THURSDAY, FEBRUARY 6, 1890.

TAVERNIER'S TRAVELS IN INDIA.

Travels in India of Jean Baptiste Tavernier, Baron of Aubonne. Translated from the original French Edition of 1676, &c., by V. Ball, LL.D., F.R.S., F.G.S., &c. In Two Volumes. (London: Macmillan and Co., 1889.)

JEAN BAPTISTE TAVERNIER was a Sindbad of the seventeenth century. To an insatiable love of travel, which prompted him even in his boyhood to rove through the greater part of Europe, and in his mature life to accomplish no less than six voyages to Persia, India, and the still more remote East, he united the faculties of a shrewd and successful trader. By his traffic in jewels and other costly commodities of small bulk, he turned his wanderings to profitable account, and amassed a fortune which enabled him to purchase the Barony of Aubonne, and to enjoy the dignified retirement of a wealthy old age. But, like a true traveller, he remained active-minded and active-bodied to the last. At the age of 79, attracted by the offer of the Elector of Brandenburg to conduct an embassy to India, he set forth on a circuitous journey through Europe, and, disposing of his estate and *château* of Aubonne, he embarked on renewed mercantile ventures. The few remaining years of his life were passed, for the most part, in journeying to and fro in Europe, and he died while so occupied. The place of his death has long been doubtful, and it has only recently been discovered, on the authority of a letter from the Swedish Resident at Moscow, that the indefatigable traveller drew his last breath at Smolensk, in February 1689, when on his journey to the ancient Russian capital.

Despite some inaccuracies and inconsistencies, due mainly to the incompetent editing of the original work, Tavernier's account of his travels has long been appealed to by Indian historians as a recognized authority—the testimony of an eye-witness to the condition of India under the later great Mogul emperors. At the time of his visits, the Mogul Empire was in the zenith of its power and splendour. On the occasion of his first journey to India, he found Shah Jehan, "the most magnificent prince that ever appeared in India," peaceably seated on the Imperial *masnad;* and throughout his dominions, though these were less extensive than in the time of his successor Aurungzebe, a degree of good administration and general prosperity surpassing that attained under any previous or subsequent emperor. He quitted India for the last time only about two months after the death of Shah Jehan, then deposed and imprisoned, when Aurungzebe was setting out on that career of conquest and oppression that in the following century brought about the wreck of the Mogul Empire, and exposed its rich cities and provinces to be wasted and despoiled by Maráthá hordes and Afghan invaders.

At a Court gathered around the famous peacock throne, where emperor and nobles vied with each other in the acquisition of costly jewels, an expert such as Tavernier was received as a welcome visitor; and in pursuit of his calling he travelled without hindrance through the length and breadth of India, visiting the European settlements of Surat, Goa, Madras, and Kásimbazár, the independent Court of Golconda (Hyderabad), and certain of the diamond-mines that were then actively worked both in Southern and Northern India. His work is a medley of historical memoranda, incidents of travel, itineraries, and details of his commercial dealings, put together without much system, but nevertheless highly instructive, and apparently far more trustworthy than was conceded to him by most of his contemporaries; altogether furnishing a fund of information respecting the state of India in the middle of the seventeenth century.

The latest English translation of Tavernier's travels appeared more than two centuries ago, and as Mr. Ball remarks, owing to the translator's misconception of the author's meaning, through want of local knowledge, and to serious abridgment, it gives a very inadequate idea of the true merits of the original work. Mr. Ball's own long experience of India, and his familiarity with its geography and the varied phases of native life, would alone have enabled him to correct most of the errors of his predecessors; and the deficiencies as a philological and historical critic which he modestly urges as having determined him, for a time, to abstain from attempting a new translation, have been made good by the invaluable assistance afforded by the late Sir Henry Yule, under whose advice he eventually undertook the work. The result is the two handsome volumes now before us, in which for the first time the old traveller's experiences are presented to English readers, elucidated by the results of modern research, and in a form which very greatly enhances their value for all purposes of future reference. Some few inconsistencies remain, and are duly pointed out in the footnotes, but they are such as relate to matters of detail, occasional confusion of dates or persons, and the like; and they do not appreciably detract from the general trustworthiness of the narration.

With the political and historical data of Tavernier's work it is hardly our province to deal in this place. Most of his facts relating to the Court of Delhi were probably furnished to him by his cotemporary and sometime fellow-traveller Bernier, and all that is important in them has been long rendered familiar to English readers in the lucid pages of Elphinstone. Neither need we dwell on his descriptions of native customs or the manner of life of those European exiles of various nationalities who were then, as pioneers, exploiting the riches of the East, with no small display of mutual jealousy and animosity, and indulgence in practices sometimes hardly less barbarous than those of the indigenous population amid which they dwelt. The social condition of the Indian people in Tavernier's day was essentially the same as when, more than a century and a half later, the British Empire having been raised and consolidated on the ruins left by Maráthás and Patháns, a new era of peace and civilization was inaugurated by Lord Bentinck, and the suppression of thuggi, dacoity, sati, and other barbarous rites of the Hindu religion, preceded the establishment of schools and Universities, and the opening up of the wilds of India by systems of roads and railways. The social regeneration of India, such as it is, has been almost exclusively the work of the last seventy years, and even now it has hardly penetrated far below the surface.

It was the information given by the traveller on the diamond-mines worked in his day, that first drew Mr. Ball's attention to the subject of Tavernier's travels. The mines visited and described by him have long been abandoned, and even their very sites forgotten. With free labour, and at its present enhanced rates, diamond-working is no longer so remunerative as under the despotic governments of the seventeenth century, and it is within the recollection of the present writer that the working of one of the most productive mines of the former Golconda State was let on behalf of the British Government at the modest rental of 100 rupees. Tavernier gives it to be understood, indeed, that only four mines were worked, all of which he visited; but Mr. Ball tells us there is ample reason for believing that they were far more numerous than he had any conception of; and in an appendix he gives a full list of all the Indian localities at which diamonds have been obtained as far as is known, together with the geographical co-ordinates of all such as he has succeeded in identifying. Owing to the vagaries of phonetic spelling, and the ignorance of Indian geography on the part of many who have dealt with this subject, this identification has been far from easy. As amusing examples of the way in which localities have been confused by some previous writers, Mr. Ball tells us that "one author gives Pegu as a diamond-mine in Southern India; in the Mount Catti of another we have a reference to the Ghâts of Southern India"; and he adds: "For some time I was unable to identify a certain Mr. Cullinger, who was quoted by one writer, in connection with diamonds. Will it be believed that this *gentleman* ultimately proved on investigation to be the *fort* of Kálinjar?"—a well-known historical fortress in Bundelkhand.

Indian diamonds are found exclusively in rocks of the Vindhyan formation or in the gravels of rivers that drain these rocks. The formation consists of sandstones, limestones, and other sedimentary rocks, certainly not more recent than the Lower Palæozoic age, but being unfossiliferous, their precise age cannot be determined. In Southern India the diamonds occur only in the Bánaganpili sandstone, at the base of the lower subdivision of the Vindhyan series, or in gravels derived from that bed. This is described by the authors of the "Manual of the Geology of India" as usually from 10 to 20 feet thick, consisting of gravelly, coarse sandstone, often earthy, and containing numerous beds of small pebbles. The diamonds are found in some of the more clayey and pebbly layers, and in the opinion of Dr. W. King, the present Director of the Indian Geological Survey, they are innate in the rock. This view does not, however, appear to commend itself to the authors of the manual. In Northern India, at Panna, in Bundelkhand, the diamond bed is in the upper division of the Vindhyan series; but it is considered not improbable that here also the original *nidus* of the diamonds was, as in Southern India, a bed of the lower subdivision, pebbles of which occur in the diamond bed, and are extracted and broken up in the search for the gem.

As is well known, Tavernier examined, and in his book described and figured, the famous Great Mogul diamond, then in the possession of the Emperor Aurungzebe; and he has been often cited as a principal witness by those who have discussed the question of the history of the Koh-i-noor. To this subject Mr. Ball devotes a long note in the appendix, arriving at conclusions which differ from those of Prof. N. S. Maskelyne, and indeed of most previous writers, with the exception of James Forbes, Major-General Sleeman, and Mr. Tennant. The argument is somewhat complex, and hardly admits of abstraction, and we must therefore refer those who are interested in the subject to the text of Mr. Ball's note. It will suffice here to indicate the main issues. They are concerned with the identification *inter se* of the three diamonds known respectively as the Mogul diamond, Baber's diamond, and the Koh-i-noor. The first of these, described and figured by Tavernier, is the largest diamond on record, and is stated to have weighed originally, before cutting, 900 *ratis* (an Indian weight still in use, but the value of which has varied greatly at different times and under different circumstances). When Tavernier saw it, it had been reduced by unskilful cutting to about two-fifths of its former size, and weighed only 379½ *ratis*, which Mr. Ball computes to be equivalent to 268 English carats. Baber's diamond, of which Tavernier makes no mention, but which is equally historic, Mr. Ball thinks was probably retained by the imprisoned Shah Jehan, and acquired by Aurungzebe only after Shah Jehan's death. The weight of this stone is computed by Mr. Ball, from the statements of Baber and Ferishta, to have been 186 English carats. The weight of the Koh-i-noor when first brought to England was exactly the same as that computed for Baber's diamond, or, accurately, 186·06 carats. Now Prof. Maskelyne, General Cunningham, and several other writers regard these three stones as identical, and the former suggests that Tavernier's estimate of the weight of the Great Mogul diamond in carats (probably Florentine) was erroneous, and due to his having adopted a mistaken value for the *rati*. This view Mr. Ball is unable to accept. Nevertheless he considers it probable that the Koh-i-noor is the remnant of the Mogul diamond, from which portions have been removed while it was in the possession of the unfortunate grandson of Nadir Shah, or some other of those through whose hands it passed before it was acquired by Runjeet Singh; and that Baber's diamond was a distinct stone, now in the possession of the Shah of Persia, and known as the Dariya-i-noor (sea of lustre), the weight of which is also 186 carats.

Mr. Ball's careful criticism of the available evidence, and his clear setting forth of the several steps of his argument, give weight to the conclusion at which he finally arrives, that will doubtless be acknowledged even by those who differ from him. But as regards the identity of the Koh-i-noor and the Mogul diamond, there remains one objection which, as it appears to us, Mr. Ball has hardly adequately disposed of. If Tavernier's figure, as reproduced by Mr. Ball, represents at all faithfully the general form and especially the height of the Mogul diamond, it is difficult to see how a comparatively flat stone like the Koh-i-noor could have been obtained from it without a much greater reduction of its weight than the 82 carats which are all that his data admit of. The lateral dimensions of the two stones accord fairly enough, so that any reduction of Tavernier's figured stone, to bring it down to the required size, could be effected only by diminishing its height; in which case it would hardly correspond to his description of its form as that of an egg cut in two. The question can only be fairly tested by the weighment of a model constructed as nearly as possible in accordance with Tavernier's figure, and of such lateral dimensions as to be capable of including the Koh-i-noor. It may be that such a model, of the specific gravity of the diamond, would be found much to exceed Tavernier's reported weight of the stone, in which case the importance of his figure as an item of evidence, would be greatly invalidated.

Whatever may be the final outcome of this controversy, Mr. Ball has done a good service to literature and science in re-translating Tavernier's work, in its careful editing, and in throwing light on much that has hitherto remained obscure. The result will certainly be that which he has anticipated, the vindication of Tavernier's claim "to be regarded as a veracious and original author."

H. F. B.

OUR BOOK SHELF.

Star Land. By Sir Robert S. Ball, LL.D., F.R.S. (London: Cassell and Co., 1889.)

THE author of this work is now so well known as a popular expounder of astronomical subjects that it is quite sufficient praise of his new book to say that it fully sustains his reputation. The book is described as "talks with young people about the wonders of the heavens," being founded chiefly on notes taken at his courses of juvenile lectures at the Royal Institution. Astronomy gives plenty of scope for the exercise of the imagination, and Dr. Ball takes full advantage of this. The book abounds with anecdotes and homely illustrations, calculated to impress the facts on the memory as well as to excite wonder at them. The startling figures dealt with in astronomy are, as usual, converted into railway train notation, and otherwise illustrated. One new illustration of the distances of the stars is that it would take all the Lancashire cotton factories 400 years to spin a thread long enough to reach the nearest star at the present rate of production of about 155,000,000 miles per day. The irregularities in the motion of Encke's comet are explained in an interesting dialogue between the "offending comet" and the astronomer, in which the comet explains that his delay was due to the fact that Mercury was "meddlesome."

The only disappointing parts of the book are those which deal with astronomical physics. One point not sufficiently insisted upon is the now generally acknowledged meteoritic constitution of comets; a connection is certainly suggested, but that comets are now supposed to be simply dense swarms of meteorites is not stated at all. Nebulæ, again, are described as "masses of glowing gas," notwithstanding the recent researches on the subject. The theory that meteorites are the products of ancient terrestrial volcanoes is also still adopted by Dr. Ball, without any consideration of the objections to such a view.

The book is well illustrated, and will undoubtedly awaken an interest in the subject in all intelligent readers.

The Magic Lantern: its Construction and Use. By a Fellow of the Chemical Society. (London: Perken, Son, and Rayment.)

THE third edition of this little book has been issued, and will be exceedingly useful to those who work with the lantern. Descriptions are given of the various lights used in lanterns, from the oil lamp to the electric arc; the methods of making simple slides are entered into, and a few experiments, illustrative of elementary scientific principles, are well included. The work is thoroughly practical; none of the little details so necessary to beginners have been omitted, whilst many of the hints it contains may be of service to all who use this optical instrument, whether it be for lecture purposes or for recreation only.

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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Acquired Characters and Congenital Variation.

I DO not see that the Duke of Argyll's last letter in any way strengthens his position. The questions at issue with regard to evolution are now, I believe, thoroughly understood by biologists. Nothing, in my opinion, can solve them in the direction the Duke desires but the evidence of fact. And that, I can only repeat, is precisely what is not forthcoming. I am equally of opinion that the discussion has been worn threadbare. I should not myself have interfered in it, had it not seemed desirable to show that the motives attributed by the Duke to those who accept Darwinian principles were destitute of foundation.

This part of his position the Duke does not attempt to defend. As to the rest he merely restates what he has said before. His remarks fall under two heads, and I shall content myself with the briefest possible comment upon these.

(1) *Acquired Characters.*—The Duke gives what I presume he intends as a logical proof of the theorem that acquired characters are inherited. It may, I think, be formally expressed as follows:—

"It is always possible to assert" that acquired characters are developed latent congenital characters.

It is admitted that congenital characters are inherited.

∴ Acquired characters are inherited.

It will be observed in the first place that this is a mere *a priori* argument. And next that, while it is not denied by Darwinians that the organism is a complex of congenital tendencies, limitations, and possibilities, this is entirely beside the question. From Lamarck to Darwin, Weismann, and Lankester, the meaning of "acquired characters" has been clearly defined. They are those changes of hypertrophy, extension, thickening, and the like, which are obviously due to the direct physical action of the environment on the body of the individual organism. It was these changes which Lamarck asserted were transmitted to the offspring; and it is this transmission which it is now maintained needs demonstration as a fact.

Let me give another illustration. I read the other day in the newspapers that the police of Paris have carried out an extremely interesting investigation. They have carefully ascertained the recognizable changes in the normal human organism produced by the prolonged pursuit of any particular occupation. The object was to obtain data for the identification of unknown dead bodies. The changes proved more numerous and characteristic than could have been supposed. They supplied, in fact, diagnostic marks by which the occupation of the individual could be accurately inferred. It seems to me impossible to have a more admirable case of the direct action of external conditions. I ask, Is there any reason to suppose that these acquired characters would be transmitted?

This appears to me an extremely plain issue, as it is certainly an extremely important one. There is not the least reluctance on the part of Darwinians to face it squarely. But the Duke appears to me to deliberately evade it.

(2) *Prophetic Germs.*—It seems to me that we are somewhat at cross-purposes. The Duke admits that I have correctly quoted him as saying: "All organs do actually pass through rudimentary stages in which actual use is impossible." When Prof. Lankester challenged the Duke to produce a single instance, he guarded himself by the remark: "The stages here alluded to are—if I understand correctly—ancestral stages, not stages in the embryological development of the individual." The Duke has never repudiated, as far as I am aware, that limitation of his meaning, if it be a limitation. And as he has

never responded to the challenge, I maintain that he has no right in a scientific discussion to reiterate a statement in support of which he has produced no definite observed evidence. He now returns the challenge to me. But it is no affair of mine. I simply take note of the fact that Prof. Lankester pointed out that the Duke's case collapsed unless the challenge was met, and that the Duke acquiesced by silence.

Just, however, as with the question of acquired characters, the Duke in defect of direct evidence now tries an *a priori* argument. He reminds us of the well known principle of embryology, sometimes called the recapitulation theory. Darwin states it in this form: the embryo is "a picture, more or less obscured, of the progenitor, either in its adult or larval state, of all the members of the same great class."

Now, of course, in the development of the individual organism, we have "a series of incipient structures on the rise for actual use," if by "on the rise" we mean in process of nutritive growth. This is, however, not necessarily true of the recapitulative structures which may or may not be temporarily utilized. When they are not so utilized they are mere survivals, and we know that survivals constantly so completely fall out of use, that by mere inspection it is often difficult to conceive what could have been their original function. I may give a single illustration. In flowering plants the homologue of the spore of the vascular cryptogams is still preserved. *Within* it, previous to fertilization, certain rudimentary structures are developed. It has been shown that these are the last recapitulative remnant of an independent series of structures developed *outside* the spore in the fern. In that type they form the prothallus, which possesses all the attributes of an independent organism, assimilates, respires, often reproduces itself asexually, and finally bears the sexual reproductive organs. All this in the flowering plant is not merely reduced to scarcely intelligible rudiments, but, in accordance with a well-known principle in embryology, it is thrown backwards in the order of development, and never emerges from the spore at all, instead of as in the fern being wholly external to and independent of it.

In this case we know the recapitulation and the thing recapitulated. We infer from their comparison that a fern-like plant was amongst the ancestry of the flowering plant. But I defy anyone, from a mere inspection of what happens in the latter, to form any idea of what happens in the former. From cases such as these it is obvious that the analogy between the development of the individual and the evolution of the race only holds for the broad facts of the sequence of stages, and does not give us any information as to the inutility of the structures of the ancestral organisms, or even, indeed, as to the precise period in their life when such structures made their appearance. The Duke's argument may now, I take it, be stated as follows:—

In the development of the individual organism, incipient organs are useless.

The development of the individual organism is a recapitulation of the evolution of the race.

∴ Incipient organs in the evolution of the race are useless.

I observe that the Duke's estimation of my logical powers is the reverse of flattering. I abstain, therefore, from criticizing this piece of reasoning. For my part I must confess I do not possess an *a priori* mind. No argument, however ingenious, is as convincing to me as accurately observed facts. If the Duke's convictions are laws of Nature, the objective verification ought to be forthcoming. W. T. THISELTON DYER.

Royal Gardens, Kew.

THE Duke of Argyll supports his assertion that "all organs do actually pass through rudimentary stages in which actual use is impossible" by reference to the stages of embryonic growth. Surely the assertion remains merely an empty repetition of the Darwinian position that the development of the embryo summarizes the morphological history of the race.

The modern dress coat has developed from a mere blanket, but even the useless parts of the modern coat can be easily shown to have had their use in some anterior forms of completed coat. The embryo, like the coat, preserves traces of evolutional stages at which what now appear useless characters were in reality actual useful characters.

What the Duke has to show is some instance of a completed organ in a completed organism, useless to that organism, not phases in the growth of an organ affording a blurred copy of some form of the organ existent at an anterior stage of the organism, and then useful to it. So far he has merely confounded ontogenal steps of growth with phylogenal phases of plan. F. V. DICKINS.

Burlington Gardens, February 3.

Eight Rainbows seen at the Same Time.

THE following letter which I have just received from Dr. Percival Frost of Cambridge, may interest your readers.

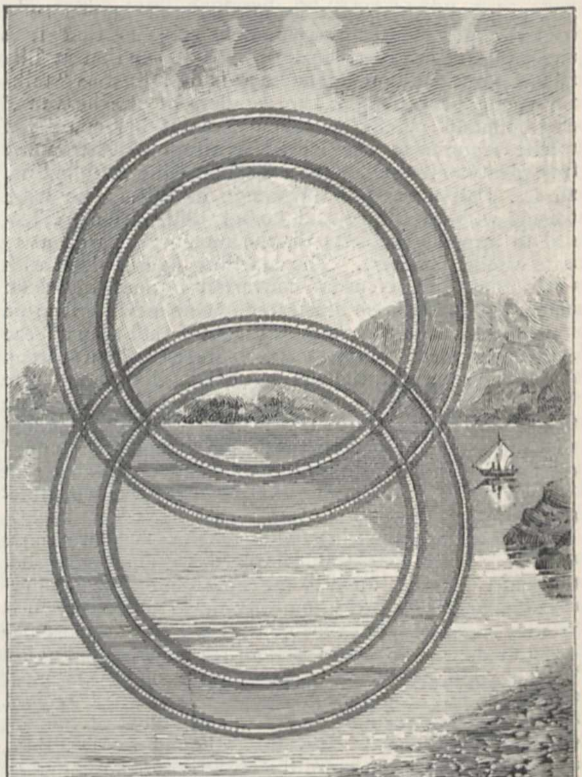
The theory of the rainbows produced by the sun itself directly, and by the image of the sun reflected from still water, is given in Prof. Tait's book on "Light." The phenomenon seems to have been observed by Halley in 1698 (see NATURE, vol. x. pp. 437, 460, and 483 for interesting correspondence on the subject).

The diffuse rainbow produced by the image of the sun reflected from a white cloud after sunset, described by Mr. Scouller, is, I believe, a novelty.

WILLIAM THOMSON.

The University, Glasgow, January 31.

IN NATURE (January 23, p. 271) you give a letter from Mr. Scouller describing an interesting case of a rainbow, due to the image of the sun in water, which, with the ordinary primary and secondary bows, make up (there being no secondary to that formed by the reflected sun) the *three* which he saw. Here is a short account of what I saw long ago, almost in prehistoric times, in Scotland, where such sights ought, according to your correspondent, to be very commonly seen. I may mention that I saw at the same time, lasting some five minutes, *eight* well-defined rainbows of one sort or another.



In 1841, during the time of a long vacation party, spent at Oban, I walked out with my brother to Dunstaffnage, and we were on the top of the Castle, somewhere between 3 and 4 p.m., on a day in the middle of August. Not a breath of wind, bright sun over, I think, Lismore Lighthouse, dusky clouds all over Ben Cruachan and Conoll Ferry; the sea in the bay (bounded by Dunstaffnage in the west) as smooth as a pond. Gradually there appeared before us the astonishing sight of the aforesaid *eight* distinct rainbows, viz. primary and secondary ordinary bows; primary and secondary bows by reflected sun; primary and secondary bows formed by light from the real sun reflected from the water after leaving certain drops; primary and secondary formed by light from the sun reflected at the water, and, after leaving certain other drops, again reflected at the water. I have called the latter four distinct bows, because, although they looked like reflections of a solid set of four arcs, they were really formed by means of drops distinct from those which helped to make the first four bows. I append a sketch of what I saw.

PERCIVAL FROST.

15 Fitzwilliam Street, January 29.

[We have received other letters on the subject of Mr. Scouller's letter.]

Thought and Breathing.

I SEND you some extracts from the Sanskrit Yoga-sûtras which treat very fully of the prâ*n*â*y*âma, or the expulsion and retention of breath, as a means of steadying the mind.

A Yogi has first of all to assume certain postures which help him to fix his mind on certain objects. He cannot concentrate his mind while walking or running. He ought to assume a firm and pleasant position, one requiring little effort. To judge, however, from the description given of some of these postures, they would seem to us anything but pleasant.

When a Yogi has accustomed himself to his posture, he begins to regulate his breath—that is, he draws in the breath through one nostril, retains it for some time in the chest, and then emits it through the other nostril. The details of this process are given in the first chapter of the Yoga-sûtras, sûtra 37. Here the commentator states that the expulsion means the throwing out of the air from the lungs in a fixed quantity through a special effort. Retention is the restraint or stoppage of the motion of breath for a certain limited time. That stoppage is effected by two acts—by filling the lungs with external air, and by retaining therein the inhaled air. Thus the threefold prâ*n*âyâma, including the three acts of expiration, inspiration, and retention of breath, fixes the thinking principle to one point of concentration. All the functions of the organs being preceded by that of the breath—there being always a correlation between breath and mind in their respective functions—the breath, when overcome by stopping all the functions of the organs, effects the concentration of the thinking principle to one object.

Râjendralal Mitra, to whom we owe a very valuable edition of the text and translation of the Yoga-sûtras, adds the following remarks:—"All other Yogic and Tantric works regard the three acts of expiration, inspiration, and retention performed in specific order to constitute prâ*n*âyâma. The order, however, is not always the same. . . . The mode of reckoning the time to be devoted to each act is regulated in one of two ways: (1) by so many repetitions of the syllable om, or the mystic mantra (formula) of the performer, or the specific mystic syllables (vîja) of that mantra; (2) by turning the thumb and the index-finger of the left hand round the left knee a given number of times. The time devoted to inspiration is the shortest, and to retention the longest. A Vaish*n*ava in his ordinary daily prayer repeats the Vîja-mantra once while expiring, 7 times while inspiring, and 20 times while retaining. A Sâkta repeats the mantra 16 times while inspiring, 64 times while retaining, and 32 times while expiring. These periods are frequently modified."

The usual mode of performing the prâ*n*âyâma is, after assuming the posture prescribed, to place the ring-finger of the right hand on the left nostril, pressing it so as to close it, and to expire with the right, then to press the right nostril with the thumb, and to inspire through the left nostril, and then to close the two nostrils with the ring finger and the thumb, and to stop all breathing. The order is reversed in the next operation, and in the third act the first form is required. The Ha*th*adîpikâ says:—"By the motion of the breath, the thinking principle moves; when that motion is stopped, it becomes motionless, and the Yogi becomes firm as the trunk of a tree; therefore the wind should be stopped. As long as the breath remains in the body, so long it is called living. Death is the exit of that breath, therefore it should be stopped."

Some of the minor works on Yoga expatiate on the sanitary and therapeutic advantages of practising prâ*n*âyâma regularly at stated times. In America some spiritualistic doctors prescribe the same practice for curing diseases.

In India prâ*n*âyâma is only a means towards a higher object—namely, the abstraction of the organs from their natural functions. It is a preliminary to Yoga, which consists in *dhâranâ*, steadfastness, *dhyâna*, contemplation, and *samâdhi*, meditation, or almost a cataleptic trance. These three are supposed to impart powers or *siddhis* which seem to us incredible, but which nevertheless are attested by the ancient Yogis in a very *bonâ-fide* spirit, and deserve examination, if only as instances of human credulity. I say nothing of modern impostures.

Oxford, January 22. F. MAX MÜLLER.

IN connection with Prof. Leumann's recent researches into the relation between changes in respiration and changes in certain cerebral functions, it seems curious that the employment of deep and rapid respiration as an anæsthetic has received so little attention. Some dentists order their patients to respire as quickly and fully as they can for a period which varies, I believe, from four to six minutes, although as to the exact duration I am insufficiently informed. At the termination of this period the patient becomes giddy, and to a great extent loses consciousness, when a short operation can be painlessly performed. The patient, while unable to move his arms, opens his mouth at the order of the operator. I have heard of no casualties or evil effects from this mode of treatment. W. CLEMENT LEY.

Chiff-Chaff singing in September.

DURING more than forty years' observation of the singing of birds, I have invariably heard the chiff-chaff singing in September, although the song is much less frequently repeated than in the spring. In connection with this observation I may mention that both the male and female birds appear to be invariably mute for two or three days after their spring arrival in Northern Europe. W. CLEMENT LEY.

Lutterworth, January 31.

Foreign Substances attached to Crabs.

I HAVE read in recent numbers of NATURE some letters on sponges attached to crabs.

There are two crabs on the east coast of Australia—one of them allied to *Dromia vulgaris*—which cover themselves with sponges or with a composite Ascidian. I have in one case counted no less than seven species of sponges on one individual crab.

The Ascidian referred to is usually from ten to thirty times as large as the crab to the back of which it is attached.

Among the specimens brought by me from Australia, and now deposited in the National Collection of the British Museum, there are some of these crabs with sponges and Ascidians attached.

These might, perhaps, be interesting to your correspondents on the subject. R. v. LENDENFELD.

University, Innsbruck, January 25.

Foot-Pounds.

"A. S. E." will find moments, of resistance, of bending, or of turning, expressed in foot-pounds (often inch-pounds or foot tons) in any treatise on civil, mechanical, or marine engineering, on architecture, land or naval, and, in fact, in every treatise on *real* mechanics he may consult. Why, then, should a different terminology be adopted in a Civil Service examination paper? In metric units, moments are given in kilogramme-metres or -centimetres; but in the C.G.S. system I do not suppose it is suggested to measure moments of dyne-centimetres in ergs.

February 3. A. G. GREENHILL.

IF "A. S. E." will push his researches further, he will find that in Government dockyards the stability moment on ships is calculated in foot-tons. V.

February 3.

PROF. WEISMANN'S THEORY OF HEREDITY.

IN NATURE of October 24, 1889 (p. 621), appeared a criticism by Prof. Vines of my essays on heredity and allied subjects. I should be glad to reply briefly to his objections, and the more so as I hope thus to be able to place the scientific problems at issue in a somewhat clearer light. With regard to the immortality which I attribute both to the unicellular organisms and to the germinal cells of the multicellular, if I understand Prof. Vines aright, he does not attack the proposition itself, but has simply overlooked the explanation in my book of the way in which mortal organisms arose out of immortal in process of phyletic development, a process which must have taken place if the Protozoa have developed in the course of the world's history into the higher Metazoa,—"the first difficulty is to understand how the mortal heteroplastides can have been evolved from the immortal monoplastides." My explanation was simply that which appears to be the true one for the origin of every higher differentiation—namely, the division of the cell-mass of the Protozoan, on the principle of the division of labour, into two dissimilar halves, differing in substance, and consequently also in function; from the one cell which performed all functions comes a group of several cells which distribute themselves over the work. In my opinion, the first such differentiation produced two sets of cells, the one the mortal cells of the body proper, the other the immortal germ-cells. Prof. Vines certainly believes in the principle of the division of labour, and in the part that it has played in the development of the organic world, as well as I; but it seems to him that this division of a unicellular being into somatic and germinal cells is impossible, and that my explanation of the process by dissimilar division is inadequate, because it strikes him as "absurd to say that an immortal substance can be converted into a mortal substance."

There certainly does seem to be a great difficulty in this idea, but in reality it arises simply from a confusion of two conceptions—immortality and eternity. That the Protozoa and the germ-cells of Metazoa are in a certain sense immortal seems to me an incontrovertible proposition. As soon as one has clearly realized that the division of a monoplastid is in no way connected with the death of one part, there can be no further question that we have to do with individuals of indefinite duration; but this in no way implies that they possess an eternal duration; on the contrary, we imagine that they have all had a beginning. The conception of eternity, however, extends into the past as well as the future; it is without beginning or end, and does not affect the present question; it is an entirely artificial conception, and has no real and comprehensible existence; to express it more accurately, eternity is merely the negation of the conception of transitoriness. Of the objects with which natural science deals, none are eternal except the smallest particles of matter and their forces, certainly not the thousandfold semblances and combinations under which matter and force meet us. As I have said years ago, the immortality of unicellular organisms, and of the germ-cells of the multicellular, is not absolute but potential; it is not that they *must* live for ever as did the gods of the ancient Greeks—Ares received a "mortal" wound, and roared for pain like to ten thousand bulls, but could not die; they can die—the greater number do in fact die—but a proportion lives on which is of one and the same substance with the others. Does not life, here as elsewhere, depend on metabolism—that is to say, a constant change of material? And what is it, then, which is immortal? Clearly not the substance, but only a definite form of activity. The protoplasm of the unicellular animals is of such chemical and molecular structure that the cycle of material which constitutes life returns even to the same point and can always begin anew, so long as the necessary external conditions are forthcoming. It is like the circulation of water, which evaporates, gathers into clouds, and falls as rain upon the earth, always to evaporate afresh. And as in the physical and chemical properties of water there is no inherent cause for the cessation of this cycle, so there is no clear reason in the physical condition of unicellular organisms why the cycle of life, *i.e.* of division, growth by assimilation, and repeated division, should ever end; and this characteristic it is which I have termed immortality. It is the only true immortality to be found in Nature—a pure biological conception, and one to be carefully distinguished from the eternity of dead, that is to say unorganized, matter.

If then this true immortality is but cyclical, and is conditioned by the physical constitution of the protoplasm, why is it inconceivable that this constitution should be, under certain circumstances and to a certain extent, so modified that the metabolic activity no longer exactly follows its own orbit, but after more or fewer revolutions comes to a standstill and results in death? All living matter is variable; why should not variations in the protoplasm have also occurred which, while they fulfilled certain functions of the individual economy better, caused a metabolism which did not exactly repeat itself, *i.e.* sooner or later came to a condition of rest? I admit that I feel such a descent from immortality into mortality far less remarkable than the permanent retention of immortality by the monoplastids and germ-cells. Small, indeed, must be the variations in the complicated qualities of living matter to bring in their train such a fall; and very sharply must the essentials of its constitution be retained, for metabolism to take place so smoothly without creating in itself an obstacle to its own continuance! Even if we cannot penetrate into the mysteries of this constitution, still we may say that a rigorous and unceasing natural selection is unremittingly active in maintaining it at such an exact standard as to preserve its immortality; and every lapse from this standard is punished by death.

I believe that I have proved that organs no longer in use become rudimentary, and must finally disappear solely by "panmixie"; not through the direct action of disuse, but because natural selection no longer maintains their standard structure. What is true for an organ is true also for its function, since the latter is but the expression of the qualities of material parts, whether we can directly perceive their relations or not. If, then, as we saw, the immortality of monoplastids depends on the fact that the incessant metabolism of their bodies is ever returning exactly to its starting-point, and produces no such modifications as would gradually obstruct the repetition of the cycle, why should that quality of the living matter which causes immortality—nay, how *could* it be retained—when no longer necessary? It is obvious that it was no longer necessary in the somatic cells of the heteroplastids. From the instant that natural selection relaxed its watch on this quality of immortality began the process of panmixia which led to its abolition. Prof. Vines will ask, How can one conceive of this process? I answer, Quite easily. When once individuals arose among monoplastids, in the protoplasm of which occurred such variation in chemical and molecular constitution as to result in a gradual check on the metabolic cycle, it would happen that these individuals died; a permanent variety could not grow out of such variations. But if there arose among heteroplastids individuals with a similar differentiation of the somatic cells, the death of these cells would not be detrimental to the species, since its continuance is ensured by the immortal germ-cells. Upon the differentiation into germinal and somatic cells, natural selection was, speaking metaphorically, trained to bear on immortality of the germ-cells, but on quite other qualities in the somatic cells—on motility, irritability, capacity for assimilation, &c. We do not know whether the attainment of these qualities was accompanied by a constitutional alteration which caused the loss of immortality, but it is at least possible; and, if true, the somatic cells will have lost their immortality even more rapidly than through the unaided action of panmixia.

In the fourth essay of my book, I have cited the two Volvocinean genera Pandorina and Volvox as examples of the differentiation of homoplastids into the lowest heteroplastids; in Pandorina the cells are still all alike and all perform the same functions, in Volvox occur somatic and germinal cells, and in the latter case we should expect to find the commencement of natural death. Recent researches of Dr. Klein ("Morphologische und biologische Studien über die Gattung Volvox," *Jahrb. wiss. Botan.*, xx., 1889) show that this is actually the case; as soon as the germ-cells are ripe and emerge from the sphere, the ciliated somatic cells begin to shrivel up, and die in one or two days. This is the more interesting, as the somatic are also the nutritive cells; for, though the germ-cells also possess chlorophyll, the rapid growth of the latter (which attain an enormous size in Volvox) is only possible by the supply of nourishment from the somatic cells. The latter are so constituted that they assimilate, but cannot grow larger when once the sphere has reached its definite size; they transfer the nourishment which they derive from the decomposition of carbon dioxide, &c., to the germinal cells by means of fine pseudopodia; and themselves wither when once the germs are ripe. In this case adaptation to the nutrition of the germinal cells might well have accelerated the introduction of a natural death of the somatic cells, the capacity for considerable assimilation combined with a drain on their nutrition may have led after a certain time to stoppage of the process of assimilation and to death. To me, the idea that modification of the living matter may have been connected with loss of immortality does not appear more unlikely or more difficult than the generally received view of the gradual differentiation of the somatic cells in the course of phylogeny into their various species of digestive, secretive, motile, and nervous cells. An immortal unalterable living substance does not exist, but only immortal *forms of activity* of organized matter.

I maintain, therefore, in its entirety, my original statement, that monoplastids and the germ-cells of higher forms have no natural death. I do not know how this can to-day be better expressed than by saying that these living units possess a real and actual immortality as against the imaginary ideal immortality of the Greek gods. If death from internal causes does not exist for them, one may yet say with certainty that the fatal hour will one day strike for them all, not from internal causes, but because the external conditions for the constant renewal of vital activity will some day cease. The physicists prophesy that the circulation of water on the globe will end, not from any alteration in the qualities of water, but because external conditions will render this form of motion of aqueous particles impossible.

Prof. Vines then attacks my view of embryogeny. He finds it "not a little remarkable that Prof. Weismann should not have offered any suggestion as to the conception which he has formed of the mode in which the conversion of germ-plasm into somatoplasm can take place, considering that this assumption is the key to his whole position." He sees here the same difficulty as in the phyletic development, and says: "There is really no other criticism to be made on an unsupported assumption such as this, than to say that it involves a contradiction in terms." He means by this that the eternal cannot pass into the finite, as must be the case if the immortal germ-cell grow into the mortal soma. At the bottom of this objection lies the same confusion between immortality and eternity which has already been made clear. I do not wish to reproach Prof. Vines with this obscurity, as I felt the same objection myself for many years, and could not at once discover the reply to it; on the contrary, I am indebted to him for the opportunity to express myself on the point. Up to this time we have had no scientific conception of immortality; if this be accepted, the significance of immortality is not life without beginning or end, but life which, after its first commencement, can continue indefinitely with or without modification (specific changes in the germ-plasm or the monoplastids); it is a cyclical activity of organic material devoid of any intrinsic momentum which would lead to its cessation, just as the motion of the planets contains no intrinsic momentum which would lead to its cessation, although it has had a commencement and will some day, through the operation of extrinsic forces, have an end.

Prof. Vines says later: "I understand Prof. Weismann to imply that his theory of heredity is not—like, for instance, Darwin's theory of pangenesis—a provisional or purely formal solution of the question, but one which is applicable to every detail of embryogeny, as well as to the more general phenomena of heredity and variation." I have, as a matter of fact, designated Darwin's pangenesis as a "purely formal" solution of the question, but should like here to give a slight explanation of the expression, as I fear that not only Prof. Vines, but also many other readers of my essays, have misunderstood me. On the one hand, I am afraid that they see in my words a definite reproach against Darwin for his theory of pangenesis, of which I had not the remotest intention; and on the other, that they incline to charge me with too great an affection for my own theory.

I believe there are two kinds of theory; one may term them the "real" and the "ideal"; practically they are rarely sharply to be discriminated; both often occur in one and the same theory, but should be conceived of separately. The "ideal" theories attempt to render conceivable the phenomena to be explained by an arbitrarily accepted principle, apart from the question whether the principle itself possesses any grain of truth or not; they seek only to show that there are hypotheses on which the phenomena in question become comprehensible. "Real" theories do not make hypotheses at pleasure, but strive to construct such as have some degree of probability; they desire to give not a formal, but, if possible, the right explanation. Sir William Thomson in endeavouring to make clear the dispersion of rays of light, never believed in the remotest degree that such molecules as he pictured really existed, but desired merely to show that there were hypotheses on which the phenomena of dispersion were comprehensible. Darwin's pangenesis was originally intended in this sense, and was by him termed a "provisional" hypothesis, although in later years he may have attributed to it the weight of a real theory. To me his "gemmules" are a pure invention, an invention in no way corresponding to the actual facts, but showing what hypotheses must be made in order to explain the phenomena of heredity. Are, however, such ideal theories worthless? Certainly not. They are often the first and essential step towards the understanding of complicated phenomena, and lay the foundation for the gradual erection of a real theory. It would perhaps never have occurred to me to deny the inheritance of acquired characters, had not Darwin's pangenesis shown me that the matter was only explicable on an hypothesis so difficult to conceive, as that of the giving off, circulation, and reassemblage of gemmules. I do not even now maintain that Darwin's pangenesis cannot possibly contain a kernel of truth; De Vries ("Intracellulare Pangenesis," Jena, 1889) has shown in a recent and most interesting memoir that the ideal impossible pangenesis may be transformed into a real and possible one by means of certain profound modifications; he accepts my view that acquired (somatogenic) modifications cannot be transmitted, and thereby puts on one side just that part of Darwin's theory which has always appeared to me to lie beyond the pale of reality—namely, the circulation, &c., of the gemmules. The future will show whether his view of modified gemmules or my hypothesis is the best explanation of the facts of heredity.

In any case, I am far from assuming that I have settled the whole question of heredity; I have undertaken researches on some of the more important parts of the problem, and have thus been compelled to formulate some fundamental principles for the explanation of the phenomena; but no one can be more convinced than I how far we are from a definite and complete explanation, not only of "every detail," but also of "the more general phenomena." My endeavour was to put forth a real, in place of the previous ideal, theory; and on this ground I took pains to make only such suppositions as might possibly correspond to actual facts. There certainly is a material carrier of heredity in the ovum; it certainly can be transported from nucleus to nucleus; it certainly can be modified in the process, or can remain the same; and even the supposition that it is able to stamp its own character on the cell contains nothing which seems to us impossible and non-existent; on the contrary, we are able now to state that it is so, even if we do not understand in what wise it happens. My hypothesis relative to the quiescent state of germ-plasma also rests on a basis of fact; we know that ancestral characteristics may be transmitted in a latent condition, and that the process of transmission is bound up with a substance, the idioplasma; there must therefore actually be an inactive stage of idioplasma.

If it could be shown that upon such principles an explanation of heredity is attainable, we should have made a distinct advance upon the ideal theory of pangenesis which is founded on unreal hypotheses. Possibly it is upon the path which I have opened up that we shall gradually attain a satisfactory solution of the numerous questions at issue; possibly further research will show that it is not the right path, and must be abandoned; no one, it appears to me, can foretell this. My reflections on heredity are not a conclusion, but a commencement—no complete theory of heredity which claims to provide a complete solution of all the problems at issue, but *researches* which, if fortunate, may sooner or later, by direct or circuitous paths, lead to a true appreciation of the question, to a "real" theory. In the preface to the English edition of my "Essays" I have stated this expressly.

I have also in that place distinctly insisted that the book was not written as a whole; that it consists rather of a series of researches, the one growing out of the other, and showing the development of my views as they shaped themselves during the course of nearly a decade's work. It is therefore unreasonable to extract ideas from an earlier essay and apply them against a later one. I have left them unaltered, and even "left certain errors of interpretation uncorrected," because, if altered, their internal connection could not have been understood.

I believe that the objections which Prof. Vines makes to my theory of the continuity of germ-plasma rest solely on an unintentional confusion of my ideas, as he compares the opinions expressed in the second essay with those of the later ones, with which they do not tally. I will endeavour to make this clear. In this second essay (1883) I contrasted the body (soma) with the germ-cells, and explained heredity by the hypothesis of a "Vererbungs-substanz" in the germ cells (in fact the germ-plasma), which is transmitted without breach of continuity from one generation to the next. I was not then aware that this lay only in the nucleus of the ovum, and could therefore contrast the entire substance of the ovum with the substance of the body-cells, and term the latter "somatoplasm." In Essay IV. (1885) I had arrived, like Strasburger and O. Hertwig, at the conviction that the nuclear substance, the chromatin of the nuclear loops, was the carrier of heredity, and that the body of the cell was nutritive but not formative. Like the investigators just named, I transferred the conception of idioplasma, which Nägeli had enunciated in essentially different terms, to the "Vererbungs-substanz" of the ovum-nucleus, and laid down that the nuclear chromatin was the idioplasma not only of the ovum but of every cell, that it was the dominant cell-element which impressed its specific character upon the originally indifferent cell-mass. From then onwards, I no longer designated the cells of the body simply as "somatoplasm," but distinguished, on the one hand, the idioplasm or "Anlagen-plasma" of the nucleus from the cell-body or "Cytoplasma," and, on the other, the idioplasm of the ovum-nucleus from that of the somatic cell-nucleus; I also for the future applied "germ-plasm" to the nuclear idioplasm of ovum and spermatozoon, and "somatic idioplasm" to that of the body-cells (*e.g.* p. 184). The embryogenesis rests, according to my idea, on alterations in the nuclear idioplasma of the ovum, or "germ-plasm"; on p. 186, *et seqq.*, is pictured the way in which the nuclear idioplasm is halved in the first cell-division, undergoing regular alterations of its substance in such a way that neither half contains all the hereditary tendencies, but the one daughter-nucleus has those of the ectoblast, the other those of the entoblast; the whole remaining embryogenesis rests on a continuation of this process of regular alterations of the idioplasma. Each fresh cell-division sorts out tendencies which were mixed in the nucleus of the mother-cell, until the complete mass of embryonic cells is formed, each with a nuclear idioplasm which stamps its specific histological character on the cell.

I really do not understand how Prof. Vines can find such remarkable difficulties in this idea. The appearance of the sexual cells generally occurs late in the embryogeny; in order, then, to preserve the continuity of germ-plasm from one generation to the next, I propound the hypothesis that in segmentation it is not *all* the germ-plasm (*i.e.* idioplasm of the first ontogenetic grade) which is transformed into the second grade, but that a minute portion remains unaltered in one of the daughter-cells, mingled with its nuclear idioplasm, but in an inactive state; and that it traverses in this manner a longer or shorter series of cells, till, reaching those cells on which it stamps the character of germinal cells, it at last assumes the active state. This hypothesis is not purely gratuitous, but is supported by observations, notably by the remarkable wanderings of the germinal cells of Hydroids from their original positions.

But let us neglect the probability of my hypothesis, and consider merely its logical accuracy. Prof. Vines says:—"The fate of the germ-plasm of the fertilized ovum is, according to Prof. Weismann, to be converted in part into the somatoplasm [!] of the embryo, and in part to be stored up in the germ-cells of the embryo. This being so, how are we to conceive that the germ-plasm of the ovum can impress upon the somatoplasm [!] of the developing embryo the hereditary character of which it (the germ-plasm) is the bearer? This function cannot be discharged by that portion of the germ-plasm of the ovum which has become converted into the somatoplasm [!] of the embryo *for the simple reason that it has ceased to be germ-plasm*, and must therefore have lost the properties characteristic of that substance. Neither can it be discharged by that portion of the germ-plasm of the ovum which is aggregated in the germ-cells of the embryo, for under these circumstances, it is withdrawn from all direct relation with the developing somatic cells. The question remains without an answer." I believe myself to have answered this above. I do not recognize the somatoplasm of Prof. Vines; my germ-plasm or idioplasm of the first ontogenetic grade is not modified into the somatoplasm of Prof. Vines, but into idioplasm of the second, third, fourth, hundredth, &c., grade, and every one impresses its character on the cell containing it.

Prof. Vines also attacks my view of the idioplasmatic nature of the *nuclear* substance (the chromatic grains); and maintains that it is as easy to speak of the continuity of the cell-body as of that of the nuclear substance, and that the one may transmit heritable qualities to progeny as well as the other. I quite understand that a botanist may easily be led to this view; and Prof. Vines is not the only one to hold it. Waldeyer ("Ueber Karyokinese und ihre Beziehung zu den Befruchtungs-vorgänge," *Arch. mikr. Anat.*, xxxii., 1888) has considered the observed facts insufficient to justify the regarding of the nuclear loops as idioplasm; Whitman ("The Seat of Formative and Regenerative Energy," Boston, 1888) among zoologists has expressed himself against this view, and the same occurs in the recent book of Geddes and Thomson ("The Evolution of Sex," London, 1889). The facts which led me to the idea that the nuclear threads were the real carriers of heredity—were, in fact, the idioplasma—are enumerated in Essay IV.; they were primarily the observations of E. van Beneden on the phenomena of fertilization in the ovum of *Ascaria megalocephala*, those of Strasburger on fertilization in the Phanerogams by a mere nucleus, and the researches of Nussbaum and Gruber on division in the Infusoria. One may further cite as of essential importance the facts of karyokinesis *per se*, and the circumstance that, only on the supposition that the nucleus contains the idioplasma can the extrusion of polar bodies from the animal ovum be rendered comprehensible. The latter process divides the nuclear substance of the ovum into two quantitatively equal halves, but the body of the ovum into two unequal halves, the size of which is different in every species. The essential part of the process must therefore be the division of the nuclear substance, not that of the cell-mass. These facts on reflection so completely convinced me that the nucleus alone acts as carrier of hereditary tendencies, that the theory of the physiological equality of the nuclei of the sexual elements which I had propounded ten years before (1873) struck me as a certainty; and I then advanced the theory of fertilization which is contained on p. 246 of Essay IV. I believe that till recently Strasburger and I alone had expressed similar views of the essence of fertilization, at least so far as relates to the homodynamy of the sexual nuclei. That most distinguished observer, E. van Beneden, who has won such renown in the investigation of the process of fertilization, took his stand with regard to its theoretical significance on the platform of the older view, which regarded it as the union of two elements intrinsically and essentially the opposite of each other. He could not free himself from that dominant and deeply rooted idea, that the difference between the sexes is something fundamental, an essential principle of existence. The fertilized oosperm is in his eyes a hermaphrodite object, uniting in itself both male and female essences, an idea in which many other observers (cf. Kölliker, "Die Bedeutung der Zellenkerne für die Vorgänge der Vererbung," *Zeit. wiss. Zool.*, xlii., 1885) have followed him, and of which the logical sequence is that all the cells of the body are to be regarded as hermaphrodite!

Van Beneden was also influenced by the idea which sways the naturalists of so many countries, that fertilization is a process of rejuvenescence, in the sense that without it life cannot be prolonged to the end. Many still hold to this idea; Maupas ("Recherches expér. sur la multiplication des infusoires ciliés," *Arch. zool. exp. gén.*, (2) vi. p. 165) very recently believed that he had found a proof of its correctness, and attempted to show that Infusoria, for a continuance of existence, must from time to time enter into conjugation, or die from internal causes if this conjugation be prevented. Even were his observations correct, they would still fall short of proving his conclusions; they would prove nothing against the immortality of the Protozoa, or for a rejuvenescence in the sense here intended; they would rather state the platitude that ovum and spermatozoon must die, if the condition of their continued existence, namely fusion, inevitable in most species of plants and animals, be prohibited; but this is an accidental, not a natural, death. Richard Hertwig ("Ueber die Conjugation der Infusorien," München, 1889) has also briefly shown that the facts, on which Maupas bases his inference, are not universally true; that Infusoria hindered from conjugation do not die, but increase by division, and may produce whole colonies of animals—nay, that they are generally thus rendered abnormally prolific.

I am distinctly opposed to the rejuvenescence theory, whether applied to unicellular or multicellular organisms; my view is expressed in Essay IV., and may be summarized in this position—we should no longer speak of the conjugating nuclei of the sexual elements as male and female, but as *paternal* and *maternal*, there is no opposition of the one to the other, they are essentially alike, and differ only so far as one individual differs from another of the same species. Fertilization is no process of rejuvenescence, but merely a union of the hereditary tendencies of two individuals; tendencies which are bound up with the matter of the nuclear loops; the cell-body of the ovum and spermatozoon is indifferent in this connection, and plays merely the part of a nutritive matter which is modified and shaped by the dominant idioplasm of the nucleus in a definite way, as clay in the sculptor's hand. The different appearance and function of ovum and spermatozoon, and their mutual attraction, rest on secondary adaptations, qualified to ensure that they shall meet and that their idioplasmata shall come into contact, &c.; and as with the cells, so the differentiation of *persons* into male and female is also secondary; all the numerous differences of form and function which characterize sex in the higher animals, the so-called "secondary sexual characters," which reach even into the highest spiritual regions of mankind, are nothing but adaptations to ensure the union of the hereditary tendencies of two individuals.

These are briefly the views of fertilization which I have indicated since 1873, but have only published in a finished and definite shape since the discovery by van Beneden of the morphological processes in the fertilization of the ovum of Ascaris (Essay IV., 1885). I concluded then with these words:—"If it were possible to introduce the female pro-nucleus of an egg into another egg of the same species, immediately after the transformation of the latter into the female pro-nucleus, it is very probable that the two nuclei would conjugate just as if a fertilizing sperm-nucleus had penetrated [the ovum]. If this were so, the direct proof that egg-nucleus and sperm-nucleus are identical would be furnished. Unfortunately the practical difficulties are so great that it is hardly possible that the experiment can ever be made; but such want of experimental proof is partially compensated by the fact, ascertained by Berthold, that in certain Algæ (Ectocarpus and Scytosiphon) there is not only a female, but also a male parthenogenesis; for he shows that in these species the male germ-cells may sometimes develop into plants, which however are very weakly."

I have since attempted to fertilize one frog's egg with the nucleus of another; the experiment was, as one would expect, not successful, owing to the enormous havoc caused by introducing a cannula into the egg; but Boveri ("Ein geschlechtlich erzeugter Organismus ohne mütterliche Eigenschaften," *Ges. Morph. Physiol. München*, 16 Juli, 1889) was more fortunate, in finding an object which allowed of the converse experiment to mine; following Hertwig's example, he removed the nucleus from an Echinoid ovum by agitation, and brought such denucleated ova to develop by introducing spermatozoa. From the spermatozoan nucleus was formed a regular segmentation-nucleus, the embryogenesis pursued its regular course, and there was formed a complete though small free-swimming larva, which lived for a week. From this experiment alone it follows that the views of Strasburger and myself on fertilization are correct, *viz.* that the sperm-nucleus can play the part of ovum-nucleus and *vice versâ*, and the older view, to which Prof. Vines ("Lectures on the Physiology of Plants," Cambridge, 1886, pp. 638-681) has also sworn allegiance, must be given up.

An interesting and important modification of Boveri's experiment confirmed both this experiment, and also, if it were necessary, the recognition of the nuclear substance as idioplasm, as maintained by O. Hertwig, Strasburger, and myself. If eggs of *Echinus microtuberculatus*, when artificially deprived of their nuclei, be fertilized with the spermatozoa of *Sphærechinus granulatus*, *larvæ are developed with the true characters of the second species*—that is to say, they have derived everything from the father, nothing from the mother; the nuclear substance alone it is which transmits heredity, and by it the cell-mass is dominated.

I have interpreted the first polar body of the Metazoan ovum as a carrier of ovogenous plasm, which has to be removed from the ovum in order that the germ-plasm may attain the predominance. It is possible that this explanation is not correct; the most recent researches on the conjugation of Infusoria, as expressed in the splendid memoirs of Maupas and R. Hertwig, argue against my interpretation; but the idea which lay at the bottom of this explanation is justified. As it is the nuclear matter which gives to the cell-body its specific character, the ovum must, previous to fertilization, be dominated by a different idioplasm to the sperm-cell, since they are, up to this point, different in appearance and function. On the other hand, when they have united, they contain the same idioplasm—namely, germ-plasm; the consequence is that the first dominant idioplasm is different to that of a later period. This was the idea at the bottom of my explanation of the first polar body, and it is correct. One might perhaps imagine that the idioplasmata of ovum and spermatozoon were originally different, but that both possessed the power of alteration into germ-plasm; but it would be then incomprehensible why parthenogenetic ova should expel one polar body. Both facts, however, are explicable, if ovum and spermatozoon are dominated up to the period of maturation by different histogenetic idioplasmata with which a small quantity of germ-plasm is mingled, and if at a later period the former be removed and the germ-plasm come to rule in both cells. This process would be by no means abnormal and unparalleled, since entirely analogous divisions of the idioplasm into qualitatively dissimilar portions must occur hundreds of times in every embryogenesis. However, I am most willing to allow that the last word has not yet been said on this question, and would only maintain that my theory of heredity is not concerned thereby. It is not the interpretation of the first polar body, but that of the second, which is decisive; and one can none the less easily think of the latter as a halving of the number of ancestral germ-plasmata, even if it be proved that my explanation of the first polar body was erroneous. I would then express the first division merely as introductory to the second, as the necessary first step in the reduction of ancestral plasmata, the necessity for which we should thus perhaps learn to understand.

The regular modification of idioplasma during the ontogeny, which I have maintained and which so many have attacked (Kölliker[1] with special vehemence) will now stand out as justified. If the nucleus of a sperm-cell is capable of impressing on the denucleated mass of an ovum its own inherited tendencies, and of calling into being an organism with specific characteristics purely paternal, it will be found difficult to explain the ontogeny otherwise than as a regular modification of the idioplasm, continuous from one cell-division to another, which stamps on the body of each separate cell at each stage its peculiar character, not only with regard to shape but also to function, and especially with regard to the "rhythm" of cell-division.

[1] "Das Karyoplasma und die Vererbung: eine Kritik der Weismann'sche Theorie von der Continuität des Keimplasma's," *Zeit. wiss. Zool.*, xliv. p. 228, 1886.

A further objection is directed by Prof. Vines against my views on the origin of variation. In the fifth essay I have sought the significance of sexual reproduction in the fact that it alone could have called into existence that multiplicity of form of the higher animals and plants, and that constantly fluctuating union of individual variations, of which natural selection stood in need for the creation of new species. I am still of the opinion that the origin of sexual reproduction depends on the advantage which it affords to the operation of natural selection; nay, I am completely convinced that only through its introduction was the higher development of the organic world possible. Still, I am at present inclined to believe that Prof. Vines is correct in questioning whether sexual reproduction is the *only* factor which maintains Metazoa and Metaphyta in a state of variability. I could have pointed out in the English edition of my "Essays" that my views on this point had altered since their publication; my friend Prof. de Bary, too early lost to science, had already called my attention to those parthenogenetic Fungi which Prof. Vines justly cites against my views; but I desired, on grounds already mentioned, to undertake no alteration in the essays. Bessides, I was well aware when the essay was first committed to paper (1886) that my current view on the radical cause of variation was possibly incomplete; and so, in order to expose the truth of the view as far as possible to a general test, I drove its logical consequences home, and enunciated the statement that species reproducing parthenogenetically could not be modified into new species. I also began myself at that time experiments on the variation of parthenogenetic species which are still being continued, and on which on some future occasion I hope to be able to report.

Even if, however, from our present knowledge it is probable that sexual reproduction is not the sole radical cause of variability of the Metazoa, still no one will dispute that it is a most active means of heightening variations and of mingling them in favourable proportions. I believe that the important part which this method of reproduction has played in calling out the existing processes of selection, is hardly diminished, even if one grants that direct influences upon the idioplasm call forth a portion of individual variability. Prof. Vines even holds it probable "that the absence of sexuality in these plants [Fungi] may be just the reason why no higher forms have been evolved from them, for in this respect they present a striking contrast to the higher Algæ in which sexuality is well marked." But when Prof. Vines says, "there can be no doubt that sexual reproduction does very materially promote variation," he does not mean to say that this is a self-evident proposition; he is well aware that prominent investigators like Strasburger see in sexual reproduction the reverse action, that of maintaining the constancy of the specific character. But I gladly accept his agreement with my view, which confirms the main position of the fifth essay, which runs: Sexual reproduction has arisen by and for natural selection as the sole means by which individual variations can be united and combined in every possible proportion.

With reference also to the problem of the inheritance of acquired (somatogenic) characters, Prof. Vines is again my opponent; he holds that such inheritance is possible. I have denied it, because it did not appear to me self-evident—as was formerly universally assumed—but rather utterly unproven; and because I think that completely unfounded assumptions of such far-reaching consequence should not be made, when requiring a large number of improbable hypotheses for their explication. I have tested all the available evidence for such inheritance as accurately as I could, and have found that none has the value of proof. There is no inheritance of mutilations, and this constitutes up to now the only basis of fact for the supposition of the inheritance of somatogenic variations. If, in the last essay, I have not denied every possibility of such a transmission, Prof. Vines should interpret that in my favour, not to my discredit; it is not the business of an investigator to set forth a proposition, which on the existing evidence he is compelled to believe, as an infallible dogma. Prof. Vines finds my "statements of opinion so fluctuating that it is difficult to determine what [my] position exactly is," but he could have easily discovered my meaning, if, instead of promiscuously contrasting the eight essays and the eight years of their production, he had merely brought the last of them to the bar of judgment. This essay is especially concerned with "the supposed transmission of mutilations," and at its conclusion my verdict on the state of the problem of the inheritance of acquired characters is thus summarised:—"The true decision as to the Lamarckian principle [lies in] the explanation of the observed phenomena of transformation. . . . If, as I believe, these phenomena can be explained without the Lamarckian principle, we have no right to assume a form of transmission of which we cannot prove the existence. Only if it could be shown that we cannot now or ever dispense with the principle, should we be justified in accepting it." The distinguished botanist De Vries has proved that certain constituents of the cell-body, *e.g.* the chromatophores of Algæ, pass directly from the maternal ovum to the daughter-organism, while the male germ-cell generally contains no chromatophores. Here it appears possible that a transmission of somatogenic variation has occurred; in these lower plants, the separation between somatic and reproductive cells is slight, and the body of the ovum does not require a complete chemical and physical alteration to become the body of the somatic cell of the daughter. But how does this affect the question whether, for instance, a pianoforte player can transmit to his progeny that strength of his finger-muscles which he has acquired by practice? How does this result of practice arrive at the germ-cells? In that lies the real problem which those have to solve who maintain that somatogenic characters are transmissible.

It is proved by the observations of Boveri, quoted above, that among animals the body of the ovum contributes nothing to inheritance. If the transmission of acquired characters should take place, it would have to be by means of the nuclear matter of the germ-cells—in fact, by the germ-plasm, and that not in its patent, but in its latent condition.

To renounce the principle of Lamarck is certainly not the way to facilitate the explanation of the phenomena; but we require, not a mere formal explanation of the origin of species of the most comfortable nature, but the real and rightful explanation. We must attempt, therefore, to elucidate the phenomena without the aid of this principle, and I believe myself to have made a beginning in this direction. A short time ago I tried this in one of those cases where one would least expect to be able to dispense with the principle of modification by use—namely, in the question of artistic endowment.[1] I proposed to myself the question whether the musical sense of mankind could be conceived of as arising without a heightening of the original acoustic faculty by use. But even here I came to the conclusion that, not only do we not need this principle, but that use has actually taken no part in the development of the musical sense.

A. WEISMANN.

THE LIFE AND WORK OF G. A. HIRN.

THE three men who worked at the experimental determination of the mechanical equivalent of heat and at practical Thermodynamics have passed away within a few months of each other—Clausius, Joule, and now Hirn.

[1] "Gedanken über Musik bei Thieren und bei Menschen," *Deutsche undschau*, October 1889.

They were much of the same age, and began their experiments while young at almost the same time; and the practical agreement of the conclusions drawn from their experimental results is our best guarantee of confidence in the modern theory of Thermodynamics which is built upon these results.

Gustave Adolphe Hirn was born at Logelbach, in Alsace, on August 21, 1815, and died on January 14 of this year, a victim to the prevailing epidemic of influenza; but for this, we might have expected still further developments of his scientific theories, as he continued at work on his favourite subjects to the last.

Self-taught, so far as his scientific education was concerned, he found himself, with his elder brother Ferdinand, a manager of the works of Haussman, Jordan, and Co., an establishment for the fabrication of *indiennes*, established in 1772. Finding the machinery antiquated and worn out, Hirn, in setting to work to make the best of it, was really better placed for theorizing and experimentalizing than if he had charge of modern works in first-rate order. The different parts of the works being at a distance from each other, his brother Ferdinand brought out his system of cable transmission of power; and it was Gustave who pointed out theoretically the advantage of a thin light cable run at a high speed.

Hirn also turned his attention to the important economic question of the lubrication of machinery, and upset the previous prejudice against the use of mineral oil for this purpose. He also demonstrated experimentally that, while the old laws of friction enunciated by Morin were sufficiently accurate for the contact of one dry metal against another, these laws are powerfully modified when the surfaces are well lubricated, as with machinery. Now the friction varies as the square root of the pressure, and as the surface and the velocity; so that the theory falls in with that of the viscous flow of liquids. These laws have received confirmation of recent years by the experiments carried out under the auspices of the Institution of Mechanical Engineers.

But it is chiefly for his experiments on a large scale on the steam-engines under his charge that Hirn is best known, and from his varied methods of determining the mechanical equivalent of heat by the friction of metals on metal or water, and finally from observation of the amount of heat consumed by the steam-engine, when every source of gain or loss is carefully followed up.

With this object he investigated experimentally the separate effects of conduction, of jacketing, of initial condensation in the cylinder, and of its prevention by superheating.

If we watch the performance of a modern marine triple-expansion engine, we notice that the high-pressure cylinder appears choked with water from initial condensation, while the intermediate and low-pressure cylinders work comparatively dry. It was considered in the early days of compound engines that this initial condensation was a source of great loss, and superheating was introduced to minimize it. But the superheated steam ruined the packings, and dried up the lubricant, so that the superheater was found practically to do more harm than good. A characteristic story is told of John Elder, the pioneer of compounding in modern marine engines, too long to insert here, which bears on this point.

Nowadays this initial condensation is looked upon as inevitable, and as not really so uneconomical as the books make out, when attendant advantages are considered; but to the theorist such as Hirn this condensation was something to be avoided at any cost, and he worked hard to make its prevention feasible.

Hirn was a man of varied reading, taste, and pursuits, and he worked into his treatises on his favourite subject of Thermodynamics a good deal of speculative metaphysics, which make his books rather curious reading sometimes to modern tastes, and we must go back to the time of Descartes and Leibnitz, when physical science and moral philosophy went hand in hand, to find an equivalent.

But it must be allowed that the science of Thermodynamics may be treated with advantage from this double point of view; for, after its First Law has been established, that heat and work are equivalent and interchangeable, the rate of exchange being fixed by the mechanical equivalent of Joule and Hirn, when we come to the Second Law, named after Carnot, we are compelled to secure conviction of its truth by an appeal to the arguments of analogy and metaphysics.

Hirn spent the last years of his life at Colmar, in the society of a few congenial friends, much interested in metaphysics and meteorology, but cut off from his native France by international strained relations.

In this age of practical Thermodynamics his work will not be lost sight of; but we are still far from a complete reconciliation of the abstract theories of the books and the observed realities of practice.

A. G. GREENHILL.

NOTES.

THE Croonian Lecture, which will be delivered before the Royal Society on February 27 by Prof. Marshall Ward, will be on "The Relations between Host and Parasite in certain Epidemic Diseases of Plants."

ON Thursday last the Astronomer-Royal was elected by ballot to fill the place of the late Father Perry upon the Council of the Royal Society.

METEOROLOGISTS will be sorry to hear of the death of Prof. C. H. D. Buys-Ballot, on Sunday last. He was born in 1817, and had been Director of the Meteorological Institute, Utrecht, for more than 30 years.

DR. DAVID SHARP, the eminent entomologist, and late President of the Entomological Society of London, has accepted the appointment of Curator in Zoology in the Museum of the University of Cambridge, rendered vacant by the resignation of the Rev. A. H. Cooke, whose labours on the Macandrew Collection in that Museum have been so highly appreciated by conchologists.

SIR WILLIAM GULL, F.R.S., was so distinguished a physician, and his name was so well known, that the tidings of his death excited a widespread feeling of regret. He died on Wednesday, January 29, from paralysis, and the funeral took place on Monday at the churchyard of Thorpe-le-Soken, Essex. He was in his seventy-fifth year.

WE regret to hear of the death of Dr. L. Taczanowski, which took place at Warsaw on January 11. He is best known for his standard work "Ornithologie du Pérou," but his contributions to the ornithology of Poland, of Siberia, and the Corea have also been numerous and important.

GERMAN papers announce the death of Otto Rosenberger, the well-known astronomer. He was born in Courland in 1810, and in 1831 was appointed to the charge of the Observatory at Halle, and at the same time was made Professor of Mathematics. This position he held during the rest of his long life. Rosenberger's name is known chiefly in association with his work relating to Halley's comet.

ANOTHER death which we are sorry to have to record is that of Prof. Neumayr, the geologist, of Vienna. He was only a little over forty years of age, and his death is a great loss.

ON February 15, Lord Rayleigh will begin a course of seven lectures at the Royal Institution. The subject will be electricity and magnetism.

THE Council of the Society of Arts have arranged that a course of lectures on "The Atmosphere" shall be given by Prof. V. Lewes on the following Saturday afternoons: March 8, 15, 22, and 29, at 3 o'clock.

MR. B. A. GOULD, Cambridge, Mass., has been appointed President of the American Metrological Society for the present year. Among the members of the Council of this Society are Messrs. Cleveland Abbe, H. A. Newton, Simon Newcomb, and S. P. Langley. The Society was founded in 1873, and its objects are to improve existing systems of weights, measures, and moneys, and to bring them into relations of simple commensurability with each other; to secure the universal adoption of common units of measure for quantities in physical observation or investigation, for which ordinary systems of metrology do not provide; to secure uniform usage as to standard points of reference, or physical conditions to which observations must be reduced for purposes of comparison; and to secure the use of the decimal system for denominations of weight, measure, and money derived from unit-bases, not necessarily excluding for practical purposes binary or other convenient divisions.

THE Committee of the Cambridge University Antiquarian Society in their fifth Annual Report state that, since the opening of the Archæological Museum in 1884, over 2800 objects and 900 books have been added to the collection. The most important additions have been made in the ethnological department, including (during the past year) General Scratchley's collections from New Guinea, a series of 500 specimens of implements and ornaments from the West Indies, presented by Colonel Fielden, who has also given many rare stone implements and weapons collected in South Africa, and a series of 70 specimens of dresses, weapons, &c., from the Solomon and Banks Islands and from Santa Cruz, presented by Bishop Selwyn. The Curator, Baron von Hügel, reports that during the long vacation he excavated with success a Roman refuse-pit and a burial-place at the eastern side of Alderney. The digging is to be resumed.

THE seventh annual dinner of the Association of Public Sanitary Inspectors was held on Saturday evening at the First Avenue Hotel, Holborn. Dr. B. W. Richardson presided, and proposed the toast of "The Association and its President, Sir Edwin Chadwick." The duties of the Association, he said, were to teach and protect its members, and all sanitary inspectors ought to belong to it. He hoped that the apathy at present shown by too many of them would not last any longer.

DR. A. N. BERLESE, of Padua, has been appointed Professor of Botany to the Royal Lyceum at Ascoli-Piceno; and Dr. J. H. Wakker, of Utrecht, Professor of Botany at the dairy school at Oudshoorn, Holland.

THE *Botanical Gazette* published at Crawfordsville, Indiana, gives some particulars of one of the most magnificent bequests ever made for scientific purposes, that of the late Mr. H. Shaw for the endowment of the Botanic Garden and School of Botany at St. Louis, Missouri, amounting to not less than between three and five million dollars. The trustees have determined to apply the income to the maintenance and increase in the scientific usefulness of the Botanic Garden; to provide fire-proof quarters for the invaluable herbarium of the late Dr. George Engelmann, and to supply means for its enlargement; to secure a botanical museum; and to gradually acquire and utilize facilities for research in vegetable physiology and histology, the diseases and injuries of plants, and other branches of botany and horticulture. To aid in the carrying out of this last purpose, travelling botanical scholarships have been established. The present very able director of the Botanic Garden is Dr. William Trelease.

THE *Kew Bulletin* for February begins with some extracts from the Annual Report on the Government cinchona plantation and factory in Bengal for the year 1888–89. The valuable information presented in these extracts is given for the benefit of persons growing cinchona in countries which the documents for the Government of Bengal are little likely to reach. The new number also deals with the use of maqui berries for the colouring of wine, vine-culture in Tunis, phylloxera in Victoria, the botanical exploration of Cuba, and the sugar production of the world. The section on the last of these subjects relates to statistics brought together in Dr. Robert Giffen's report on the progress of the sugar trade. Commenting on the figures supplied in this report, the writer in the *Bulletin* says that if they "do not justify a gloomy view of the present position of the cane-sugar industry in British colonies, they scarcely justify a very optimistic one. It is obvious that the capital which should be applied to the improvement of manufacturing processes and machinery is, under present circumstances, practically diverted to the mere maintenance of the cultivation. And this in the long run must be a losing game. At present the fact stands that West Indian sugar has to a large extent been driven from the home market to that of the United States. If in time it should lose that, its fate apparently is sealed."

AT the last meeting of the Paris Biological Society, Prof. Raphael Blanchard gave an interesting account of a peculiar pigment, hitherto found in plants only, *carotine*, which he has discovered in a crustacean in one of the Alpine lakes, near Briançon. Its functions are not yet known, but M. Blanchard intends to pursue his study of the subject on the spot. The animals cannot be transported alive to lower levels.

WE are glad to welcome the first number of *The University Extension Journal*. The Society by which it is issued has become important enough to need an organ of its own; and the new periodical, which will appear at the beginning of every month, ought to be of service to all who are in any way interested in the movement.

THE *Engineer* of January 31 contains a leading article on "Colour-blind Engine-drivers," and it is interesting to note what the leading technical journal has to say on the subject: "We do not say that no accident was ever brought about by the inability of a driver to distinguish between a green light and a red one, but we can say that nothing of such an accident is to be met with in the Board of Trade Reports." Our contemporary is of opinion that the testing of the sight of locomotive men should be made under working conditions, *i.e.* with actual signal lights.

A PAPER on mortality from snake-bite in the district of Ratnagherry was read lately before the Bombay Natural History Society by Mr. Vidal, of the Bombay Civil Service. Many of the deaths in that district are, he says, due to a small and insignificant-looking snake, called "foorsa" by the natives. It is a viper rarely more than a foot long, and is so sluggish that it does not move out of the way till trodden on. Thus it is much more dangerous than the stronger and fiercer cobra.

DURING the year 1889 no fewer than 28 bears, 115 wolves, and 45 wolf-cubs were shot in the single district of Travnik, in Bosnia.

Das Wetter for January contains:—(*a*) An article by Dr. R. Assmann on climatological considerations about the prevalent epidemic of influenza. From an experience of many years in dealing with the connection between climatic conditions and the state of health, the author gives the following conditions as the most favourable for spreading organisms in the air: (1) dryness of the soil, (2) deficiency of snow covering, (3) deficiency of rainfall, (4) existence of fog or low-hanging clouds, (5) prevalence of high barometer with a small intermingling of air in the vertical direction; and he shows that these conditions were prevalent in Eastern and Central Europe from the beginning of November; that atmospheric dust existed in great quantities, and was propagated westward by easterly, north-easterly, and south-easterly winds. He considers that changes of temperature had no important relation to the spread of the epidemic. (*b*) A lecture recently delivered to the Scientific Club in Vienna, on the general circulation of the atmosphere, by Dr. J. M. Pernter. He refers to the idea of the conflict of polar and equatorial winds so long supported by Dove and others, and shows that the publication of synoptic charts since the year 1863 has demonstrated that the above theory does not hold good for temperate and northern latitudes, that the circulation there depends upon the positions of the areas of high and low pressures, producing cyclones and anticyclones. Many dark points require explanation, such as the tracks which the cyclones follow, but much new light has recently been thrown upon the subject, especially by the researches of Ferrel, Oberbeck, and Abercromby.

DR. ALBRECHT PENCK, Professor of Physical Geography at the University of Vienna, lately called attention to the fact that no two official accounts of the area of the Austro-Hungarian monarchy agree. The difference between the highest and the lowest estimates amounts to 3313·75 square kilometres. By an examination of the new special map constructed by the Army Geographical Institute, which is on the scale of 1 to 75,000, and occupies 400 sheets, Prof. Penck has satisfied himself that the actual area of the Empire is 3247·12 square kilometres greater than is given in the latest published official account. The error arose chiefly from an incorrect triangulation of the Hungarian portion of the Empire, which is 3054·02 square kilometres larger than has been supposed.

IT has hitherto been generally believed that the Montgolfier or hot-air balloon cannot be used in tropical climates. If this were true, ballooning for war purposes would of course be impossible in places where coal-gas could not be obtained. We learn from the *Times* that Mr. Percival Spencer, who has been making a series of interesting balloon experiments in Central India, has succeeded in showing that the theory is without foundation. At Secunderabad, in presence of the garrison and a crowd of European and native spectators, he lately made an ascent in his patent asbestos balloon. The inflation was effected by the burning of methylated spirit inside the balloon, which was held in place by 25 soldiers of the Bedford regiment until the word to "let go" was given. After rising to a considerable height, the aëronaut descended by means of his parachute. The spot where the ascent was made is over 2000 feet above the level of the sea, and the achievement was all the more remarkable because of the sultry climate and the great rarity of the air.

AN interesting paper on "Some Terraced Hill Slopes of the Midlands," by Mr. Edwin A. Walford, has been reprinted from the Journal of the Northamptonshire Natural History Society. The factors in the formation of these terraced slopes Mr. Walford groups as follows:—(1) The slipping and sliding outwards of the saturated porous marls upon the tenacious clays at the line of drainage, aided doubtless by the pressure of the superincumbent rock bed. (2) Displacements caused by the removal by chemical and mechanical solution of certain constituents of the marls and marlstone by the passage of the surface water through them. (3) The sliding downwards of the surface soil, as described by Dr. Darwin, and latterly illustrated by Mr. A. Ernst. The suggestions offered by Mr. Walford agree in the main, as he himself points out, with those adopted by Mr. A. Ernst in his paper in NATURE, February 28, 1889.

MESSRS. GAUTHIER-VILLARS (Paris) have recently added three new works to their already large list of photographic treatises. One is the "Manuel de Phototypie," by M. Bonnet, giving full details of the various processes for the rapid reproduction of photographs, such as is now demanded for many purposes. The formulæ are stated very clearly, and the apparatus required is sufficiently illustrated by diagrams. The treatise is thoroughly practical, and will be very valuable to all interested in the subject, whether as amateurs or for trade purposes. The second—"Temps de Pose"—is by M. Pluvinel, and deals with the difficult question of the time of exposure. It is shown that what is generally regarded as a rule-of-thumb process can be reduced to a scientific one. The various functions of the duration of the exposure are first considered mathematically, and it is then shown how the results of the investigations are to be applied practically, the method being illustrated by worked-out examples. To simplify matters, tables are given showing the different elements, such as coefficient of brightness, for all ordinary photographic subjects. The treatise is chiefly interesting as a scientific contribution, as few photographers will care to take the trouble of working out the time of exposure, now that they have found that good work can be done by judgment alone. The third book is in two volumes, and treats of the various "film" processes ("Procédés Pelliculaires," by George Balagny). It claims to give a full account of all that has been said and done in connection with the subject since the introduction of photography, and as far as we can judge, this claim is fully justified. Every detail of the subject is considered in a very practical manner. One of the most interesting applications of flexible films mentioned is the registration of flash signals in "optical telegraphy."

THE "Year-book of Photography" (Piper and Castle) for 1890 fully bears out the good reputation gained by its predecessors. In addition to the information relating to the various photographic societies, there are several articles on the advances in photographic processes which have been made during the past year, and other useful notes. One of the most interesting articles is that by the editor on photography in natural colours, from which we learn that "processes of practical value, to achieve the end, are likely to be discovered by the exercise of ability and perseverance." The only important omission we notice is a record of the remarkable achievements in astronomical photography. The volume contains a portrait and short biographical notice of Edmond Becquerel. The whole forms an invaluable book of reference to all photographic matters, with the exception referred to.

MESSRS. GEORGE BELL AND SONS have published "The School Calendar and Hand-book of Examinations, Scholarships, and Exhibitions, 1890." This is the fourth year of issue, and great pains have been taken, as in former years, to secure that the information brought together shall be full and trustworthy. A preface is contributed by Mr. F. Storr.

THE sixteenth part of Cassell's "New Popular Educator" has been issued. It includes a map of Australasia.

THE Proceedings of the International Zoological Congress, held in Paris last summer, will be ready for distribution in a fortnight.

A NEW and very simple method of synthesizing indigo has been discovered by Dr. Flimm, of Darmstadt (*Ber. deut. chem. Ges.*, No. 1, 1890, p. 57). In studying the action of caustic alkalies upon the monobromine derivative of acetanilide, $C_6H_5.NH.CO.CH_2Br$, a solid melting at 131°·5, it was found that when this substance was fused with caustic potash a product was obtained which at once gave an indigo blue colour on the addition of water, and quite a considerable quantity of a blue solid resembling indigo separated out. The best mode of carrying out the operation is described by Dr. Flimm as follows:—The monobromacetanilide is carefully mixed with dry caustic potash in a mortar, and the mixture introduced into a retort and heated rapidly until a homogeneous reddish-brown melt is obtained This is subsequently dissolved in water, and a little ammonia or ammonium chloride solution added, when the liquid immediately becomes coloured green, which colour rapidly changes into a dark blue, and in a short time the blue colouring matter is for the most part deposited upon the bottom of the vessel in which the operation is performed. The fused mass may also conveniently be dissolved in dilute hydrochloric acid, and a little ferric chloride added, when the formation of indigo takes place immediately. The collected blue colouring matter may be readily obtained pure by washing first with dilute hydrochloric acid and afterwards with alcohol. That this blue substance was really common indigo was proved by the fact that it yielded several of the most characteristic reactions of indigotin, such as solubility in aniline, paraffin, and chloroform, its sublimation, and the formation of sulphonic acids, which gave similar changes of colour with nitric acid to those of indigotin. The final proof was afforded by its reduction to indigo white and re-oxidation to indigo blue by exposure to air. Moreover, the absorption spectrum of the colouring matter was found to be identical with the well-known absorption spectrum of indigo. Hence there can be no doubt that indigo is really formed by this very simple process. The chemical changes occurring in the reaction are considered by Dr. Flimm to be the following:—Indigo blue is not produced directly, but first, as a condensation product of the monobromacetanilide, indoxyl is formed, $C_6H_4\left<\begin{matrix} NH \\ COH \end{matrix}\right>CH$, or more probably a pseudo-indoxyl of the isomeric constitution $C_6H_4\left<\begin{matrix} NH \\ CO \end{matrix}\right>CH_2$. This intermediate substance then passes over by oxidation into indigo, $C_6H_4\left<\begin{matrix} NH \\ CO \end{matrix}\right>C{=}C\left<\begin{matrix} NH \\ CO \end{matrix}\right>C_6H_4$, two molecules each losing two atoms of hydrogen by oxidation, and then condensing to form indigo. It was not found possible to isolate the intermediate pseudo-indoxyl, owing to its extreme instability; indeed, the all-important point to be observed in the practical carrying out of the synthesis by this method is that the fusion must be performed quickly and the temperature raised rapidly to a considerable height, the whole process occupying only a few minutes. The yield of pure indigo under the conditions yet investigated is not very large, amounting to about four per cent. of the weight of the original anilide.

THE additions to the Zoological Society's Gardens during the past week include thirteen Cuning's Octodons (*Octodon cuningi*) from Chili, presented by Mr. W. H. Newman; five Common Dormice (*Muscardinus avellanarius*), British, presented by Mr. Florance Wyndham; a Large Hill-Mynah (*Gracula intermedia*) from India, deposited; a Dingo (*Canis dingo*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

OBJECTS FOR THE SPECTROSCOPE.

Sidereal Time at Greenwich at 10 p.m. on February 6 = 7h. 7m. 56s.

Name.	Mag.	Colour.	R.A. 1890.	Decl. 1890.
			h. m. s.	° ′
(1) G.C. 1515	—	—	7 17 14	+69 14
(2) 51 Geminorum ...	5·5	Yellowish-red.	7 7 3	+16 21
(3) v Geminorum ...	4	Yellow.	7 27 26	+32 8
(4) α Geminorum ...	2	White.	7 11 48	+16 44
(5) DM. + 3°·1381 ...	9	Reddish-yellow.	6 38 54	+ 3 24
(6) U Monocerotis ...	Var.	Orange.	7 25 32	− 9 33

Remarks.

(1) The spectrum of this nebula has not yet, so far as I know, been recorded, but the observation will not be difficult, if one may judge from the description given by Herschel, namely: "Very bright, pretty large, round, much brighter in the middle, mottled as if with stars."

(2) This star has a spectrum of the Group II. type, Dunér describing it as very beautiful. He states that all the bands, 1-9, are very wide and dark. The observations most likely to extend our knowledge of the group of bodies to which this star belongs are (1) observations of the bright carbon flutings (see p. 305); (2) comparisons with the flame spectra of manganese, magnesium, and lead; (3) observations made with special reference to the presence or absence of absorption lines, of which Dunér makes no mention.

(3) Gothard classes this with stars of the solar type. The usual differential observations are required.

(4) A star of Group IV. The usual observations of the relative intensities of the hydrogen and metallic lines (*b*, D, &c.), as compared with other stars, are required.

(5) A rather faint star of Group VI., in which the character of band 6 (near λ 564), as compared with the other carbon bands (9 and 10), requires further attention. Secondary bands should also be looked for.

(6) This variable is stated by Gore to have a continuous spectrum, but it seems probable that lines or flutings will be found if the star be examined under the most favourable conditions—that is, when near maximum. Rigel was formerly said to have a "continuous" spectrum, but the lines are now by no means difficult to see. The star ranges from magnitude 6 at maximum to 7·2 at minimum, and the period is 31–50 days (Gore). A. FOWLER.

TOTAL SOLAR ECLIPSE OF 1886.—Dr. Schuster has thus summarized the spectroscopic results he obtained at this eclipse (Phil. Trans., vol. 180, 1889):—

(1) The continuous spectrum of the corona has the maximum of actinic intensity displaced considerably towards the red, when compared with the spectrum of sunlight.

(2) While, on the two previous occasions on which photographs of the spectrum were obtained, lines showed themselves outside the limits of the corona, this was not the case in 1886.

(3) Calcium and hydrogen do not form part of the normal spectrum of the corona. The hydrogen lines are visible only in the parts overlying strong prominences; the H and K lines of calcium, though visible everywhere, are stronger on that side of the corona which has many prominences at its base.

(4) The strongest corona line in 1886 was at $\lambda = 4232·8$; this is probably the 4233·0 line often observed by Young in the chromosphere.

(5) Of the other strong lines, the positions of the following seem pretty well established:—

4056·7 **4084·2** 4089·3 **4169·7** **4195·0** **4211·8**
4280·6 4365·4 **4372·2** 4378·1 4485·6 4627·9

The lines printed in thicker type have been observed also at the Caroline Island and Egyptian Eclipses.

(6) A comparison between the lines of the corona and the lines of terrestrial elements has led to negative results.

ANNUAIRE DU BUREAU DES LONGITUDES.—In the volume for 1890, MM. Lœwy and Schulhof contribute a list of the comets which appeared from 1825 to 1835 inclusive, and in 1888, being a continuation of the lists given in former years. M. Lœwy also gives a complete table of the appearances of the planets throughout 1890, and ephemerides of a considerable number of variable stars. An elaborate comparison of the various calendars is from the pen of M. Cornu, and under the head of the solar system a rich store of information is included. With the notices we find an account of the meeting of the permanent committee of the photographic chart of the heavens and the Photographic Congress of September last. This year's *Annuaire* is as completely filled with information as it has ever been and doubtless will be as much appreciated by astronomers.

ANNUAIRE DE L'OBSERVATOIRE ROYAL DE BRUXELLES.—The volume for 1890 is the fifty-seventh annual publication from this Observatory. It contains tables of the mean positions of the principal stars and their apparent right ascensions, of the occultation of stars by the moon, and of eclipses of Jupiter's satellites, mention being also made of remarkable phenomena relating to the moon and the planets. M. Folie gives a biographical sketch of his predecessor, J. C. Houzeau, which is embellished with the portrait of this deceased bibliographer. Considerable attention has been paid to the researches on diurnal nutation and the determination of the constant. M. Spee discusses the tabulated observations of the condition of the sun's surface during 1888, and M. Moreau contributes an interesting note on the movement of a solid about a fixed point. A list is also given of the comets and asteroids discovered in 1889, and some of the particulars relating to their orbits.

ROYAL ASTRONOMICAL SOCIETY.—The annual general meeting of the Fellows of this Society will be held at Burlington House on Friday, the 14th inst., for the purpose of receiving the Report of the Council, electing officers for the ensuing year, and transacting other business of the Society. The chair will be taken at 3 o'clock precisely.

Erratum.—In the elements of companion C of Brook's comet (p. 305), read $\Omega = 17° 52' 24''·5$, and log a = 0 565059.

GEOGRAPHICAL NOTES.

BARON NORDENSKIÖLD has announced in the Swedish Academy of Sciences, that he and Baron Oscar Dickson, with assistance from the Australian colonies, will start on an expedition in the South Polar regions next year.

A RECENT telegram from Tashkent announced that Colonel Pevtsoff and M. Roborovsky had discovered a convenient pass to the north-western part of Tibet, from Nia, and had mounted to the great table-land. The plateau has there an altitude of 12,000 feet above the sea, and the country round is desolate and uninhabited, while towards the south the plateau is well watered and wooded. The Tashkent telegram is so expressed that it might be supposed to mean that two separate passes had been discovered by the two explorers. But the news received from the expedition at St. Petersburg on December 26, and dated October 27, shows that both explorers proposed to leave the oasis of Keria (100 miles to the east of Khotan) on the next day, for Nia (65 miles further east) and there to search for a passage across the border-ridge which received from Prjevalsky the name of the "Russian ridge." This immense snow-clad chain separates the deserts of Eastern Turkestan from the trapezoidal space, the interior of which is quite unknown yet, and which is bordered by the "Russian" ridge and the Altyn-tagh in the north-west; the ridges of Tsaidam and those named by Prjevalsky "Columbus" and "Marco-Polo" in the north-east; the highlands (explored by Prjevalsky in 1879–80) at the sources of the Blue River, in the south-east; and a long, yet unnamed ridge which seems to be a prolongation of the Tan-la, in the south-west. The pass leading to that plateau from Nia, and now discovered by the Russian expedition, is situated some 80 miles to the east of the well-known pass across the Kuen-lun Mountains which leads from Southern Khotan to Lake Yashi-kul. M. Roborovsky's intention is evidently next to move up the Tchertchen river and to endeavour to reach the ridges "Moscow" and "Lake Unfreezing" (11,700 feet high), which were visited by Prjevalsky from the east during his last journey. Having succeeded in finding a pass to Tibet in the south of Nia, Colonel Pevtsoff proposes, as soon as the spring comes, to proceed himself by this pass to the table-land, while M. Roborovsky probably will be despatched to explore the same border-ridge further east, in the south of Tchertchen.

THE *Boletin* of the Madrid Geographical Society for the last quarter of 1889 contains a most valuable memoir by Dr. Fernando Blumentritt, on the intricate ethnology of the Philippine Islands. The author classifies the whole of the native population in three broad divisions—Negrito, Malay, and Mongoloid; the last comprising those tribes which in their physical appearance betray certain Chinese or Japanese affinities. All are grouped in an admirably arranged alphabetical table, where their names, race, language, religion, culture, locality, and numbers are briefly specified in seven parallel columns. With a few variants and cross-references this table contains no less than 159 entries, and thus conveys in summary form all the essential particulars regarding every known tribe in the Philippine Archipelago. From it we gather that the Negritoes—that is, the true autochthonous element, variously known as Aetas, Attas, Atés, Etas, Itas, Mamánuas, &c., and physically belonging to the same stock as the Samangs of the Malay Peninsula—

are now reduced to about 20,000, dispersed in small groups over the islands of Luzon, Mindoro, Tablas, Panay, Negros, Cebu, Paragan (Palawan), and Mindanao. A few also appear still to survive in Alabat, Busuanga, and Culioú. Of the Malay peoples by far the most numerous and important are the southern Bisayas (Visayas), and the northern Tagalas, both described as "civilized Christians," and numbering respectively 1,700,000 and 1,250,000. These two peoples are steadily encroaching on all the surrounding tribes, causing them to disappear by a gradual process of absorption or assimilation, and the time is approaching when the whole of the islands will be divided into two great nationalities bearing somewhat the same relation to each other that the High German does to the Low German branch of the Teutonic family.

SMOKELESS EXPLOSIVES.[1]

I.

THE production of smoke which attends the ignition or explosion of gunpowder is often a source of considerable inconvenience in connection with its application to naval or military purposes, its employment in mines, and its use by the sportsman, although occasions not unfrequently arise during naval and military operations when the shroud of smoke produced by musketry or artillery fire has proved of important advantage to one or other, or to both, of the belligerents during different periods of an engagement.

Until within the last few years, however, but little, if any, thought appears to have been given to the possibility of dispensing with or greatly diminishing the production of smoke in the application of fire-arms, excepting in connection with sport. The inconvenience and disappointment often resulting from the obscuring effects of a neighbouring gun-discharge, or of the first shot from a double-barrel arm, led the sportsman to look hopefully to gun-cotton, directly after its first production in 1846, as a probable source of greater comfort and brighter prospects in the pursuit of his pastime and in his strivings for success.

A comparison between the chemical changes attending the burning, explosion, or metamorphosis of gun-cotton and of gunpowder, serves to explain the cause of the production of smoke in the latter case, and the reason of smokelessness in the case of gun-cotton. Whilst the products of explosion of the latter consist exclusively of gases, and of water which assumes the transparent form of highly-heated vapour at the moment of its production, the explosive substances classed as gunpowder, and which consist of mixtures of saltpetre, or another nitrate of a metal, with charred wood or other carbonized vegetable matter, and with variable quantities of sulphur, furnish products, of which very large proportions are not gaseous, even at high temperatures. Upon the ignition of such a mixture, these products are in part deposited in the form of a fused residue, which constitutes the fouling in a fire-arm, and are in part distributed, in an extremely fine state of division, through the gases and vapours developed by the explosion, thus producing smoke.

In the case of gunpowder of ordinary composition, the solid products amount to over fifty per cent. by weight of the total products of explosion, and the dense white smoke which it produces consists partly of extremely finely-divided potassium carbonate, which is a component of the solid products, and, to a great extent, of potassium sulphate produced chiefly by the burning of one of the important solid products of explosion—potassium sulphide—when it is carried in a fine state of division into the air by the rush of gas.

With other explosives, which are also smoke-producing, the formation of the smoke is due to the fact that one or other of the products, although existing as vapour at the instant of its development, is immediately condensed to a cloud composed of minute liquid particles, or of vesicles, as in the case of mercury vapour liberated upon the explosion of mercuric fulminate, or of the aqueous vapour produced upon the ignition of a mixture of ammonium nitrate and charcoal, or ammonium nitrate and picric acid.

Until within the last half-dozen years, the varieties of gunpowder which have been applied to war purposes in this and other countries have exhibited comparatively few variations in chemical composition. The proportions of charcoal, saltpetre, and sulphur employed in their production exhibit slight differences in different countries, and these, as well as the character of the charcoal used, its sources and method of production, underwent but little modification for very many years. The same remark applies to the nature of the successive operations pursued in the manufacture of black powder for artillery purposes in this and other countries.

The replacement of smooth-bore guns by rifled artillery which followed the Crimean war, and the increase in the size and power of guns consequent upon the application of armour to ships and forts, soon called for the pursuit of investigations having for their object the attainment of means for variously modifying the action of fired gunpowder, so as to render it suitable for the different calibres of guns, whose full power could not be effectively, or in some instances safely, developed by the use of the kind of gunpowder previously employed indiscriminately in artillery of all known calibres.

In order to control the violence of explosion of gunpowder, by modifying the rapidity of transmission of explosion from particle to particle, or through the mass of each individual particle, of which the charge of a gun is composed, the accomplishment of the desired results was, in the first instance, and indeed throughout practical investigations extending over many years, sought exclusively in modifications of the size and form of the individual masses composing a charge of powder, and of their density and hardness, it being considered that, as the proportions of saltpetre, charcoal, and sulphur generally employed in the production of gunpowder very nearly correspond to those required for the development of the greatest chemical energy by those incorporated materials, it was advisable to seek for the attainment of the desired results by modifications of the physical and mechanical characters of, rather than by any modification in the proportions and chemical characters of, its ingredients.

The varieties of powder, which, as the outcome of careful practical and scientific researches in this direction, have been introduced into artillery service from time to time, and some of which, at any rate, have proved fairly efficient, have been of two distinct types. The first of these, produced by breaking up more or less highly-pressed cakes of black powder into grains, pebbles, or boulders, of approximately uniform size and shape, the sharp edges and rough surfaces being afterwards removed by attrition (reeling and glazing), are simply a further development of one of the original forms of granulated or corned powder, represented by the old F. G., or small arms, and L. G., or cannon powder. Gunpowder of this class, ranging in size from about 1000 pieces to the ounce, to about six pieces to the pound, have been introduced into artillery service, and certain of them, viz. R. L. G. (rifle large grain), which was the first step in advance upon the old cannon-powder (L. G.); pebble-powder (P.), and large pebble or boulder-powder (P. 2), are still employed more or less extensively in some guns of the present day.

The other type of powder has no representative among the more ancient varieties; it has its origin in the obviously sound theoretical view that uniformity in the results furnished by a particular powder, when employed under like conditions, demands not merely identity in regard to composition, but also identity in form, size, density, and structure of the individual masses composing the charge used in a gun. The practical realization of this view should obviously be attained, or at any rate approached, by submitting equal quantities of one and the same mixture of ingredients, presented in the form of powder of uniform fineness and dryness, to a uniform pressure for a fixed period in moulds of uniform size, and under surrounding conditions as nearly as possible alike. The fulfilment of these conditions would, moreover, have to be supplemented by an equally uniform course of proceeding in the subsequent drying and other finishing processes to which the powder-masses would be submitted.

The only form of powder, introduced into our artillery service for a brief period, in the production of which these conditions were adhered to as closely as possible, was a so-called pellet powder, which consisted of small cylinders having semi-perforations with the object of increasing the total inflaming surface of the individual masses.

Practical experience with this powder, and with others prepared upon the same system, but with much less rigorous regard to uniformity in such details as state of division and condition of dryness of the powder before its compression into cylindrical or other forms, showed that uniformity in the ballistic properties

[1] Friday Evening Discourse delivered by Sir Frederick Abel, F.R.S., at the Royal Institution of Great Britain, on January 31, 1890.

of black powder could be as well and even more readily secured by the thorough blending or mixing together of batches presenting some variation in regard to density, hardness, or other features, as by aiming at an approach to absolute uniformity in the characters of each individual mass composing a charge.

At the time that our attention was first actively given to this subject of the modification of the ballistic properties of powder, it had already been to some extent dealt with in the United States by Rodman and Doremus, and the latter was the first to propose the application, as charges for guns, of powder-masses produced by the compression of coarsely grained powder into moulds of prismatic form. In Russia the first step was taken to utilize the results arrived at by Doremus, and to adopt a prismatic powder for use in guns of large calibre.

Side by side with the development and perfection of the manufacture of prismatic powder in Russia, Germany, and in this country, new experiments on the production of powder-masses suitable, by their comparatively gradual action, for employment in the very large charges required for the heavy artillery of the present day, by the powerful compression of mixtures of more or less finely broken up powder-cake into masses of greater size than those of the pebble, pellet, and prism powders, were actively pursued in Italy, and also by our own Government Committee on Explosives, and the outcome of very exhaustive practical investigations were the very efficient Fossano powder, or *poudre progressif*, of the Italians, and the boulder and large cylindrical powders known as P^2 and C^2, produced at Waltham Abbey, which scarcely vied, however, with the Italian powder in the uniformity of their ballistic properties.

Researches carried out by Captain Noble and the lecturer some years ago with a series of gunpowders differing considerably in composition from each other, indicated that advantages might be secured in the production of powders for heavy guns by so modifying the proportions of the constituents (*e.g.* by considerably increasing the proportion of charcoal and reducing the proportion of sulphur) as to give rise to the production of a much greater volume of gas, and at the same time to diminish the heat developed by the explosion.

These researches served, among other purposes, to throw considerable light upon the cause of the wearing or erosive action of powder-explosions upon the inner surface of the gun, which in time may produce so serious a deterioration of the arm as to diminish the velocity of projection considerably, and so affect the accuracy of shooting, a deterioration which increases in extent in an increasing ratio to the size of the guns, in consequence, obviously, of the large increase in the weight of the charges fired.

Several causes undoubtedly combine to bring about the wearing away of the gun's bore, which is especially great where the products of explosion, while under the maximum pressure, can escape between the projectile and the bore of the gun. The great velocity with which the very highly heated gaseous and liquid (fused solid) products of explosion sweep over the heated surface of the metal gives rise to a displacement of the particles composing it, which increases as the surface becomes roughened by the first action upon the least compact portions of the metal, and thus opposes greater resistance; at the same time, the effect of the high temperature to which the surface is raised is to reduce its rigidity and power of resisting the force of the gaseous torrent, and lastly some amount of chemical action upon the metal, by certain of the highly heated non-gaseous products of explosion, contributes towards an increase in the erosive effects. A series of careful experiments made by Captain Noble with powders of different composition, and with other explosives, afforded decisive evidence that the material which furnished the largest proportion of gaseous products, and the explosion of which was attended by the development of the smallest amount of heat, exerted least erosive action.

It is probable that important changes in the composition of powders manufactured by us for our heavy guns would have resulted from those researches, but in the meantime, two eminent German gunpowder manufacturers had occupied themselves independently, and simultaneously, with the important practical question of producing some more suitable powder for heavy guns than the various new forms of ordinary black powder, the rate of burning of which, especially when confined in a close chamber, was, after all, reduced only in a moderate degree by the increase in the size of the masses, and by such increase in their density as it was practicable to attain. The German experimenters directed their attention not merely to the proportions in which the powder ingredients are employed, but also to a modification in the character of charcoal, and the success attending their labours in these directions led to the practically simultaneous production, by Mr. Heidemann at the Westphalia Powder Works, and Mr. Düttenhofer at the Rottweil Works near Hamburg, of a prismatic powder of cocoa-brown colour, consisting of saltpetre in somewhat higher proportion, of sulphur in much lower proportion, than in normal black powder, and of very slightly burned charcoal, similar in composition to the charcoal (*charbon roux*) which Violette, a French chemist, first produced in 1847 by the action of superheated steam upon wood or other vegetable matter, and which he proposed for employment in the manufacture of sporting powder. These brown prismatic powders (or "cocoa-powders," as they were termed from their colour), are distinguished from black powder not only by their appearance, but also by their very slow combustion in open air, by their comparatively gradual and long-sustained action when used in guns, and by the simple character of their products of explosion as compared with those of black powder. As the oxidizing ingredient, saltpetre, is contained, in brown or cocoa powder, in larger proportion relatively to the oxidizable components, sulphur and charcoal, than in black powder, these become fully oxidized, while the products of explosion of the latter contain, on the other hand, larger proportions of unoxidized material, or only partially oxidized products. Moreover, there is produced upon the explosion of brown powder a relatively very large amount of water-vapour, not merely because the finished powder contains a larger proportion of water than black powder, but also because the very slightly charred wood or straw used in the brown powder is much richer in hydrogen than black charcoal, and therefore furnishes by its oxidation a considerable amount of water. The total volume of gas furnished by the brown powder (at 0° C. and 760 mm. barometer) is only about 200 volumes per kilogramme of powder, against 278 volumes furnished by a normal sample of black powder, but the amount of water-vapour furnished upon its explosion is about three times that produced from black powder, and this would make the volume of gas and vapour developed by the two powders about equal if the heat of its explosion were the same in the two cases; the actual temperature produced by the explosion of brown powder, is, however, somewhat the higher of the two.

Although the smoke produced upon firing a charge of brown powder from a gun appears at first but little different in denseness to that of black powder, it certainly disperses much more rapidly, a difference which is probably due to the speedy absorption, by solution, of the finely divided potassium salts by the large proportion of water-vapour distributed throughout the so-called smoke.

This class of powder was substituted with considerable advantage for black powder in guns of comparatively large calibre; nevertheless it became desirable to attain even slower or more gradual action in the case of the very large charges required for guns of the heaviest calibres, such as those which propel shot of about 2000 pounds weight. Accordingly, the brown powder has been modified in regard to the proportions of its ingredients to suit these conditions, while, on the other hand, powder intermediate with respect to rapidity of action between black pebble powder and the brown powder, has been found more suitable than the former for use in guns of moderately large calibre.

The recent successful adaptation of machine guns and comparatively large quick-firing guns to naval service, more especially for the defence of ships against attack by torpedo boats, &c., has rendered the provision of a powder for use with them, which would produce comparatively little or no smoke, a matter of very considerable importance, inasmuch as the efficiency of such defence must be greatly diminished by the circumstance that, after a very brief use of the guns with black powder, the objects against which their fire is destined to operate, become more or less completely hidden from those directing them, by the dense veil of powder-smoke produced. Hence much attention has been directed during the last few years to the production of smokeless, or nearly smokeless powders for naval use in the above directions. At the same time, the views of many military authorities regarding the importance of dispensing with smoke in land engagements has also created a demand, the apparent urgency of which has been increased by various circumstances, for a smokeless powder suitable for field artillery and small arms.

The properties of ammonium nitrate, of which the products of decomposition by heat are, in addition to water-vapour, entirely gaseous, have rendered it a tempting material to work upon in the hands of those who have striven to produce a smokeless powder, but its deliquescent character has been the chief obstacle to its application as a component of an explosive agent susceptible of substitution for black powder for service purposes.

A German chemical engineer, F. Gäus, conceived that, by incorporating charcoal and saltpetre with a particular proportion of ammonium nitrate, he had produced an explosive material which did not partake of the hygroscopic character common to other ammonium-nitrate mixtures, and that, by its explosion, the potassium in the saltpetre formed a volatile combination with nitrogen and hydrogen, a *potassium amide*, so that, although containing nearly half its weight of potassium salt, it would furnish only volatile products. The views of Mr. Gäus regarding the changes which his so-called *amide powder* undergoes upon explosion were not borne out by existing chemical knowledge, while the powder compounded in accordance with his views proved to be by no means smokeless, and was certainly not non-hygroscopic. Mr. Heidemann has, however, been successful, by modifications of Gäus's prescription and by application of his own special experience in powder-manufacture, in producing an ammonium-nitrate powder possessed of remarkable ballistic properties, furnishing comparatively little smoke, which speedily disperses, and exhibiting the hygroscopic characteristics of ammonium-nitrate preparations in a decidedly less degree than any other hitherto prepared. The powder, while yielding a very much larger volume of gas and water-vapour than black or brown powder, is considerably slower than the latter; the charge required to produce equal ballistic results is less, while the chamber-pressure developed is lower, and the pressures along the chase of the gun are higher, than in the case of brown powder.

The ammonium-nitrate powder contains, in its normal, dried condition, more water than even brown powder; it does not exhibit any great tendency to absorb moisture from an ordinarily dry or even a somewhat moist atmosphere, but if the amount of atmospheric moisture approaches saturation, it will rapidly absorb water, and when once the process begins it continues rapidly, the powder-masses becoming speedily quite pasty. The charges for quick-firing guns are enclosed in metal cases, in which they are securely sealed up; the powder is therefore prevented from absorbing moisture from the external air, but it has been found that if the cartridges are kept for long periods in ships' magazines, in which, from their position relatively to the ships' boilers, the temperature is more or less elevated, sometimes for considerable periods, the expulsion of water from some portions of the powder-masses composing the hermetically sealed charge, and its consequent irregular distribution, may give rise to want of uniformity in the action of the powder, and to the occasional development of high pressures. Although, therefore, this ammonium-nitrate powder may be regarded as the first successful advance towards the production of a comparatively smokeless artillery powder, it is not uniformly well adapted to the requirements which it should fulfil in naval service.

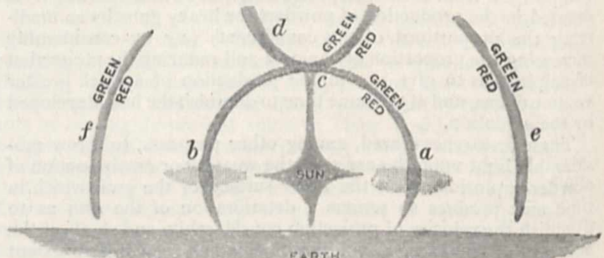
Attention was first seriously directed to the subject of smokeless powder by the reports received about four years ago of remarkable results stated to have been obtained in France with such a powder for use with the magazine rifle (the Lebel) which was being adapted to military service. These reports were speedily followed by others, descriptive of marvellous velocities obtained with small charges of this powder, or some modifications of it, from guns of very great length. As in the case of mélinite, the fabulously destructive effects of which were much vaunted at about the same time, the secret of the precise nature of the smokeless powder was so well preserved by the French authorities, that surmises could only be made on the subject even by those most conversant with these matters. It is now well known, however, that more than one smokeless explosive has succeeded the original powder, the perfection of which was reported to be beyond dispute, and that the material now adopted for use in the Lebel rifle bears, at any rate, great similarity to preparations which have been made the subject of patents in this country, and which are still experimental powders in other countries.

(*To be continued.*)

SOLAR HALOS AND PARHELIA.

THE recent appearance of solar and lunar halos, parhelia, and paraselene, has called forth a considerable amount of correspondence from all parts of the country, and the accompanying figure may be taken as a composite representation of the solar phenomenon observed. A glance at the times at which the halos were observed on the 29th ult., makes it apparent that they occurred earliest in places of highest latitudes. At Driffield, in lat. 54°, the halo, with its attendant parhelia, was observed at 1.34 p.m., and the whole phenomenon disappeared at 2.8 p.m.; at Burton-on-Trent, lat. 52° 48′, the halos and parhelia were first observed at 2 p.m., and lasted more or less distinctly until 3 p.m.; whilst about a degree south of this, at Oxford, Colnbrook, and Walton-on-Thames, the phenomena occurred from about 3.30 to 4.30. The uniform difference in the times when the halos were observed at the places of different latitudes necessarily follows from the fact that they are formed by the action upon solar rays of prismatic crystals of ice suspended in the air by the ascending currents which especially occur in the spring and autumn. Those prisms that are in such positions that the rays from the sun in transmission through them suffer minimum deviation are the cause of the formation of halos, and since the angular distance of the sun equal to minimum deviation is about 22°, this must be the radius of the halo, and the external circle, being produced by two such refractions in succession, has a radius of about 46°.

The halos recently observed do not differ in the main from those frequently seen in higher latitudes, and consisting of (1) a first circle or halo concentric with the sun, red within, violet without, and at an angular distance of 22° or 23°; (2) a second circle or halo, similar to the preceding, but at an angular distance of 46°; (3) a portion of the *parhelic* circle appearing horizontal and diametral, and at the points of junction of this circle with the two halos, there is increased luminosity, which have been taken for images of the sun; (4) horizontal arcs, tangents to the circular halos, and a vertical line making a cross with the horizontal portion of the parhelic circle.



a was seen at 3.35 p.m.; *b* at 3.45 p.m.; *c* and *d* at 3.50 p.m.; *e* at 4.0 p.m.; *f* at 4.10 p.m.

Mr. John Lovell thus describes the phenomena observed at Driffield:—"A splendid solar halo, with its attendant parhelia, was observed this afternoon at 1.34 local time. The halo (diameter 45°) was almost perfect, the lower part only being slightly obliterated by the thick atmosphere near the horizon. Attached to the upper side, an inverted portion of a similar halo appeared, brilliantly illumined on the concave side, the lower part giving out a dull red light. Again, 22½° above this, and also inverted, about 60° of arc beautifully coloured with rainbow colours was clearly visible, the red side lowest. This arc, if it had been produced, would have circled the zenith. The mock lights on each side of the halo were drawn out into long cones of intensely bright light, while the inner sky of the halo was of a very dark shade. The most noteworthy feature of the display was a brilliant patch of pure white light in the north-western sky, at a distance of 90° from the western mock sun, and undoubtedly emanating from it, and which remained visible for nearly ten minutes. The whole phenomena disappeared at 2.8 p.m., the sky then being covered with streaky cirro-stratus haze from the north-north-west."

The patch of white light referred to by Mr. Lovell was doubtless produced by the junction of the parhelic circle with one of the halos concentric with the sun. It is perhaps hardly necessary to note the relation that exists between halos and cirro-stratus clouds, and that the space included within the halo is frequently of a more intense grey, or of a deeper blue than the rest of the sky.

The son of Sir W. Herschel observed the phenomena at Oxford, and noted:—"The sun was near the horizon. On either side of it, at a distance of five or six diameters of the sun, was a mock sun, not very bright, of the colours of the rainbow, the one on the right being the brighter. There was a scarcely perceptible rainbow, of which red was the only colour visible, joining the two mock suns. This rainbow was brightest directly over the sun. As far off again as the first was a second rainbow, hazy, but fairly bright, which was equally visible from earth to earth. Vertically above the sun, a third, a very bright rainbow, touched the second, being inverted, and having its centre straight overhead. It did not look quite as large as the second. The weather was clear, but the clouds on and above the horizon were of a uniform grey colour, fading off gradually to a nearly clear sky overhead. There seemed always to be a much lighter shade of grey in the clouds where the sun and the two mock suns were."

The coloured parhelia observed indicates the refraction and dispersion of solar light by vertical prisms, whilst the phenomena of inverted arches are produced by the light which passes through horizontal crystals, at different azimuths.

Mr. Frank E. Lott, at Burton-on-Trent, observed a third parhelia on the part of the first halo vertically above the sun, whilst Mr. H. G. Williams, of Caterham, observing the phenomena about 4 p.m., noted that the sun appeared about 10° above the horizon. So far, the observations of two or three parhelia with two halos and two inverted arches agree with many former descriptions. In the diagram appended, however, and in the majority of sketches received, the inverted arch is not given as the arc of a circle, but hyperbolic.

Mr. A. J. Butler, observing at Walton-on-Thames, remarks: "The hyperbolic band above the sun was carefully noted;" and Mr. C. A. Carus-Wilson, in the following observation made at Staines, supports this view:—

"The sun was just setting behind a bank of hazy mist, appearing as a crimson disk enveloped in blue grey cloud; I first noticed a distinct bow, of light grey tint, and coloured for a short distance at its left extremity with the ordinary rainbow tints—red inside. There then appeared a part of a second bow outside the other, coloured throughout the whole length visible—red inside. From the sun vertically upwards to the first bow, there was a band of white light, quite distinct from the light grey tint of the lower bow, and above the lower bow this band continued as a hyperbolic brush of white light: this brush was much brighter and better defined than the vertical band. A hasty measurement, with a pocket sextant, of the angular radii of the two bows, gave 46° and 23° for the outside and inside bows respectively."

Mr. H. W. Pyddoke also remarks:—"The most noticeable thing of all was the shape of the upper bow, which was like a hyberbole except at its ends where it bent round again very slightly;" and other correspondents concur in this description of the shape of the first inverted arch.

From the descriptions and figures given it is evident that the two parhelia on the parhelic circle are the respective centres of halos similar to the first halo concentric with the real sun; the intersection of these two circles with that surrounding the sun gives the appearance of a hyperbolic curve at the top of it. An exactly similar appearance was drawn by Pastorff as occurring on December 29, 1789, and is found in his "Beobachtungen der Sonnenflecke"; and *L'Astronomie* for August 1889 contains a drawing and description of a very similar appearance.

Lunar halos followed the solar halos on the 29th ult., and on the following day Mr. G. B. Buckton, F.R.S., observed three fine parhelia and a halo at Haslemere, and describes them as follows:—

"The sun shone brightly, but through a moderate haze. On the right and on the left, at equal altitudes with the sun, an oblong bright patch of light appeared. That on the left was the brightest, and formed a blurred image of the sun with all the prismatic colours of the rainbow, but the colours were reversed in order. The upper and lower parts of these mock suns were drawn out, and formed portions of a large circle of about (by eye estimate) 20° radius. These images were connected with the haze, but a lower stratum of finely striated cloud came between the eye and these patches. Immediately above the true sun a third patch of light occurred, through which a portion of an inverted circle was seen, the greater part of which was lost in the blue of the sky above. The right-hand mock sun was fainter than the other, on account of the grey haze being more dense."

Mr. Buckton's observation is a demonstration of the principle laid down—namely, that parhelia always appear at the same elevation as the true sun, and are united to each other by a white horizontal circle, whose pole is the zenith. This circle changes in elevation with the true sun; and the apparent semidiameter is always equal to the distance of the luminary from the zenith.

Mr. Nagel, of Trinity College, Oxford, notes that:—"The solar halos on the afternoon of January 29 were very clearly seen in Oxford; the tangential arc to the outer halo was extremely brilliant, and the two mock suns at the extremities of the horizontal diameter of the inner halo were well marked. During part of the time the halos lasted, a whitish incomplete circle was seen about 80° from the sun; and consequently beyond the zenith. This circle seemed to correspond to that first described by Helvelius in 1661."

It is evident from the descriptions given that the parhelia are not, as is sometimes supposed, images of the real sun at all, but only the junctions of two of the circles formed. The upper and the lower parts of these mock suns were drawn out and connected with the first halo, whilst their sides were observed to be drawn out and to merge into the parhelic circle.

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE forty-third annual general meeting of the Institution of Mechanical Engineers took place on January 29, 30, and 31, in the theatre of the Institution of Civil Engineers.

The papers down for reading and discussion were as follows: on the compounding of locomotives burning petroleum refuse in Russia, by Mr. Thomas Urquhart, Locomotive Superintendent, Grazi and Tsaritsin Railway, South-East Russia; on the burning of colonial coal in the locomotives on the Cape Government railways, by Mr. Michael Stephens, Locomotive Superintendent; and on the mechanical appliances employed in the manufacture and storage of oxygen, by Mr. Kenneth S. Murray, of London. The latter paper was communicated through Mr. Henry Chapman.

Mr. Urquhart's paper is one of a series of excellent and thoroughly useful descriptions of work done by that gentleman on his railway, and had been for some time promised to the Institution. In order to satisfy himself as to the utility and saving of fuel in compound locomotives, he obtained the sanction of the Government for altering one locomotive by way of experiment. The altered engine was put to work, and the driver was allowed over a month's running to get fully acquainted with the handling in regular service. Comparative trials were then made of the compound against a non-compound locomotive with the same weight of train, on the same days, so as to expose them both to the same circumstances in regard to weather. It was clearly proved that the compound burnt 22 per cent. less of the petroleum refuse used as fuel than the non-compound engine, and the author's experience has left no doubt in his own mind that compound locomotives are the engines of the future in all countries. Mr. Urquhart's results are thoroughly borne out by those obtained in this country by Messrs. Worsdell and Webb. Some engineers suppose that this great economy in fuel is due to the higher working steam pressure, and therefore greater expansion in the compound engines as compared with the non-compound engines.

The paper by Mr. Michael Stephens is a description of the South African coal-fields, their discovery, and general working within the last sixteen years. It appears from the paper that the local coal cannot be burned to advantage without a special arrangement of fire-bars—as may be well imagined, since it contains nearly 30 per cent. of incombustible matter.

Mr. Kenneth S. Murray gives an interesting account of the commercial preparation of oxygen from the atmosphere by means of the alternate heating and cooling of the monoxide of barium. About thirty years ago the eminent French chemist Boussingault made the discovery that, at a temperature of about 1000° F., the monoxide of barium would absorb oxygen readily from the atmosphere, with the resulting formation of the dioxide;

and that at a higher temperature of about 1700° F. the oxygen thus absorbed would be given off again, and the monoxide would apparently be restored to its original condition. The paper clearly describes the machinery required for the manufacture of oxygen by means of barium oxide.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The lecture lists for this term include the following courses:—Prof. Clifton, Magnetism; Mr. Baynes, Thermodynamics; Prof. Odling, Diacidic Olefine Acids; Mr. Veley, Physical Chemistry. Prof. Burdon-Sanderson has resumed his lectures, and Mr. Gotch is treating of the Physiology of Muscle. Dr. Tylor lectures on the Development of Religions.

An open Fellowship in Mathematics at Christ Church has been awarded to Mr. C. H. Thompson, Queen's College, Lecturer in Mathematics at Lampeter. No other mathematical Fellowship has been awarded for about seven years.

The arrangement of the Pitt-Rivers anthropological collection at the Museum is proceeding as rapidly as the constant acquisition of new material allows, and a large portion of the collection is now open for public inspection.

CAMBRIDGE.—At the next meeting of the Cambridge Philosophical Society, on Monday, February 10, the following papers will be read:—

(1) W. Bateson (St. John's), on the perceptions and modes of feeding of fishes.

(2) A. C. Seward (St. John's), notes on Lomatophloios.

(3) S. F. Harmer (King's), on the origin of the embryos in the ovicells of Cyclostomatous Polyzoa.

Prof. Stuart has communicated to the Vice-Chancellor his intention of resigning the Chair of Mechanism and Applied Science before the end of the current academical year.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xii., No. 2 (Baltimore, January 1890).—The number opens with the concluding part of Mr. Forsyth's paper on "Systems of Ternariants that are Algebraically Complete" (pp. 115–160). It is illustrated with numerous tables and closed with a useful abstract of contents.—In the following memoir (pp. 161–190), by Prof. Franklin, on "Some Applications of Circular Co-ordinates," the author investigates, with the aid of these co-ordinates, some interesting theorems relating to the orientation of systems of lines given in a recent volume (vol. x. p. 258) by M. Humbert. Several further illustrations are given, and the memoir closes with a discussion of the curve given by the equation $\sin x dx = \sin y dy$.—Mr. F. N. Cole writes (pp. 191–212) on "Rotations in Space of Four Dimensions." The present article is preliminary to a second paper on groups of rotations in four-dimensional space which is to follow.

Bulletins de la Société d'Anthropologie, tome xii., série iii., fasc. 3 (Paris, 1889).—Continuation of M. Dumont's paper on the natality of Paimpol, in which he treats at great length of the causes which influence the ratio of marriages contracted in every hundred of the population in the maritime districts of Brittany, and of the number of children born in each family. In both these respects the means rank amongst the lowest for all France. One cause for this may be the preponderance of women over men, a large number of the latter being engaged as seamen, or taking part in the Iceland and other distant fisheries. Another factor in this problem is probably the subdivision of property among all the members of a family, who in the peasant and small burgher classes, not uncommonly remain together all their lives, and avoid marriage in the fear of diminishing their individual shares of the patrimony. This, coupled with the repugnance, so common among the French peasantry, against large families, leads indirectly to late marriages or to celibacy, and has thus exercised a baneful influence on the normal increase of the population.—An essay on the classification of human races, based entirely on physical characters, by M. Denniker. Believing in the long persistence of types in spite of the constant intermixture of races, the author thinks that it is only by a careful study of the typical characteristics in a so-called ethnic group that we can arrive at any correct idea of the affinities between different races. In an elaborate synoptical table he enumerates the thirteen races which he proposes for his classification, adding separate remarks on the varieties of each.—The dog, by M. G. de Mortillet. Assuming from negative evidence the non-existence of the dog in the Quaternary age, the author traces his presence onwards from the Kjökkenmöddings, in which abundant remains of this animal are to be found. Passing from the prehistoric ages in Europe he considers at length the evidence that can be advanced of the existence of several varieties of the dog among the Egyptians, and later on among the ancient Greeks and Romans; and in the fact of the innumerable varieties of *Canis domesticus*, M. de Mortillet believes we have one of the most conclusive proofs of evolution.—Observations on the skeletons of two young orangs, by M. Hervé.—Pre-Columbian ethnography of Venezuela, by Dr. Marcano. The most interesting report in this treatise is that referring to the Grotto de Cerro de Luna, owing to the almost absolute certainly that it had never been entered since Guiana was first visited by white men. Here Dr. Marcano recovered fifty-two male, and forty-three female skulls, with five of children, together with numerous long bones. Among these skulls many were painted red, and others had obviously been embalmed. The general mean of their cephalic index was 79, while the facial characters were mesorrhinic and prognathic.—On correlative variations in the biceps, by M. G. Hervé.—A report of the Seventh Conference on Transformism, by M. M. Duval. The author here gives an interesting biographical notice of the great French *savant* Lamarck, entering at the same time fully into the character and scope of his researches, and showing how far his views differed from, or approximated to, those of Darwin. As a *résumé* of what Lamarck attempted on the same lines of inquiry so successfully followed by Darwin, M. Duval's report presents much interest for the English reader.—On the menhirs of Morbihan, by M. Gaillard.—On the discovery of Robenhausian flint implements near Macon, by M. Lafay.—Comparison of three sub-species of man, by M. Lombard.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 23.—"On a Photographic Method for determining Variability in Stars." By Isaac Roberts, F.R.A.S. Communicated by Prof. J. Norman Lockyer, F.R.S.

Some of the uncertainties which necessarily attend the determination of variability in the brightness of stars by eye observations are removed by the application of photographic methods, and particularly by that of giving two or more exposures of the same photographic plate to a given sky space, with intervals of days or weeks between each exposure.

In this way any errors caused by atmospheric, actinic, or chemical changes, together with those due to personal bias, are eliminated, and the study of stellar variability can be pursued under conditions that admit of the necessary exactitude.

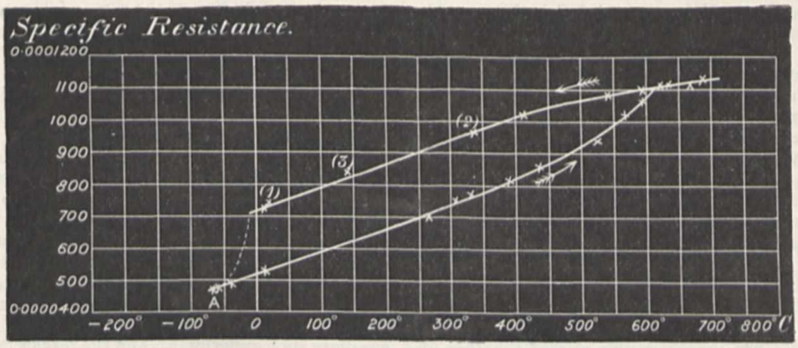
As an illustration of the applicability of this dual photographic method, the enlargement on paper from the negative is now submitted. It shows the results obtained by two exposures of the same plate to the sky in the region of the great nebula in Orion. The first exposure was of two hours' duration on January 29, and the second of two and a half hours on February 3, 1889. The stellar images formed during the two exposures are 0·0122 of an inch apart, measured from centre to centre, and therefore comparable with each other in the field of a microscope. When the images are examined in the manner thus indicated, and their diameters also measured by means of a suitably made eye-piece micrometer, it is found that at least ten of the photographed stars, the magnitudes of which are estimated to range between the 7th and 15th, have changed to a considerable extent in the short interval of five days.

The ten stars referred to are to be found within an area of less than two square degrees of the sky, and in the table given are the co-ordinates of their positions with reference to θ Orionis. The measurements of the diameters of their photo-images on a scale of 0·00002 of an inch are also given.

"Physical Properties of Nickel Steel." By J. Hopkinson, D.Sc., F.R.S.

Mr. Riley, of the Steel Company of Scotland, has kindly sent me samples of wire drawn from the material concerning the magnetic properties of which I recently made a communication to the Royal Society.[1] As already stated, this material contains 25 per cent. of nickel and about 74 per cent. of iron, and over a range of temperature from something below freezing to 580° C. it can exist in two states, magnetic and non-magnetic.

The wire as sent to me was magnetizable as tested by means of a magnet in the ordinary way. On heating it to a dull redness, it became non-magnetizable, whether it was cooled slowly or exceedingly rapidly by plunging it into water. A quantity of the wire was brought into the non-magnetizable state by heating it, and allowing it to cool. The electric resistance of a portion of this wire, about 5 metres in length, was ascertained in terms of the temperature; it was first of all tried at the ordinary temperature, and at temperatures up to 340° C. The specific resistances at these temperatures are indicated in the curve by the numbers 1, 2, 3. The wire was then cooled by means of solid carbonic acid; the supposed course of change of resistance is indicated by the dotted line on the curve; the actual observations of resistance, however, are indicated by the crosses in the neighbourhood of the letter A on the curve. The wire was then allowed to return to the temperature of the room, and was subsequently heated, the actual observations being shown by crosses on the lower branch of the curve; the heating was continued to a temperature of 680° C., and the metal was then allowed to cool, the actual observations being still shown by crosses. From this curve, it will be seen that in the two states of the metal, magnetizable and non-magnetizable, the resistances at ordinary temperatures are quite different. The specific resistance in the magnetizable condition is about 0·000052, in the non-magnetizable condition it is about 0·000072. The curve of resistance in terms of the temperature of the material in the magnetizable condition has a close resemblance to that of soft iron, excepting that the coefficient of variation is much smaller, as, indeed, one would expect it to be in the case of an alloy; at 20° C. the coefficient is about 0·00132, just below 600° C. it is about 0·0040, and above 600° it has fallen to a value less than that which it had at 20° C. The change in electrical resistance effected by cooling is almost as remarkable as the change in the magnetic properties.



Samples of the wire were next tested in Prof. Kennedy's laboratory for mechanical strength. Five samples of the wire were taken which had been heated and were in the non-magnetizable state, and five which had been cooled and were in the magnetizable state. There was a marked difference in the hardness of these two samples; the non-magnetizable was extremely soft, and the magnetizable tolerably hard. Of the five non-magnetizable samples the highest breaking stress was 50·52 tons per square inch, the lowest 48·75; the greatest extension was 33·3 per cent., the lowest 30 per cent. Of the magnetizable samples, the highest breaking stress was 88·12 tons per square inch, the lowest was 85·76; the highest extension was 8·33, the lowest 6·70. The broken fragments, both of the wire which had originally been magnetizable and that which had been non-magnetizable, were now found to be magnetizable. If this material could be produced at a lower cost, these facts would have a very important bearing. As a mild steel the non-magnetizable material is very fine, having so high a breaking stress for so great an elongation at rupture. Suppose it were used for any purpose for which a mild steel is suitable on account of this considerable elongation at rupture, if exposed to a sharp frost its properties would be completely changed—it would become essentially a hard steel, and it would remain a hard steel until it had been heated to a temperature of about 600° C.

Geological Society, January 22.—W. T. Blanford, F.R.S., President, in the chair.—The following communication was read:—On the crystalline schists and their relation to the Mesozoic rocks in the Lepontine Alps, by Prof. T. G. Bonney, F.R.S. In the debate upon the paper on two traverses of the crystalline rocks of the Alps (read December 5, 1888) it was stated that rocks had been asserted on good authority to exist in the Lepontine Alps, which contained Mesozoic fossils, together with garnets, staurolites, &c., and thus were undistinguishable from crystalline schists regarded by the author as belonging to the presumably Archæan *massifs* of that mountain-chain. In reply the author stated that he regarded this as a challenge to demonstrate the soundness or unsoundness of the hypothesis to which he had committed himself. The present paper gives the result of his investigations, undertaken in the month of July 1889, in company with Mr. James Eccles, to whom the author is deeply indebted for invaluable help. The paper deals with the following subjects:—(1) *The Andermatt Section.* By the geologists aforesaid, a highly crystalline white marble which occurs on the northern side of the Urserenthal trough, at and above Altkirch, near Andermatt, is referred to the Jurassic series (members of which undoubtedly occur at no great distance, almost on the same line of strike). The author describes the relation of the marble to an adjacent black schistose slate, and discusses the significance of some markings in the former which might readily be considered as organic, but to which he assigns a different origin. He shows that there are most serious difficulties in regarding these two rocks as members of the same series, and explains the apparent sequence as the result of a sharp and probably broken infold, as in the case of the admitted band of Carboniferous rock at Andermatt itself. That the section is a difficult one on any hypothesis the author admits, but in regard to the former of these, after a discussion of the evidence, he concludes, "that tendered on the spot demands a verdict of 'not proven'—that obtainable in other parts of the Alps, will compel us to add, 'not provable.' (2) *The Schists of the Val Piora.* These schists, already noticed by the author in his Presidential address to the Society in 1886, occur in force near the Lago di Ritom, and consist of two groups—the one dark mica-schists, sometimes containing conspicuous black garnets, banded with quartzites, the other various calc-mica schists; between them, apparently not very persistent, occurs a schist containing rather large staurolites or kyanites. On the north side is a prolongation of the garnet-actinolite (Tremola-) schists of the St. Gothard and then gneiss, on the south side gneiss. There is also some rauchwacké. This rock, at first sight, appears to underlie the Piora schists, and thus to be the lowest member of a trough. If so, as it is admittedly about Triassic in age, the Piora schists would be Mesozoic. The author shows that (1) the latter rocks do not form a simple fold; (2) they are, beyond all question, altered sediments; (3) they have often been greatly crushed subsequent to mineralization; (4) the garnets, staurolites, &c. (if not injured by subsequent crushing) are well developed and characteristic, and are authigenous minerals. (3) *The Rauchwacké and its Relation to the Schist.* (*a*) *The Val Piora Sections:* The author shows that the rauchwacké, which

[1] See Address to the Institution of Electrical Engineers (NATURE, January 23, p. 274).

at first sight seems to underlie the dark mica-schist, is inconstant in position (on the assumption of a stratigraphical sequence); that its crystalline condition does not resemble that of the schist-series, but is rather such as is common in a rock of its age; that it contains mica and other minerals of derivative origin, and in places rock-fragments which precisely resemble members of the Piora schist series. (*b*) *The Val Canaria Section:* This section, described by Dr. Grubenmann, is discussed at length. It is shown that the idea of a simple trough is not tenable, for identical schists occur above and below the rauchwacké; that there is evidence of great pressure, which, however, acted subsequently to the mineralization of the schists; and that in one place the rauchewacké is full of fragments of the very schists which are supposed to overlie it. (*c*) *Nufenen Pass, &c.:* Other cases, further to the west, are described, where confirmatory evidence is obtained as to great difference in age between the rauchewacké and the schists, and the antiquity of the latter. The apparent interstratification is explained by thrust-faulting. (4) *The Jurassic Rocks, containing Fossils and Minerals.* The author describes the section on the Alp Vitgira, Scopi, and the Nufenen Pass. Here indubitable Belemnites and fragments of Crinoids occur in a dark, schistose, somewhat micaceous rock, which is often very full of "knots" and "prisms" of rather ill-defined external form, something like rounded garnets and ill-developed staurolites. These rocks at the Alp Vitgira appear to overlie, and in the field can be distinguished from the black garnet schists. In one place the rock resembles a compressed breccia, and among the constituent fragments is a rock very like a crushed variety of the black-garnet mica-schist. These Jurassic "schists" are totally different from the last-named schists, to which they often present considerable superficial resemblance; for instance, their matrix is highly calcareous, the other rock mainly consisting of silicates. Some of the associated mica may be authigenous, but the author believes much of it and other small constituents to be derivative. There is, however, a mineral resembling a mica, exhibiting twinning with (?) simultaneous extinction, which is authigenous. The knots are merely matrix clotted together by some undefinable silicate, and under the microscope have no resemblance to the "black garnets." The prisms are much the same, but slightly better defined; they present no resemblance to the staurolites, but may be couseranite, or a mineral allied to dipyre. Hence, though there is rather more alteration in these rocks than is usual with members of the Mesozoic series, and an interesting group of minerals is produced, these so-called schists differ about as widely as possible from the crystalline schists of the Alps, and do not affect the arguments in favour of the antiquity of the latter. In short, they may be compared to rather poor forgeries of genuine antiques. Incidentally the author's observations indicate (as he has already noticed) that a cleavage-foliation had been produced in some of the Alpine schists anterior to Triassic times. After the reading of this paper, Dr. Geikie stated that he had sent to Prof. Heim an abstract of the paper read by Prof. Bonney to the British Association at Newcastle, and Dr. Heim had favoured him with a *résumé* of his views on the subject of the present discussion. Having read a translation of this *résumé*, Dr. Geikie complimented the author on his courage in returning to this difficult ground, but, notwithstanding the arguments so skilfully brought forward that evening, he was not convinced of an error on the part of the Swiss geologists. Even the author's own sections gave some countenance to their views, since the dark garnetiferous schists might quite well be part of the same series as the Belemnite-schists. In metamorphic regions there must be some line, on one side of which fossils are recognizable, on the other not so. In the Alps, as Heim and his associates contend, the Belemnite-schists, which have become markedly crystalline, may be less altered portions of masses from which all trace of fossils has been generally obliterated. Remarks were also made by Mr. Eccles, Mr. Teall, Dr. Irving, Prof. Hughes, the Rev. E. Hill, and Prof. Bonney.

Entomological Society, January 15.—Fifty-seventh Annual Meeting.—The Right Hon. Lord Walsingham, F.R.S., President, in the chair.—An abstract of the Treasurer's accounts, showing that the finances of the Society were in a thoroughly satisfactory condition, was read by Dr. Sharp, one of the Auditors, and the Report of the Council was read by Mr. H. Goss. It appeared therefrom that the Society had lost during the year several Fellows by death and had elected 24 new Fellows; that the volume of Transactions for the year extended to nearly 600 pages, and comprised 23 memoirs, contributed by 20 authors and illustrated by 17 plates; and that the sale of the Society's Transactions and other publications is largely on the increase. It was then announced that the following gentlemen had been elected as Officers and Council for 1890:—President, The Right Hon. Lord Walsingham, F.R.S.; Treasurer, Mr. Edward Saunders; Secretaries, Mr. Herbert Goss and the Rev. Canon Fowler; Librarian, Mr. Ferdinand Grut; and as other Members of Council, Mr. J. W. Dunning, Captain H. J. Elwes, Mr. F. DuCane-Godman, F.R.S., Dr. P. B. Mason, Prof. R. Meldola, F.R.S., Mr. R. South, Mr. Henry T. Stainton, F.R.S., and Mr. Roland Trimen, F.R.S. Lord Walsingham nominated Mr. J. W. Dunning, Captain Elwes and Mr. F. DuCane-Godman, Vice-Presidents for the Session 1890-91, and he then delivered an address. After remarking on the attractive beauty of some of the larger diurnal Lepidoptera, and the brilliant metallic colouring of certain species of Coleoptera, the influence that such magnificent examples of the wealth of design in Nature might have upon artistic taste, and the consequent refinement and increased enjoyment of life, Lord Walsingham referred, in illustration of the practical usefulness of entomological studies, to the successful importation into California of the Australian parasites infesting the scale insect (*Icerya purchasi*), which had proved so noxious to the orange plantations. Through the efforts of Prof. Riley, upwards of 10,000 parasites had been distributed and had since spread very widely, so that in many localities the orange and other trees hitherto thickly infested with this noxious insect had been practically cleared of it by their aid. He also referred to the successful fertilization of red clover in New Zealand by the importation of impregnated queens of the common humble-bee, and to the uses to which the silk produced by various exotic species of Bombycidæ had now been successfully applied. Reference was then made to the investigation instituted by Mr. Francis Galton, F.R.S., and to the experiments of Mr. F. Merrifield, with the view of determining the percentage of hereditary transmission to successive offspring by different generations of successors, and to the valuable auxiliary such experiments and the researches of Prof. Weismann, Mr. Poulton, F.R.S., and others might prove to the study of the laws of heredity, protective resemblance, and natural selection. It was then observed that even if the study of entomology could claim to have conferred no greater benefits upon the human race than to have afforded to many members of our urban population an inducement to improve their minds and recreate their bodies, it would have contributed in no small degree to the sum of human health, happiness, and morality; in connection with these remarks he quoted the words of the Abbé Umhang in the obituary notice of Henri de Peyerimhoff, "J'ai connu plus d'un jeune homme qui s'est passionné pour une branche de l'histoire naturelle, et je n'en ai vu aucun s'écarter du chemin de la vertu et de l'honneur." Attention was then drawn to the enormous numbers of species of Insecta as compared with the numbers of species of other orders of the animal kingdom, and an approximate estimate was made of the extent of the field of entomology, and of its relation to other branches of biological study. In connection with the subject of the principal works in entomology continued or completed during the year, special mention was made of the "Biologia Centrali Americana," by Messrs. Godman and Salvin, and the "Revisio Insectorum Familiæ Mantidarum," by Prof. Westwood. In conclusion, Lord Walsingham referred to the losses by death during the past year of several Fellows of the Society and other entomologists, mention being made of Mr. F. Bond, Dr. Signoret, Mons. Puls, Colonel C. J. Cox, Pastor Holmgren, Dr. Franz Löw, Dr. Karl Venus, and the Rev. J. G. Wood. Votes of thanks having been passed to the President, Secretaries, and Librarian, Lord Walsingham, Mr. H. Goss, Canon Fowler, and Mr. Grut replied.

Linnean Society, January 16.—Mr. J. G. Baker, F.R.S., Vice-President, in the chair.—Mr. Clement Reid exhibited and made some remarks upon a collection of fruit of *Trapa natans*, from the Cromer Forest bed at Mundesley.—Mr. J. G. Baker exhibited and described a collection of cryptogamic plants from New Guinea, upon which Mr. A. W. Bennett and Captain Elwes made some critical remarks.—In the absence of the author, Mr. A. Barclay, a paper was read by Mr. B. D. Jackson on the life-history of a remarkable Uredine on *Jasminum grandiflora*. A discussion ensued in which Mr. A. W. Bennett and Prof. Marshall Ward took part.—This was followed by a paper from Mr. Edward E. Prince, on certain protective provisions in some larval British Teleosteans.

Royal Microscopical Society, January 8.—Rev. Dr. Dallinger, F.R.S., Vice-President, in the chair.—Mr. T. F. Smith exhibited to the meeting, by means of the oxyhydrogen lantern, a series of photomicrographs of various diatoms taken with Zeiss's apochromatic objectives and projection eye-pieces, giving powers of 1000 to 7500 diameters. At the conclusion of the exhibition Mr. Smith presented the series of slides—52 in number—to the Society for future use and reference.—Mr. T. C. White exhibited a specimen of a parasite found in the cockroaches which infest sugar-ships; also a slide containing bacilli in large numbers from a urinary deposit.—A paper by Dr. R. L. Maddox, on a small glass rod illuminator, was read.—Owing to the lateness of the hour, the reading of papers by Mr. Michael and Dr. Czapski was postponed until the March meeting.

Chemical Society, January 16.—Dr. W. J. Russell, F.R.S., in the chair.—The following papers were read:—A new method of estimating the oxygen dissolved in water, by Dr. J. C. Thresh. The process is based on the fact that whereas, in the absence of oxygen, nitrous acid and hydrogen iodide interact, forming iodine, water, and nitric oxide, in the presence of oxygen the nitric oxide becomes re-oxidized, and, serving as a carrier of the oxygen, brings about an additional separation of iodine, equivalent in amount to the oxygen present; hence, deducting the amount of iodine liberated by the nitrous acid and by the oxygen dissolved in the solutions used from the total amount, the difference will be that corresponding to the oxygen dissolved in the water examined. The apparatus required is a very simple one, the analytical operations are conducted in an atmosphere of coal gas, and the results in the case of freshly distilled water agree closely with those recently published by Sir H. E. Roscoe and Mr. Lunt (Chem. Soc. Trans., 1889, 552).—Note on a milk of abnormal quality, by Mr. F. J. Lloyd. The author gave the results of an examination of the milk of two cross-bred shorthorns, and called attention to the abnormally low proportion of solid constituents other than fat.—The sulphates of antimony, by Mr. R. H. Adie.

Zoological Society, January 14.—Prof. A. Newton, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's menagerie during the month of December 1889.—Mr. Sclater exhibited and made remarks on a specimen of a very singular duck from North-East Asia, apparently referable to the genus *Tadorna*, sent to him for determination by Dr. Lütken, of Copenhagen. After a careful examination Mr. Sclater was inclined to think that it was probably a hybrid between *Tadorna casarca* and *Querquedula falcata*.—Mr. Sclater exhibited and made remarks on a set of small birds' bones obtained from beneath some deposits of nitrate in Southern Peru, transmitted to the Society by Prof. W. Nation.—Mr. David Wilson-Barker exhibited and made remarks on some specimens of Teredos taken from submarine telegraphic cables off the Brazilian coast.—Prof. F. Jeffrey Bell exhibited and made remarks on some living specimens of *Bipalium*, transmitted to the Society by the Rev. G. H. R. Fisk, of Capetown.—A communication was read from Mr. R. Lydekker, containing an account of a new species of extinct otter from the Lower Pliocene of Eppelsheim. The author described part of the lower jaw, which he had previously referred to *Lutra dubia*, from the deposits indicated. Having, however, now seen a cast of the type of the latter, he found that the present specimen indicated a distinct species, for which the name *L. hessica* was proposed.—A communication was read from Prof. Bertram C. A. Windle and Mr. John Humphreys, on some cranial and dental characters of the domestic dog. The paper was based on the results of the measurements of a large number of dogs' skulls of various breeds. Its object was to ascertain whether cranial and dental characteristics afforded sufficient information to permit of a scientific classification of the breeds, or would throw any light upon their origin. The conclusion so far arrived at was that interbreeding had been so extensive and complicated as to make it impossible to distinguish the various forms scientifically from the characters examined. Several points with regard to the shape of head and palate and the occasional occurrence of an extra molar were also touched upon.—Mr. G. A. Boulenger read the fourth of his series of contributions to the herpetology of the Solomon Islands. The present memoir gave an account of the last collection brought home by Mr. C. M. Woodford. Besides known species, this collection contained examples of a new snake, proposed to be called *Hoplocephalus elapoides*.—A second paper by Mr. Boulenger contained a list of the reptiles, batrachians, and freshwater fishes collected by Prof. Moesch and Mr. Iversen in the districts of Delhi and Langkat, in North-Eastern Sumatra.—Dr. Günther, F.R.S., read a paper entitled "A Contribution to our Knowledge of British Pleuronectidæ." The author described the true *Arnoglossus grohmanni*, a Mediterranean scald-fish, recently discovered by the Rev. W. S. Green on the Irish coast, and quite distinct from *Arnoglossus lophotes*. Dr. Günther also stated that the Mediterranean lemon-sole (*Solea lascaris*) was specifically identical with the British species (*Solea aurantiaca*), but was distinct from that of the Canary Islands and Madeira (*Solea scriba*); and gave it as his opinion that the Mediterranean *Solea lutea* and British *Solea minuta* cannot be separated by any constant character.

EDINBURGH.

Royal Society, January 6.—Lord Maclaren, Vice-President, in the chair.—Bailie Russell read an obituary notice of the late Sir James Falshaw, Bart.—Prof. Tait read a paper on the effect of friction on vortex-motion.—Dr. A. Bruce described a connection (hitherto undescribed) of the inferior olivary body of the medulla oblongata, which has a function in the maintenance of equilibrium of the body.—Dr. W. H. Perkin read a paper on the internal condensation of some diketones.—A photograph of a group of sun-spots and of the surface of the sun was presented by Mr. James Naismith. The photograph was from a drawing made in 1864.

PARIS.

Academy of Sciences, January 27.—M. Hermite in the chair.—On clasmatocytes, by M. L. Ranvier. The author gives this name (from κλάσμα, fragment, and κύτος, cell) to certain elements which are easily detected under the microscope in the thin connective membranes of the vertebrates when they are prepared by a process here described. They are not migratory cells, but have their origin in the leucocytes, or lymphatic cells, though it is not to be supposed that all leucocytes develop into clasmatocytes.—On the theorem of Euler in the theory of polyhedrons, by M. de Jonquières. The paper deals with Lhuilier's objection, accepted by Gergonne, against the generalization of Euler's formula, which is shown to be applicable to all polyhedrons, whether convex or not. It is further placed beyond doubt that Euler not only enounced, but gave a full demonstration of the formula in question.—On the roots of an algebraic equation, by Prof. A. Cayley. Assuming $\int(u)$ to be a rational and integral function, with real or imaginary coefficients, of the n order; and supposing that the equation $\int'(u) = 0$, of the order $n - 1$, has $n - 1$ roots, then it is shown that the equation $\int(u) = 0$ will have n roots. The demonstration rests on the same principles as those of Gauss and Cauchy.—Researches on the cultivation of the potato, by M. Aimé Girard. The author communicates the results of his experiments, continued for three years at the Ferme de la Faisanderie, Joinville-le-Pont, with the variety of the potato known as Richter's Imperator, which is shown to yield a far larger crop of starch-bearing tubers than any other variety cultivated in France. The paper was supplemented by some remarks by M. P. P. Dehérain, who stated that his own experiments fully confirmed those of M. Girard. There could be no doubt as to the great superiority of Richter's Imperator, especially as a starch-producing tuber.—Remarks on the *Annuaire du Bureau des Longitudes* for 1890, by M. Faye. In presenting a copy of this valuable annual for 1890, M. Faye remarked that the astronomic section of the work became more important every year. The present volume contains a table of the planetary phenomena, the most accurate available data for the variable stars, a catalogue of the chief stars whose magnitudes correspond to Pickering's photometric scale, papers on the use of the aneroid barometer, on the elasticity of solids and the neutral temperature of thermo-electric couples, together with the magnetic elements for France and its seaports on January 1, 1890, and at various Mediterranean stations for 1887.—On the simply rational transformations of algebraic surfaces, by M. Paul Painlevé. In this paper the author extends to the transformations in question M. Picard's method relative to the birational transformations of algebraic surfaces.—On the substitution of the salts in mixed solutions, by M. A. Etard. In his previous researches the author determined the lines of complete solubility for a mixture of potassium and sodium chlorides, varying the quantity of the *metals* saturated by the same metalloid as a function of the temperature. He studies the reverse case here, determining the results when in a solution of the same metal the *metalloids* are varied.—On the different states of iodine in solution, by MM. Henri Gautier and Georges Charpy. Iodine solutions are usually divided into two classes—brown (alcohol, ether, &c.) and violet (sulphur of carbon, chloroform, benzine, &c.). The molecular weights have been determined by Raoult's method, and results were obtained varying from 330 to 489, according to the solvent; Loeb's results are thus confirmed and amplified.—Calorimetric study of the phosphites and pyro-phosphite of soda, by M. L. Amat. These researches fully confirm the author's previous conclusion that the acid phosphite of soda, $PO_3H.NaH$, may, by the simple process of drying, lose water and become transformed into pyrophosphite of soda, a substance differing in many of its properties from the acid phosphite.—A study of the pneumococcus occurring in the fibrine pneumonia consecutive to *la grippe* (influenza), by MM. G. Sée and F. Bordas. From these clinical researches, made on a large number of patients in the Hôtel-Dieu, the authors conclude that pneumonia is not only a local affection caused by infection, but that it is itself infecting in the sense that it may invade other organs.—Papers were read by M. Chr. Bohr, on pulmonary respiration; by M. Abel Dutartre, on the poison of the land salamander; by M. Ch. Musset, on "selenotropism" (influence of moonlight on plants); by M. A. de Schulten, on the artificial reproduction of malachite all but identical in density, hardness, and crystallization with the natural stone; by M. A. de Grossouvre, on the presence of Alpine fossils in the Callovian formation of the west of France; and by M. Ch. V. Zenger, on the magnetic storms and auroræ boreales of the years 1842–57.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, FEBRUARY 6.

ROYAL SOCIETY, at 4.30.—A New Theory of Colour-blindness and Colour-perception: Dr. Edridge Green.—Memoir on the Symmetrical Functions of the Roots of Systems of Equations: Percy A. MacMahon, Major R.A.

LINNEAN SOCIETY, at 8.—On the Stamens and Setæ of Scirpeæ: C. B. Clarke, F.R.S.—On the Flora of Patagonia: John Ball, F.R.S.

CHEMICAL SOCIETY, at 8.—Ballot for the Election of Fellows.—The Oxides of Nitrogen: Prof. Ramsay, F.R.S.—Studies on the Constitution of Tri-Derivatives of Naphthalene: Dr. Armstrong and W. P. Wynne.—On the Action of Chromium Oxychloride on Nitrobenzole: G. G. Henderson and J. Morrow Campbell.

ROYAL INSTITUTION, at 3.—Sculpture in Relation to the Age: Edwin Roscoe Mullins.

FRIDAY, FEBRUARY 7.

PHYSICAL SOCIETY, at 5.—Annual General Meeting.—On Galvanometers: Prof. W. E. Ayrton, F.R.S., T. Mather, and W. E. Sumpner.—On a Carbon Deposit in a Blake Telephone Transmitter: F. B. Hawes.

GEOLOGISTS' ASSOCIATION, at 7.30.—Annual General Meeting.—Notes on the Nature of the Geological Record: The President.

SOCIETY OF ARTS, at 5.—The Utility of Forests and the Study of Forestry: Dr. Schlich.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Reclamation of Land on the River Tees: Colin P. Fowler.

ROYAL INSTITUTION, at 9.—The London Stage in Elizabeth's Reign: Henry B. Wheatley.

SATURDAY, FEBRUARY 8.

ROYAL BOTANIC SOCIETY, at 3.45.

ROYAL INSTITUTION, at 3.—The Natural History of the Horse, and of its Extinct and Existing Allies: Prof. Flower, C.B., F.R.S.

MONDAY, FEBRUARY 10.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—Search and Travel in the Caucasus; an Account of the Discovery of the Fate of the Party lost in 1888: Douglas W. Freshfield (illustrated by Photographs by Signor V. Sella and H. Woolley).

SOCIETY OF ARTS, at 8.—The Electromagnet: Dr. Silvanus P. Thompson.

TUESDAY, FEBRUARY 11.

ANTHROPOLOGICAL INSTITUTE, at 8.30.—Exhibition of some Skulls, dredged by G. F. Lawrence from the Thames, in the Neighbourhood of Kew: Dr. Garson.—Characteristic Survivals of the Celts in Hampshire: T. W. Shore.

SOCIETY OF ARTS, at 8.—Cast Iron and its Treatment for Artistic Purposes: W. R. Lethaby.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Bars at the Mouths of Tidal Estuaries: W. H. Wheeler.

ROYAL INSTITUTION, at 3.—The Post-Darwinian Period: Prof. G. J. Romanes, F.R.S.

UNIVERSITY COLLEGE BIOLOGICAL SOCIETY, at 5.15.—Some Aberrant Coleoptera: S. V. Tebbs.

WEDNESDAY, FEBRUARY 12.

ROYAL MICROSCOPICAL SOCIETY, at 8.—Annual Meeting.—President's Address.

SOCIETY OF ARTS, at 8.—Modern Improvements in Facilities for Railway Travelling: George Findlay.

THURSDAY, FEBRUARY 13.

ROYAL SOCIETY, at 4.30.

MATHEMATICAL SOCIETY, at 8.—Concerning Semi-invariants: S. Roberts, F.R.S.—Ether-Squirts: Prof. K. Pearson.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

ROYAL INSTITUTION, at 3.—The Three Stages of Shakspeare's Art: Rev. Canon Ainger.

FRIDAY, FEBRUARY 14.

ROYAL ASTRONOMICAL SOCIETY, at 3.—Anniversary Meeting.

ROYAL INSTITUTION, at 9.—Problems in the Physics of an Electric Lamp: Prof. J. A. Fleming.

SATURDAY, FEBRUARY 15.

ROYAL INSTITUTION, at 3.—Electricity and Magnetism: Right Hon. Lord Rayleigh, F.R.S.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Medical Electricity and Massage: H. N. Lawrence (Gill).—A Theory of Lunar Surfacing by Glaciation: S. E. Peal (Thacker).—Einleitung in die chemische Krystallographie: Dr. A. Fock (Leipzig, Engelmann).—Elemente der Paläontologie, 2 Hälfte: Dr. G. Steinmann and Dr. L. Doderlein (Leipzig, Engelmann).—L'Évolution du Système Nerveux: H. Beaunis (Paris, J. B. Baillière).—A Theory of Gravitation: T. Wakelin (Petherick).—The Psychology of Attention: T. Ribot (Chicago, Open Court Publishing Company).—English Intercourse with Siam in the Seventeenth Century: Dr. J. Anderson (K. Paul).—Contributions to the Fauna of Mergui and its Archipelago, 2 vols. (Taylor and Francis).—Report of the Commissioner of Education for the Year 1887–88 (Washington).—The Library Reference Atlas of the World: J. Bartholomew (Macmillan).—Science and Scientists: Rev. J. Gerard (London).—Le Climat de la Belgique en 1889: A. Lancaster (Bruxelles).—Tylar's Practical Hints and Photographic Calendar, 1890 (Tylar, Birmingham).—Results of Astronomical Observations made at the Melbourne Observatory in the Years 1881–84 (Melbourne).—Babbage's Calculating Engines (Spon).—Practical Hints for Electrical Students, vol. i: Kennelly and Wilkinson (*Electrician* Office).—Lehrbuch der Meteorologie: Dr. W. J. Van Bebber (Stuttgart, Enke).—Is the Copernican System of Astronomy True?: W. S. Cassedy (Kittanning, Pa.).—New Zealand for the Emigrant, Invalid, and Tourist: J. M. Moore (S. Low).—Fauna der Gaskohle und der Kalksteine der Permformation Böhmens, Band 2, Heft 4: Dr. Ant. Fritsch (Prag).—The Extermination of the American Bison: W. T. Hornaday (Washington).—Iowa Weather Report, 1878-79-80-82-83-84-85-87 (Des Moines, Iowa).—U.S. Commission of Fish and Fisheries; Part XIV., Report of the Commissioner for 1886 (Washington).—Report on Insect and Fungus Pests, No. 1: H. Tryon (Brisbane, Beal).—La Photographie à la Lumière du Magnésium: Dr. J. M. Eder (Paris, Gauthier-Villars).—Notes upon a Proposed Photographic Survey of Warwickshire: W. J. Harrison (Birmingham).—Chinese Games with Dice: S. Culin (Philadelphia).—Ancient Symbolism among the Chinese: Dr. J. Edkins (Trübner).—Journal of the Royal Statistical Society, December (Stanford).—Charts showing the Normal Monthly Rainfall in the United States (Washington).

CONTENTS.