THURSDAY, JANUARY 15, 1880

ERASMUS DARWIN

Erasmus Darwin. By Ernst Krause. Translated from the German by W. S. Dallas. With a Preliminary Notice by Charles Darwin. Portrait and Woodcuts. (London : Murray, 1879.)

THE memory of this great man has suffered from the florid and spiteful biography written by Miss Seward. That she was animated by a feeling of bitterness towards Erasmus Darwin, engendered by disappointment, is clearly shown in these pages; she was an unsuccessful candidate for the post of his second wife, and she seems never to have forgiven him for his blindness towards her merits. A trustworthy life of the author of the "Botanic Garden" was therefore much wanted, and no one could have been better qualified for the task than his grandson, Charles Darwin. He has done his work so well and completely as to leave no room for any subsequent biography; further criticism there may well be, but the facts of the life of Erasmus Darwin can never be better put together, and they are as fully given as there is any need for. The critical essay by Herr Krause forms only little more than one-third of the modest volume, and is really an appendix to the life by Mr. Darwin.

Erasmus Darwin was born of a good family at Elston Hall, Notts, on December 12, 1731. He was educated at a school at Chesterfield, from which he went to St. John's College, Cambridge, and subsequently to Edinburgh to study medicine. In 1756 he settled at Lichfield as a physician, and married in 1757, his wife dying in 1770. He married a second time in 1781, when he settled at Derby, where he died in 1802. From his earliest years he seems to have had a taste for versifying and mechanics, and when very young he made experiments in electricity with a rude apparatus of his own invention. Mr. Darwin gives a most amusing letter addressed to his grandfather when at Chesterfield, by Susannah, the sister of the latter, in which she sets down in a very incongruous fashion the details of four days' fasting in Lent. The reply of Erasmus (ætat 16) was characteristic :—

"I fancy you forget in Yours to inform me y^t your Cheek was quite settled by your Temperance, but however I can easily suppose it. For y^e temperate enjoy an ever-blooming Health free from all y^e Infections and disorders luxurious mortals are subject to, the whimsical Tribe of Phisitians cheated of their fees may sit down in penury and Want, they may curse mankind and imprecate the Gods and call down y^t parent of all Deseases, luxury, to infest Mankind, luxury more destructive than y^e Sharpest Famine; tho' all the Distempers that ever Satan inflicted upon Job hover over y^e intemperate; they would play harmless round our Heads, nor dare to touch a single Hair. We should not meet those pale thin and haggard countenances which every day present themselves to us. No doubt men would still live their Hunderd, and Methusalem would lose his Character; fever banished from our Streets, limping Gout would fly y^e land, and Sedentary Stone would vanish into oblivion and death himself be slain."

Even at this early age is seen his leaning towards vegetarianism and abstinence from alcoholic drinks, which he subsequently carried into almost regular practice. This was not the only respect in which Erasmus Darwin was far ahead of his own time and even of ours. In sanitary matters he could read a lesson even to our advanced age, and with his mechanical genius he carried out his ideas in this respect into practice as far as the circumstances of the time would permit. He advocated the abolition of intra-mural interments, a rational treatment of the insane, radical reform in female education, and the abolition of slavery at a time when all the world, including the Society for the Propagation of the Gospel, regarded it as a divine institution. His little work on female education was translated into German, where it was regarded as an authority, and he carried out his ideas on the subject in the case of his own daughters, whom, for example, he taught to swim. He was a radical in politics, and a theist in religion, as his works amply testify, though his indiscriminating and bigoted contemporaries stamped him as an atheist. His friendship was wide, both in England and on the Continent, and included many of the most eminent men of his time. He was a man of great influence among his neighbours, and was specially beloved by the poor and needy, a common epithet coupled with his name being that of Benevolent. He was slightly irascible in temper, his massive face pitted from smallpox, he walked with a limp, and although he stammered in speech, he was one of the best conversationalists of his time. He soon acquired a good practice in Lichfield, and as a physician his fame reached George III., who wanted him to settle in London; but Darwin's desires in regard both to fame and income were moderate, and he preferred the quiet of Lichfield. His chief recreation was in tending eight acres of land near the city, which he converted into a botanic garden. Apart altogether from his position in the history of science, it will thus be seen that Erasmus Darwin was a man of unusual originality and independence of mind, who could rise far above the beliefs and customs of his time. But for us he is mainly interesting for the position his works hold in the history of the doctrine of evolution. We are inclined to think that had Erasmus Darwin not chosen to throw his ideas on this and other scientific matters into the form of verse, the theory itself and his claim to be the originator of it in its modern form would have been much sooner recognised. The works in which he embodied his speculations and theories are "The Botanic Garden," in its two parts, "The Loves of the Plants," and "The Economy of Vegetation," the latter, although the first part, having been published last; the former probably first appeared in 1788. Then followed the "Zoonomia" in 1794, soon after translated into German, French, and Italian; the "Phytologia" was published in 1800, and "The Temple of Nature, or the Origin of Society," the year after the author's death. In England, at least, where these works first appeared, they were treated mainly as poems, the scientific speculations which they contained, if referred to at all, being generally regarded as the mere fancies of a poet, or the dreams of a rhapsodist. As poems they had a reputation which must seem to the readers of to-day wonderful. Such men as Walpole and Edgeworth spoke of them with rapture, though the parody of the "Loves of the Plants," known as the "Loves of the Triangles," seems to have done much to destroy the reputation of the original.

Coleridge invented the term "Darwinising" to express his contempt for the speculations of the elder Darwin, and the *Edinburgh Review* treated his poems in its well-known "this-will-never-do" style. Still Darwin's poems contain many brilliant passages, yet we fear no reader of the present day would care to read them through merely as literary productions. Any one, however, who desires to master the history of the progress of scientific theory, must study them carefully; and this is what Herr Krause has done in order to be able to write the critical essay appended to the biography by Mr. Darwin, an essay which Mr. Dallas has turned into excellent English.

Herr Krause, then, claims for Erasmus Darwin that he is the real father of the doctrine of evolution in its modern form, and that much of the credit which has been ascribed to Lamarck is really due to his predecessor. No one can read Herr Krause's careful paper, fortified as it is with numerous extracts from the elder Darwin's works, without being convinced that the claim he upholds is just True, Darwin often saw as in a glass darkly, what his greater grandson has been able to see and to show us face to face. But when we remember the state of scientific theory in his time, and the scanty store of data at his command, we cannot but be struck with the real penetrative genius of the man, and wonder that he was able to see so much. His powerful and thoroughly scientific imagination helped him to leap over many difficulties, which the Darwin of to-day has been able to bridge by an abundance of fresh facts. As might be expected, the elder Darwin's ideas are sometimes crude and undeveloped; when he seems in a fair way to arrive at the full-blown ideas connected with the doctrine of evolution such as we have it now, he sometimes turns aside ere the goal is reached, and concludes with something that is only half the truth. Here is how Herr Krause speaks of him:—

"I was speedily convinced that this man, equally eminent as philanthropist, physician, naturalist, philosopher, and poet, is far less known and valued by posterity than he deserves, in comparison with other persons who occupy a similar rank. It is true that what is perhaps the most important of his many-sided endowments, namely, his broad view of the philosophy of nature, was not intelligible to his contemporaries; it is only now, after the lapse of a hundred years, that by the labours of one of his descendants we are in a position to estimate at its true value the wonderful perceptivity, amounting almost to divination, that he displayed in the domain of biology. For in him we find the same indefatigable spirit of research, and almost the same biological tendency, as in his grandson; and we might, not without justice, assert that the latter has succeeded to an intellectual inheritance, and carried out a programme sketched forth and left behind by his grandfather.

"Almost every single work of the younger Darwin may be paralleled by at least a chapter in the works of his ancestor; the mystery of heredity, adaptation, the protective arrangements of animals and plants, sexual selection, insectivorous plants, and the analysis of the emotions and sociological impulses; nay, even the studies on infants are to be found already discussed in the writings of the elder Darwin. But at the same time we remark a material difference in their interpretation of nature. The elder Darwin was a Lamarckian, or, more properly, Jean Lamarck was a Darwinian of the older school, for he has only carried out further the ideas of Erasmus Darwin, although with great acumen; and it is to Darwin, therefore, that the credit is due of having first established a complete system of the theory of evolution."

Herr Krause then proceeds to analyse "The Botanic Garden" and other works, in order to produce evidence of the claim he maintains on behalf of Erasmus Darwin. It is interesting to read in a note appended by Darwin to a verse in "The Botanic Garden," the following idea and first scheme of the theory of evolution:—

"'From having observed the gradual evolution of the young animal or plant from its egg or seed; and afterwards its successive advances to its more perfect state, or maturity; philosophers of all ages seem to have imagined that the great world itself had likewise its infancy and its gradual progress to maturity; this seems to have given origin to the very antient and sublime allegory of Eros, or Divine love, producing the world from the egg of Night, as it floated in chaos.'"

It is in the "Economy of Vegetation" that the well-known prophetic lines on the power of steam occur:—

"'Soon shall thy arm, Unconquer'd Steam, afar
Drag the slow barge, or drive the rapid car;
Or on wide-waving wings expanded bear
The flying-chariot through the fields of air.
—— Fair crews, triumphant, leaning from above,
Shall wave their flutt'ring kerchiefs as they move;
Or warrior-bands alarm the gaping crowd,
And armies shrink beneath the shadowy cloud.'"

Darwin goes on then to describe the formation of the earth, which he maintains was shot forth from a volcano in the sun, the formation of a nucleus, the precipitation of water, the formation of clouds, &c.:—

"In this connection the fossil marine animals also come under discussion; and after mentioning the singular circumstance that most fossil marine animals as, for example, the ammonites, are no longer found living, whilst the living animals do not occur in the fossil state, the author raises the questions, 'Were all the ammoniæ destroyed when the continents were raised? Or do some genera of animals perish by the increasing power of their enemies? Or do they still reside at inaccessible depths in the sea? Or do some animals change their forms gradually and become new genera?'"

How very near the now accepted truth is this! While he divined the principle of mimicry in plants, and speculated on the interesting subject of their fertilisation and their relation to insects, he here just missed the truth from his want of knowledge of facts; had he had as much power of patient observation as his grandson, he would have come nearer the truth in this matter. While he held even bold speculation to be of value to science, he distinctly recognised observation and experiment as the only true bases of scientific progress, as will be seen in his admirable address to the Philosophical Society of Derby, of which he was one of the founders; he defined a fool as "A man who never tried an experiment in his life." The fundamental idea of Darwin's "Zoonomia," it seems to Herr Krause—

"Is that in plants and animals a living force is at work, which, endowed in both with sensibility, is enabled *spontaneously* to adapt them to the circumstances of the outer world, so that the assumption of innate ideas, of divinely implanted impulses and instincts is rendered unnecessary, and even the process of thought appears attainable as the legitimate activity of a mechanical analysis and combination. All kinds of human knowledge originate from the senses, the action of which is

regarded as the chief source of knowledge, and is accordingly first of all investigated.

"As regards the apparently inborn faculties which young animals bring with them into the world, the author explains them by repeated exertions of the muscles under the guidance of the sensations and stimuli.

"The author very carefully studied this subject, which has been elaborated by his grandson with so much success, and deduces his formulæ especially from the *first* impressions of new-born creatures. The trembling of fear may perhaps be referred back to the cold shivering of the new-born infant; and weeping to the first irritation of the lachrymal glands by cold air, as well as by pleasant and disagreeable odours. That anger and rage are universally expressed by animals taking the position of attack, is immediately intelligible. As regards smiling and the expression of the agreeable sensations, the author refers them, as well as the feeling of the beauty of undulating lines and of rounded surfaces, to the pleasure of the first nourishment derived from the soft and gently rounded maternal breast."

Here also is a remarkable passage in which the principle of heredity is distinctly recognised:—

"'The ingenious Dr. Hartley in his work on man, and some other philosophers,' says Darwin, 'have been of opinion, that our immortal part acquires during this life certain habits of action or of sentiment, which become for ever indissoluble, continuing after death in a future state of existence; and add, that if these habits are of the malevolent kind, they must render the possessor miserable even in heaven. *I would apply this ingenious idea to the generation, or production of the embryon, or new animal which partakes so much of the form and propensities of the parent.*' And he continues as follows: 'Owing to the imperfection of language the offspring is termed a *new* animal, but is in truth a branch or elongation of the parent; since a part of the embryon-animal is, or was, a part of the parent; and therefore in strict language it cannot be said to be entirely *new* at the time of its production; and therefore it may retain some of the habits of the parent-system.'"

In the "Zoonomia" there are many passages we should like to quote, in which many of the doctrines associated at the present day with the name of the younger Darwin, are enunciated with wonderful clearness, even to sexual selection, the struggle for existence, and the survival of the fittest. Speaking of the weapons with which the males of animals are armed, and their contest for the possession of the female, he says:—

"'*The final cause of this contest amongst the males seems to be, that the strongest and most active animal should propagate the species, which should thence become improved.*'"

He concludes as follows the long passage in which this idea occurs:—

"'From thus meditating on the great similarity of the structure of the warm-blooded animals, and at the same time of the great changes they undergo both before and after their nativity; and by considering in how minute a portion of time many of the changes of animals above described have been produced; would it be too bold to imagine, that in the great length of time, since the earth began to exist, perhaps millions of ages before the commencement of the history of mankind, would it be too bold to imagine that all warm-blooded animals have arisen from one living filament which THE GREAT FIRST CAUSE endued with animality, with the power of acquiring new parts, attended with new propensities, directed by irritations, sensations, volitions, and associations; and thus possessing the faculty of continuing to improve by its own inherent activity, and of delivering down those improvements by generation to its posterity, world without end!'"

In his "Temple of Nature":—

"About the first hundred verses are devoted to a description of the pitiless struggle for existence which rages in the air, on the earth, and in the water, making the earth, with its incessantly warring inhabitants, like a vast slaughter-house:—

"'Air, earth, and ocean, to astonish'd day
One scene of blood, one mighty tomb display!
From Hunger's arm the shafts of Death are hurl'd,
And one great Slaughter-house the warring world!'"

Many more passages might be quoted all tending to prove what a really wonderful grasp the elder Darwin had of these doctrines, to which, through his grandson, his name is now so justly attached. But enough has been given to show that he deserves one of the highest places among those who have contributed to the progress of true science, and that the verdict of Herr Krause is amply borne out:—

"*That he was the first who proposed and consistently carried out, a well-rounded theory with regard to the development of the living world,* a merit which shines forth most brilliantly when we compare with it the vacillating and confused attempts of Buffon, Linnæus, and Göthe. It is the idea of a power working from within the organisms, to improve their natural position; and thus, out of the impulses of individual needs, to work towards the perfection of Nature as a whole."

Erasmus Darwin's system was in itself, as Herr Krause puts it, a most magnificent first step in the path of knowledge which his grandson has opened up for us. We ought to be grateful to Herr Krause for taking the pains he has done to show the true place of Erasmus Darwin in the history of science. There are many points in Mr. Darwin's intensely interesting, simple, and characteristic memoir we should have liked to notice, did space permit. The memoir is eminently candid and free from bias or anything like strong language, even when rebutting calumnies. Mr. Charles Darwin, we may say, is the son of Robert Waring Darwin, the third son of Erasmus by his first wife. A genealogy of the family is given which is of great interest in connection with the subject of hereditary genius, so well treated by Mr. Francis Galton, himself a descendant of the elder Darwin.

NORTH AMERICAN ETHNOLOGY

Contributions to North American Ethnology. Vol. iii. *Tribes of California.* By Stephen Powers. (Washington, 1877.)

"IT has been the melancholy fate of the Californian Indians to be more vilified and less understood than any other of the American aborigines. They were once probably the most contented and happy race on the continent, in proportion to their capacities of enjoyment, and they have been more miserably corrupted and destroyed than any other tribes within the union. They were certainly the most populous, and dwelt beneath the most genial heavens and amidst the most abundant natural productions, and they were swept away with the most swift and cruel extermination." Words such as these are now only too familiar to the ethnologist, and do not refer alone to the Californian Indians. As the ethnographic

facts are slowly accumulated upon which general anthropology is founded, there comes the conviction in many cases that we are dealing with customs and beliefs that have already reached the twilight of their existence. The crania in our museums will soon be the only physical relics of many races that can just be said to still exist. The desire has therefore long been expressed for the immediate production of faithful and exhaustive monographs, by competent observers, of many of these fast-fading anthropological shadows. To this demand America is now contributing largely; the present volume relates to the habits, customs, legends, religious beliefs, and geographical distribution of the Californian Indians, information collected by Mr. Powers during three years' residence and travel among those tribes.

This work being the result of personal experiences, and as such of the greatest value, other authorities are necessarily not much quoted, but the reader who would desire a more exhaustive treatment of the subject, can supplement his perusal of this volume by that of the fourth chapter of the first volume of Bancroft's "Native Races of the Pacific States," and it is possible that little more in the way of general Californian ethnological information can be desired. In the introduction it is acknowledged that there is a difficulty in drawing a fine distinction between the Californian Indians and their neighbours, and although there are some customs which appear to differentiate them, these alone are not sufficient, and Mr. Powers considers the "crucial test is that of language." Twenty-eight principal tribes and some smaller ones related thereto are recognised and receive separate treatment.

La Pérouse compared these Californians from their dark colour to the negroes in the West Indies, and Prichard, generalising from the accounts of Rollin and Kotzebue, boldly compared the shape of their heads and features to those of the "Negroes of Guinea, New Guinea, and the New Hebrides." Without however ourselves attaching importance to these assumed or superficial resemblances, the above writers might have adduced another fact for the supposed negroid type from Mr. Powers, who tells us that "all Californian Indians emit an odour peculiar to themselves."

The women of the Kā-rok tribes are described as having, apart from tattooed chins, "a piquant and splendid beauty," which has resulted in much intermarriage with the whites, many pioneers, "including four county officers and the only editor in Klamath County, having taken them to wives." This is another illustration of the pregnant fact, pointed out by many writers during the last few years, that with the Indian tribes in North America there has in many cases been much assimilation, and not only extermination. It is stated, however, that among "half-breed" children a decided majority are girls. "Often I have seen whole families of half-breed girls, but never one composed entirely of boys, and seldom one where they were more numerous." It would be interesting to know the proportion of male to female births among the Californians themselves. Franz Mayer has stated that there are more boys than girls in Upper California. Among the Nozi tribe, whose stature is short, the children "often remain mere dwarfs until they are ten or fifteen years old, when they start and shoot up suddenly eighteen inches or so."

These Californian Indians have a considerable power of botanical discrimination. Of the Patawat tribe it is stated, "there is not the smallest moss or lichen, not a blossoming shrub, or tree, or root, not a flower or vine, no forest parasite, bulrush, or unsightly weed, growing in the water or out, or any sea-weed or kelp, for which they have not a specific name;" and Mr. Powers asserts without hesitation that an average intelligent Indian, even if not a shaman, has at command a much greater catalogue of names than is possessed by nine-tenths of Americans. It is, however, incorrect to say, at p. 419, that savages have no systematic classification of botanical knowledge —"there are no genera, no species." The same remark has been made by Dr. Peschel in his "Races of Man." Dr. Hector, on the contrary, has informed us that the Maoris of New Zealand have not only distinct names for nearly all their plants, but generic names, by which they group plants according to their affinities, in a way impossible to most people who were not educated botanists. These Californians sometimes exhibit morbid anticipations of death. Amongst the Pomo, the authority of Robt. White is given for the fact that aged men and women in early expectation of their demise, frequently dig their own burial-place, and then repair thither daily for months together, eating their repast at the mouth of the grave; whilst amongst the Wintun, Mr. Powers relates that sometimes an aged woman will wear around her for months, the rope wherewith she is to be wrapped when a corpse.

Mr. Powers also contributes a most valuable addition to Californian folk-lore in the numerous legends that have been exhaustively collected and excellently narrated. It becomes a question, however, whether some of these are truly aboriginal, and uninfluenced by the teachings of the early Spanish missionaries; the "Legend of Gard," p. 80, seems to have had a very possible inspiration from Eastern sources.

This work is more intended for the careful study of the ethnologist than to afford extracts for a reviewer; and though naturally a great part of the information has been previously collated, there is not only much that is decidedly new to science, but the whole forms an excellent example of what can be observed and collected by an ethnologist for ethnology. It is likewise another token of "American Progress;" published by the "Department of the Interior," it is distributed to European students, without barter and without price. It is well illustrated, many patterns of facial tattoo marks being shown. A good map also accompanies the volume, the value of which is farther enhanced by an appendix on Linguistics, edited by J. W. Powell, in which a number of vocabularies are given.

W. L. DISTANT

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.]

Sunshine Cycles

IN NATURE, vol. xxi. p. 81, your correspondent, Mr. B. G. Jenkins, with the best intentions, yet does me too much honour

in quoting my name from a former volume of NATURE in 1872, as having then announced a cold wave of climate temperature being due to commence in 1878·8; or, as he is kind enough to say, the very day on which the severe winter of the years 1878 and 1879 did begin.

I must decline, at all events, the full honour, for these two reasons: *firstly*, the quoted datum was intended by me to signify the middle, not the beginning, of that cold wave; and as that lasted thirteen months, my date was between six and seven months too soon. *Secondly*, when I published again on the subject in the *Edinburgh Astr. Observations* of 1877, I indicated not 1878·8, but 1877·8 as the probable date of the then coming, now past, wave of cold, or erred much more than in 1872.

I have recently been looking into the reason of that latter calamitous failure, and find it clearly enough now on the plates of projections in that volume in this fact—that the then expected minimum of solar spots was inserted for 1877·5, or nearly two years sooner than sun observations have since then shown it to be. Rectifying, therefore, now that quantity, and in the then absence of our rock thermometers (which had first indicated the remarkable cycles of temperature in striking connection with the whole period, though not the details, of the sun-spots) using merely air-temperatures and re-drawing the curves, the middle epoch of the late cold wave comes out 1879·1, and of the next hot wave 1880·8.

Now these hot waves, I have always maintained, are more important, more regular, and more directly of solar origin, than the cold waves which are rather due to some indirect earthly effects of watery vapour and its transformations. It may be worth while, therefore, when we are now, as I believe, on the threshold of one of the heat-waves, to mention shortly the data on which the expectation is founded.

These are mainly the actual observations of no less than five occasions of such maxima of temperature occurring in a peculiar kind of dependence on successive sun-spot minima; or rather on the beginning, in each case, of the forces of a new spot cycle. The last such heat-wave was in 1868·8, or 1·8 year after the then last sun-spot minimum. Next before that in 1857·9, or 1·9 years after its previous sun-spot minimum. Before that, in 1846·4, or 2·4 years after the same solar test. Before that again, but then depending only on old atmospheric temperature observations, as our rock thermometers were not then existent, in 1834·5, or 1 year after; and still further back, in 1826·5, or also about 1 year after the then last sun-spot minimum.

These intervals from one to the other of the hot waves are by no means arithmetically equal, being 10·9, 11·5, 11·9, and 8·0. This last (really the earliest of the series) is a frightful inequality, but is borne out, *first*, by the sun-spot period of that cycle as given by M. Schwabe, having been also anomalously short; and *second*, by this remarkable testimony, which I have only just become acquainted with, in a pamphlet of most independent character by Sir Robert Christison, Bart., M.D., on tree-growth, read before the Botanical Society in Edinburgh:—

"The wonderful season of 1826, when warmth and sunshine, commencing with March, ended only with September, and when the summer was continuously such as to change in some respects the habits of the people."

The year 1826 was therefore a crucial case, not only for a maximum of temperature and sunshine in Scotland, but for its keeping such remarkable pace with the then anomalously shortened period of sun-spots. While the presently coming case of 1880 will equally prove, by what I have detailed above, that no certain success in weather predictions for several years beforehand can be hoped for, unless the dates of sun-spot minima can be also announced by authority beforehand to a very much smaller quantity than two years of error; and that no *mean* duration of the sun-spot cycle comes close enough to the fact of the large variations between one cycle and another. We must have therefore each cycle of sun-spots fixed by its own dates alone, and not smoothed away and improved out of creation by being made apparently conformable to others.

I have not yet seen, by those able men who believe they have traced sun-spots to planetary influences of position on the sun, any attempt, from the planetary places in almanacs, to compute the dates of all the solar minima of spots, say from 1825 to 1900. But something of that kind appears now to be necessary for the next steps of the science of the future.

PIAZZI SMYTH

15, Royal Terrace, Edinburgh, January 9

Cranial Measurements

IN the notice of my catalogue of crania which you have been good enough to insert in NATURE, vol. xxi. p. 222, your reviewer has given me credit for originality of method, to which I have no wish to lay claim.

1. In reference to the mode of taking the horizontal circumference, it is said that I pass "the tape line, not over the prominence of the glabella, as is customary with craniologists, but above it, around the supra-orbital line." The fact is, that the method which I have adopted, so far from being a deviation from what is customary, is that recommended in the valuable "Instructions Craniologiques," drawn up by Broca and published by the French Anthropological Society, and which is used, certainly by the large majority, if not by all the craniologists with whose writings I am acquainted.

2. With regard to the more important measurement of the antero-posterior diameter of the cranium, more important on account of its influence on some of the most characteristic indices, there are, unfortunately, still considerable differences of method, and it was only after very full consideration of the subject that I decided not to follow the French instructions, but to adopt the plan used by Rolleston in "British Barrows," by Barnard Davis in his "Thesaurus Craniorum," and by the majority of German anthropologists. So fully was I convinced of the expediency of the latter, that after having already measured the whole of the crania in the collection, and calculated the indices by the method which included the prominence of the glabella in the cranial length, I took the trouble to remeasure them, with the results given in the catalogue. The object being to obtain, as near as may be, an idea of the form of the brain-case, it appears desirable to exclude all extraneous projections which have no relation to this form. The impossibility of eliminating every source of fallacy, such as those occasioned by the varying thickness of bone or of diploë, is no argument against endeavouring to reduce them, as far as we can, to a minimum. The projection caused by the development of the frontal sinuses should certainly not be omitted in a complete description of a skull, but it no more affects the form of the cranium proper, than the prominence of the nasal bones or of the maxilla, which, important and instructive as they are from other points of view, are usually ignored in giving what is called the maximum length of the skull, although if the term is to be taken in its literal sense, they have as much claim to be included as the glabella or supra-orbital ridges.

Many other arguments might be adduced and authorities given for the usage I have adopted, but I will bear in mind your request for brevity.

W. H. FLOWER

Royal College of Surgeons, January 11

"Why the Air at the Equator is not Hotter in January than in July"—Freezing of the Neva

IN NATURE, vol. xxi. p. 129, Mr. Croll gives his reasons why the equator is not much warmer in January than in July, notwithstanding the greater nearness of the sun at the former season. To state the case briefly, he, having recalled the fact that the whole earth is colder in January than in July, because in the former the cold winter of the northern (or principally land) hemisphere coincides with the mild winter of the southern (or principally water) hemisphere, he continues: "Consequently the air which the equatorial regions receive from the trades must have a higher temperature in July than in January. The northern is the dominant hemisphere; it pours in hot air in July and cold air in January, and this effect is not counterbalanced by the air from the opposite hemisphere. The mean temperature of the air passing into the equatorial regions ought therefore to be much higher in July than in January, and this it no doubt would be were it not for the counteracting effects of eccentricity." And further: "There is another case which must also tend to lower the January and raise the July temperature of the equator: the northern trades pass farther south, and consequently cool the equatorial regions more during the former than the latter season."

I maintain that there is no such influence of the northern trades on the temperature of the equator, because they scarcely anywhere reach it, and then because the lower latitudes of the northern hemisphere are not colder in January than those of the southern hemisphere in July. *In the Atlantic the northern trades do not reach the equator at all in January*, but only in February, March, and April, and this but in the western part of the ocean.

The same may be said of the Pacific in its eastern part, where alone the trades are regular. In the Western Pacific, as well as in the Western Indian Ocean, I admit that air from the northern hemisphere reaches to the equator and somewhat beyond in January, but not that this tends to give the equator a lower temperature in this month than in July. According to Dove, the mean temperature of 10° N. in January is 77°·2; of 10° S. in July, 76°·1.

So far as the temperature of the equator is concerned, the southern is the dominant hemisphere, and the equator is certainly cooled by winds coming from the south. If the equator is not everywhere warmer in January than in July, this is caused by the rainy season, which on the equator, and even a few degrees north of it, generally coincides with the southern summer. Where the rains are not very heavy, as, for example, on the Isle of St. Thomas, West Africa, we have: January, 78°·3; July, 75°·7; at Padang, Sumatra, where the rains are exceedingly heavy all the year, there is scarcely any difference at all between the months. Somewhat to the south of the equator, where to the difference in the nearness of the sun is added a much greater height above the horizon in January, we have—

	Jan.	July.	Rainy Season.
Amboina, Molucca Islands, 4° S.	80·9	77·4	May to August.
Batavia, Java, 6° S.... ...	77·7	78·8	December to February.
Pernambuco, Brazil, 8° S.	80·6	75·0	April to July.

so, by the first-rate observations of Batavia, it is established that, so far as 6° S., January is 1°·1 colder than July, because the former is very rainy, while the latter has little rain. Even to 9° lat. N., July is colder than January, if the former has much more rain, so for example—

Island of Fernando Po, W. Africa, 4° N.	79·9	76·5	March to November.
Gondokoro, Upper Nile, 5° N.	81·3	75·7	April to August.
Freetown, W. Africa, 8½° N.	80·4	77·0	June to October.

Thus, in the lowest latitudes of the northern hemisphere, we find differences amounting to 5°·6, while in the southern greater differences than 1°·1 are not known, which may, to a certain degree, be ascribed to the nearness of the sun in January.

I think I have proved that, as to what we call the temperature of the air (really that of the lowest stratum), it is, on the equator and a few degrees north and south from it, far more influenced by the yearly distribution of clouds and rain than by the different amount of heat received from the sun. The result would be different if we knew the temperature of the whole stratum of air. The heating of the upper surface of the clouds by the sun, and especially the heat liberated in the condensation of water must give to the higher strata a superior temperature than that they had in the dry season; in other words, the decrease of temperature with elevation is much slower during the rains than in the dry season, as was shown for India by Mr. Blanford. This is true for other regions, and where the sky is cloudy and rains abundant in the greater part of the year, the temperature of the whole air may yet be higher than in drier climates, where the soil and the lower stratum of air are hotter.

I do not agree with Mr. Croll in what he states at the end of his letter as to the effect of winds in cooling the equatorial regions and rendering them habitable, as they would be too hot for man without the cool air brought from the temperate regions. I think Mr. Croll has enormou ly over-stated the effects of winds on the temperature of the equator. The extent of the tropical zone is so great, its temperature so very near to that of the equator, the winds which blow across it so gentle, that I consider the effect of the winds from the temperate regions in directly cooling the temperature of the equator to be nearly imperceptible. The following is a good illustration:—Nowhere is the winter temperature so low near the tropics as in Southern China, for example, in January, Canton, 55°·6, Victoria, Hong-Kong, 59°·2. Yet Saigon, in Cochin China, but 11° to the south of Hong-Kong, and subjected to the full force of the north-east monsoon from the China seas, has a January temperature above 77°. Clearly the thermal effect even of the cold winter monsoon is scarcely perceptible farther south.

I consider water to be the only direct cause of the mildness and uniformity of equatorial temperatures, and this in three ways—(1) by the great heat-capacity of water; (2) by the clouds which interpose a screen between the sun and the surface of the earth; (3) by the evaporation of rain-water by the soil and plants.

The first cause is especially powerful on the ocean, while the two latter act especially on land, even very far from the sea. If it was not for the clouds and evaporation, how could we explain, for example, the absence of great heat (hottest month, 78°·6) at Iquitos, on the Amazons, 4° S., and more than 1,000 miles from the Atlantic, where the winds are generally weak?

As to the winds, I admit of their effect in this case; but (1) in causing ocean-currents, and thus removing the heated water from the equator; (2) in spreading the cold air from over the cold currents over a greater distance. The latter is the cause of the low temperature in the equatorial regions of the Eastern Atlantic and Eastern Pacific.

Where the sky is clear and humidity and rains deficient, very high temperatures of the air are attained, even at a great distance from the equator (10°–30°) and this notwithstanding winds of considerable force blowing from cooler regions. So, for example, the north winds blowing in the summer in the Sahara, and coming from the cooler Mediterranean, are certainly stronger than the trades of the ocean and yet do not prevent the desert from attaining a higher temperature than known in any equatorial region.

In the same number of NATURE you committed an error by giving the dates of freezing of the Neva in *old style*. The dates in *new style* are: Mean day of freezing, November 25, earliest October 28 (1805), latest (not quite certain), January 9 (1711), next latest December 26 (1826); mean day of opening, April 21, earliest, March 18 (1822), latest, May 12 (1810); number of days open, 218, least, 172 (1852), greatest, 279 (1822).

A. WOEIKOF

Sékpaleruaya 8, St. Petersburg, December 5-17, 1879

Hearing through the Mouth

THE principle of the so-called "Audiphone," described in NATURE, vol. xxi. p. 243, is by no means a new discovery, although the application of it may be novel. It has long been known that sounds may be conveyed to the auditory nerves through the mouth when the drum of the ear is defective in its action, although the principle has, perhaps, been little acted upon by aurists. Mr. Rhodes's system is to press the edge of a vibrating metal disk against the upper teeth, and "the vibrations thus taken up by the disk are transmitted through the teeth and bones of the skull to the auditory nerve." (?) Such a remedy will, in many cases, be thought more inconvenient than the defect, and it is by no means necessary thus to jar the teeth and the bones. Although I am not deaf, some years ago I practised the listening to very feeble sounds through the mouth instead of by the outward ear, at the recommendation of the late Sir Charles Wheatstone. The inducing cause was to verify by experiment the true character and the notes of resultant tones, or Tartini's tones, about which no two authors had agreed. Sir Charles lent me one of his symphoniums—little instruments made like his concertinas, except that they were blown by the mouth directly upon the metal springs instead of by bellows. According to his directions I stopped my ears lightly with cotton, but pressed it into the *concha* with a thumb upon the lip of each ear. The little instrument was supported by my third and fourth fingers, leaving the notes to be touched by the first and second fingers of each hand. By thus excluding external sounds I could hear the deep and soft resultant tones to perfection; the instrument should not be tempered because they result from coincident vibrations of the notes sounded above. In these experiments I touched the symphonium as lightly as possible with elongated lips, the cavity of the mouth receiving the sounds. The teeth were covered by the lips.

WM. CHAPPELL

Strafford Lodge, Oatlands Park

Intellect in Brutes

THE numbers of NATURE containing the interesting discussion on this subject have only lately reached us, and it is late to bring forward anything on the question, yet the readers of NATURE will be interested in two instances of "calculation" on the part of wild birds that I have noticed. Some years ago I was overlooking a penguin "rookery" as it is called, at the Falklands, and watching the goings on of the numerous colony below me. It was breeding season, and the birds were sitting on their eggs on

the bare earth, crowded together with hardly walking room between them. Amongst the birds stepped a pretty Sheath-bill (*Chionis alba*) with a quiet jaunty stride, picking what he could, and apparently perfectly indifferent to the motions of the penguins, who drove at him with their beaks as he passed, but never struck him. I saw him pass and repass one bird always just out of reach, till the bird could stand it no longer, but reached off her nest about an inch to strike him; he was still just out of reach and busy with something, apparently not noticing the penguin; she reached further, he crossed her again, still just out of reach, and this went on till he had drawn her about two feet from the nest, then in one stride he was beside the egg, had punched a hole, and was sipping the contents before the slow penguin could turn and hop back to save it; he again led her away by the same manœuvre and increased the hole and got a greater part; a third time he led her off and was eating the egg when he was driven right away by another penguin, who was wandering at liberty, the mate, I suppose, being on turn on the egg. The proceeding on the part of the sheath-bill was a perfect trap for the poor foolish old penguin.

The other instance I will give occurred in the Pacific, where some albatross were circling about, and frequently settling on the water in flocks; some sharks were about, and I watched to see if any albatross would be pulled down; then I noticed a cordon of sentries round the flock, who were relieved at times from the flock, a single bird going out and sitting near the sentry who flew in. Whenever a shark's fin approached the sentry he flew in, and the whole flock took up new position. Here was distinct organisation. The ship was going very slowly through the water, and I was able to study the whole carefully. J. P. MACLEAR

H.M.S. *Alert*, Straits of Magellan, November 5, 1879

THE following account of an incident in the early life of a South African baboon may not be out of place in your journal The person who witnessed it was a very trustworthy native attached to Bishopstowe, Natal, and who is employed from time to time to get game for the house. He used to find it an agreeable addition to this duty to collect objects of natural history for the cabinet as well as for the table. He was fully alive to, and took a keen interest in, what went on among the animals of all kinds, and was much struck with what he saw take place one day at the bottom of a little *iDonga*, or dry watercourse, over the brink of which he peeped, on the slope of a table-mountain, the grand object that lies in the front of Bishopstowe, some twelve miles distant.

It was a hot day, and a number of baboons were sunning themselves along the bottom of the *iDonga*. They lay upon their backs, with half-closed eyes, rubbing their stomachs in a state of placid enjoyment. Two or three young baboons had wandered to a little distance down the *iDonga*, searching for scorpions from stone to stone just below them. They were not very successful, and it did not appear that their movements were of much concern to their elders. Presently, however, one of the young ones, turning up a stone, lit upon a particularly fine and fat scorpion, which, with a furtive glance round at his elders, he seized and popped into his mouth, having first pinched off the sting. He at once proceeded to turn the stone over again with great assiduity, as though in further unsuccessful search for scorpions. He had not escaped notice, however, for down the gully in a sluggish roll came a great baboon, who seized the young one by the scruff of the neck, shaking him vigorously until the plump morsel dropped from his pouch. Having gobbled this up, the elder baboon at once regained his lounge, and all went on as before in the sleepy hollow.

London, January 8 FRANCIS E. COLENSO

Notes on the Papuans of Maclay Coast, New Guinea

THE articles on the above subject which have appeared in NATURE, vol. xxi. pp. 204, 226, have been read by me with great interest in consequence of the resemblance which certain of the customs therein described have to some which have come under my observation among the inhabitants of the Andaman and Nicobar Islands.

With regard to the custom of the relatives of deceased persons in the Andamans ornamenting and carrying about the skulls of the departed, which is alluded to in a note on p. 205, I believe I may claim to have first described and figured such a skull. My paper entitled "On a Visit to the Andamanese Home, Port Blair, Andaman Islands" (of which I inclose a copy), was published in the *Proceedings* of the Royal Irish Academy for 1871. In it I mentioned, together with some other facts, that I had witnessed the process of making flakes from a piece of bottle-glass which I saw subsequently employed for shaving.

An Andamanese necklace made of human clavicles and turtles' ribs is now in my possession, and I believe human finger and toe bones are also sometimes strung together and worn round the neck. In reference to the ideograph discovered by Mr. Maclay among the Papuans I would refer to a paper "On Nicobarese Hieroglyphics or Picture Writing," which I communicated to the pages of the *Indian Antiquary* (Bombay) in the year 1875.

The screen which is figured is one out of many which I saw in the Nicobar Islands. It consists of the spathe of a palm and is covered with representations, done in vermilion, of men (in various attitudes), pigs, fish, houses, canoes, weapons, &c., &c. It would take up too much space to give details of it here, but I may state that my conclusion regarding it was that it was the pictorial record of some past event. Both skull and screen are described, but unfortunately not figured, in my recently-published work, "Jungle Life in India."

To any of your readers who may be specially interested in the matter I shall be happy to forward, on application, copies of the papers above mentioned and photographs of the screen so far as the numbers available for the purpose will go. V. BALL

37, Northumberland Road, Dublin, January 9

The Word "Telegraph"

THE word "telegraph" appears to have been naturalised in our language at a much earlier date than that given by Mr. Warren de la Rue in his letter in NATURE, vol. xxi. p. 226. There are several references to the apparatus in the *Gentleman's Magazine* for July, December, 1794, and the next three volumes. At first the word appears in its French form with the final "e," but the sign of its foreign origin soon disappears. Under the date January 28, 1796, we find amongst "Domestic Occurrences" a paragraph stating that "a telegraph was this day erected over the Admiralty." This, I think, was removed about thirty years ago. In case your correspondent should wish to verify the references in the indexes to the *Gentleman's Magazine*, I may point out that there are two pages numbered 106 in the volume for January to June, 1795, and that there is an article at p. 1176 of that for July to December, 1794, not mentioned in the index.

Watt, in his "Bibliotheca Britannica," gives a still earlier reference to the word in R. H. Gower's "Theory and Practice of Seamanship," but there is no mention of a telegraph in the first edition of that work, published in 1793.

The word occurs many times in Dr. Thos. Young's "Lectures on Natural Philosophy," 1807, and some interesting information on the subject may be found in Gregory's "Treatise of Mechanics" (2nd ed), vol. ii. p. 434 (London, 1807), where several sorts of telegraphs are described.

H.M. Patent Office, January 12 RICHARD B. PROSSER

Stags' Horns

THOUGH, no doubt, as Mr. Stokoe suggests, many antlers are picked up and sold to knife-handle makers, or, if they happen to be good ones, used for "making up" deers' heads, yet many, I believe, are really eaten by the deer themselves. I have never myself seen a deer engaged in eating a fallen antler, nor, though I have more than once found cast horns on the hills, did the latter present any appearance of having been gnawed.

All the hill men will tell you, however, that it is a well-known fact that red-deer eat the horns that are shed every year, and the late Sir Thomas Moncreiffe once told me that he watched a hind—a cervine Delilah—gnawing the tips of the tines of the horns of a stag that was lying beside her, and which he afterwards shot. In Blair Castle there is a magnificent stuffed stag—"Tilt"—which was reared by the late Duke of Athole, who fed it upon, amongst other things, ground deer-horns. As tame stags often do, "Tilt" became dangerous, and had to be killed when he was eight or nine years old. In size he far surpassed any of the wild red-deer, and had most magnificent antlers. As each year's antlers fell off they were preserved, and form an interesting and instructive series.

Considering how fond cows are of gnawing bones, and also how they will eat any woollen garment they can get at, there does not seem to be anything very remarkable in the fact of red deer consuming shed horns. F. BUCHANAN WHITE

Perth, January 2

VISUALISED NUMERALS

I HAVE lately been occupied in eliciting the degree and manner in which different persons possess the power of seeing images in their mind's eye, and am collecting a large and growing store of materials, partly of verbal answers made by friends to my inquiries, but principally by means of written replies to a printed list of questions that I am distributing. The subject bears in many ways upon psychological and ethnological studies, and I should be glad if the present memoir upon one particular branch of it should induce correspondents to furnish me with authentic information of the kind I seek.

The various ways in which numerals are visualised is but a small subject, nevertheless it is one that is curious and complete in itself. My data in respect to it are already sufficiently numerous to be worth recording, and they will serve to show that parallel results admit of being arrived at in other directions.

I may begin by mentioning one or two general experiences. I have been astonished to find how superior women usually are to men in the vividness of their mental imagery and in their powers of introspection. Though I have admirable returns from many men, I have frequently found others, even of the highest general ability, quite unable for some time to take in the meaning of such simple questions as these. "Think of some definite object,—say your breakfast table, as you sat down to it this morning, and consider carefully the picture that rises before your mind's eye. Is the image dim, or fairly clear? Is its brightness comparable to that of the actual scene? Are the objects sharply defined? Are the colours quite distinct and natural, &c.?" On the other hand, I find the attention of women, especially women of ability, to be instantly aroused by these inquiries. They eagerly and carefully address themselves to consider their modes of thought, they put pertinent questions, they suggest tests, they express themselves in well-weighed language and with happy turns of expression, and they are evidently masters of the art of introspection. I do not find any peculiar tendency to exaggeration in this matter either among women or men; the only difference I have observed between them is that the former usually show an unexpected amount of intelligence, while many of the latter are as unexpectedly obtuse. The mental difference between the two sexes seems wider in the vividness of their mental imagery and the power of introspecting it, than in respect to any other combination of mental faculties of which I can think.

Another general experience is that the power of seeing vivid images in the mind's eye has little connection with high or low ability or any other obvious characteristic, so that at present I am often puzzled to guess from my general knowledge of a friend, whether he will prove on inquiry to have the faculty or not. I have instances in which the highest ability is accompanied by a large measure of this gift, and others in which the faculty appears to be almost wholly absent. It is not possessed by all artists, nor by all mathematicians, nor by all mechanics, nor by all men of science. It is certainly not possessed by all metaphysicians, who are too apt to put forward generalisations based solely on the experiences of their own special ways of thinking, in total disregard of the fact that the mental operations of other men may be conducted in very different ways to their own.

I have much to say on this and cognate topics which I pass by on the present occasion, that I may at once proceed to the subject of this paper. The first section of it is of minor interest and may be quickly dismissed. It is the power of mentally seeing numerals, of holding them fast in the field of view, of perusing them when there, and of working sums by mental imagery in the same form as that in which they are usually carried on with pen and paper.

Here is a well marked case of the power of visualising numerals. The writer is an office-bearer of one of our scientific societies :—

1. If words such as fifty-six be spoken, I most clearly, easily and instantly visualise the figures. I do so almost automatically. I perceive that when I speak the word "thousand" or hear it spoken, the figures at once group themselves together. I find it quite impossible to think of the date of a year without remembering and visualising the figures, though I express myself in words. The figures are always printed; in type and size they resemble those commonly used for the headings of newspapers. I cannot, however, appreciate a back-ground, the figures appear simply in space. I think that by practice and concentration I could hold fast many figures.

The next is by a friend who has a most tenacious memory for numerical administrative details :—

2. I can see and mentally retain many figures, and can multiply four figures by four figures without practice, the operation proceeding visibly in my mind like a sum upon paper.

The following is by a school-boy who is a near relation of a man of the highest mark in science :—

3. I can visualise a fairly long line of figures, and I do mental sums by putting down the working of them in my mind's eye, up to square roots with two figures in the root, and in algebra, to simple quadratics.

A schoolmistress writes :—

4. I can retain several figures in my mental view and work examples, seeing every figure in the process.

A late Fellow of Trinity College, Cambridge, states :—

5. All arithmetical processes performed mentally, are exactly the processes I should perform on paper.

It must not, however, be imagined for a moment, that the processes of mental arithmetic are necessarily wholly dependent on the faculty of visualising numerals. Here is a good instance to the contrary. The writer is the author of a valuable work on a branch of Mental Philosophy :—

6. The numerals are merely ideal sounds [to me], not ideal sights in any way. I have, or used to have, very considerable powers of mental arithmetic and mental algebra, but always used in thought the sounds of the signs. In the process I always forgot every step as soon as I had reached the result of that step.

This last sentence is exceedingly suggestive, and reminds one that many so-called "unconscious" acts are not really unconscious, but are acts characterised by an exceedingly brief and evanescent period of consciousness.

The processes of mental arithmetic are commonly dependent on the representation of more than one sense, as in the following instance :—

7. I can multiply with effort four figures by four; but partly only by images, chiefly by memory.

I am as yet unable to determine the percentage of persons who possess in the various degrees, the power of visualising numerals, because my returns are chiefly derived from persons who are exceptionally gifted. An excellent way of obtaining average returns to psychological questions would be by the help of schoolmasters, who have an admirable field of psychological research immediately before them, which they wholly neglect. If a hundred boys in a large school could be set simultaneously to answer such questions as those I am putting, after their masters had clearly explained their purport to them, and had taken common precautions to insure independent replies, and to sift away lax and untrustworthy statements, the thing would be effected by a single stroke, and both boys and masters would enjoy the satisfactory feeling of having accomplished a substantial piece of scientific research.

I have many curious cases of colour association with the various numerals, but shall only give a very few instances of them, and those incidentally, in the present paper. I shall also abstain at present from speaking of the many different ways in which dates, days of the week, and months of the year are apt to be visualised.

The topic to which I especially wish to direct attention, is the innate and hereditary tendency of certain persons to see numbers in definite and constant arrangements or schemes, whose various characters will be easily understood from the extracts I am about to give and by the accompanying illustrations, which are reductions to a small scale of the pictures I have received, with a necessary sacrifice of detail in a few cases.

The simplest instances do not seem to be the commonest; thus, I have very few indeed that could be classed with the following :—

8. When a child, I counted by means of imaginary cards from ace to ten. My little boy in the same way, used an imaginary domino.

Or this :—

9. I picture numbers in groups, thus 5 is sometimes ⁘, sometimes ⋰, 8 is ⁞⁞, 7 is ⁞⋮, 100 is ten rows of ten.

I may as well give the remainder of this communication here; it is written by a lecturer upon mental philosophy. He says :—

10. The numerals 1, 2, 3, 4, &c., from the part they play in the multiplication table, have been personified by me from childhood. 9 is a wonderful being of whom I felt almost afraid, 8 I took for his wife, and there used always to seem a fitness in 9×9 being so much more than 8×8. 7 again is masculine; 6, of no particular sex but gentle and straightforward; 3, a feeble edition of 9, and generally mean; 2, young and sprightly; 1, a common-place drudge. In this style the whole multiplication table consisted of the actions of living persons, whom I liked or disliked, and who had, though only vaguely, human forms.

The schemes in which numerals appear are usually fantastical and sometimes very elaborate. I will (by permission) give the name of the writer of the first instance about to be adduced, on account of the hereditary interest that is attached to it. It is by Mr. George Bidder, Q.C., a son of the late eminent engineer, who was known in early life as the calculating boy. Mr. George Bidder inherits much of his father's marvellous power of mental arithmetic, being able, though not with equal precision and rapidity, to mentally multiply fifteen figures by another fifteen figures. This faculty has been again transmitted, though in an again reduced degree, to the third generation. (*See* letter in the *Spectator*, December 28, 1878, also the early numbers of that paper in 1879.)

He writes to me as follows :—

11. One of the most curious peculiarities in my own case, is the arrangement of the arithmetical numerals. I have sketched this to the best of my ability. Every number (at least within

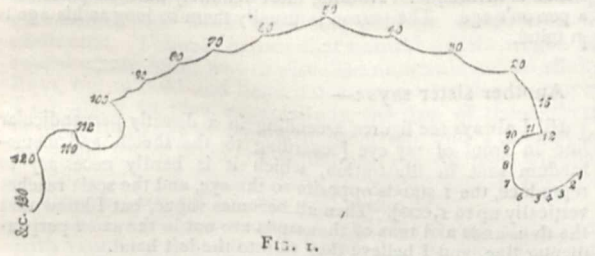

Fig. 1.

the first thousand, and afterwards thousands take the place of units) is always thought of by me in its own definite place in the series, where it has if I may say so, a home and an individuality. I should, however, qualify this by saying that when I am multiplying together two large numbers, my mind is engrossed in the operation and the idea of locality in the series for the moment sinks out of prominence. You will observe that the first part of the diagram roughly follows the arrangement of figures on a clock-face, and I am inclined to think that may have been in part the unconscious source of it, but I have always been utterly at a loss to account for the abrupt change at 10 and again at 12.

It occurs to me that the change is probably due to the wrench given to the mental picture of the clock dial in order to make its duodecimal arrangement conform to the decimal system, and that the same action is repeated at 110.

The next diagram exhibits the most compact of all the mental schedules which I have as yet received :—

12. The representation I carry in my mind of the numerical series is quite distinct to me, so much so that I cannot think of any number but I at once see it (as it were) in its peculiar place in the diagram. My remembrance of dates is also nearly entirely dependent on a clear mental vision of their *loci* in the diagram. This, as nearly as I can draw it, is the following :—

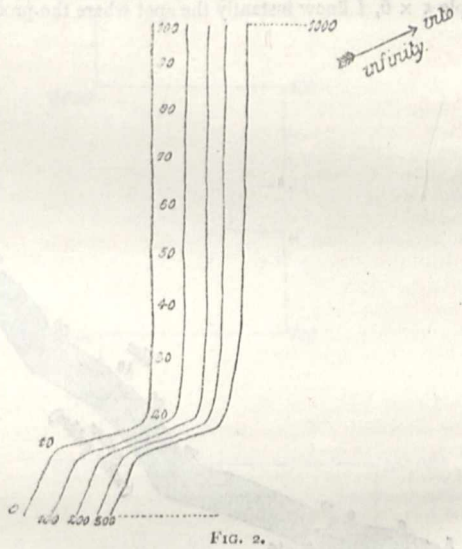

Fig. 2.

It is only approximately correct (if the term "correct" be at all applicable). The numbers seem to approach more closely as I ascend from 10 to 20, 30, 40, &c. The lines embracing a hundred numbers also seem to approach as I go on to 400, 500, to 1,000. Beyond 1,000 I have only the sense of an infinite line in the direction of the arrow, losing itself in darkness towards the millions. Any special number of thousands returns in my mind to its position in the parallel lines from 1 to 1,000. The diagram was present in my mind from early childhood; I remember that I learnt the multiplication table by reference to it, at the age of seven or eight. I need hardly say that the impression is not that of perfectly straight lines, I have therefore used no ruler in drawing it.

Some writers have somewhat rashly asserted that our idea of numbers is always based on our ten fingers and ten toes. There are, however, other forms in use by various nations than those of decimal arithmetic, and the last paragraph of the foregoing seems sufficient to show that the finger and toe hypothesis is not universally true. This opinion was strongly maintained by the lady writer of the following remarks, whose imagery dates beyond her earliest recollections :—

13. The annexed column [a portion only of it is represented here] represents how I see the numbers from 1 to 140. There is no break up to 30, and none from 90 to 130, but I think this is because the three figures at 100 make a sort of break of themselves. After 140 they go on regularly, but farther off. The figures are not one *above* the other, as they appear in the diagram, but are one *beyond* the other, stretching away into space. They are about half an inch long, of a light grey colour on a darker and brownish grey ground.

The next example is very curious; the diagram which accompanies it is carefully and minutely drawn on a large sheet of paper and looks like a detailed route survey made by a careful traveller. I have been obliged to treat it much as a map maker would treat such a survey.

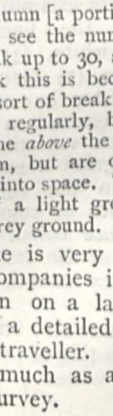

Fig. 3.

14. I find it very difficult to represent my visualisation of numerals diagrammatically. I scarcely ever see the lower numbers written; I simply know exactly where 6, 7, 4, &c., are to be found. I cannot properly represent the crowding of numbers in some places, nor the edgewise positions they occupy, nor can I at all adequately express the compactness and yet extent of the line. On either side of it there seems to be indefinite space. But there is a boundary at 1, *beyond* which I have to look for *minus* quantities. After 108 the notion of place becomes hazy and indistinct, though I can visualise the higher numbers in respect to their position, if I make the effort. I think of a million as *very* far off and high up. When multiplying for example 5 × 6, I know instantly the spot where the product will be, and look to see what number it is. But if asked to multiply 14 × 17 I first go up to the place whereabouts I expect it will be, and am baffled. I do not know where to look. In the coloured parts, it is the place rather than the number that is coloured, and the number is connected with colour because it happens to be in that place. The brightness and darkness may possibly in the lower numbers have some connection with the events of my life, the numbers which correspond to years of my age which were eventful, being as a rule much more distinct. As a child I had great liking for the number six, arising I fancy from a keen desire to be six years old. I had also an excessive love for blue, so perhaps this accounts for the connection between them. N.B.—I learnt arithmetic in a thorough old-fashioned unintelligent style, the first step being to learn to count without the least conception as to what the numbers meant.

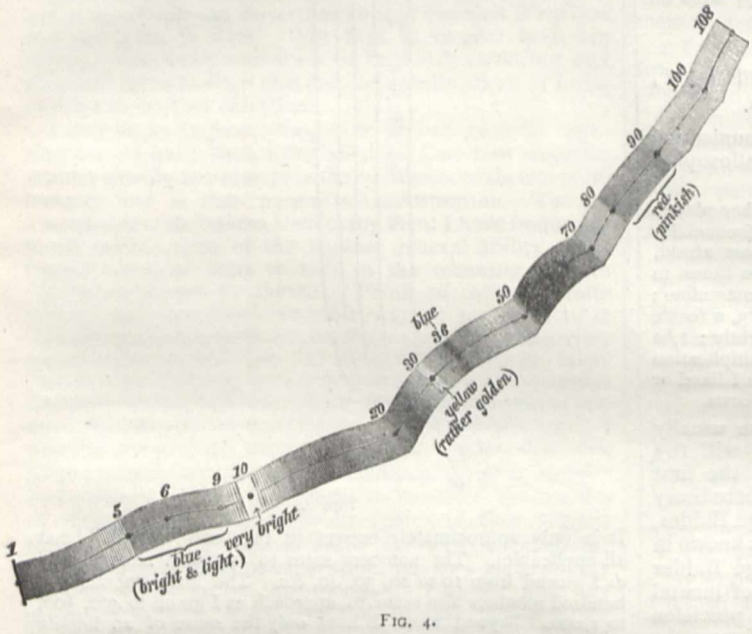

FIG. 4.

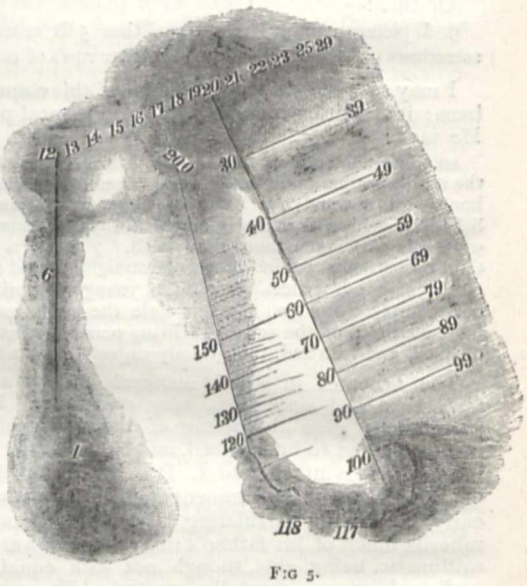

FIG 5.

The writer of the foregoing has two sisters and a brother. One of the sisters sees numerals in a differently arranged diagram, and the figures themselves are coloured, (1) black, (2) white, (3) yellow, (4) red, (5) greenish yellow, (6) blue, (7) black, (8) red, (9) grey, (0) gold. The other sister has a fainter, but still a decided tendency to see figures in a mental diagram. It is without colour but has variations of shade. The brother has a definite diagram of numbers arranged in a line sloping upwards to the right as far as 120, and absolutely devoid both of colour and variations of shade. No trace of these colour-peculiarities has yet been made out on either the father or the mother's side, but there is a tendency in both father and mother to visualise in diagrams.

The effects of heredity are also strongly marked in the next set of instances, consisting of two families of cousins. A sister in the first family writes:—

15. From the very first I have seen numerals up to nearly 200, range themselves always in a particular manner, and in thinking of a number it always takes its place in the figure. The more attention I give to the properties of numbers and their interpretations, the less I am troubled with this clumsy framework for them, but it is indelible in my mind's eye even when for a long time less consciously so. The higher numbers are to me quite abstract and unconnected with a shape. This rough and untidy production is the best I can do towards representing what I see. There was a little difficulty in the performance, because it is only by catching oneself at unawares, so to speak, that one is quite sure that what one sees is not affected by temporary imagination. But it does not seem much like, chiefly because the mental picture never seems *on* the flat but *in* a thick, dark grey atmosphere deepening in certain parts, especially where 1 emerges, and about 20. How I get from 100 to 120 I hardly know, though if I could require these figures a few times without thinking of them on purpose, I should soon notice. About 200 I lose all framework. I do not see the actual figures very distinctly, but what there is of them is distinguished from the dark by a thin whitish tracing. It is the place they take and the shape they make collectively which is invariable. Nothing more definitely takes its place than a person's age. The person is usually there so long as his age is in mind.

Another sister says:—

16. I always see figures ascending in a directly perpendicular line in front of my eye [according to the sketch and memorandum sent in illustration, which it is hardly necessary to reproduce, the 1 stands opposite to the eye, and the scale reaches vertically up to 1,000]. Then all becomes vague, but I know that the thousands and tens of thousands are not in the same perpendicular line, and I believe they turn to the left hand.

A maternal aunt of these ladies "sees figures in a diagram," which has not yet reached me, and the other family that I am now about to mention are the children of a maternal uncle. There are three sisters and a brother who have the same faculty in varying degrees.

The brother writes from Cambridge:—

17. Numerals are always pictured by me in a straight line from left to right. They are black, on a ground varying in illumination, which is bright up to 10, then getting very shady from 10 to 20; 20 to 40, bright; 40 to 60, moderate; 60 to 80, shady. Shadiest are from 10 to 20, 60 to 80 or 90, 1,000 to

2,000. The millions are in a vague, bright distance to the right.

One of the sisters writes :—

18. Figures present themselves to me in lines [as in the annexed diagram]. They are about a quarter of an inch in length,

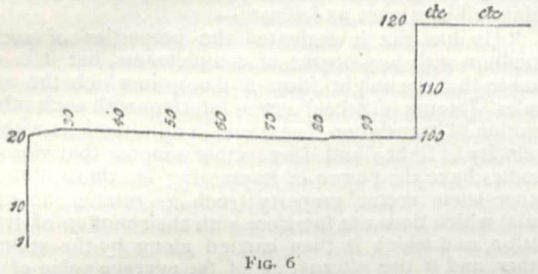

Fig. 6

and of ordinary type. They are black on a white ground. 200 generally takes the place of 100 and obliterates it. There is no light or shade, and the picture is invariable.

Another sister gives a picture in which the numbers form a vertical line from 1, opposite to the eye, up to 100, at which point the scale appears to recede from her.

The third sister writes :—

19. Figures always stand out distinctly in Arabic numerals ; they are black on a white ground, of this size [the specimen was

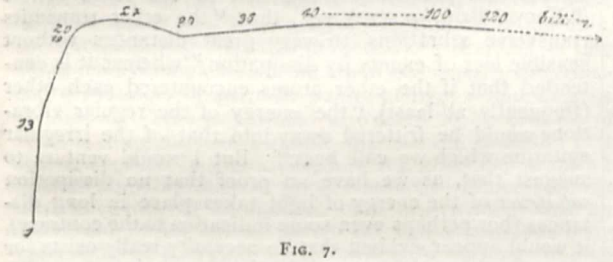

Fig. 7.

clear and round, and in rather large ordinary handwriting], but the numeral 19 is smaller than the rest.

It is curious that the lines of most of the diagrams I have thus far given should be so feeble and, to appearance, wandering, although as a matter of fact they are firmly fixed. Artists speak of the "leading lines" in a picture, and commend pictures in which the leading lines are graceful. I have little doubt that one of the reasons why minds vary in artistic power is that the leading channels in the blank schedules of their minds vary in character. I should expect that natural artists might be found whose habit was to visualise numerals not in shaky lines, but in bold and beautiful curves. In the instances I am about to give, especially in the first of them, there is more tendency to geometric precision, and I should be most curious to learn (by actual and careful test) whether or no such cases are generally correlated with a true eye to straightness, squareness, and symmetry.

In the following example the numbers are not associated with visual figures, but with points on an ascending and descending scale, which is a pure line having neither breadth nor colour. It is described as perfectly flexible and extensible, much, I suppose, as if it were printed on a strip of india-rubber sheeting, and it is applicable to the measurement of large distances or small ones, to fractions, and to straight lines or curves. A very curious description is given in detail, which I will not here reproduce, of the way in which the scale is used in mental arithmetic. The writer adds :—

20. The accompanying figure lies in a vertical plane, and is the picture seen in counting. The zero point never moves, it is *in* my mind ; it is that point of space known as "here," while all other points are outside or "there." When I was a child the zero point began the curve ; now it is a fixed point in an infinite circle . . . I have had the curious bending from 0 to

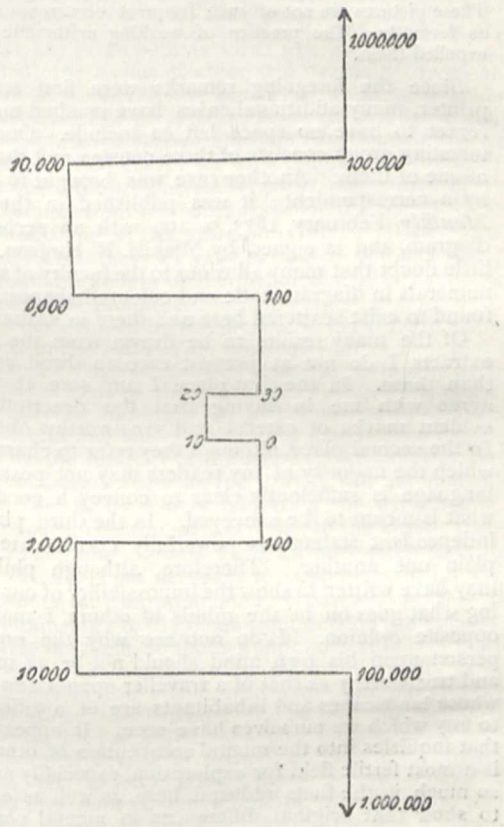

Fig. 8.

30 as long as I can remember, and imagine each bend must mark a stage in early calculation. It is absent from the negative side of the scale, which has been added since childhood.

Another correspondent sees figures in a circle, having 0 at the right hand of its horizontal diameter and 100 at the left hand. Positive numbers are reckoned from 0 to 100 from the right, over the top to the left, and negative numbers the other way. The same takes place with figures between 100 and 200, 200 and 300, &c.

Another correspondent sees them for the most part in a regular row like park palings. The description and sketch are as follows :—

21. As far as 12 the numerals appear to be concealed in black shadow ; from 12 to 20 is illuminated space, in which I can distinguish no divisions. This I cannot illustrate, because it is simply dark and light *space*, but with a tolerably sharp line of division at 12. From 20 to 100 the numerals present themselves as follows, but less distinctly :—

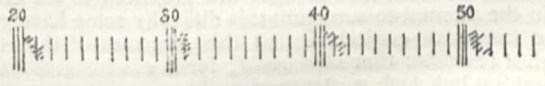

Fig. 9.

An account is appended of the way in which simple mental arithmetic is effected by this arrangement, which at present I pass over.

I will conclude my list with a statement written by a mathematical astronomer of rapidly rising reputation, whose "practice of working arithmetic" mentioned in the concluding paragraph must be understood to signify "performing masses of laborious calculations" :—

22. The numbers 1, 2, 3, 4, &c., are in a straight row, and I

am standing a little on one side. They go away in the distance, so that 100 is the farthest number I can see distinctly. It is dusky grey, and paler near to me; up to 20 it occupies a disproportionate size. There are sorts of woolly lumps at the tens. These pictures are not of such frequent occurrence in my mind as formerly. The practice of working arithmetic has rather expelled them.

Since the foregoing remarks were first sent to the printer, many additional cases have reached me, which I regret to have no space left to include. One very interesting group consists of three cousins and the daughter of one of them. Another case was brought to my notice by a correspondent; it was published in the *Atlantic Monthly*, February, 1873, p. 199, with an accompanying diagram, and is signed by Miss H. R. Hudson. I have little doubt that many allusions to the faculty of visualising numerals in diagrammatic and coloured shapes might be found to exist scattered here and there in various books.

Of the many results to be drawn from the foregoing extracts, I do not at present care to dwell upon more than these. In the first place I am sure that all will agree with me in saying that the descriptions bear evident marks of careful and trustworthy observation. In the second place, although they refer to characteristics which the majority of my readers may not possess, their language is sufficiently clear to convey a good idea of what is meant to be conveyed. In the third place, these independent statements powerfully corroborate and explain one another. Therefore, although philosophers may have written to show the impossibility of our discovering what goes on in the minds of others, I maintain an opposite opinion. I do not see why the report of a person upon his own mind should not be as intelligible and trustworthy as that of a traveller upon a new country, whose landscapes and inhabitants are of a different type to any which we ourselves have seen. It appears to me that inquiries into the mental constitution of other people is a most fertile field for exploration, especially as there is so much in the facts adduced here, as well as elsewhere, to show that original differences in mental constitution are permanent, being little modified by the accidents of education, and that they are strongly hereditary.

I trust, therefore, that the publication of this memoir may prove to be the means of inducing some persons to furnish me with information of the kind I am now seeking. I want to hear of well-marked and properly-authenticated instances of persons who are able to recall, or represent to their imagination, with great vividness, either sights, sounds, smells, or tastes, and to obtain information that may throw light on the peculiarities of the representative faculty in different families and races.

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ON A MODE OF EXPLAINING THE TRANSVERSE VIBRATIONS OF LIGHT

THERE has been considerable difficulty in arriving at a satisfactory conception of the means by which the transverse vibrations of light are produced in the ether. In the attempt to surmount this difficulty some have gone so far as to conjecture that this structure of the ether must resemble that of a *solid;* for it was imagined that nothing but such a structure could propagate transverse vibrations. Yet the supposition of the ether being anything like a solid appears to be in direct antagonism to the evidence of our senses; for we move about so freely in this "solid" as to be unconscious even of its existence.

My object here is to direct attention more especially to a suggestion thrown out by the late Prof. Clerk Maxwell in regard to this point. This suggestion is contained in the article, "Ether," in the new edition of the "Encyclopædia Britannica," in connection with a notice of a theory of the constitution of the ether (considered in special relation to the problem of gravitation) by the present writer, and published in the *Philosophical Magazine* for September and November, 1877, and February, 1878. After referring to the fact that the present writer "has supposed that the ether is like a gas whose molecules very rarely interfere with each other, so that their mean path is far greater than any planetary distances," Prof. Maxwell continues as follows:—

"He has not investigated the properties of such a medium with any degree of completeness, but it is easy to see that we might form a theory in which the molecules [1] [atoms of ether] *never* interfere with each other's motion of translation, but travel in all directions with the velocity of light; and if we further suppose that vibrating bodies have the power of impressing on these atoms of ether some vector property (such as rotation about an axis) which does not interfere with their motion of translation, and which is then carried along by the atoms of ether, and if the alternation of the average value of this vector for all the atoms of ether within an element of volume be the process which we call light, then the equations which express this average will be of the same form as that which expresses the displacement in the ordinary theory."

There is one point in the above suggestion I would briefly remark upon, viz., the supposition made by Prof. Maxwell that the atoms of ether "*never* interfere with each other's motion of translation" [*i.e.*, never encounter each other]. This supposition seems to have been called for by the fact previously mentioned in the same article ("Encyc. Brit.," p. 572), viz., that "the ether transmits transverse vibrations to very great distances without sensible loss of energy by dissipation," whereas it is contended that if the ether atoms encountered each other (frequently at least), "the energy of the regular vibrations would be frittered away into that of the irregular agitation which we call heat." But I would venture to suggest that, as we have no proof that no dissipation *whatever* of the energy of light takes place in long distances (but perhaps even some indication to the contrary), it would appear evident that no necessity really exists for supposing that the atoms of ether *never* interfere with each other's motion of translation. I think it will be admitted as a reasonable conclusion that so long as the dissipation of the energy (of the light) attendant on the mutual encounters of the ether atoms is no greater than observation allows us to suppose it to be, all conditions are satisfied. Moreover, it would seem that to suppose the ether atoms *never* to interfere with each other's motion of translation would be equivalent to assuming that their mean path is *indefinitely* great, which appears to involve the assumption that the atoms have no finite size or dimensions, which would put a difficulty in the way of a satisfactory or consistent conception of matter. On this ground I would therefore suggest that the atoms of ether may be considered to have a reasonably long free path [which may be conceived as great as we please, by simply conceiving the atoms small], and thus the dissipation of the energy of the light may be reduced within the limits required by observation. This does not alter in the least in its essential details the above suggestion by Prof. Maxwell as to the mode of production of the transverse vibrations of light, which I would accordingly enlarge upon and elaborate somewhat here (in connection with the special structure of gross matter required by the physical theory of gravity). First it is important to observe that many observed facts lead us to infer that gross matter (probably the molecules [2] themselves) possesses a more or less *open* structure (or possesses a high degree of porosity). The transparency of some bodies, the free passage of the magnetic disturbance through all bodies,

[1] I merely substitute "atoms of ether" in the above passage for "molecules," to avoid any possible ambiguity, as the word "molecules" is often applied to the parts of gross matter.

[2] This is also in harmony with the modern theory of vortex-atoms.

and many other well known independent facts render this inference necessary. The fact that gravity is proportional to mass, on the basis of the dynamical theory (first started by Le Sage) also renders it essential to conclude that gross matter possesses an open structure [so that the atoms producing gravity can penetrate and act upon the interior of bodies]. If we admit this, and figure to ourselves the streams of ether atoms passing in all directions freely through the open structure of gross matter, and further, if we conceive the molecules of gross matter to be in a state of vibration (of regular periods, as proved by the spectroscope), then it is evident that these streams of ether atoms during their passage can, from the very nature of the case, be solely effected by the *transverse* component of the motion of the molecules of the luminous body. It is much as if the meshes of a sieve were in vibration, and a continuous stream of fine particles of sand (impelled by a current of air) were urged through it, when in however many different directions the filament forming the meshes of the sieve might be conceived to be vibrating, the sand particles that passed through in the onward stream could be only affected by the *transverse* component of the motion of the meshes. So the atoms of ether in their passage through the vibrating molecules of gross matter are solely affected by the *transverse* component of the motion of the molecules. The ether atoms passing through the open structure of gross matter would be thus periodically deflected (or the ether atom itself thrown into vibration or rotation), and as the transverse impulses (whatever their exact nature) thus received by the stream of ether atoms would be perfectly ryhthmical or periodic, in harmony with the known periodic vibrations of the molecules through which the ether atoms pass, the *transverse* pulsatory or periodic nature of light would thus be produced. This view would also seem to be capable of surmounting in a very simple manner the difficulty that there has been in conceiving how the ether can transmit transverse vibrations to great distances without sensible loss of energy by dissipation. For it is evident that an ether atom after having passed through a luminous body and received energy from it, would have nothing[1] to give that energy (say vibration or rotation) to during its transit, since, by assuming the ether atom small, we may conceive its mean path as long as we please; so, therefore, the energy carried by the ether atom from the luminous body could not possibly be dissipated during the transit of the atom, but this energy would be carried intact by the ether atom (through its normal motion of translation) until the distant object is reached, where the energy is given up in the form of heat and light. The normal motion of translation possessed by the ether atom performs the part of simple carrier of the energy received by the atom from the luminous body.

It might possibly be thought at first sight that this theory had some resemblance in principle to the emission theory of light, but this is evidently not the case, as no atoms are emitted by the luminous body, but simply the atoms of ether in their normal state of translatory motion pass through objects in streams equally in all directions —the ether being regarded simply as a gas (according to the modern kinetic theory) with atoms of very long free path. It is a known mathematical fact that no consequence how close the atoms of ether may be together (*i.e.*, no consequence how many in unit of volume) their mean path may become as great as we please, by simply conceiving the atoms adequately small. It further follows from the known principles investigated in connection with the kinetic theory of gases, that these atoms will of themselves automatically adjust their motions so as to move with perfect uniformity or *equally in all directions*; this adjustment being of such a rigid character that if the atoms were imagined to be disturbed or made to move in the most chaotic manner, they would, when left to themselves, instantly correct the irregularity, and return to the above regular form of motion, *i.e.*, so that the atoms move *equally in all directions*. It follows from this, therefore, that if we take any given point (such as where a luminous body is situated), the atoms of ether will "radiate" from and to this point along all the imaginary radii of a sphere described from this point as a centre; so that those ether atoms which have passed through the luminous point (and have carried energy off with them) will diminish in number (per unit of spherical area) as the *square* of the radial distance from the luminous point, the energy, therefore, diminishing in the same ratio, which is the "law" of light. The "law" of gravity (which is found also to diminish as the *square* of the distance) may be accounted for on the same principle.

It has been shown by the present writer (in the papers published in the *Phil. Mag.* previously alluded to) that in accepting Le Sage's ingenious sheltering principle as the fundamental basis of the explanation of gravity, there is no necessity for admitting any of his postulates regarding the particular motions of the atoms (corpuscles) required to produce the result. For it may be shown that the whole of the conditions requisite for gravity will automatically fulfil themselves by simply admitting the existence of a body in space, constituted according to the kinetic theory of gases (and whose atoms have an adequately long free path). There is no necessity to suppose, with Le Sage, the existence of "ultramundane corpuscles," or that the atoms producing gravity come from outside the bounds of the visible universe, so that a continuous supply of matter from without is necessary to maintain gravity within the confines of the visible universe. On the contrary, the conditions are satisfied by merely supposing the universe to be immersed in a gas, which, *as a whole* (like any other gas) is at rest. The motion (in streams) requisite for gravity takes place solely *within the range of free path* of the atoms of the gas; just (as is known) in every ordinary gas the atoms within the range of free path are moving in streams equally in all directions. The only difference is that in the case of the ether, on account of the smallness of the atoms (which is in harmony with their high velocity), the range of free path is great—equal to the range of gravity. We have no proof that the range of gravity extends across stellar distances, and there is clearly no necessity for assuming it to prevail over greater distances than observation warrants. By the explanation of gravity by the physical theory, the remarkable and anomalous distinction between *two kinds* of matter ("ponderable" and "imponderable") vanishes. Matter is shown of its essence to be all alike, "ponderability," or the tendency to approach, not being an occult or magic quality, but simply an effect dependent on differing dynamical conditions,[1] and the variation of the intensity of which as the *square* of the distance it is as necessary to account for dynamically as in the analogous case of light.

It appears, therefore, from the above considerations, that the same medium shows itself to be capable of accounting for, in their essential groundwork, the phenomena of both gravity and the propagation of light. The theory of gravity is based upon the well-known sheltering principle of Le Sage, which has already found favour with some eminent physicists. The normal translatory motion of the atoms of the medium produces gravity, and this motion serves as a vehicle for the propagation of light, while the light itself consists in the

[1] This holds equally true, whether we conceive space as empty, or space to be filled with a perfect (frictionless) liquid that *plays the exact part of empty space*, in so far as it is known to be impossible to operate upon or communicate energy to such a liquid.

[1] The fact of the property of "ponderability" having been attributed to gross matter as an occult quality (not an effect depending on dynamical conditions), has naturally brought the ether—which does not possess this property—into direct contrast with gross matter, as if it were an anomalous substance, of its essence distinct from gross matter. This circumstance has no doubt naturally contributed to produce a distaste for the study of the ether and to cause some to treat this magnificent physical agent as if it were desired rather to ignore than to take a rational interest in its existence.

abnormal *transverse* disturbance produced in the streams of atoms in their passage through the vibrating molecules of luminous bodies.

Finally, it may be shown that, quite independently of any consideration of the effects of gravity and light, the inference is a necessary one that the constitution of the ether must in principle be that of a gas, because this solution to the problem of the constitution of the ether exhausts the limits of the conceivable: *i.e.*, if any *rational* solution to the problem or *explanation* capable of appreciation by the reason exists, then it would follow that this must be the true solution to the problem. This will become more and more evident on reflecting on the subject. For it is clear that a motion *in straight lines* is the only motion possible to particles of matter moving freely in space; for particles of matter cannot of themselves change the directions of their motions. They can only do this at their encounters. Hence the inference is necessary that the particles of ether move *in straight lines* (and therefore that the ether is constituted as a gas). Hence in principle it seems apparent that the above is essentially the only conceivable solution of which the problem of the constitution of the ether admits. It seems remarkable that this fact (important as it is) is not more generally recognised and appreciated. Can this be referable in any way to the influence the theory of "action at a distance" has had, and that the endless empty and profitless speculations arising therefrom have diverted attention from the subject?

It might be said that we have expressly assumed the existence of "particles" (or atoms) of matter in the above result as to the constitution of the ether, whereas some might contend that the ether is not *atomic* at all. My answer to this is, that unless we assume the ether to be atomic,[1] we cannot give any explanation of its properties, and these properties can exist solely in virtue of the explanation that underlies them. In connection with this the following remark of Prof. Clerk Maxwell ("On the Dynamical Theory of Gases," *Phil. Trans.*, 1867, p. 49) may be quoted, as to the point:—

"In certain applications of mathematics to physical questions, it is convenient to suppose bodies homogeneous, . . . but I am not aware that any theory of this kind has been proposed to account for the different properties of bodies. Indeed, the properties of a body supposed to be a uniform *plenum* may be affirmed dogmatically, but cannot be explained mathematically."

Hence to assume the ether to be anything else than *atomic*, would be to affirm its properties "dogmatically." If we avoid this, therefore, we must consider it *atomic* (and therefore a gas): for (as we pointed out), atoms in free motion can only move *in straight lines*. It is of course evident that, unless the atoms of ether be *in motion*, we cannot account for any of its properties, unless, indeed, we resort to the now practically defunct theory of "action at a distance," and assume the atoms to be endowed with mysterious and occult powers, which renders any *explanation* impossible, and only increases instead of diminishing the difficulty.

To illustrate somewhat further the insuperable nature of the difficulty involved, unless we assume the atoms of ether to be in motion in their normal state, I quote the following passage from a lecture by Prof. Tait on "The Position and Prospects of Physical Science," delivered November 7, 1860 (p. 15 in pamphlet):—

"If we suppose it [the ether] to consist . . . of detached particles . . . we are met by the further difficulty, *how* do these particles act on each other, and without some such action there could be no transmission of motion—they are not in contact, there must therefore be something between them to convey the effect. This appears certain, for how can action be conceived as exerted across empty space?"

I will merely here remark parenthetically that the fact appears to have escaped notice here that this difficulty is got over by assuming the particles (of ether) to be *in motion*; for then the particles can act on each other by direct impact without the necessity for anything "between them to convey the effect." The passage goes on to say:—

"We must, therefore, have a second medium to fill the interstices between the particles of ether. If this again consist of detached particles, there will be a third required that these may act on each other—and so on. If, then, we would not have an infinite number of different kinds of matter in each element of space, we must suppose one of these—say the ether itself—to be *continuous*, that is, not consisting of ultimate parts. *How* vibratory motions could be transmitted through such a substance, it is difficult to imagine—the whole subject is beset with overwhelming difficulties."

In the above passage the difficulties that attend the assumption of the ether being a *continuous* substance, or uniform *plenum*, are well illustrated. It will be seen that the main dilemma vanishes by assuming the particles of ether to be *in motion* in their normal state. Indeed, this is evidently the only conceivable way of solving the difficulty.

I would, therefore, venture to suggest that the result above arrived at as a solution to the problem of the constitution of the ether might be worthy of the attention of physicists, especially in its bearing on the explanation of gravity (on the basis of Le Sage's fundamental principle now recognised by several eminent authorities, including Sir W. Thomson)—also in relation to a mode of explaining the *transverse* vibrations of light, the main idea involved in which was suggested by Prof. Clerk Maxwell.

Addendum.—I may mention that I have lately received a book ("Das Räthsel von der Schwerkraft"—Wieweg und Sohn, Braunschweig) through the kindness of the author, Dr. Isenkrahe, of Crefeld, where a theory is applied to the constitution of the ether and to gravity, which resembles in some points that adopted by the present writer. This book bears date 1879, but the MS. was prepared earlier (1877). I may note that a book, "Physics of the Ether" (E. and F. N. Spon), was published by me in 1875, where in principle the same theory of the ether as here given is developed, though it was not applied by me to gravity until 1877. The work of Dr. Isenkrahe contains, in addition, a valuable description and criticism of the various attempts to solve the problem of gravitation. Perhaps I may append, for the benefit of those who are interested in the question, the references to some of the chief of these here, viz.:—

Huyghens' "Discours sur la Cause de la Pesanteur." Leyden, 1690.

Le Sage's theory 1764, given in "Deux Traités de Physique mécanique," by Pierre Prevost.

Sir W. Thomson's development of Le Sage's theory, Royal Society of Edinburgh, 1872, and *Philosophical Magazine*, May, 1873.

Schramm, "Die allgemeine Bewegung der Materie als Grundursache der Naturerscheinungen," Vienna, 1872.

Secchi, "Die Einheit der Naturkräfte" (German edition). Leipzig, 1876.

These facts may show an awakening to the importance of the problem of gravitation, and I may conclude with

[1] It is almost needless to add that the vortex-atom theory is essentially an *atomic* theory. Although it assumes a perfect liquid to fill all space, yet this liquid (outside the portions of it that form the atoms) plays the part of pure space, since it is impossible to communicate energy to the liquid outside the atoms, or to act upon it at all. It is therefore (as far as practical effects are concerned) as if the liquid exterior to the atoms did not exist. The vortex-atom theory does not, therefore, essentially alter (as some might be disposed to imagine) the conceptions of the ancients of indestructible atoms surrounded by space in which they can freely move. The main purpose of the vortex-atom theory is to prove dynamically how atoms can be "*elastic*," and be capable of executing regular vibrations as the spectroscope proves (and actually measures the number of vibrations executed per second, in the case of the molecules of gross matter).

the words of Dr. Isenkrahe [translation]: "One is beginning to recognise that physics has been quietly sleeping for two centuries upon [in the words of Newton] 'a great absurdity,' for which no one less than Newton can be made responsible" [page 125].

S. TOLVER PRESTON

THE NATURAL HISTORY OF THE TRANSIT OF VENUS EXPEDITION[1]

IN 1870 one of our correspondents called attention[2] to the favourable opportunity that would then shortly present itself for the exploration of some very little known parts of the earth's surface. Some of the positions selected by our astronomers for the observation of the Transit of Venus of 1874 were in little known islands of the Pacific and Indian Oceans, and it was pointed out that the addition to the astronomers' staff of a small corps of naturalists would not materially increase the expenditure, and would possibly lead to very interesting scientific results.

The same subject was brought before the notice of the British Association at their Meeting in 1871 by Mr. Sclater,[3] who likewise suggested that so good a chance of adding to our knowledge of the natural history of some of the least known parts of the world should not be thrown away, and urged that Naturalists should be appointed to at least three of the stations to be occupied by the astronomers, namely Kerguelen Island, Rodriguez and the Sandwich Islands.

Subsequently the same idea was taken up by the Council of the Royal Society, who resolved to request the Treasury to attach naturalists to the expeditions destined for the two first above-named localities—"two of the least explored and most inaccessible islands in the southern hemisphere"—and appointed a committee consisting of Sir Joseph Hooker, Prof. Huxley, and Mr. Sclater, to prepare the necessary application to the Government for this purpose. We need not now repeat the arguments which these gentlemen brought before Her Majesty's chief advisers—it is enough to say that they were of a sufficiently cogent character to obtain the sanction of the Treasury to the appointment of four naturalists for the purposes required; three for Rodriguez and one for Kerguelen's Land.

The gentlemen selected for the work by the Council of the Royal Society were for Rodriguez, Mr. George Gulliver, Dr. I. B. Balfour, and Mr. H. H. Slater, and for Kerguelen's Land the Rev. A. E. Eaton. Mr. Gulliver was directed to investigate the fauna of Rodriguez generally, Dr. Balfour was charged with the duties of botanist and geologist, and Mr. Slater was set to dig out the caves of the same island, and to collect the fossil remains of extinct birds known to be imbedded in them. Mr. Eaton was thought to be specially qualified to investigate the fauna and flora of Kerguelen's Land, as having been previously naturalist to one of the Arctic expeditions.

The collections and observations made by these naturalists fulfilled, as we are informed, the expectations of the Council. The results of them are given in the present work, which is issued as an extra volume (No. 168) of the *Philosophical Transactions*.

The plan upon which the collections were worked out, and which is in fact the only plan upon which a mass of heterogeneous materials can be properly worked out now-a-days, is an admirable one. The different objects were assigned for examination to experts in different branches of science, each of whom has prepared his own report on what was submitted to him. These reports, prefaced by a few introductory remarks, and illustrated by notes of the collectors, constitute the volume now before us. It is divided into two sections, the first relating to Kerguelen, and the second to Rodriguez.

After a chapter by Mr. Eaton on the physical features of Kerguelen, and on the previous visits to it by naturalists, we find a series of essays on the botany by Sir J. Hooker, Mr. Mitten, the Rev. J. M. Crombie, Dr. Dickie, and other well-known authorities. Then follows a similar series of memoirs upon the zoology of the same island.

The zoological and botanical collections made in Rodriguez are next treated of in the same way, and we have here also a valuable memoir on the petrology of Rodriguez by Mr. N. S. Maskelyne. From the last-named essay it turns out that the notion that this island consists of "granite overlaid with limestone, and other recent rocks," which was entertained by the Committee of the Royal Society, misled by previous inaccurate observations, is altogether erroneous. Mr. Maskelyne tells us that "the numerous specimens illustrating the rock formations of the Isle of Rodriguez, collected by Mr. I. Bayley Balfour from different localities, need only a cursory inspection to attest the volcanic character of the whole mass of the island.

"Rodriguez, in fact, consists of doleritic lavas that appear to have been poured out at a considerable number of volcanic orifices at successive periods. It would be difficult, without more minute description of the physical geography of the island than is accessible, to assign any precise date of duration to these volcanic eruptions, or to trace with any certainty the degree to which, and the mode in which, subsequent denudation has helped in giving the island its present remarkable aspect.

"But the fact of that denudation and the degree to which alteration has proceeded in affecting the minerals composing rocks that by their position must have been among the later of the out-poured lavas, would point to a remote date, possibly to one contemporary with the tertiary period, as that of the volcanic activity of Rodriguez."

We have not space here to go separately into the numerous essays that compose this work. For many of them, the name of the author is quite sufficient to assure us of their excellence, some of the most accomplished naturalists of the present epoch having contributed to the volume. But it is quite evident that a thoroughly good and satisfactory piece of scientific work has been thus accomplished at a very small cost, and that the council of the Royal Society, who planned the whole scheme and carried it out, and especially those two members of it (Sir J. Hooker and Dr. Günther), who have so efficiently edited this account of the results attained, are entitled to the warmest thanks of all naturalists. Several other nations sent out expeditions to observe the Transit of 1874, and likewise had naturalists attached to their staffs, who have published some valuable observations. But nothing like the handsome and solid volume now before us, with its fifty-five admirably executed lithographic plates, has been produced on this occasion in France, Germany, or America. There are certainly some advantages in having a Royal Society at the head of Science instead of a Royal Academy!

Having said this much, we will venture on two small criticisms:—First, it is a great pity that there are no *maps* given in the volume now before us. Without reference to maps it is not possible to appreciate the significance of many of the observations made by the naturalists, and as no generally available atlas contains charts of such obscure islets as Rodriguez and Kerguelen, maps ought to have been attached to the work itself. In fact, every zoo-geographical memoir now-a-days ought to be illustrated by a map.

[1] "An Account of the Petrological, Botanical, and Zoological Collections made in Kerguelen's Land and Rodriguez during the Transit of Venus Expeditions, carried out by Order of Her Majesty's Government in the Years 1874-75." *Philosophical Transactions* of the Royal Society of London, vol. clxviii. Extra volume, 1879.

[2] See article on the Transits of Venus in 1874 and 1882. NATURE, vol. i., p. 526.

[3] See "Remarks on a Favourable Occasion for the Establishment of Zoological Observatories." By P. L. Sclater, M.A., Ph.D., F.R.S. Rep. Brit. Ass., 1871, pt. ii., p. 134.

Secondly, it is unfortunate that naturalists were not likewise sent to Oahu, in the Sandwich Islands, where there was likewise an astronomical station in 1874. The Sandwich Islands, as was pointed out by our correspondent in 1870, are the seat of a most peculiar indigenous flora and fauna, which is now fast perishing beneath the assaults of European weeds and animals introduced from other countries. Dr. Finsch, who was lately at Honolulu on his way to the Northern Pacific, tells us (*Ibis*, 1880, p. 79) that during a week's stay in that city and its vicinity, he saw *no birds* except introduced species, and had to go far into the interior to obtain examples of the indigenous Avi-fauna, and that the "native forests are going in the same way." It is a great misfortune, then, that this should happen before we have any good account of this peculiar flora and fauna which rivals in eccentricity even that of the Galepagos. And as another Transit occurs in 1882, we trust that should our astronomers again visit any one of the Sandwich Islands group, a staff of efficient naturalists will be sent in their company.

ARTIFICIAL DIAMONDS

UNDER the heading of "The Crystallisation of Carbon" Mr. Crookes writes as follows in the last number of the *Chemical News*:—

Since sending the telegram[1] announcing that carbon crystals, apparently diamond, could without difficulty be produced from any carbon compound, Mr. Mactear has sent me several specimens of his supposed artificial diamond. He has also called upon me with other specimens, and has explained the whole process by which he obtains such remarkable results. As, however, he has sent to the Royal Society a paper which will probably be read in the course of a week or two, I am not yet at liberty to give details of the process.

The general character of the specimens now in my possession may be described as irregularly shaped masses from 1 mm. downwards in diameter, with rounded angles, and showing no definite crystalline appearance. They are whitish looking, translucent, and as a rule lustreless; many pieces are almost spherical and appear like fragments of corundum which have been water worn. Amongst these are perfectly clear fragments larger in size, some being 3 or 4 mm. across, having a conchoidal fracture exactly like glass.

In a paper "On Molecular Physics in High Vacua," read before the Royal Society in March last, and now being published in the *Philosophical Transactions*, I referred to the remarkable power possessed by the molecular rays in a high vacuum of causing phosphorescence in bodies on which they fall, and I remarked that the only body which surpassed Becquerel's luminous sulphides both in brilliancy and variety of colour is the diamond. Most of these gems, whether cut or in the rough, when coming from the South African fields, phosphoresce of a brilliant light blue colour. Diamonds from Brazil shine with different colours, such as bright blue, pale blue, apricot, red, yellowish green, orange, and light green. A beautiful collection of diamond crystals, kindly lent me by Prof. Maskelyne, phosphoresced with nearly all the colours of the rainbow, the different faces glowing with different shades of colour. On receiving the specimens from Mr. Mactear, I immediately submitted them to the molecular discharge. The following are the results I have at present obtained:—

In a high vacuum the specimens phosphoresce brightly of different colours—pale blue, orange, apricot, and yellowish green. The clear glassy fragments are also phosphorescent. The appearance of the phosphorescence is very similar to that shown by small, rough diamonds from Brazil, called in the trade "Boart;" indeed, had I not known the history of the fragments in my tube, I should, from their appearance, have said that they were small fragments of Brazilian Boart.

The opaque rounded appearance of the fragments is unlike that of the natural diamond, but by heating a rough diamond before the blowpipe until it has partly burnt away, it assumes a very similar appearance to that of Mr. Mactear's crystals, and it is therefore not unlikely, from their mode of preparation, that these crystals have undergone partial combustion after their formation—a fact which would explain this difference in appearance. Other specimens having been placed by Mr. Mactear in competent hands, with a view of determining their hardness and chemical properties, I have refrained from making experiments in this direction. W. C.

[1] *Chemical News*, vol. xl. p. 306 (December 26, 1879).

We append a letter on the subject from Prof. Maskelyne in the *Times* of the 8th inst.:—

As I know that a portion of the public is very much interested in the diamond question, and in the result of the interview Mr. Mactear announced that he and I were to have in connection with it, I think, perhaps, it will be well to say that I have had the pleasure of working with that gentleman many hours yesterday and to-day, and that our results so far convince me that, while my own conclusions, as announced in the *Times*, are borne out as regards at least the portion of the substances on which I worked, there are other portions of those substances that differ from these in properties and still require investigation; that, in fact, the material is a mixture of different bodies. When I say that I have as yet no evidence of the existence of crystalline carbon, whether as diamonds or in some other condition, among these bodies, I feel that Mr. Mactear makes a reasonable request of me in asking that I should invite a suspension of opinion regarding a discovery he believes that he has made. A portion of the material he has produced is very hard, and, I believe, bears out his claim to have scratched topaz and sapphire. Mr. Mactear wishes me to add that the diamond has been also abraded by his product and to inclose to you a certificate to that effect. Mr. Mactear wishes me also to state that he claims simply to have produced a crystalline form of carbon irrespective of the question of whether this is the diamond.

I am, Sir, your obedient servant,
NEVIL STORY-MASKELYNE

British Museum, January 7

We may state that in the *Times* of the same date is a certificate from Mr. L. Boston, of Glasgow, that he has been able "to scratch a diamond and to engrave two rubies, two sapphires, an amethyst, and a cairngorm" with Mr. Mactear's "crystallised carbon sand."

THE "TIMES" ON BRITISH BIRDS

NATURALISTS live a life of surprises, but the surprise with which ornithologists must have one day last week received certain positive assurances of the leading journal would surely overstep the bounds of ordinary astonishment. We have, no doubt, been passing through a "silly season" of unwonted severity, as the morris-dance of late performed by many of the pseudo-ornithological correspondents of the *Times* proves; but a recent leading article in that journal eclipses all else that it has published on the subject.

After declaring that "our birds are the glory of the land," and piously ascribing that glory to the upper regions, the writer goes on to compare England with France in the matter of its birds, saying, of course, nothing that was not quite well known before, except the extraordinary statement that "France has produced ornithologists, but they have had to leave her shores." The meaning of this is entirely beyond us, for every one knows who cares to know that France now possesses a large number of ornithologists—and one indeed, M.

Alphonse Milne-Edwards, who on some points is the greatest ornithological authority that has ever lived. We are then told of an Oxford undergraduate who "took a walk with his gun in Bagley Wood and brought home fifty different specimens which he carefully stuffed." "He had a museum," it is added, "of several hundreds." We are not told whether this Oxford undergraduate's conduct is worthy of praise or blame, nor would it much signify, for the writer is evidently confused in his notion of "specimens" and "species." To kill specimens of fifty different *species* in one day and in one wood, though not easy, could no doubt be done in many places, but it would be hard to kill fifty birds that were not different *specimens!* Would the writer also be surprised to learn that "a museum," or a collection, as people nowadays more humbly style it, "of several hundreds," was some fifty years ago by no means uncommon, and that of late years private collections include not only thousands of specimens, but thousands of species?

But now comes the most astonishing assertion of all. We are told that "Mr. Morris describes more than twelve hundred birds," and that there may be no mistake in the writer's meaning, he subsequently repeats the statement in this wise: "Of the twelve hundred British birds, a good many are represented by a single stray specimen," and so on! The ornithologists of this country have hitherto been deemed by their continental brethren somewhat too hasty in enrolling as "British" every chance waif from foreign lands and seas that has had the ill luck to show itself (and of course be shot) within the limits of the United Kingdom, and we have never understood that on the most liberal interpretation of the expression, "British birds," the number has exceeded four hundred. How blind and inefficient have they been when they have omitted more than two-thirds of the species that occur here! It is really to be hoped that the writer of the leading article on English birds in last Thursday's *Times* will bring them to a due sense of their neglected duties by furnishing a list of the 800 species whose rights of citizenship have been so shamefully ignored, and if he will at the same time say in which edition of Mr. Morris's work "more than twelve hundred" British birds are described, he will possibly contribute to a more comfortable understanding of the matter, for Mr. Morris has hitherto been supposed to follow very closely the late Mr. Yarrell in the information he gives, so that when the latter in his last edition included 354 species, the former a few years later made the number 358!

There are many other assertions in the same article which excite a degree of amazement inferior only to the last particularised, and we have heard persons suggest that the writer must have been all the while perpetrating a solemn joke.

EDISON'S ELECTRIC LIGHT

THE *Times* New York correspondent gives some interesting details in Monday's paper of Mr. Edison's new form of electric lighting and the steps by which he was led to its discovery. So far the light has withstood every test that has been tried, and so confident do the public seem that success has been attained at last, that the shares of the Edison Company have risen from 20 dollars to 3,500 dollars.

The Philadelphia correspondent of the same journal gives some further information in yesterday's issue. Probably 200 people make up the population of Menlo Park, we are told, nearly all Edison's workmen and their families. He gets an income of 40,000 dollars to 50,000 dollars a year from his various inventions, and he spends it all, the most of it for machinery and wages, and the balance in charity. The correspondent then gives some interesting details concerning Mr. Edison, his habits, his enthusiasm, and his relations with his numerous employés. There is no discipline enforced or any apparent time-table for work, yet with all hands it seems a labour of love, and if you pick out from the crowd the grimiest and most woe-begone of the whole party of overworked alchemists it will be Edison himself. It appears to have been the system at Menlo Park, as with the alchemists of old, to do most of the work at night, and it seems the regular habit of Edison and his chief subordinates to work straight through the twenty-four hours without stopping, until tired nature compels them to drop down in any handy place and go to sleep. "We went there," the correspondent writes, "hoping that Edison had succeeded, but nevertheless sceptics, and we came away thorough believers. His lamps were burning when we arrived, and they burnt continuously until our departure, excepting from half-past four to half-past five P.M., when about an hour's time was taken in putting in a new generator to do the work, which he had just finished and desired to try. During the daylight we could see the lamps burning, supplied by the first generator, and perceived that the little carbon loop or horseshoe giving the light remained intact. After dark, when the second generator went to work, we saw for three hours the lamps successfully burning as a complete substitute for gas for every purpose for which illumination was necessary at Menlo Park. The gas jets were idle, being put out of use by the steadier and more genial glow of the electric light. We ate our supper by it in the little restaurant that has been established at the Park, and I sat down in Edison's office under two of his lamps attached to a gas bracket and wrote the rough draft of the telegram sent to the *Times*. In this room a telegraph operator worked in a corner with an Edison lamp in a movable table stand illuminating his work. Down stairs his bookkeeper was paying off the hands by the aid of two more electric lights on a gas bracket. Out in the roadway in front of the building two street lamps were set up with the Edison light in full operation. In his workshop the engineer was running his engine and a couple of men watching the operation of the new generator by the light of more Edison's lamps, while in the laboratory some fifteen of them were giving light for various operations, and downstairs a young man sat at the regulator, and, watching another light, by the aid of the galvanometer, kept the flame steady, just as the regulator is worked constantly in the gas-house to adjust the gas pressure, so that it will compensate for turning lights on or off throughout the town. It was between seven and eight o'clock on a dark winter evening, and the electric light had put into disuse both the gas jets and the petroleum lamps that were in profusion around. I visited four dwellings in the village and saw the Edison lamps doing the work of illumination for all household purposes in each of them. In Edison's own house, where he had at least a dozen of them, we remained over half an hour, and I shall never forget the glee with which Edison listened to the reading of a newspaper slip, wherein an ambitious 'expert' offered to forfeit 100 dollars for every lamp that Edison could keep burning over twenty minutes."

NOTES

ON Friday, the 9th inst., the St. Andrew's University Court agreed to report to the Queen in Council in favour of an application by Prof. Swan to be permitted to retire, on the usual retiring allowance, from his Chair of Natural and Experimental Philosophy in the University, on the ground of failing health.

MR. E. W. COOKE, R.A., F.R.S., whose death at the age of sixty-nine years, took place at Groombridge on the 4th inst., deserves some notice in these pages for his connection in various ways with science. From his boyhood he had the keenest interest in natural history, and was probably one of the first amateur horticulturists. He was connected with most of our scientific societies, and was an early member and constant

attendant at the meetings of the British Association. His first artistic work was botanical, the drawing of many hundred of the illustrations to London's "Encyclopædia of Plants," all drawn from living specimens. The professional work of Mr. Cooke as an artist was throughout an advancement of science through a channel which we have often had occasion to point out, is generally too independent of the claims of science, and suffers accordingly. Mr. Cooke's representations of natural objects, of plants and animals and rocks, were always scientifically accurate, and his coast scenes are in themselves a geological study. He was always ready to help other artists whose ignorance of natural science was apt to lead them into ludicrous blunders. He was, we believe, one of the first who ever attempted to grow ferns and tropical plants under conditions similar to those under which they are found in nature. Both at Kensington and at Tunbridge Wells his fernery and tropical garden were masterpieces in their way. For his eminence as a horticulturist and for his contributions to geological science by his series of pictures and drawings illustrating the principal geological features of the British Islands, Mr. Cooke was, in 1863, elected a Fellow of the Royal Society. His series of drawings of "Grotesque Animals," published a few years ago, afford a remarkable example of his intimate knowledge of comparative anatomy, as well as of his sense of humour. Mr. Cooke counted among his friends nearly all the leading men both in science and art.

MR. WILLIAM ALEXANDER FORBES, B.A., F.Z.S., Scholar of St. John's College, Cambridge, has been appointed by the Council Prosector to the Zoological Society of London in succession to the late Prof. Garrod. Mr. Forbes, who is already well known for his contributions to scientific literature, obtained a first-class in the Natural Sciences Tripos at the late examination at Cambridge, and was designated as specially distinguished in the sciences of comparative anatomy and zoology.

WE understand that, at the suggestion of several practical teachers of botany, a new piece of ground at the Royal Gardens, Kew, will, during the ensuing season, be set apart for the study of botany, and that students will, under certain regulations, be able to carry home specimens for examination. Papers recently read at the Chemical Society by Mr. Church on the respiration and transpiration of albino foliage, and at the Linnean Society by Mr. Marshall Ward on the embryology of phanerogams, were in both cases founded on observations made in the laboratory.

THE *Journal of Botany* announces the death, at the early age of twenty-eight, of one of the most promising of the younger generation of physiological botanists, Dr. H. Bauke, of Berlin. His researches on cryptogams, and especially on the phenomenon of bilateralness in the prothallia of ferns, gave promise of a brilliant future.

THE Fourth Annual Report of the Johns Hopkins University contains much that is of great interest. It gives a sketch of the foundation and plan of instruction of the University, showing how the latter has been based on the best ideas as to what ought to be the functions of a university. The system of fellowships at the Johns Hopkins institution is one calculated to encourage and call forth the best energies of the Fellows, and in the short career of the University the success of these fellowships has been fully shown. The University has the use of the magnificent library of the Peabody Institute of Baltimore, and in its own various laboratories much good work is being done. The University has contrived happily to combine teaching and research in such a way as to give students real help and yet leave the teachers ample time to carry on original work. The *American Journal of Mathematics* and the *American Journal of Chemistry* both emanate from this Institution, while special publications contain the results of biological work, and a long list of papers in various departments by members of the University is appended to the Report. We have also a long list of apparatus for scientific researches involving accurate measurements in the physical laboratory, and of some of the most important apparatus in the biological laboratory. Altogether from this Report it will be seen that the Johns Hopkins University is doing its best to carry out the noble purpose of its founder.

FROM the Twelfth Annual Report of the Peabody Institute of Baltimore, we see that the magnificent new buildings are now complete and occupied. It now forms one of the best equipped centres of culture in the United States.

IN the *Bulletin* of the Paris Anthropological Society (tome ii. fasc. 3) M. J. Geoffroy gives a *résumé* of his great work on the knowledge and denominations of colour, in which he attempts to controvert the views of Magnus and Geiger, and those of Mr. Gladstone, which ascribe colour-blindness to Homer. On the grounds taken by these writers he insists that we should be equally justified in asserting that Corneille, La Fontaine, and others who happen not to mention in their works any one special colour, must have been blind to it; he considers that the delight taken by savages in bright colours is a sufficient proof that the sense of colour is not due to culture.

IN the same number M. de Jouvencel draws attention to the curious circumstances that the Latin races by preference take the right side, where the Teutonic races, including our own, and that of Scandinavians, take the left. With regard to the former, he finds a sufficient explanation in the superstition of the Romans, who deemed all omens favourable which manifested themselves on their right side, and *vice versâ;* while the barbarian enemies of Rome may be assumed to have regarded as favourable to themselves whatever the Romans accepted as of evil portent. The Saxon races as masters of the sea and pioneers in the laying of railways, have imposed their own rules of the left side on the French and other Latin nations, who, however, still in driving, riding, &c., keep to the practice of their progenitors.

M. ZABOROWSKI recently communicated to the Paris Anthropological Society his discovery, on the banks of the Lower Vistula, of certain sepulchral vessels of a kind never before described. At the depth of 50-80 centimetres below the surface he found cinerary urns filled with bones, in the midst of which were various objects in bronze, iron, and bone, and over each urn there was a cover, like an inverted bell, resting in some cases on a kind of stand, or plateau. He proposes to give to these singular urns the name of *tombeaux sous cloches;* of which outline drawings with full description of their form and size are given at pp. 337-8 of the *Bulletin* (t. ii. fasc. 3).

THE *North American Entomologist* for August, 1879, contains a paper by Mr. A. R. Grote "On the Neuration in certain Genera of *Pyralidæ*," illustrated by a plate with outline figures of the neural characters of fourteen genera, which should prove of great service to students of *Lepidoptera*.

MR. T. R. ARCHER BRIGGS, of Plymouth, announces the early publication of a Flora of Plymouth, including the Flowering Plants and Ferns growing within a distance of about twelve miles from the town. The almost unrivalled critical knowledge of our native plants possessed by Mr. Briggs will render this a valuable contribution to geographical botany.

MESSRS. D. M'ALPINE and A. N. M'Alpine announce the publication of a Biological Atlas, being a guide to the practical study of plants and animals, illustrating the characters of typical forms by drawings of the object, dissections, microscopic preparations, and diagrams, with explanatory text, specially designed for the London University, Science and Art, Medical, and other examinations, and for use in schools and colleges. The Atlas will consist of 24 plates, containing 423 coloured figures and

diagrams, and is to be published by Messrs. W. and A. K. Johnston.

THE Göttingen Royal Society of Sciences offers a prize of 50 ducats for the best treatment, by new researches, of the question as to the processes of development of the adult echinoderm. In addition to what is known of the embryonal development of echinoderms, it must specially be shown how the animal grows from the larva form to the completed system of organs. It is open to competitors either to examine a characteristic kind of development-process in all its features, or by exhibiting the development of different forms, to establish a common behaviour for the whole; in the latter case, the chief agreements and divergences in the formation of the organic system in different forms of echinoderms must be indicated from their earliest occurrence. The Society re-propose their question as to the nature of the *unpolarised light-ray*, researches being desired which will bring conceptions as to natural light of any source, near, in definiteness, to those which theory associates with polarised light. Papers on these subjects have to be sent in before the end of September in 1881 and 1882 respectively.

THE Reale Istituto Lombardo offers prizes of various value in connection with the following among other subjects:—The climatology of Italy; Critical history of the telephone; Œnology, especially in ancient Italy; The nature of miasma and contagion; Motor centres of the cerebral cortex; Etiology of cretinism and idiocy; Demonstration by experiments, whether the generative matter of hydrophobia is a virulent principle or an organic germ; Elucidation of some facts of the macro- or microscopical anatomy of the human brain. Particulars with reference to these will be found in the *Rendiconti* of the Institute (vol. xii. fasc. xvii.–xviii.).

A GERMAN translation of Schiaparelli's work on the planet Mars has just been published by Herr Georgi, of Leipzig.

MR. SHRUBSOLE asks us to say that he will exhibit specimens of the diatoms he states he has found in the London Clay at the annual meeting of the Geologists' Association on February 6.

A SEVERE earthquake was felt at Coire, in the Grisons, early on the morning of the 7th inst.

DURING these last twenty years numerous complaints have been published or sent to the public authorities with regard to the organisation of the observatory of Algiers. This unhappy state of things has now come to an end. This establishment has been placed under the authority of the rector of the Academy, and a lectureship in astronomy has been created. The same decree has organised the several preparatory schools recently created by law. An Oriental Section has been organised, and the lectureship for Arabic existing in Algiers, Oran, and Constantine have been connected with it. Chairs for Mussulman Law, African Geography, African Antiquities or History, have been created by the same decree. M. Pomel, one of the Senators for the Algerian provinces, has been appointed director of the School of Sciences and Professor of Mineralogy in the same schools. He will be obliged to resign his senatorship.

MR. C. LLOYD MORGAN, Associate of the Royal School of Mines, F.G.S., Lecturer on Science and English Literature at the Diocesan College, Rondebosch, Cape Town, has been appointed Examiner in Natural Science at the Cape Town University.

WE have received the first number of the *Angler's Note-Book and Naturalists' Record*, a repertory of fact, inquiry, and discussion on field sports and subjects of natural history. It is a neat small quarto, and might serve a very useful purpose; the first number, however, contains far too many extracts from othe journals, many of them years old. The publishers are Satchell and Co.

THE *New York Herald* articles and telegrams relating to the new Edison light have created much sensation in Paris, and caused a fall of 3*l.* in the shares of the Compagnie d'Eclairage et de Chauffage par le Gas. It is said that the judicial authorities are engaged in an inquiry directed against the *Figaro*, which published the news with aggravating embellishments.

M. FERRY has taken an important resolution obliging students to make use of the magnificent opportunities afforded by the Jardin des Plantes. The professors of botany and natural history of the schools of medicine and pharmacy have been authorised to deliver their lectures in the amphitheatre of that establishment. A special commission has been created consisting of these professors and the professors of the museum. A new chair has been instituted of vegetable physiology, and M. Dehairain has been appointed professor. M. Dehairain has edited during a series of years the *Annuaire du Progrès des Sciences*, written by himself and a large staff of contributors selected from among the most popular scientific writers.

THE new number of the *Proceedings* of the Berwickshire Naturalist's Field Club is as varied and interesting as usual, with papers on the natural history, antiquities, folk-lore, and local history of the Border.

THE new volume of the "Year Book of Facts in Science and the Arts" (Ward, Lock, and Co.) is no improvement on its predecessor; it is solely the work of unintelligent scissors and paste, and no more represents the science of the year than a few clippings from a third-rate illustrated journal would do the art.

DR. SCHOMBURGK, the director of the Botanic Garden, Adelaide, has issued a little pamphlet "On the Naturalised Weeds and other plants in South Australia." As this writer truly says, "From the past and present constant intercourse with Europe and other parts of the world, and the abundant importation of seeds into Australia for agricultural and horticultural purposes, it is no wonder that a very great number of the weeds most troublesome at home are now naturalised in South Australia." It is shown that a point of interest might occur whether the altered circumstances which now seem to be so favourable to the growth of the acclimatised weeds will prove permanent, or, by a change effected by over-stimulation, whether degeneracy and subsequent extinction might not follow. Such an effect, however, is not yet observable, the growth being quite as luxuriant as they were eighteen to twenty-five years since. The list contains the names of many of our best, or worst, known weeds, some of which have so firmly established themselves that it is almost impossible to eradicate them. Thus the extension of *Onopordium acanthium* was so rapid that the Legislature passed an Act in 1862 for preventing the further spread of this plant as well as those of *Carduus marianus*, and *Xanthium spinosum*. "According to the Act every owner or occupier of land upon which, or upon the adjacent half of any road, the above-mentioned thistles are growing, is obliged in twenty-one days after notice, signed by any chairman of a Road Board or District Council, has been served upon such owner, to destroy the thistles on his land; otherwise he is liable to a penalty not exceeding ten pounds. The Government must, on all unoccupied Crown lands, employ the necessary labour to eradicate the thistles. This stringent measure it is true has decimated the plants, but without effecting the object desired. Although thousands of pounds have been spent for the purpose, the destruction of thistles is generally commenced too late to prevent the dispersion of the developed seed." The pamphlet, though composed of only thirteen pages, appears to have been hurried through the press,

for numerous mistakes occur in the spelling both of the common as well as of the scientific names; thus we have Spury for Spurry, Cormwell for Gromwell, Torn-apple for Thorn-apple, *Hordeum murianum* for *H. murinum*, *Anthoxanthum oderatum* for *A. odoratum*, &c.

In four bone-caves of Upper Franconia different proportions of mammalian remains are met with ("Some Franconian Cave Faunas," by A. Nehring, in Report of Proceed. of the Imperial Geolog. Instit. Vienna, August 31, 1879). The bones of the older layers are darker in colour, and belong to the collared lemming and other decidedly arctic species. Bats are absent. This fauna probably existed at the end of the glacial period, when there were as yet few forests, or none, in the surrounding region. The bones belonging to a later period are lighter in colour, and indicate a post-glacial forest-fauna, mingled with a few arctic species. Bats requiring a temperate climate are abundant. These more delicate remains may have been brought to the caves by owls. This later cave-fauna of Upper Franconia agrees with that of Balve in Westphalia.

In the United States a series of experiments has been made by the Ordnance Department in the use of the telephone to assist in determining the time of flight of small-arm projectiles, which has hitherto been a matter of great difficulty at long ranges, owing to the impossibility of seeing them strike. One telephone was placed within a few feet of the gun, and the other (both being provided with Blake's transmitters) in the shelter, about thirty feet in front of the target. The telephone being placed to the ear, a stop-watch, beating fourths of a second, was started at the moment of firing, and stopped on the bullet striking. The observations founded on a large number of experiments never differed more than a quarter or half of a second from each other, the slight delay in starting the watch being neutralised by the delay in stopping it. It was found that the time of transit was affected by the wind, being shortened by a rear and lengthened by a head wind.

From the Canaries we continue to receive the *Revista de Canarias*, which we are pleased to see has reached its twenty-third number, and still continues to devote a fair amount of its space to science.

A second edition of Mr. W. H. Penning's "Text-Book of Field Geology" has been published by Baillière, Tindall, and Cox, with several additions and improvements.

It is stated that a seam of exceedingly good coal has been opened up on the Irwin River in Western Australia. Its existence appears to have been known, though no attempt had been previously made to work it.

M. Cochery, Minister of Postal Telegraphy, has asked from the French Parliament a credit of 320,000*l.* for establishing a subterranean telegraphic communication between the principal French cities and Paris. This resolution has been taken in consequence of the number of interruptions experienced in the aërial service during the present winter. For days the communication with Marseilles was conducted by a single line.

The number of the *Transactions* of the Asiatic Society of Japan which has just come to hand, contains several papers of interest from different points of view. Among these may be mentioned "Analyses of Surface Waters in Tokiyo [Yedo]" by Mr. R. W. Atkinson; "The Chemical Industries of Japan," by the same; "A History of Japanese Art," by W. Anderson; and notes by the Rev. J. Summers on Osaka, usually known to the outer world as the commercial capital of Japan.

The additions to the Zoological Society's Gardens during the past week include a Brown Bear (*Ursus arctos*), three — Snakes (*Tropidonotus tigrinus*) from Japan, presented by Messrs. James Veitch and Sons and Mr. Chas. Maries; an Arabian Gazelle (*Gazella arabica*) from Arabia, presented by Miss M. Murray; two Corean Pigs (*Sus sp. inc.*) from the Island of Quelpart, Corea, presented by Dr. Sydney Ringer; a Japanese Hawk Eagle (*Spizaëtus orientalis*) from Japan, presented by Mr. Harry Pryor, C.M.Z.S.; two Common Gulls (*Larus canus*), British, presented by Mr. George Weaver; a Robben Island Snake (*Coronella phocarum*), four Rufescent Snakes (*Leptodira rufescens*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; a Rhomb-marked Snake (*Psammophylax rombeatus*) from South Africa, presented by Mr. Eustace Pillans; three Oyster-catchers (*Hæmatopus estralegus*), three Brant Geese (*Berniela brenta*), British, purchased; a Yellow Conure (*Conurus solstitialis*) from Guiana, received in exchange.

OUR ASTRONOMICAL COLUMN

Winnecke's Comet.—The only known comet of short period due at perihelion within the present year is that discovered by Winnecke in March, 1858, which was soon found to be identical with the third comet of 1819, detected by Pons at Marseilles on June 12, having completed seven revolutions in the interval. Encke had shown that the observations in 1819, extending over thirty-six days, were best represented by an ellipse, with a period of 2052 days, or 5·618 years, but it is not upon record, so far as we know, that any serious attempt was made to recover the comet when with Encke's period it might be expected to be near perihelion, and thus it remained for Winnecke to find it again after a lapse of nearly forty years. The perturbations by Jupiter and Saturn during this period have been calculated by Clausen, with the view to fix the precise value of the mean motion at the perihelion passage in 1858. Another revolution would be completed in November, 1863, but the comet's track in the heavens under that condition is so unfavourable, that no observations were secured. At the next return in 1868, however, it was well observed, and again in 1875. The calculations for this comet are understood to be in the hands of Oppölzer, of Vienna. With his elements for 1875, the next perihelion passage, without having regard to perturbations which must be small in the present revolution, would fall at the beginning of December next, in which case, the comet's apparent track must be again an unfavourable one; indeed it seems questionable if it will be possible to obtain observations. The most likely time will perhaps be late in January, but the intensity of light will then be very small.

Oppölzer has suggested that the comet imperfectly observed by Pons in February, 1808, in the constellation Ophiuchus, may have been identical with Winnecke's, if it were in perihelion on or about April 12. The following particulars relating to the comet of 1808 appear to have been obtained from Pons's papers, and were communicated to Schumacher by Inghirami:—"La comète du 6 fevrier 1808, est une des comètes qui ont echappé aux astronomes sans pouvoir en calculer les elemens à cause que l'on n'en a pu avoir que quelques positions très-douteuses par méprise avec d'autres nébuleuses. Elle était très faible et difficile à voir. La nébulosité était ronde, elle s'étendait à peu près un dégré et on y soupçonnait par intervalle un très faible noyau en deux parties. Son mouvement était assez rapide vers le sud et l'on n'a pu l'apercevoir que 3 jours parceque la clair de lune était très-fort, de sorte que malgré de recherches très-opiniatres, on ne pouvait pas même la soupçonner le 10." There is then given a "Configuration renversée du 3 fevrier vers les 5h. du matin dans le grand chercheur qui à peu près a 3 dégrés de champ;" and it is added: "Les deux nébuleuses marquées dans la figure sont sur le ventre d'Ophiuchus un peu au-dessous de l'Equateur." Oppölzer identifies the nebulæ as Nos. 9 and 10 of Messier. In Zach's *Correspondenz* the comet is called a very small one, and nothing is said as to its rapid motion. It is evident that if the statement forwarded to Schumacher is the correct one, the comet moving quickly and with an apparent diameter of nearly a degree must have been in near proximity to the earth. Winnecke's comet in perihelion, on April 12, would have had about the following positions:—

	h.	° ′		° ′	
Feb. 5 at	16 ...	R.A. 237 56 ...	Decl.	− 7 0 ...	Dist. 1·04
,, 8 at	17 ...	,, 241 39 ...	,,	− 7 31 ...	,, 1·01

So that the motion, though southerly, would be but small. The identity of the comet of 1808 with Winnecke's comet is therefore at least doubtful.

In 1833 Clausen made what appears a more likely suggestion, that the comet of July, 1819, was identical with the second comet of 1766, which was observed for a short time only by Messier at Paris, before perihelion passage, and after perihelion by La Nux in the Isle of Bourbon, though but roughly. Burckhardt found, in 1817, that the whole of the observations could be represented within their probable limits of error by an ellipse with a period of revolution little over five years. The planet Jupiter must have acted powerfully upon Winnecke's comet towards the end of the last century, and, so far as we can see, it appears possible that the perturbations occasioned at that time may account for the differences in the orbits of 1766 and 1819. If Burckhardt's elements for the comet of 1766 are approximately correct, as seems probable, it may have been detected at its first visit to perihelion in the actual form of orbit, perhaps at its first visit after being fixed in the system through the agency of Jupiter. We know that Brorsen's comet of short period was discovered under similar conditions.

METEOROLOGICAL NOTES

SIX years ago we remarked (NATURE, vol. ix. p. 164) that what was required in order to describe and classify many forms of clouds, were accurate delineations of these forms in their different aspects, and systematic inquiries as to the relations of clouds to the mode of their formation, to the states of the aqueous vapour composing them, and to the varying elasticity, temperature, and electricity of the atmosphere. Since then but slow progress has been made, the great desideratum being the contribution of data in a form on which science can lay its hands. A contribution of data of this sort has just been made by Dr. Hildebrandsson, the director of the meteorological observatory of Upsala, in a memoir on the "Classification of Clouds employed at the Observatory," illustrated with sixteen photographs of clouds. The photographs, which are about nine by seven inches, are very fine ones, and well chosen out of a large number taken under the direction of Dr. Hildebrandsson, to illustrate the different forms of cloud and their more important modifications and transitional states. The series representing the more marked changes from the delicately-pencilled cirri of the flimsiest texture to the nimbus of a rain-cloud is a most instructive one; as is also the series showing the strato-cumulus as commonly observed during the winter season in Scandinavia. The relations of the varying forms of clouds to cyclones and anticyclones which pass over Sweden is just touched on, but this important phase of the inquiry we hope Dr. Hildebrandsson will again return to, seeing he can so readily refer to the observations of his observatory, which give so complete and satisfactory a record of the various fugitive phenomena of the weather changes of that part of Sweden. Dr. Hildebrandsson's photographs of clouds may be studied with equal interest and professional advantage by artists as well as by meteorologists, it being scarcely possible to point to any department of art standing more in need of a thorough reformation than the cloudscapes of our landscape painters.

THE Hydrographic Committee of the French Marine has at a recent sitting sanctioned the publication of the last four of the series of sixteen wind-charts prepared by M. L. Brault. In these four charts the winds of the Pacific are dealt with, the winds of the North Atlantic, the South Atlantic, and the Indian Ocean being discussed in the twelve charts previously prepared. In preparing these sixteen charts M. Brault has made use of upwards of 3,000,000 observations made over the oceans and continents of the globe. The chief results referring to the circulation of the atmosphere show as regards the South Pacific, which presents the largest expanse of ocean least influenced by land, a belt of calm or light winds near the equator; then the well-known south trades; to these succeeds a belt of winds variable as regards direction, but blowing with a force at least as great as the trades; and lastly, westerly winds, varying little, though more than the trades, in direction, and incurving upon the South Pole the nearer they approach it, and blowing much stronger than the trades and variables. As regards the other oceans, the disturbing influence of the land is felt in proportion to the extent of the continents which surround them, the disturbing influence reaching its maximum in August and January, in other words in those months when atmospheric pressure of the continents is in greatest excess or defect compared with that of the ocean as shown by the isobaric charts of the globe.

M. L. TEISSERENC DE BORT has prepared isabnormal charts of the temperature and pressure of the atmosphere, with the view of comparing, with some exactness, these two all-important factors of atmospheric circulation. He finds that when any region presents an excess of temperature, either absolute or relative to that of places in the same latitudes, a barometric minimum tends to be formed, and that the coincidence between the minimum of pressure and the maximum of temperature is almost complete. The tendency results in either a well-defined area of low pressure, or in the less pronounced form of a simple distortion of the isobaric lines as they cross the region of relatively high temperature. On the other hand, barometric maxima tend to establish themselves over regions whose temperature is either absolutely high or relatively so to the latitude, and the tendency to an increased pressure is the more decided when the region in question is surrounded by regions of low pressure.

AT a meeting of the Botanical Society of Edinburgh, held on Thursday, the 8th inst., Sir Robert Christison read a paper of very considerable importance on the relative growth of the trunks of trees during 1879 as compared with 1878. Upwards of two years ago Sir Robert set on foot a system of measurement of the girths of a large number of well-grown trees in Edinburgh and neighbourhood, the measurements being made by himself with the same measuring-line, and the same circumference to be measured secured by marking it at the time of the first measurement with paint. The inclement character of the summer months of 1879 as compared with 1878 was described by a reference to the daily maximum temperatures noted at the Edinburgh station of the Scottish Meteorological Society, from which it appeared that for the six months ending with September the mean for 1879 was fully 5°·0 less than for 1878, and the deficiency of day temperature amounting to nearly 10°·0 Of 11 deciduous trees, exclusive of oaks, the deficiency of growth during 1879 as compared with 1878 was 41 per cent.; of 17 evergreens of the pine tribe, the deficiency was 20 per cent.; and of 7 oaks the deficiency was 10 per cent. The 7 oaks were of different species, but they all gave results closely agreeing with each other. We shall look forward with the greatest interest to the annual reports of this investigation, which may be expected to reveal novel and valuable results illustrative of the bearings of meteorology on the growth of our forest trees.

AN interesting account of waterspouts observed on November 10, 1879, off Cape Spada, west of Canea, by Herr Miksche, has been communicated by him to the Vienna Academy. About 9 A.M. some heavy thunder-clouds rose in the west in a clear sky, reaching the zenith only after noon. One in advance, very black, and low-hanging, gave, about ten minutes to one o'clock, the phenomenon of the waterspout, a thick descending column, of milk-white appearance, being formed from it. The amount of downward gyrating force may be approximately estimated from the fact that at the distance of some eighteen miles one could distinctly see with the naked eye, a high round pedestal, formed by the foaming sea-water, like the socle of a monument. After ten minutes' duration, the column lost its conical form and began to assume a rectangular one; while, at the extreme eastern point of the cloud, a second waterspout was formed, conical in shape and of the same hue and intensity as the first. To this column also the sea presented a pedestal visible to the eye. For fully five minutes the water discharge continued with like intensity in both trombes. Precisely at five minutes after 1 P.M., *i.e.*, about a quarter of an hour after formation of the first trombe, an angular discharge of lightning (without audible thunder) took place from the clouds at that part into the sea; then the trombe suddenly ceased, only the pedestal continuing some time to show where it had been. The second trombe remained unaffected five minutes longer, then was extinguished without lightning discharge, and without reverting to the original conical form (as the first did). This fine display of natural forces was quite finished at 1.16 P.M., the clouds then uniting and pursuing their course eastwards.

GEOGRAPHICAL NOTES

AT the meeting of the Geographical Society on Monday last, a letter was read from Mr. Thomson which had that day been received *viâ* Mozambique, announcing the arrival of the East African expedition at Mbungo, at the north end of Lake Nyassa, on September 22. Mr. Thomson was unable to discover the Uranga country and river, described by the late Capt. Elton as lying near Merere's town, but he believes the river to be the

Mbangala, which flows into the Ruaha. According to Mr. Thomson, the formidable range, called the Konde mountains, is simply the termination of a plateau which rises from an altitude of 3,500 feet in 8° 50′ S. lat. to not more than 9,000 feet at the lake. Mr. Thomson was to leave for Lake Tanganyika on September 28, and we may fairly hope that by now he has completed his explorations, and is on his way back to the coast. The papers of the evening were "The Grand Canal and Yellow River of China," and "Hankow to Canton overland," by Mr. G. J. Morrison. During the journey referred to in the former, Mr. Morrison was enabled to examine some 200 miles of the Yellow River, a portion of which has materially altered since it was described by any traveller, and his observations are, therefore, very useful. Mr. Morrison, it may be noted, is of opinion that the Yellow River is now flowing in its natural channel, and that in former times it discharged its waters into the sea north of the Shantung promontory. His description of the condition of the Grand Canal is also interesting, as he looks at it from the point of view of a practical engineer. The other paper, from which only extracts were read, described a journey undertaken with the object of getting some idea of the country through which one of the great railway lines of the future may be expected to run, and a portion of which embraced the rich mineral field of Southern Hunan examined by Baron Richthofen a few years ago.

AN interesting piece of exploration has just been successfully accomplished by the Church Missionary Society's agents in Western Africa. In a small steamer they have ascended the River Binue from its confluence with the Niger to a point probably about 800 miles from the sea. The party penetrated 150 miles beyond Hamaruwa, which was reached by Dr. Baikie when in search of Dr. Barth in 1854, and a careful survey of the river has been executed.

M. PÉTRIMENT (*Bulletin* of Paris Anthropological Society, t. ii. fasc. 3), in confirmation of M. Madaillac's assertion that a blonde race existed in Persia, had engaged a Persian doctor, Mirzâ Mohammed, some time resident in Paris, to obtain definite information on this point. According to this gentleman there are about 2 per cent. of blonde persons in the Persian population, blonde children appearing in brunette families after the lapse of a generation or two. According to local tradition, the white men came from the north, and were *sheitâns*, or demons; this evil character is still attached to blonde individuals in Persia, where they are generally impetuous and artful, and seldom possessed of a lymphatic temperament.

M. DE UJFALVY, in his recent travels through the Russian territories of Central Asia, has visited the lands of the Galtchas, Sarts, and Tadjiks, where he found that caste and patriarchal authority were rigidly observed. The people are Mussulmans, and consequently polygamists, and the women are held in great subjection. The Galtchas in their nomadic wanderings ascend the mountain-slopes of Kohistan in search of pasture. To the east of their country we would seem, although close to the plains of Pamir, to be on the extreme limits of the Aryan race, for here in the Kuldja district the oblique-eyed Mongolians begin to predominate. At this point, where the Mountains of Heaven form a line of division, the white and yellow races meet, and even overlap one another to some extent, although the strict observance of caste has hitherto prevented their complete fusion, and has left the Aryan races to form isolated ethnic groups in the midst of an otherwise Mongolian population. M. de Ujfalvy is at present engaged in completing the narrative of his travels in this part of Central Asia, and his observations on the distinct characters of the Galtchas and other kindred races can scarcely fail to afford valuable aid in the solution of the vexed question of the limits of demarcation between the Mongolian and Aryan races.

IN No. 83 of the *Zeitschrift* of the Berlin Geographical Society Dr. Hildebrandt concludes the narrative of his journey from Mombassa to Kitue, and this is followed by some remarks on his measurements of heights in the Wakamba land. *Apropos* of the recent Karl Ritter celebration, we have two papers on that geographer; one by Pastor Tallin on Michael Servetus as a predecessor of Ritter and Humboldt, and the other by Dr. Marthe on what Ritter did for geography. In a letter from Gerhard Rohlfs, that traveller maintains that none of the greater carnivora are found in the Sahara, while, in reply, Drs. Ascherson and Hartmann endeavour to show that this statement must be received with some modifications The *Verhandlungen* (Nos. 1 and 9, Band vi.) of the same Society contains a paper by Herr Schütt on his travels in Central Africa.

TWO important congresses will be held next year by the French geographers. The first will be held at Lyons, and will deliberate on the means of regulating the explorations of Africa by French travellers or colonists. The second will be held at Nancy in Summer, at the conclusion of the meeting of the French Association, which will meet at Rheims, on general subjects.

A DEPUTATION waited on the Lord Mayor last week to bespeak his patronage in behalf of Commander Cheyne's elaborate and expensive scheme for reaching the North Pole. The Lord Mayor promised the use of the Egyptian Hall to have the scheme "thrashed out" at a public meeting.

IN connection with letters from Lieut. Bove on the work of the North-East Passage Expedition, the *Bolletino* of the Italian Geographical Society publishes several sheets of illustrations of the natural features along the routes, heads of the natives met with, sledges, implements, and weapons, native houses, &c., besides two excellent maps.

THE *Bulletin* of the Paris Geographical Society for November contains a translation, by M. Barrande, of the memoir by the Russian Grand Duke Nicholas on the Amu and Uzboi. Also an important paper by Dr. Lange, on the cartography of the Brazilian province of Santa Catharina, and the continuation of Admiral Fleuriot de Langle's article on African migrations.

THE new *Bulletin* of the Geographical Society of Oran, Algeria, is largely occupied with the Trans-Saharan Railway. The question is dealt with from a commercial point of view, and among the other contributions to the subject is a note on the western route and that proposed by General Colonieu.

THE new number of the *Bulletin* of the Société de Géographie Commerciale of Bordeaux contains the first portion of an address delivered by M. Soleillet on the Trans-Saharan railway project, in connection with which he is about to undertake explorations in West Africa.

THE publication of a new geographical journal is announced, the *Revista Geografica Internationale*. It will appear fortnightly, and will contain original articles in Italian, English, French, and Spanish, not a happy group, we think; French, English, German, and perhaps Italian, would have been much more representative. The editor is M. A. M. Mizzi, and the journal is published at Malta.

PHYSICAL NOTES

AN attempt is made in *L'Electricité* by M. C. E. Séguin, fils, to claim for France the honour of the invention of the phonograph; firstly, by the plea that M. Léon Scott (who died only last July) patented the instrument under the name of the phonautograph in 1857, and secondly, by the statement that M. Charles Cros deposited before the Académie des Sciences, in April, 1877, seven months before the date of Edison's patent, a sealed packet describing the possible reproduction of sounds from recorded traces. In justice to Mr. Edison, we can hardly admit the validity of either of these claims. The phonautograph of M. Scott merely recorded the graphic traces of vibrations in sinuous scratches upon a smoked surface, which, therefore, was useless for the purpose of reproduction of the sounds; and, moreover, Dr. König, who worked upon the instrument with M. Scott, and perfected it, has stated to us most candidly that the idea of reproducing the sounds from the recorded traces never occurred either to M. Scott or to himself; and that neither of them attempted or proposed to obtain graphic traces in hollows and ridges in tinfoil or soft metal, or otherwise than as plane curves. And as for the claims of M. Cros, we have yet to learn that he constructed an actual phonograph, or that his sealed packet contained any descriptions of a sufficiently detailed or practical nature to enable any instrument to be made from them.

PROF. BORLINETTO, of Padua, has devised two very simple and effective pieces of apparatus for showing the passage of electric sparks through such non-conducting liquids as turpentine, petroleum, &c. They consist of U-tubes of glass, with or without an intermediate branch, and having platinum wires led down the two branches or introduced through the glass walls, so as nearly to meet, the other extremities of which can receive the discharge from a Leyden jar or from an induction-coil.

To study the fluorescent spectrum many physicists adopt the method of projecting a spectrum sufficiently pure to show the principal Fraunhofer lines, on a fluorescent body, solid perhaps, or the side of a glass vessel containing a fluorescent liquid, and determining the parts where the fluorescence appears, reaches a maximum, and disappears. Others develop the direct spectrum on the surface of a liquid; Herr Hagenbach places the slit and the prism horizontally, and projects the spectrum on the free surface of the liquid. The disadvantages of these two methods M. Lamansky (*Jour. de Phys.*, Dec.) has sought to avoid in a spectroscope he has had recently constructed by M. Duboscq, and which he finds very convenient. The collimator and the telescope of this direct vision spectroscope are fixed separately on a graduated circle; they may be placed at various angles in the vertical plane. The collimator is furnished with a small adjustable mirror for directing the luminous rays along the optic axis. In the prolongation of the collimator tube is placed the direct-vision prism and a lens which throws the spectrum on the surface of the liquid contained in a small vessel on a table which can be raised or lowered. The telescope is directed to the same liquid surface, and the focal distance of the ordinary telescope is shortened by the addition of a second object-glass, which may be removed at will. The division of the circle allows of determining the angles at which the coloured rays fall on the liquid surface and the angles at which the fluorescent spectrum is observed. A dark cloth may be thrown over the apparatus to exclude disturbing light.

An interesting observation on the supernumerary or spurious rainbows occasionally seen lining the inner edge of the primary arc of a rainbow has been made by M. Montigny. These supernumerary rainbows usually consist of a red band touching the violet on the inner side of the bow, followed by green and violet, and passing again to red. Indeed it is possible occasionally to observe as many as four or five recurrences of the red and green tints. They are, however, almost always confined to the highest portion of the bow, and are rarely observed near the ground. M. Montigny, on August 30, 1879, watching a rainbow near Rochefort, a little before sunset, noticed that while the upper portion of the primary bow showed no trace of supernumerary bows, the lower portions on each side, which came out brilliantly against a stratum or zone of misty air, were furnished with no fewer than four supernumeraries of paler tint. According to the received theory of Young and Airy these bows are due to diffraction, caused by very small drops, the smallest drops giving the broadest and most brilliant fringes of colour. Usually it happens that in the higher regions of the air the falling drops are smaller than they are at the lower regions; hence the occurrence of supernumerary arcs at the upper part of the bow. In M. Montigny's observation, doubtless, the misty zone lying near the ground provided the drops of the requisite degree of smallness to produce the diffractive effects. This is, at least, his view of the case.

In the December number of *Silliman's Journal* is a memoir of extreme interest by Dr. E. L. Nichols on the character and intensity of the rays emitted by glowing platinum. Several tables of statistics of observations are given, and two graphic charts which embody the tabular results. Reviewing the *à priori* law of Kirchhoff, concerning the emission of rays of greater refrangibility at higher and higher temperatures, he remarks: "Strictly speaking, however, the temperature at which each individual wave length becomes visible depends solely upon the sensitiveness of the observer's eye. We are furthermore forced to conclude from experiment that the more refrangible rays really exist at temperatures far below those at which we begin to see them. The directions of the curves (Plates I. and II.) seem to denote that all the rays studied begin to be emitted at some temperature not included in the interval embraced by the experiments. *I suspect indeed that all of them originate at some very low degree (the absolute zero?), and are recognizable no sooner, simply because the various instruments at command, the thermopile, eye, photographic plate, &c., are not more delicate.* That the various colours do not appear simultaneously, follows from the very different degrees of sensitiveness shown by the eye for different rays."

An interesting electric toy, contrived by M. Pfeiffer, is described in a recent number of *La Nature*. It is a small electrophorus consisting merely of a thin plate of ebonite about 1 mm. in thickness; the usual wooden disk with tinfoil is replaced by a small piece of tin about the size of a playing-card, attached to one of the faces of the ebonite plate. This electrophorus produces electricity with great facility. You have merely to place it on a wooden table and rub it successively on its two faces with the open hand; then on lifting it with the left hand and bringing the right hand near the tin plate, a spark is obtained 1 to 2 centimetres long. Several small accessories, skilfully contrived, are added to the electrophorus; among these are dancing puppets made of pith, which manifest very amusingly the phenomena of electric attraction or repulsion. Electrify the ebonite plate, put the three puppets on the tin, and then raise the plate from its support. One small personage lifts his arms above his head; the hair of a second stands out; and the third, lighter than the others, jumps about like a clown, while two pith balls placed at his side dance with him. M. Pfeiffer has also collected in one small box all the known accessories of an electric machine; a miniature Leyden jar, an electric carillon, a Volta pistol, a Geissler tube, &c., these being operated with the electrophorus.

SCIENTIFIC SERIALS

American Journal of Science and Arts, December, 1879.—Mr. Brooks here calls attention to an important difference in the breeding habits of American and European oysters; the eggs of the former are fertilised *outside* the body of the parent; and during the period which the European oyster passes inside the mantle cavity of the parent, the young American oyster swims at large in the open ocean. Mr. Brooks traces the successive stages of oyster development.—Mr. Harting writes on triple objectives with complete colour-correction.—There are geological papers on Virginia, on Galisteo Creek, New Mexico, and on Catrosa Co., Georgia.—Prof. Verrill describes two new species of cephalopods caught off the coast of Massachusetts; also what is the second known representative of the remarkable family of *Cirroteuthidæ*.—Dr. Nichol's researches on the character and intensity of the rays emitted by glowing platinum (see NATURE, vol. xxi. p. 184) are here given in detail.—Prof. Marsh's notes on Jurassic dinosaurs, and Dr. Draper's researches in photography of stellar spectra have been already noticed in our columns.—Prof. Peters contributes observations on the planets Hersilia and Dido; and in the "Scientific Intelligence" we note two useful lists of the (209) minor planets, numerical and alphabetical.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 8.—"On the Photographic Method of Mapping the Least Refrangible End of the Solar Spectrum (with a map of the Solar Spectrum from 7600 to 10750)," by Capt. W. de W. Abney, F.R.S., R.E.

The author refers to the sensitiveness of different forms of silver salts when exposed to the action of the spectrum, and shows how he has been able to prepare, by methods indicated, silver bromide which absorbs the red and ultra-red rays, and which is sensitive to these rays.

In his paper he describes the apparatus employed by him in the photography of the invisible least refrangible rays, both with a prismatic, and also with the diffraction apparatus. From photographs taken with the latter, he has constructed a map extending from λ 7600 to λ 10750, which he submits to the Society. He shows also that in the photographs of the prismatic spectrum, he has apparently reached the limiting length by comparing it with photographs of the diffraction spectrum. The author has also compared Lamansky's prismatic thermograph with his photograph. The paper closes with some theoretical remarks on the silver compounds employed.

Mathematical Society, January 8.—C. W. Merrifield, F.R.S., president, in the chair.—Prof. W. S. Burnside was elected a Member.—Prof. Cayley, F.R.S., communicated two formulæ in spherical trigonometry which are included in the one form—

$$\tan \tfrac{1}{2} c\,(\cos B - i \sin B) = \tan\left(\frac{c}{2} - \phi\right),$$

where $i = \sqrt{-1}$ and $\tan \phi = \tan \tfrac{1}{2} b\,(\cos A + i \sin A)$.

The note which the President read at the last meeting simply gives (as has been pointed out to him since) some symmetrical cases of the orthogonal transformation, of a much more general character (but unsymmetrical) given by Mr. Cayley, and reproduced in Salmon's "Higher Algebra" (3rd edition, p. 39). The symmetrical form may be obtained from the one there given by writing—

$$\frac{\mathrm{I}}{\Delta} = k, \quad \frac{\lambda}{\Delta} = a, \quad \frac{\mu}{\Delta} = b, \quad \frac{\gamma}{\Delta} = c,$$

and then putting $k = 0$. We thus get for the determinant the symmetrical form—

$$\begin{vmatrix} a^2 - b^2 - c^2, & 2ab, & 2ca \\ 2ab, & -a^2 + b^2 - c^2, & 2bc \\ 2ca, & 2bc, & -a^2 - b^2 + c^2, \end{vmatrix}$$

the value of which is r^3, and the sum of the terms on the leading diagonal $= -r$, where $r = a^2 + b^2 + c^2$. The terms of this determinant will be integral if a, b, c are either integral, or of the form integer $\times \sqrt{2}$, or indeed if they contain any common factor entering under the square root only. It has been shown by Legendre and Gauss that every integer, or its double, is the sum of three squares. It follows that an orthogonal transformation of the above symmetrical character can be found for every whole number r. The transformation is, however, nugatory for certain low values of r. The symmetrical transformation means a turn of two right angles about an axis whose direction cosines are proportional to abc. That is to say, if a cube be taken, with the axes for edges, and those of rational length, in a cubical system, it is always possible to find one or more axes, inclined to the co-ordinate axes, such that if we turn the cube about them through two right angles, its points will still rest on points of the system.—Mr. Hammond gave a form for the complementary function in fractional differentiation. Messrs. Cayley, Merrifield, Roberts, Glaisher, and Freeman took part in a discussion upon Mr. Hammond's communication.

EDINBURGH

Royal Society, January 5.—The Right Hon. Lord Moncrieff, president, in the chair.—At the request of the Council an address on the Trigonometrical Survey of Palestine was given by Lieut. Conder, R.E., late in command. Apart from its more technical nature, the paper contained many details of archæological, ethnological, and geological interest, including the discovery of the positions of not a few historic localities, such as the Cave of Adullam, Bethabara beyond Jordan, the vineyard of Naboth, &c.—Prof. Tait communicated a note on Minding's theorem by Prof. Chrystal. This beautiful theorem in rigid dynamics, the proof of which originally occupied many quarto pages of *Crelle's Journal* with elaborate analysis, had been proved by Prof. Chrystal by means of Plücker's congruencies, in a manner almost rivalling in brevity the quaternion demonstration by Prof. Tait. A generalisation of the theorem led to the discovery of a *volume-locus*.—Prof. Tait then communicated two mathematical notes: (*a*) on a problem in arrangements; (*b*) on a graphical solution of the equation $V\rho\phi\rho = 0$. The former was given under the name of The Mad Schoolmaster. A schoolmaster went mad, and began to operate upon his class of boys according to the following method:—The dux he put down one place, the new dux two places, the next dux three places, and so on till every boy in the class had been shifted at least once. He then began again putting the first dux down one place, the next dux down two, and so on as before. After 306 operations, he found the boys arranged exactly as they had been at the beginning. He then cast one out, and set to work operating similarly upon the remainder; but to his dismay found that he had to operate 1,120 times before they were brought back to their old arrangement. The problem is to find how many boys were in the class, and is of course a particular case of a much more general problem in arrangements. Prof. Tait gave a graphical method by which the inverse problem could be solved by a simple inspection for any number.

PARIS

Academy of Sciences, January 5.—M. Edm. Becquerel in the chair.—M. Wurtz was elected vice-president for 1880.—M. Daubrée gave information as to the Academy's publications and changes in members and correspondents. Two members have died in the year—MM. de Tessan and Gervais—and one correspondent—Mr. MacLear.—The following papers were read:—On the motion engendered by diffusion of gases and liquids, by M. Sainte-Claire Deville. The difference of velocity in passage of gases through a porous septum is utilised in raising liquid, a machine being thus produced which apparently does not consume heat. M. Debray's diffusion apparatus is used, being changed into a machine simply by adding tubes of discharge and valves. Dutrochet's endosmometer may be similarly changed to a machine.—On the hydride of copper; reply to M. Berthelot, by M. Wurtz.—On the heat of formation of hydrate of chloral; reply to M. Berthelot, by M. Wurtz.—Remarks on a recent communication regarding the photospheric network, by M. Janssen. The reticulated aspect produced by faculæ round spots has been long known, but has nothing in common with the photospheric network revealed by photographs. This is formed by the totality of points where the solar granulation is disturbed by upward currents of hydrogen, while the faculæ are due to gaseous masses above the granulated region. The former is in the photospheric layer, the latter above it; the network seen chiefly in the central parts, the faculæ only easily visible at the border. M. Lamey seems to have confounded the phenomena. The network is only visible in photographs 0·25 m. to 0·30 m. in diameter.—On treatment of phylloxerised vines, by M. Maren.—M. Perry was elected Member in Geography and Navigation in room of the late M. de Tessan.—Carbonic acid in the air in its relations with the great movements of the atmosphere, by M. Marié-Davy. A discussion of daily mid-day analyses (April, 1876, to June, 1879) at Montsouris, by MM. Levy and Allaire. The quantity of CO_2 in 100,000 parts of air in volume varied between twenty-two and thirty-six. Winds blowing from Paris contained, on an average, less CO_2 than those from the country. This might be explained by CO_2 occurring more largely in air *below* than *above* the layer of clouds. Three periods are noted: in the first, to November, 1877, the CO_2 was below the mean, and sometimes very low; in the second, to September, 1879, it was considerably above the mean; the third, commencing in October, 1879, showing very little CO_2. The second period was one of wet weather, and comprised two bad harvests; the equatorial current was predominant in France. This current had less extension in the first, which was also less wet, and gave better harvests. A complete change in the atmospheric circulation seems to have occurred since October.—On an application of the pre-existence of Ampère's currents in soft iron, by M. Trève. With iron solenoids he gets much better effects than with copper.—On new luminous tubes, by M. Trève. Into a large Geissler tube he introduces a Fizeau condenser, and fixes the electrodes (connected with the two poles of the induced current of a Ruhmkorff coil) to the eleventh and twelfth tin sheets. On reducing the pressure to 0·003m. or so, sound is no longer heard, but a brilliant white light springs in pearls from the sheets of the condenser, quite distinct from the common light of Geissler tubes.—Action of acetic anhydride on some phenol-aldehydes, by M. Barbier.—On a new synthesis of saligenine, by M. Greene. This is by reaction of chloride of methylene with phenate of sodium in presence of hydrate of sodium.—On the preparation of iodised and bromised derivatives of benzine, by M. Greene.—On the comparative value of monochromatic impressions in invertebrates, by M. Chatin. In arthropods, decapod crustaceans, and some insects, Dewar's current of the retina is well marked, and varies with different rays, reaching a maximum in the yellow-green region. In molluscs, and especially in pulmonated gasteropods, the differences are still more pronounced.—Histology, development, and origin of the testicle and ovary of *Campanularia angulata* (Hincks), by M. Fraipont. M. Touchimbert presented a photograph of forms of snow (resembling small roses) observed at Poitiers.

CONTENTS