produced with an intervening space of 3 inches between the two substances. But on attempting to apply nitro-glycerine to the detonation of gun-cotton, the quantity of the former, which was detonated in *close contact* with compressed gun-cotton, was gradually increased in the first instance to ¾ oz. and subsequently even to 2 oz. without accomplishing the detonation of the latter, which was simply dispersed in a fine state of division, in all instances but one in a large number of experiments.

The force developed by the detonation of nitro-glycerine was proved to be decidedly greater than that of the fulminate, of which from 2 to 5 grains suffice for developing the detonation of gun-cotton, when it is in close contact with them. The non-susceptibility of gun-cotton to detonation by nitro-glycerine is therefore, it need scarcely be said, not ascribable to any deficiency in mechanical force suddenly applied when the nitro-glycerine is detonated.

(*To be continued.*)

INTELLECT IN BRUTES

FROM several additional letters which we have received on this subject we select the following:—

Mr. Claypole, of Antioch College, Ohio, writes:—A friend of mine is employed on a farm near Toronto, Ontario, where a horse belonging to the wife of the farmer is never required to work, but is allowed to live the life of a gentleman for the following reason: Some years ago the lady abovementioned fell off a plank bridge into a stream where the water was deep. The horse, which was feeding in a field close by, ran to the spot and held her up with his teeth till assistance arrived, thus probably saving her life. Was this reason or instinct? Again, a gentleman engaged in the business of distilling at Cincinnati has more than once told me that the rats in his distillery are in the habit of drinking any spirits spilt on the ground or left in open vessels, and that they often become, in consequence, so tipsy that they cannot run, and are easily taken by hand. Which is this?

Mr. J. J. Furniss, of New York, writes:—Since the publication of my letter (NATURE, vol. xix. p. 385) on the evidence of reasoning power in an elephant, afforded by the fact that he thatched his back with grass when exposed to the heat of the sun, I have received additional data bearing on the subject from Mr. W. A. Conklin, the superintendent of the Central Park Menagerie. I am informed by him that he has frequently observed elephants, when out of doors in the hot sunshine, thatch their backs with hay or grass; that they do so to a certain extent when under cover in the summer time, and when the flies which then attack the animals, often so fiercely as to draw blood, are particularly numerous; but that they never attempt to thatch their backs in the winter. This seems to prove that they act intelligently, and for the attainment of a definite end. It would be interesting to learn whether elephants in their wild state are in the habit of so thatching their backs. It seems more probable to suppose that in their native wilds they would avail themselves of the natural shade afforded by the jungle, and that the habit is one which has been developed in consequence of their changed surroundings in captivity. I am also informed by Mr. Conklin that when taken to the water in summer the elephants first sprinkle their bodies all over with water, and then quench their thirst. They never so sprinkle themselves in cold weather. Their reasoning in this case seems to be, "I cool my mouth by pouring water into it, now if I pour water over my back it will cool that also." Am I not justified in calling this "abstract" reasoning?

Mr. Charles Stewart, of Tighnduin by Killin, Perthshire, sends the following story:—A few years ago I kept a collie dog named "Bodach" at my farm, for herding the milk cows, and who recognised the dairy-maid as his mistress. On her directing him to keep the cows on a certain part of a field, he would lay himself down in the centre of a line fixed by him as the proper limit. Patiently and vigilantly he would remain in quietness until any of the cows passed his limit, when he would swoop down on the trespasser, take her by the heels, and drive her back. It was wonderful in how short a time the cows came to recognise and respect the arrangement. He also came to know some of the cows by name. One of them named "Aggi" required at certain seasons to be milked oftener than the others, and the dairymaid had only to say in Gaelic "Bodach, go and bring home Aggi," when he would start for the pasture, single out Aggi, and bring her carefully home.

O. J. H. sends the following:—An ordinary domestic cat was equally fond of a friend of mine and of myself. As a test, we resolved to try the following experiment. We each held a piece of bread, of the same size, shape, &c., above the eyes of the animal. He looked at each hand and its contents alternately, attempting to solve the problem of getting at the bread without exhibiting partiality for either of his friends. He at last seemed to decide upon an expedient, for, raising himself upon his hind legs, he simultaneously seized a piece of bread in each of his front paws, and conveyed the food thus obtained to his mouth. On repeating the experiment after a lapse of some time, no difficulty was experienced in dealing with the matter, as the expedient just mentioned was resorted to without a moment's hesitation.

Prof. Nipher, of Washington University, St. Louis, U.S., writes:—A friend of mine living at Iowa City, had a mule, whose ingenuity in getting into mischief was more than ordinarily remarkable. This animal had a great liking for the company of an oat-bin, and lost no opportunity, when the yard gate and barn-door were open, to secure a mouthful of oats. Finally the mule was found in the barn in the morning, and for a long time it was impossible to discover how he had come there. This went on for some time, until the animal was "caught in the act." It was found that he had learned how to open the gate, reaching over the fence to lift the latch, and that he then effectually mystified his masters, by turning round, and backing against it, until it was latched. He then proceeded to the barn-door, and pulling out the pin which held the door, it swung open of its own accord. From the intelligence which this animal displayed on many occasions, I am of the opinion that had not discovery of his trick prevented, it would soon have occurred to him to retrace his steps before daylight, in order to avoid the clubbing which the stable boys gave him in the morning. It may be added that this animal had enjoyed no unusual educational advantages, and his owners found it to their interest to discourage his intellectual efforts as much as possible.

The Rev. George Henslow endeavours to sum up as follows from the stories that have already appeared:—I am quite ready to admit that more than one instance (notably Dr. Frost's cat, which spread crumbs to catch birds, and which is paralleled by one mentioned in Wood's "Natural History," which "chirped" like a sparrow, and so enticed and caught them), if correctly stated, and *if the motive* of the animals could in every case be proved, will completely overthrow my supposition that animals never copy us with the same or a rational purpose. I cannot help thinking, however, that such cases are very rare. Moreover, I will abandon my notion of abstract reasoning, at least, as hitherto described, for I now think that what I meant by the want of the faculty would be better described as an impotence, or, at least, a feebleness of mind in concatenating correlative ideas; or, perhaps, a want of a receptivity of the suggestiveness of things will express my meaning. On the other hand, I still see no reason for believing that animals can conceive of a purely abstract idea. Thus, "V. I." says a mule would turn on a tap, but did not turn it off again. The reason I would suggest is that *wastefulness* being an abstract conception, the mule could not entertain it. If this be correct, we may now proceed a step further. The idea of a personal *Ego* is purely abstract; hence I am led to believe that no animals can be *self-conscious*, and as a direct consequence, they cannot be either moral or immoral, but are simply automata and non-moral. Like children, they can learn by being scolded, when they displease their master, so that a conscience similar to a child's can be produced in them;

yet they cannot naturally be moral. Thus, *e.g.*, self-interest is all in all with animals, but it can never lapse into selfishness, which is the *conscious* abuse of self-interest. We "punish" a dog, but we never look upon it as a criminal. So, too, no animal can ever act unjustly towards another, because it cannot be conscious either of justice or injustice. The abstract conceptions of righteousness and justice are only applicable to acts done *under a sense of righteousness and justice*. The same remark applies to personal immoralities; so that no animal can be immoral. That animals cannot entertain abstract ideas is not at all surprising, seeing how slow children are to do the same. A somewhat grotesque illustration will show this. A class of boys was asked what conscience was. None could explain it, so the teacher defined it as "something within you that tells you when you have done wrong." A boy at once exclaimed it was a stomach-ache. On inquiry it turned out that he had stolen and eaten some unripe fruit, and doubtless felt the *remorse* of conscience accordingly! If, then, my former position be qualified, I would restate it as corrected by the cases recorded as follows:—Animals reason as we do, but always in connection with concrete phenomena whether immediately apprehended by the senses, or present to consciousness through memory; but like children they are slow to perceive the suggestiveness of things. They have, morever, no power of conceiving truly abstract ideas. Hence they cannot be self-conscious, cannot conceive of God, and can neither be moral nor immoral, but are simply non-moral automata. On the other hand, that which rescues man from being an automaton pure and simple, is his power of conceiving of abstract ideas, which enables him to be self-conscious; consequently he can conceive of a personal, *i.e.* self-conscious Deity, so that he at once becomes a responsible being, and can be positively moral or immoral.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

AT a recent meeting of the governors of Owens College, Manchester, the Committee on the proposed University charter presented a report. It appears that "negotiations have been actively carried on with the Council of the Yorkshire College, Leeds, partly by letter and partly by means of interviews between members of the respective committees. The suggestions agreed to by the Council of the Yorkshire College, Leeds, provide that the Owens College shall be named in the charter establishing the University as the first college in it; that the president and the principal of the Owens College shall be the first chancellor and vice-chancellor of the new University; that its *locus* shall be Manchester; and that in the system of proportionate representation proposed for the governing and the executive bodies of the University, the Owens College shall in either case begin with the maximum number of representatives allowed by the scheme." To obviate objection to a local name, that of Victoria University is suggested. The report and draft memorial were approved of, and the Committee were requested to make arrangements for the presentation of the memorial to the Lord President of the Privy Council at as early a date as possible, and for carrying out the other suggestions of the report, which was passed.

THE British Medical Association are getting up a memorial to the House of Commons urging the immediate institution at Oxford of a thorough medical curriculum, on the same basis as the medical schools of other English towns, in the following subjects at least:—Human anatomy, physiology of man, general pathology, materia medica, clinical medicine and surgery for beginners, State medicine, including jurisprudence and public health.

SCIENTIFIC SERIALS

American Journal of Science and Arts, April.—An opening obituary notice of the distinguished botanist, Dr. Jacob Bigelow, who died in January, aged 92, is here followed by a note in which Prof. Marsh traces the connection between the two widely divergent forms of vertebræ of the toothed birds *Ichthyornis* and *Hesperornis*. In the former the articulation of the centrum is cup-shaped; in the latter the ends of the centrum are saddle-shaped, as in ordinary birds. The third cervical vertebra of Ichthyornis, however, has a transition form, affording a ready solution of the development of the modern avian vertebra from the fish-like. The order of development of vertebræ seems this: Biconcave vertebræ (fishes and amphibians), plane vertebræ (mammals), cup-and-ball vertebræ (reptiles), saddle vertebræ (birds).—The double stars discovered by Mr. Alvan G. Clark, which (except Sirius) have not been brought to the attention of astronomers generally, are the subject of a paper by Mr. Burnham.—Interesting details are furnished by Prof. Church of underground temperatures in the Comstock lode in Nevada, where are, apparently, the hottest mines in the world. (The rock in the lower levels seems to have a pretty uniform temperature of 130° F.)—Prof. Lesquereux contributes a review of Count Saporta's valuable work on the plants of the world before man, taking occasion to compare the essential characters of certain tertiary groups of the North American continent, in order to determine some points still under discussion as to their age.—Mr. Palsinger indicates a method of estimating the thickness of Young's reversing layer; and among other subjects dealt with are, the lower jaw of Loxolophodon and the presence of chlorine in scapolites.

Journal of the Franklin Institute, April.—We note here the following:—Reports of the Committee on Science and the Arts, on Ainsworth's automatic switch for railroads, and a machine for treating flax, hemp, &c.—Tests of a Baldwin locomotive, by Mr. Hill.—The Franklin Institute standard screw thread.—The Butler mine fire cut off, by Mr. Drinker. In the course of investigations described in this last paper, Mr. Drinker thought it established that coal *in situ cannot* be burned *en masse*, but that the walls of carbonaceous slaty rock inclosing solid coal *can* be burned or calcined *in situ*. The mining engineers who discussed his paper seemed generally to be of opinion that the slates in the old fire were not actually burned, but that the carbonaceous matter in them was rather subjected to a process of distillation.

THE *Jornal de Sciencias mathematicas physicas e naturaes* (No. xxiv., December, 1878) contains the following papers:—On the oblique projection of a circle, by L. P. da Motta Pegado.—Contributiones ad floram mycologicam lusitanicam, by F. de Thuemen.—Ornithological notes, by J. V. Barboza du Bocage.—On the birds of the Portuguese possessions in West Africa (continuation), by the same.—On electrical condensation and the condensing force, by A. A. de Pina Vidal.—On a new densimeter, by Virgilio Machado.

THE quarterly *Revue des Sciences naturelles* (tome vii. No. 4) contains the following original papers:—Morphological researches on the family of *Gramineæ*, by D. A. Gordon.—Note on the genital organs and the propagation of some *Limacidæ*, by S. Jourdain.—Observations on the destruction and the development of the ovigerous capsule of *Blatta orientalis*, by G. Duchamp.—Catalogue of the land and river molluscs of the Hérault department, by E. Dubrueil (continuation).—Note on the soil of Montpellier, by P. de Rouville.—Note on the Pyrenees of the Aude, by M. Leymerie.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 6.—"On the Characters of the Pelvis in the Mammalia, and the Conclusions respecting the Origin of Mammals which may be based on them." By Prof. Huxley, Sec. R.S., Professor of Natural History in the Royal School of Mines.

In the course of the following observations upon the typical characters and the modifications of the pelvis in the mammalia, it will be convenient to refer to certain straight lines, which may be drawn through anatomically definable regions of the pelvis, as *axes*. Of these I shall term a longitudinal line traversing the centre of the sacral vertebræ, the *sacral axis*; a second, drawn along the ilium, dorso-ventrally, through the middle of the sacral articulation and the centre of the acetabulum, will be termed the *iliac axis*; a third, passing through the junctions of the pubis and ischium above and below the obturator foramen, will be the *obturator axis*; while a fourth, traversing the union of the ilium, in front with the pubis, and behind with the ischium, will be the *iliopectineal axis*.

The least modified form of mammalian pelvis is to be seen, as might be expected, in the Monotremes, but there is a great difference between *Ornithorhynchus* and *Echidna* in this respect, the former being much less characteristically mammalian than the latter.

The distinctive features of the mammalian pelvis have been clearly indicated by Gegenbaur,[1] who points out that in mammals, in contradistinction from reptiles, "the longitudinal axis of the ilium gradually acquires an oblique direction, from in front and above, backwards and downwards. The part which represents the crista above thus becomes turned forwards, or more or less outwards, with increase of lateral surface, the acetabular part backwards and downwards; hence the ischium retains its original direction in the produced long axis of the ilium, and, at the same time, takes up a position in relation to the vertebral column similar to that which obtains in birds. The conditions of this position are, however, to be sought in factors of a totally different nature in mammals from those which produce it in birds; for, in the former, the ischium follows the changed direction of the ilium, whilst in birds the ilium has nothing to do with the matter, and the ventral elements of the pelvis appear to pass towards the caudal region, independently of the ilium."

On one point, however, I cannot agree with Gegenbaur's conclusions. He is of opinion that the ilium of mammals answers to the post-acetabular part of the ilium of birds, and that "the *crista ossis ilii* of mammals corresponds with the posterior edge of the post-acetabular part of the bird's ilium. Between the two parts, therefore, there is the difference of a rotation through an angle of almost 180°." On the contrary, it appears to me evident that the whole *crista ilii* in a mammal corresponds with the whole dorsal edge of the ilium in a bird or a reptile, and that the angle through which the iliac axis rotates amounts to not more than 90°. I cannot reconcile the contrary view either with the relations of the ilium to the sacrum, or with the attachment of the muscles.

On comparing the pelvis of *Ornithorhynchus* with that of a lizard, or that of a chelonian, it will be observed that the resemblance between the former and the sauropsidan pelvis is, in most respects, closer than that which it bears to the higher mammalian pelvis. In the reptiles both the pubes and the ischia unite in a ventral symphysis; the pubis has a strong pectineal process, which acquires very large dimensions in the *Chelonia*; the metischial processes are also often very strong. Nevertheless, there is an important difference, for in all these animals the iliac axis is either nearly perpendicular to the sacral axis, or slopes from above downwards and forwards; the obturator axis also inclines downwards and forwards. Hence in most *Lacertilia* and *Chelonia*, the pubes slope forwards very obliquely, while the ischia come more and more forwards.

In other words, such modifications of the pelvis as occur in the Lacertilia and the Chelonia are of an opposite kind to those which take place in mammalia.

The same thing is true of the *Crocodilia*.

Thus it appears to be useless to attempt to seek among any known Sauropsida for the kind of pelvis which analogy leads us to expect among those vertebrated animals which immediately preceded the lowest known mammalia. For, if we prolong the series of observed modifications of the pelvis in this group backwards, the "pro-mammalia" antecedent to the Monotremes may be expected to have the iliac and obturator axis perpendicular to the sacral axis, and the iliopectineal axis parallel with it; something, in short, between the pelvis of an *Ornithorhynchus* and that of a land-tortoise; and provided, like the former, with large epipubes intermediate in character between those of the lower mammals and those of crocodiles. In fact, we are led to the construction of a common type of pelvis, whence all the modifications known to occur in the Sauropsida and in the mammalia may have diverged.

It is a well-known peculiarity of the urodele amphibia, that each *os innominatum* consists of a continuous cartilage, the ventral half of which is perforated by a foramen for the obturator nerve, but has no large fibrous fontanelle or obturator foramen in the ordinary sense of the word. As the junction of the dorsal with the ventral moiety, the acetabulum marks off the iliac portion of the pelvic arch above, from the pubic and ischial regions below; and these are further distinguishable, even apart from their ossifications, by the position of the foramen for the obturator nerve and the origins of the muscles. In full-grown specimens of *Salamandra maculosa* the pelvis presents the following characters:—The iliac axis is slightly inclined forwards, while the iliopectineal axis is practically parallel with the sacral axis. The iliac ossification extends into the acetabulum, and forms a triangular segment of its roof with the apex downwards, exactly as in lizards. The posterior and inferior side of the triangle is separated by a thin band of the primitive cartilage from the upper edge of the similarly triangular cotyloid end of the ischial ossification, the anterior edge of which is vertical again as in lizards. Between this edge and the anterior and inferior edge of the iliac ossification there is a cartilaginous interspace, as in crocodiles, which represents the cotyloid end of the pubis. This cartilaginous part of the pubis gives rise to a pectineal process, which has the same position as in birds and in *Ornithorhynchus*. In the floor of the acetabulum the pubic ossification makes its appearance as a very thin lamina, which extends, underneath the pectineal process, inwards; and gradually surrounds the whole of the thickened transverse ridge of cartilage which corresponds with the pubis. The pubis is thus represented by an axis of cartilage surrounded by bone, and the thick inner extremities of the two pubes are largely united by fibrous tissue. The ischia are relatively large, and are united, partly by cartilage and partly by ligament, in a long symphysis. Their posterior and external angles are produced into short metischial processes. In one specimen I observed a distinct sutural line between the anterior curved edge of the right ischium and the corresponding pubis, while no such suture could be traced upon the other side.

The pelvic arch of *Salamandra*, therefore, contains all the elements which are found in the higher vertebrata, but the obturator fontanelle is wanting, and it seems to me that in such a pelvis we have an adequate representation of the type from which all the different modifications which we find in the higher vertebrata may have taken their origin.

In the lizards and the *Chelonia* the iliac and obturator axes have inclined forwards, and the epipubes have been reduced to such rudiments, as have been described in chameleons and in some tortoises.[1]

In the crocodiles, with the same general pelvic characters, the cotyloid end of the pubis retains its imperfectly ossified condition, while the epipubes represent the vastly enlarged rami of the salamandrine epipubis.

In the Ornithoscelida and in birds, the ilia elongate, but it is the modification of the pubes and ischia which is the most characteristic feature of the pelvis, and the epipubis vanishes.

In the Pterosauria and in the Dicynodonts, the salamandrine non-development of an obturator fontanelle persists; and, in the former, the sessile rami of the epipubis appear to be represented by the so-called marsupial bones.

Unless the like should prove to be the case in the Dicynodonts, it is in the mammalia alone that the subsacral portion of the ilium elongates backwards, carrying with it the pubis and the ischium, between which a large rounded obturator fontanelle is developed.

These facts appear to me to point to the conclusion that the mammalia have been connected with the amphibia by some unknown pro-mammalian group, and not by any of the known forms of Sauropsida; and there is other evidence which tends in the same direction.

Thus, the amphibia are the only air-breathing vertebrata which, like mammals, have a dicondylian skull. It is only in them that the articular element of the mandibular arch remains cartilaginous; while the quadrate ossification is small, and the squamosal extends down over it to the osseous elements of the mandible; thus affording an easy transition to the mammalian condition of these parts.

The pectoral arch of the Monotremes is as much amphibian as it is sauropsidan; the carpus and the tarsus of all Sauropsida, except the Chelonia, are modified away from the urodele type, while those of the mammal are directly reducible to it; and it is perhaps worth notice, that the calcar of the frogs is, in some respects, comparable with the spur of the Monotremes.

Finally, the fact that in all Sauropsida it is a right aortic arch which is the main conduit of arterial blood leaving the heart, while, in mammals it is a left aortic arch which performs this office, is a great stumbling-block in the way of the derivation of the mammalia from any of the Sauropsida. But if we suppose the earliest forms of both the mammalia and the Sauropsida to have had a common amphibian origin, there is no difficulty in the supposition that, from the first, it was a left aortic arch in the one series, and the corresponding right aortic arch in the other, which became the predominant feeder of the arterial system.

The discovery of the intermediate links between reptilia and

[1] "Beiträge zur Kenntniss des Beckens der Vögel," *Jenaische Zeitschrift* vi.

[1] Hoffman, "Beiträge zur Kenntniss des Beckens der Amphibien und Reptilien," *Nied. Archiv für Zoologie*, 1876.

aves, among extinct forms of life, gives every ground for hoping that, before long, the transition between the lowest mammalia at present known and the simpler vertebrata may be similarly traced. The preceding remarks are intended to direct attention to the indications of the characters of these pro-mammalian vertebrata, which the evidence at present forthcoming seems to me to suggest.

In the relatively large size of the brain, and in the absence of teeth, the only existing representatives of the Ornithodelphia present characters which suggest that they are much modified members of the group. On comparing the brain of *Echidna*, for example, with that of many marsupialia and insectivora, its relative magnitude is remarkable: and, in view of the evidence which is now accumulating, that the brain increases in size in the later members of the same series of mammalia, one may surmise that *Echidna* is the last term of a series of smaller-brained Ornithodelphia. Among the higher vertebrata I think that there is strong reason to believe that edentalous animals are always modifications of toothed forms.

Institution of Civil Engineers, April 22.—Mr. Bateman, president, in the chair.—The paper read was on dioptric apparatus in light-houses for the electric light, by Mr. James T. Chance, Assoc. Inst. C.E.

PARIS

Academy of Sciences, April 24.—M. Daubrée in the chair. —The following papers were read:—On the condition of the roadstead of Port Said, by M. De Lesseps. The bottom appears to have reached a state of equilibrium, and the dredging operations carried out annually will suffice to maintain this state. The sand deposits, opposed by dredging, are chiefly formed to the north and north-east of the large jetty, in a region reaching about 800 to 1,000 metres from its base. Beyond this, as also to the west, the deposits are more muddy, and are carried away by the action of the sea. M. De Lesseps also spoke hopefully of the Congress to meet on May 15, for determining the best course for an inter-oceanic canal (which he thinks will be achieved before the close of this century).—Complementary researches on the products of distillation of alcohols, by MM. Pierre and Puchot. The authors reproduced synthetically most of the phenomena observed, by operating on aldehydes.—On the navisphere, a nautical instrument, by M. De Magnac. This gives, without calculation, and in a few seconds, the names of the stars that are above the horizon at a given moment; also very approximately, the altitudes and azimuths of these stars; also the angle of route for going from one point to another by the arc of a great circle, and the distance between these points. The instrument has been tried on the steamship *Washington* with excellent results.—Experimental researches on the metallic grains of sporadosideric meteorites, by M. Meunier. The grains are essentially angular and branching, and do not seem to have passed through fusion. They often form envelopes round stony elements of cosmic rock. The Greenland masses of native iron (whose grains are of this character) cannot be thought the product of reduction of the dolerite by the lignite through which they have been erupted. M. Meunier considers them brought from a great depth with ordinary basalt, in which they had been embedded.—On the artificial production of bioxide of manganese, by M. Gorgeu. Artificial bioxide, having all the properties of polianite and pyrolusite, was got by heating, gently and long, at a temperature of 155° to 162°, nitrate of manganese in a glass phial placed in a bath of oil or paraffin. Other methods were tried without success. The authors are of opinion that, in formation of polianite and pyrolusite, the iron suspended in the very fluid mass of fused nitrate of manganese was decanted before decomposition of the nitrate occurred; and the same with all other powdery products mixed with the nitrate.—On tritungstates, by M. Lefort.—On the methodic employment of coloured glasses in achromatopsy, by M. Coursserant. May not the exclusive excitation of certain nerve elements of the retina cause to be produced and accumulated, in certain elements in repose, a quantity of work which will manifest itself in the form of variously coloured light, when these rested elements, solicited in turn, come into action?—Observations of Jupiter's satellites, at the Toulouse Observatory in 1878, by M. Baillaud. —Formation of a function, $F(x)$, possessing the property $F[\phi(x)] = F(x)$, by M. Appel.—Letter to M. Dumas on the apparatus of Lavoisier, by M. Truchot. The Conservatoire des Arts et Métiers contains about a dozen of Lavoisier's instruments, chiefly relating to synthesis of water and calorimetry. But this is not all that remains; his chemical laboratory and physical cabinet have been piously preserved by his family. They are now in possession of M. de Chazelles, at Canière, near Aigue-perse (Puy de Dôme), and M. Truchot has made an inventory of them, which he here gives briefly. Many of the instruments are of great interest.—Chemical function of anhydrous acetic acid, by M. Loir. It presents the general properties characterising aldehydes.—On nitrosoguanidine, by M. Jousselin. He indicates a method of obtaining it in considerable quantities, and describes several of its reactions.—On the value of certain chemical agents employed in dyeing with aniline black, by M. Witz. The proved inertia of chromium in mixtures with chlorates contrasts singularly with the marvellous energy of vanadium, the industrial use of which presents the greatest economical advantages.—On the formation of hail, by M. Oltromare. Suppose the temperature of a considerable cloudy mass (formed by cooling and condensation of saturated air and electricity keeping the molecules apart) to go down to −14°, implying a state of *surfusion*,—and the electricity of the mass suppressed by discharge, the molecules then clashing together will be changed into pieces of ice more or less coherent.—On the amyloid appearance of cellulose in champignons, by M. de Seynes.—On the mode of formation of biliary canaliculi in hepatitis, and the consecutive production of tubulated glands in the liver of the rabbit, by MM. Nicati and Richaud.—M. Jaubert claimed priority with regard to the MM. Henry's new catadioptric telescope. M. Faye pointed out, however, that MM. Henry did not seek to modify the optical power of reflectors by addition of a large refracting lens, but simply to close the tube so as to suppress movements of the interior air.—M. Larry presented the catalogue of the South Kensington Loan Collection (third edition), accompanied with a French Guide.

VIENNA

Imperial Academy of Sciences, March 6.—The following among other papers were read:—On the new recurrence of halotrichite and melanterite at Idria, by Prof. Zepharovich.—On the electrical perforation of glass, by Prof. Waltenhofen.—On the decomposition of formiate of ammonium at a high temperature, by Herr Andreasch.—On determination of the co-efficient of internal friction in viscous liquids by gravitation experiments, by Herr Schröttner.—On direct introduction of carboxyl groups into phenols and aromatic acids, by Prof. Senhofer and Dr. Brunner.—On facts of experience lying at the base of mechanics, by Herr Heller.—Muscular system of the extremities of the orang, by Prof. Langer.—On lacunar consumption of striped muscular fibres, by Prof. Klemmsiewicz.—Eruptive rocks of the western Balkans, by Prof. Niedzwiedzki.—Theory of the metallic thermometer, by Herr Jüllig.

March 13.—Remarks on the telephone, by Prof. Boltzmann.—On a new substance, nitroso-sulphhydantoin, by Prof. Naly and Herr Andreasch.—On resorcin-sulpho-acids, by Herr Fischer.

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