THURSDAY, JANUARY 24, 1889.

MR. GRANT ALLEN'S NOTIONS ABOUT FORCE AND ENERGY.

Force and Energy: a Theory of Dynamics. By Grant Allen. Pp. 161. (Longmans, Green, and Co., 1888.)

THERE exists a certain class of mind, allied perhaps to the ancient Greek Sophist variety, to which ignorance of a subject offers no sufficient obstacle to the composition of a treatise upon it. It may be rash to suggest that this type of mind is well developed in philosophers of the Spencerian school, though it would be possible to adduce some evidence in support of such a suggestion.

In the volume before us Mr. Grant Allen sets to work to reconstruct the fundamental science of dynamics, an edifice which, since the time of Galileo and Newton, has been standing on what has seemed a fairly secure and substantial basis, but which he seems to think it is now time to demolish in order to make room for a newly excogitated theory. The attempt is audacious, and the result—what might have been expected. The performance lends itself indeed to the most scathing criticism; blunders and misstatements abound on nearly every page, and the whole structure is simply an emanation of mental fog.

Thus much it is necessary to say in order to give an adequate idea of the nature of the book; but, having said as much as this, it is possible to speak quite otherwise of the friendly tone and apparently candid modesty in which the preface has been written. The preface, indeed, almost disarms a critic, or at any rate it causes him to use the blunt end of his lance; and were it not necessary to call attention to the erroneousness of such of the doctrines as are new, and expose their hollowness to a number of unlearned persons who are always eager to see some flaw found in a universally accepted theory the difficulties of which they have never mastered, and which they therefore vainly hope will turn out incorrect, one would gladly accept the apology and explanation of the preface and let the work sink into oblivion unnoticed.

Its pretentious form, however, renders criticism necessary; and indeed criticism is expected by the author, though, as he naïvely confesses, it will not undermine his own opinion of the truth and value of his work.

The book is in two parts: Part I. "Abstract or Analytic"; Part II. "Concrete or Synthetic." The second part consists of more or less popular illustrations of the doctrines inculcated in the first part: it need not therefore much concern us. The first part starts with chapters on "Power," "Force," "Energy"; later on it has three chapters called respectively, "The Persistence of Force," "The Conservation of Energy," "The Indestructibility of Power." These last headings are not very promising; and the performance does not belie the promise.

Chapter II. leads off with the following definition:—

> "A Force is a Power which initiates or accelerates aggregative motion, while it resists or retards separative motion, in two or more particles of ponderable matter (and possibly also of the ethereal medium)."

In other words, the author agrees to limit the term "force" to that which is commonly known as attraction. Very good. Now take Chapter III.:—

> "An Energy is a Power which resists or retards aggregative motion, while it initiates or accelerates separative motion, in two or more particles of ponderable matter, or of the ethereal medium."

In other words, he is going to denote that which is commonly known as repulsion by the name "energy." He by no means always adheres to the limitations imposed by this definition, and frequently he means by the term energy the same as is ordinarily understood by the term, though he does so most happily in cases where the result can be looked upon as a separation of some bodies.

Thus, for instance, the upward motion of a cannon-ball he would style an energy, because it separates the ball from the earth; the strain in a bow he would call an energy, because it can separate the arrow from the bow; heat he would call energy, because it usually expands things; but the *horizontal* motion of a cannon-ball or a railway train he will have a difficulty in calling an energy, and, in fact, is unable to do so without a flagrant mistake of principle in the case of the ball, and some manifest special pleading in the case of the train. Still more unable is he properly to apply the term energy to the *downward* motion of a falling body. We shall see what he makes of this obvious difficulty shortly. Nevertheless, this is the main doctrine in the book—viz. that anything tending towards aggregation is a force, while anything tending to disgregation is an energy.

Passing, then, to the next chapter, "The Species of Force," he subdivides force into attraction between large masses, or gravitation—between molecules, or cohesion—between atoms, or chemical affinity—and, lastly, between electric charges. It may as well be said, once for all, that throughout the book there are many lame and hesitating references to electricity and ether, which are so vague as to be quite harmless, and which it will be the most charitable plan to simply ignore. We had better ignore most of the chemical and molecular statements also, for the same reason, and attend only to those which deal with ordinary lumps of matter. But even here it will not do to criticize the language too closely, or one would have enough to do. This, for instance, is one of the first sentences about gravitation: "When an aërolite comes within the circle of the earth's attraction, it is Gravitation which makes them leap towards one another." "The circle of the earth's attraction" and the sudden "leap" of the aërolite when it comes within this circle, are phrases which scarcely express quite accurately the facts of the case!

The next sentence throws a flood of light upon the state of the writer's mind when he formed his conception of the difference between force and energy, and explains also, I venture to surmise, why his doctrine found so blithe an acceptance with Mr. Edward Clodd, as Mr. Clodd's own book and the preface to this one inform us. It is the orbital energy of the moon which counteracts the aggregative power of gravity. "If the moon were to lose its orbital Energy, Gravitation would pull it to the earth."

This sentence is not indeed untrue, but it is significant as showing that it is the old puzzle of centrifugal force or of radial acceleration that is at the bottom of this whole "new theory." The puzzle was solved completely long ago, in the clearest possible manner, and the "Principia" is the witness to it; but it is still felt to be a difficulty by beginners, and I suppose there is no offence in applying this harmless epithet to both Mr. Grant Allen and Mr. Clodd, so far as the truths of dynamics and physics are concerned.

"Energy" and "Force" are always opposed to one another, and the meaning of the author is well illustrated by the following quotation:—

"Again, when two masses are in a state of aggregation, the Force of gravitation resists any attempt to sever them. If a cannon-ball lies upon the ground, it cannot be raised without an expenditure of Energy, and the amount of the Energy required to lift it to a given height is the measure of the resistance offered by Gravitation."

That is to say, the energy required to lift a body to a given height is a measure of the force of gravitation. This reminds one of some of the sentences in Tyndall's "Heat."

The next chapter, "The Species of Energy," classifies energy as molar, molecular, atomic, and electrical, just as in the case of force. "Of Molar Energies employed in resistance to aggregation, the most familiar instance is that of orbital movement"—moon and earth, &c.

Now we come to the *crux* of how horizontal motion can be an "energy" in the sense defined by Mr. Allen.

"On a smaller scale, the Energy of a bird in flying, or of a cannon-ball fired horizontally, is largely employed in counteracting gravitation"!!

May I inform the author of an elementary fact? A cannon-ball fired horizontally falls just as quickly, and reaches the ground (if flat) in precisely the same time, as if it were merely dropped; its energy of motion has no power of counteracting gravitation. The same is really true of the moon: it falls towards the earth just as much in each second as if it had no orbital velocity; in no case does motion influence the effect produced by a given force. But this is just the fact which, if he had been able to recognize it, would have saved him the trouble of writing this treatise.

I said I would charitably omit reference to the molecular, chemical, and electrical statements, but I cannot resist one quotation from the next page: "Large masses of water before freezing part with their Energy in the visible form of heated mist."

Chapter VII. is headed "The Kinds of Kinesis," by which is meant apparently the varieties of motion. Motion is subdivided, not into rotation and translation, but thus:—

"Motion has three Kinds; . . . it may be separative, or it may be aggregative, or it may be continuous and neutral. Each species of Kinetic Energy has a form of each kind."

The author now finds it necessary to grapple with the difficulty which we guessed he would sooner or later feel, viz. how it is possible to bring falling motion, or motion of bodies towards one another, under his category of "Energies or separative Powers." A body thrown up, and a body thrown horizontally, he has already tackled by simply committing some convenient errors of fact. He surmises that such bodies fall less than they would if dropped, and thus that their energy of motion counteracts gravitation; but the case of a body thrown down is not to be thus managed, so he proceeds to get over the difficulty in three ways. First, by raising a cloud of words; second, by asserting that when a body strikes the earth, although its potential energy of separation has disappeared, yet the heat of its collision separates atoms just as much, and results in the same ultimate amount of separation again (a statement which in no sense can be considered true), so that the motion which intervened between the start and stop of the falling body "we are justified in regarding as essentially a transitory form of separative Power."

"Throughout we see that aggregative Energy" (*i.e.* energy apparently aggregative) "is merely Potential Energy in course of transformation to another form. While the really aggregative Power of Force is causing these bodies to combine, the Energy of their motion represents for a while their original separateness, and is finally transformed into a similar separateness between other bodies."

So, while motion *from* the earth is true kinetic energy, motion *towards* it is a transitory form of potential energy, and represents for a while the original separateness of the bodies!

When a doctrine requires a statement like this to bolster it up, it is wise to take the need as a sure indication that we are somewhere off the track, and had better get back to the turning whence the path which has led us into such a jungle diverged. It is a pity the author did not take the hint thus clearly vouchsafed to him. His difficulties about understanding normal acceleration and the generality of Newton's second law were natural and excusable, though hardly the subject to write a book about; but after encountering and being worsted by this last thicket, it was very unwise to go on plunging madly forward, and exhibit his scratches as signs of victory.

But he has not yet made his last struggle into still more hopeless entanglement. Here is his third attempt at extrication:—

"We see that the Energy of a falling body does not consist in its mere downward movement, but rather in that accelerating motion which is capable of being transformed into heat when the masses aggregate."

If this statement means anything, it means that the kinetic energy of a down-moving body consists, not in its velocity, as is the case with an upward-moving body, but in its acceleration, and that it is this acceleration which ultimately gets turned into heat!

"So the Energy of Kinesis is seen to be a mere transferential mode from one kind of separation to another." "Motion is the redistribution of Separations."

One may at least acknowledge the ingenuity as well as the gallantry with which the author endeavours to get clear of his impenetrable jungle.

In passing, here is a curious definition of friction, given in an explanatory note.

"From the point of view of the Force involved, friction means the cohesion which must be overcome; but from the point of view of the Energy employed, friction means the separative power of heat which overcomes."

Next we come to the extraordinary but fortunately short chapter entitled "The Persistence of Force," which "must be carefully distinguished from the opposite principle of the Conservation of Energy."

From the examples given it appears that the author means by "the persistence of force" the universal prevalence and unalterable character of gravitation. This, if you please, is a principle "opposite" to the "conservation of energy."

It may be asked how on earth the author manages to make out any doctrine of conservation for what he is pleased to style "Energy." It may be asked, but scarcely answered. First, there is a momentous difference of language to be attended to, a difference of which we are told the concrete and practical results are enormous. "While Forces *persist*, Energies are *conserved*." This is a most satisfactory beginning, and makes one feel quite smooth and comfortable. But unfortunately the author is not original in the rest of this chapter, and his illustrations of conservation of energy do not flow from his definition, but are quite common-place. I cannot resist one little extract from this chapter wherein the property of "inertia" has an altogether new light thrown upon it, a light even more brilliant than that just thrown upon "friction."

"Two molecules of water vapour are prevented from aggregating under the relatively feeble attraction of cohesion at a distance, by their inertia—that is, by the relatively strong cohesion of surrounding or intervening matters (just as a mass on a table is prevented from aggregating with the earth by the cohesion of its boards). Two atoms having affinities for one another are similarly prevented from aggregating by inertia. . . . So also two electrical units are prevented from aggregating in the Leyden jar by the electrical neutrality of the glass partition."

Chapter X., "The Indestructibility of Power," asserts that

"the total amount of Power, aggregative or separative, in the Universe is constant, and no Power can ever disappear or be destroyed. This sums up the two preceding generalizations of the Persistence of Force and the Conservation of Energy in one still wider generalization."

Chapter X. is very short.

The chapter on "Liberating Energies" is a dissertation on pulling a trigger, and quite mistakenly supposes that some expenditure of energy is essential to the performance of this act.

"A ball suspended by a thread is released by the separative Energy of a knife or scissors."

The separative Energy of a knife is a good phrase.

"The stronger Force necessarily outweighs the weaker, and as Forces cannot increase or decrease in intensity, the only manner, &c."

Forces cannot increase or decrease in intensity? No, certainly not; this is proved by the existence of the phrase "persistence of force." Well, this is logical at any rate, after the bookish manner of argument, and that is some comfort.

"Electrical Liberating Energies are those which release Electrical Units from the interference of a Force antagonistic to Electrical Affinity."

"They are such as close the circuit of a battery, or bring a discharging tongs to a Leyden jar."

"The usual vagueness of electrical science prevents any definite treatment of these phenomena."

Are we, then, to conclude that the author, in all this treatise, has hit on no germ of truth—nothing but what was well known before, or what is erroneous? I fear that, with the possible exception of the idea of classifying energies by reference to the sizes of the bodies concerned, this must be our conclusion. He has regarded the universe from the point of view of action at a distance, and has been struck with one or two salient features: (1) the universality of gravitation at great distances, and of cohesion at small; (2) the existence of what has been styled "repulsive motion"—that is, a motion which simulates the effects of repulsion, as when two particles or two molecules, rushing together, swing each other round and separate again; or when a heated gas expands. One knows that, in old text-books, heat was often spoken of as a repulsive force. And a sufficient velocity imparted to a satellite *does* keep it clear of the earth.

Struck with these facts, he has proceeded to take gravitative separation as the typical and fundamental form of energy; motion being a form of energy only because it tends to separation (in some cases, at any rate), so that he defines motion, in the last chapter of Part I., as "the redistribution of separations," while energy he defines as "a Power which separates." On the other hand, he has taken gravitation and cohesion as his typical forms of force; and, because these tend to pull bodies together, he has defined force as "a Power which aggregates," and has proceeded to write a treatise on the subject, showing how force and energy are opposed to one another.

The thing which strikes one most forcibly about the physics of these paper philosophers is the extraordinary contempt which, if they are consistent, they must or ought to feel for men of science. If Newton, and Lagrange, and Gauss, and Thomson, to say nothing of smaller men, have muddled away their brains in concocting a scheme of dynamics wherein the very definitions are all wrong; if they have arrived at a law of conservation of energy without knowing what the word energy means, or how to define it; if they have to be set right by an amateur who has devoted a few weeks or months to the subject, and acquired a rude smattering of some of its terms,—what intolerable fools they must all be!

But this does seem the attitude of many literary men, and that must be one reason why they dislike and despise science. If such a view were just or true, dislike and contempt would be the only reasonable attitude.

A scientific man may often feel harassed by being unable to express in literary form what he has to say; but, though this is an evil, it is surely a lesser evil than to have the knack of writing and no matter to write. It is as when the Sophists proceeded to teach rhetoric, heedless of whether either their pupils or themselves possessed any real knowledge about which to be eloquent.

Mr. Grant Allen has apologized for his Mistake in the preface, and one has no quarrel with him. One might, if it were worth while, have a quarrel with a certain class of literary men for the shallow and flippant way in which they occasionally refer to Science; but it is not worth while.

The disciple is not above his master in this respect.

Here is one quotation of many which might have been chosen from Mr. Herbert Spencer:—

"Newton described himself as unable to think that the attraction of one body for another at a distance could be exerted in the absence of an intervening medium. But now let us ask how much the forwarder we are if an intervening medium be assumed. This ether, whose undulations, according to the received hypothesis, constitute heat and light, and which is the vehicle of gravitation,—how is it constituted? We must regard it in the way that physicists do regard it, as composed of atoms which attract and repel each other—infinitesimal, it may be, in comparison with those of ordinary matter, but still atoms. And remembering that this ether is imponderable, we are obliged to conclude that the ratio between the interspaces of these atoms and the atoms themselves is incommensurably greater than the like ratio in ponderable matter; else the densities could not be incommensurable. Instead, then, of a direct action by the sun upon the earth, without anything intervening, we have to conceive the sun's action propagated through a medium whose molecules are probably as small relatively to their interspaces as are the sun and earth compared with the space between them; we have to conceive these infinitesimal molecules acting on each other through absolutely vacant spaces which are immense in comparison with their own dimensions. How is this conception easier than the other? We still have mentally to represent a body as acting where it is not, and in the absence of anything by which its action may be transferred; and what matters it whether this takes place on a large or a small scale?" ("First Principles," chap. iii. § 18).

Omitting any reference to the absurd reasoning about "incommensurable" densities in this quotation, and about the ether being "imponderable," as if Mr. Herbert Spencer or anyone else knows anything whatever on the subject, I wish to call attention to the words regarding the structure of the ether "in the way physicists do regard it"! If they do, if they are unable to see that action at a distance across a small space is just as inexplicable as action at a distance across a large one, and if the ether they imagine is not thought of as in some sense or other a *continuum* for this very reason; if, in fact, they are unable to appreciate, in all the years they have been thinking on the subject, what is obvious on the face of it to someone who steps in, so to speak, for the first time, —what singularly incompetent persons they must be!

That seems to be the real upshot and natural meaning of many of these criticisms of science from the outsider's point of view. O. J. LODGE.

ROCKS AND SOILS.

Rocks and Soils: their Origin, Composition, and Characteristics. By Horace Edward Stockbridge, Ph.D., Professor of Chemistry and Geology in the Imperial College of Agriculture, Sapporo, Japan; Chemist to the Hokkaido Cho. (New York: John Wiley and Sons. London: Trübner. 1888.)

CHEMIST to the Hokkaido Cho! It is not the least striking feature of our time that there should be an Imperial College of Agriculture at Sapporo whose Professors publish researches in New York and London. This is not exactly a novel experience, for events crowd upon us thick and fast in these days; but those of us who can look back forty years must be struck when confronted with the Chemist of the Hokkaido Cho. Dr. Stockbridge is not, be it understood, the alchemist to an Eastern potentate, nor yet one of the astrologers, Chaldæans, or soothsayers of a modern Belshazzar, but an agricultural chemist and geologist discoursing upon rocks and soils, nitrates and microbes, and suggesting processes by which atmospheric nitrogen is fixed in the soil by the action of living organisms. The great Mikado, "virtuous man," has, we know, transplanted full-grown and fully-equipped knowledge from the West to his remote dominions; and so successfully, that it has rooted, and now is become an article for exportation—as witness the volume before us. To some of our readers it may appear unnecessary to dilate upon a fact which springs naturally out of the most recent developments of civilization. We need not now despair of openings for aspiring young chemists under the protection and pay of the King of Dahomey or of Ashantee, or of an Imperial Institute at Khartoum or some other part of the Dark Continent; and truly the missionaries of science are in a fair way to rival those of religion in their ubiquity.

The volume before us is of attractive appearance. It is, however, hard upon the reader who takes it up in order to learn something about rocks and soils to be carried through the entire history of the planet on which his lot is cast. Deeply interesting as are the cosmic questions bearing upon the original nebulous mist, "in glowing gaseous condition," they scarcely affect even scientific agriculture. Besides, it is open to doubt whether an agricultural chemist and geologist is within his province in explaining the differences between white stars, red stars, and habitable planets which have gone through phases thus indicated. Such information belongs to the domain of the astronomer and the physicist, and the agricultural study of rocks and soils should be taken up at a later date of the earth's history. It is not our object to criticize Dr. Stockbridge's book severely, but it appears to us that if he had cut out 100 pages at the beginning, and added 100 pages at the end in harmony with his concluding sections, his work would have been more useful.

The two features of this book which seem to us the most important are, first, Dr. Stockbridge's views as to the "fixation of atmospheric nitrogen independent of ammoniacal condensation and of nitrification." The compounds thus formed in the soil are, we are told, complex insoluble amides resembling those existing in living organisms, and must have resulted through the vital activity of the micro-organisms present in the soil. If soils have the power of fixing atmospheric nitrogen through the action of living organisms, they possess a means of recruiting fertility independent of plant action, and of so fundamental a nature that, supposing such action to take place, the question of the source of nitrogen and the supply of nitrogen in soils would be set at rest. Another novel view is that propounded with reference to dew-formation. Here, we have a subject which is not very clearly related to that of rocks and soils. So far as the soil is a vehicle of plant nutrition, its conditions as related to moisture are of course important, and it is in this connection that the theory of dew as propounded by Dr. Stockbridge finds a place in his work. It is not necessary here to explain Dr. Wells's explanation of the fall of dew.

It is sufficient to state that it is unsatisfactory to our author, who holds that dew on the leaves of plants is (we presume he means occasionally and not universally) derived from the plant itself rather than from condensation from the atmosphere. Dew on growing vegetables is produced by the condensation of the transpired moisture from the plant on its own leaves. This explanation is proved by direct experiment, and we are not disposed to deny its truth. It is probable, and, in fact, more than probable, that plants which are giving off large quantities of water into a cold or overcharged atmosphere should have a portion of their own moisture thrown back upon them. This fact is asserted in Marshall Ward's translation of Sachs's "Physiology of Plants," when he says, "Much of the water we find early in the morning on the margins of the leaves of many field and garden plants in the form of large drops, and which are generally taken for drops of dew, is really water excreted from the plants themselves." That the air is really the cooling medium by which the moisture rising from the warm soil or the growing plant is condensed is no doubt often true. The mist which stretches over the meadows at sundown is moisture condensed in the cool air, and thus becomes precipitated upon vegetation, and not only on vegetation, but upon everything else. Where we think Dr. Stockbridge has overstated his case is when he writes in italics, "*The declaration is here made that dew is the condensed exhalation of the plant.*" The statement is too general, and the assertion has too much of the character of a supposed new discovery on the part of the author. The real facts of the case are, that dew is produced in some cases from condensed exhalations from the plant, or from condensed moisture rising from the soil; but also from the precipitation of moisture from higher sections of the air during the night; especially when the sky is clear. The collection of water in the form of hoar frost upon leafless trees or lawns must be derived from the condensation of atmospheric moisture upon the tree, or upon the grass, cooled by radiation; and we have no doubt that grass radiates heat on a moonlight night more rapidly than does bare ground. Dr. Stockbridge lays too much stress on the fact that the earth is warmer than the air when dew is falling. This he asks us to believe is fatal to the theory that the earth condenses or can condense atmospheric moisture. The radiating power of the earth is very great, and exceeds that of the air, which, in fact, absorbs and retains much heat which otherwise would be immediately lost in space. Dr. Stockbridge argues that the surface of the earth is invariably warmer than the air at the dew point, but this is not likely to be the case. Even the temperature of grass land is affirmed to be always warmer than the air, and hence it is contended that in no instance can the earth or vegetation be the condenser. We are disposed to think that observation will throw more light on this point than such experiments as are quoted or were made by Dr. Stockbridge.

It is probable that dew may be precipitated at times by a colder air on a warmer surface, and at other times by a cold soil or cold expanse of leaf from a warm atmosphere. Whether the leaf of a grass or the air above it acts the part of "the cold pitcher" is not always to be predicated, but in either case dew would be the result. We may point out that, while Dr. Stockbridge is disposed to assert that the soil is always warmer than the air, other authorities are of opinion that the surface, especially of grass, is colder by many degrees than the air. A thermometer laid upon grass would, we believe, recede further and record a lower minimum than one suspended 2 feet above the grass. The freezing of dew on grass during summer nights, which is always an unpleasant sight to gardeners and farmers, appears to be accounted for by radiation of heat from the grass surface, while the substance of the grass cuts off radiated heat from below. The absence of dew under shade also is apparently due to radiation from the earth being checked, and the cooling process of the surface of the earth or its vegetable covering being prevented.

Dr. Stockbridge's book suffers from careless reading of the proofs. This work, the author tells us, he was compelled to depute to others. In one place (p. 183) the word "soil" is evidently used instead of air, thereby reversing the author's obvious meaning, and the word "not" is interpolated, which further confuses the sentence hopelessly. Names of authors quoted are mis-spelt in several cases. The book, especially in the earlier pages, is somewhat bombastic and provincial in its style, and, as we have before stated, many of the earlier pages might have been omitted with advantage. The tone of the writing becomes more modest, precise, and student-like as the author approaches the topics which we are informed upon the title-page he professes.

JOHN WRIGHTSON.

OUR BOOK SHELF.

The Kingdom of Georgia. By Oliver Wardrop. (London: Sampson Low, 1888.)

THE author of this work sees no reason why Georgia should not become as popular a resort as Norway or Switzerland. Perhaps he takes rather too sanguine a view of the energy even of the British tourist, but everyone who reads his book will certainly wish to have a chance of visiting the country he describes. A brighter or pleasanter book of the kind we have not seen for many a day. The style is fresh and sparkling, and Mr. Wardrop has the secret of awakening and maintaining the interest of his readers without any attempt at picturesque fine-writing. He conveys a remarkably vivid impression of the splendid natural scenery of Transcaucasia; but it is in describing the Georgian people that he displays most effectively his powers as an observer. He has the warmest appreciation of the intelligence, bravery, and generosity of the Georgians, and, in the course of his narrative, the extent to which these and other qualities affect their social life is shown with much force and animation. There are valuable chapters on the history, the political condition, and the language and literature of Georgia; and an excellent bibliography of works relating to the country is given as an appendix. The book is also supplied with good maps and illustrations.

The British Journal Photographic Almanac for 1889 Edited by J. Traill Taylor. (London: Henry Greenwood and Co., 1889.)

THIS work, as a compendium of photographic art science, could scarcely be more complete. It contains about 300 closely-printed pages, consisting of articles written by men who are eminent in connection with photography in its various and ever-increasing branches. Great advance has been made in this year's volume, both as regards size and the number of articles included; the pages of the calendar which were formerly devoted to the dates of meetings of Societies have here been left blank for the sake of persons desiring to make notes or memoranda.

Valuable hints on all topics are given both to amateurs and professionals, no single department of the work, as far as we can find, having been neglected.

A brief summary of the year's work is given by e editor, touching upon the gradual merging of the brown and purple tones into those of darker and more engraving-like type, the advancement made in flash-light photography, and the new method of platinum-printing. The summary concludes with an obituary of those who have passed away since the last issue.

Next follow series of articles, commencing with one on "Iron Printing," by the editor, and continuing with those contributed by Abney, Burton, Perry, Piazzi-Smyth, and many others.

Twenty pages are devoted to an epitome of progress during the year 1888, and then are added a list of useful receipts, standard formulæ, reference tables, &c.

The Photographer's Diary and Desk-book for 1889. Compiled by the Editor of the *Camera*. (London: Published at the Office of the *Camera*, 1889.)

OF the various diaries brought out for the present year, that issued by the proprietors of the *Camera* will be sure to give great satisfaction to photographers, both amateur and professional. This differs from other photographic diaries in two respects: in the first place, it is much larger, there being plenty of room for notes on experiences of various kinds, results of manipulating, developing difficulties, and many other details well worth recording, which are often so useful and valuable for reference. In the second place, there is a great amount of useful information condensed in the first fifty pages. Besides various tables and processes of developing, printing, &c., information similar to that included in almanacs and other diaries is inserted; the tables and standard formulæ relating to photography being printed in larger type, to enable the worker, when in the dark room, to refer to them. This diary is a very complete and useful publication, and, as a book of reference, is most handy.

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.*]

The Climate of Siberia in the Mammoth Age.

A SHORT time ago I was discussing with my friend Mr. Henry Seebohm the various problems connected with the distribution and migration of birds in Siberia, about which he has collected so many facts. One fact which he mentioned to me seemed to have a much wider interest than a merely ornithological one, and to illustrate from an unexpected quarter a conclusion which you have allowed me to urge in your columns, and which forms a notable postulate in my recent work on "The Mammoth and the Flood." I mean in reference to the climate of Siberia during the Mammoth age. The views I have advanced on this subject are not my own. I have merely followed in the footsteps of almost every recent Continental authority, especially the authorities with the greatest claims to attention—namely, the Russian naturalists who have visited Northern Siberia. They maintain—and I think the position is unassailable—that during the Mammoth period that district which is now a bare *tundra*, on which neither in summer nor winter could herds of pachyderms find food or shelter, was marked by a temperate climate, and was probably occupied by forests to the very borders of the Arctic Ocean.

This view, which is supported by so many facts, was finally established when it was shown by Schmidt and others that rooted trunks of trees are found in the beds containing Mammoth remains far north of the present range of trees, and that southern forms of fresh-water mollusks, such as the *Cyrena fluminalis*, are also found preserved in the same beds in Siberia far to the north of any place where they will now live. These facts are consistent only with the former existence of a temperate climate in Siberia.

It is interesting to meet with support for this position from the present avifauna of the Palæarctic region. Mr. Seebohm, who has an unrivalled collection of skins, illustrating the ornithology of this region from Britain to Japan, assures me (and, in fact, he showed me the evidence) that certain birds—notably the jay, the nuthatch, the marsh-tit, coal-tit, and long-tailed tit, the great and little spotted woodpecker of England and Japan, and in one case of Northern China—are virtually undistinguishable. Similarly, the hazel grouse of Japan resembles that of the Pyrenees, and the nutcracker of Japan and China is like that of Western Europe. While this is so, the forms of these same birds found in the intervening district of Siberia differ very materially, and have, I believe, in almost every case, been treated as specifically distinct. This is assuredly a very interesting fact. Both Britain and Northern Japan are in the same zoological province—namely, the Palæarctic region, over which there is a singular constancy of types and forms, and yet we find that in certain birds the forms at either extremity of the province are closely allied, while the intermediate form differs. This is at one with the fact that the climate of the two extremities is very similar, and that of the intervening district is very much more severe in the winter. We can hardly doubt that the general adherence to a normal type which marks the fauna and flora of the Palæarctic region (and which was even more marked, and amounted, so far as we know, to identity, in the Mammoth period) is due to the fact that formerly, and in every probability in the Mammoth period, an equality of conditions prevailed throughout. This equality has been maintained at the extremities of the region, with the result of maintaining the old forms and types unaltered; while it has changed and grown more severe in the intervening region, with the corresponding result of altering the types there. The conservatism of forms at either end of the province proves unmistakably a conservatism of conditions. This is assuredly a very interesting independent proof, if proof be now needed, that the climate of Siberia was once much more temperate throughout, and like that of Britain and Japan, and this doubtless in the Mammoth age.

I may add that it seems to me very nearly certain that this change of climate in Siberia was the cause of the conversion of what were once sedentary birds there into birds that migrate to South Africa and elsewhere—a migration which has been very well illustrated by Mr. Seebohm. That the date of the commencement of this migratory tendency is not very remote in time is shown by the fact that the birds have not been more differentiated, notwithstanding the very various conditions prevailing in their several winter-quarters. I believe myself that in the Mammoth period, when the climate of Siberia was temperate, there was no need for these tremendous migrations, which were, no doubt, originally induced by the necessity for finding food in winter; but that most, if not all, of these migratory birds were then either stationary in Siberia, or were only local migrants, like so many of our own birds are now.

Mr. Seebohm, in his recent work on the Charadriidæ, has invoked the Glacial epoch to account for the facts presented by that singularly distributed genus. I know of no Glacial epoch in Siberia before the present. The last epoch there, as we can test and prove by the presence of the undecayed carcasses in the frozen ground, was the period when the Mammoth lived. It was when that period closed (and as I claim to have proved closed very rapidly) that the present Arctic conditions of the Siberian climate were introduced, and I would urge it was from this date that the *present* laws controlling the migration of Siberian birds arose.

This seems an inference of some importance; and when the ornithological history of the eastern half of the Palæarctic region is written in detail, it will very probably be shown that the peculiar sub-specific types found there are many of them *new* forms, which have arisen since the Mammoth age, having been altered from the old ones, which live on under old conditions in the West of Europe and the Japanese Archipelago.

I should like to specify one particular bird in regard to which this notion seems to point a special moral. This is the British red grouse, the only peculiar bird of these islands. Its nearest ally on the Continent, and a very near ally, is the willow grouse. I have little doubt that the willow grouse of the Continent is an altered form, and that our red grouse is the parent, since the evidence we have, and it is not slight, goes to show that England has preserved better than Scandinavia the climatic conditions of the Mammoth period. The white marks and other characteristics of the willow grouse are evidences of the effect of colder winter-quarters, as they are in the case of the ptarmigan.

Lastly, I cannot avoid emphasizing once more the conclusion which I have pressed in my book that this milder climate in Siberia during the Mammoth age entirely does away with the necessity for invoking quite transcendental seasonal migrations for the fauna there which have been postulated by Prof. Dawkins and others. That the Mammoth, with its immature young, should be able to pass to and fro between the south of Siberia and the New Siberian Islands and Kamchatka, between summer and winter, has always seemed to me incredible. If they could compass the journey they would either find a temperate conditions of things, which is alone consistent with their finding food, when there would be no occasion for them to migrate, or they would find the conditions which prevail now, when no pachyderms could find food *even in summer*, since they are physically incapable of browsing the short herbage of the *tundra*. Nor could the trees and the southern mollusks, like the *Cyrena*, migrate, even if the young Mammoths could. This theory of migrations finds no support, so far as I know, among those who have studied the problem on the ground; and it is put entirely out of court when we realize that, Siberia having had a temperate climate, there was no necessity to migrate.

A similar argument applies to the theory invoking the transport of the Mammoth carcasses by means of the Siberian rivers, which has always seemed to me untenable when the conditions are faced. I would mention that in Baron Toll's recent journey to the New Siberian Islands, situated a long way north of the Siberian coast-line, and entirely out of the reach of any possible river portage, he not only found remains of a carcass of a Mammoth preserved in the flesh, but found them in a bed situated to the north of a ridge. This fact may be put beside those already mentioned by Wrangell and others long ago, that the carcasses and skeletons and *cachés* are found chiefly on the hillocks and higher ground of the *tundra*, out of the reach of river-floods altogether, and found most frequently near the small rivulets and feeders of the greater streams, which could not float them, and found also near those flowing south. This theory of portage and that of seasonal migrations have been nursed and maintained in this country in spite of evidence of every kind, because they are supposed in some way to buttress the theory of uniformity, as taught by Lyell and Ramsay. An appeal to them and to similar complicated physical causes becomes not merely unwarrantable, but unscientific and illogical, when we realize that from one end of Siberia to the other the climate was sufficiently temperate when the Mammoth lived there to enable trees to grow and vegetable food to be found everywhere, and the physical surroundings of the country were probably such as may be measured by those still prevailing in Britain or Japan.

HENRY H. HOWORTH.

Bentcliffe, Eccles, January 10.

The Crystallization of Lake Ice.

ON returning recently from North Wales, I was very pleased to meet with a description, by Mr. James C. McConnel (NATURE, December 27, p. 203), of the elaborate experiments performed by himself and Mr. Dudley A. Kidd on glacier and lake ice at St. Moritz. An experiment I had made on the ice of Llyn Creigenen, a small lake to the north-west of Tyrau Mawr, seems to me to be, in some measure, confirmatory of the results obtained in connection with the crystallization of lake ice.

By sharply striking the ice, which was only about half an inch thick, with the rounded end of a stick, fractures were produced, which invariably adopted the form of a six-rayed star-like figure. The beautiful regularity of these figures, in regard to the number, position, and perfect straightness of their rays, at once reminded me of the well-known percussion- and pressure-figures produced in mica plates by Reusch and Bauer. Mr. Grenville Cole, who was with me at the time, repeated the experiment, and obtained precisely similar results. We found that over a certain area a large number of these figures could be produced, in each of which there were corresponding parallel rays—that is to say, every percussion-figure was similarly disposed with regard to a fixed line. Outside this area, the figures produced, although preserving the characters of those first made, exhibited a change in the direction of the rays. In this way we could determine the boundaries of a number of adjacent areas, separated from one another by definite lines of demarcation. These areas averaged about two feet across.

We were unable to conceive of any conditions of stress which would, in a homogeneous solid plate, give rise to such phenomena—fractures of such beautiful regularity, and so constant in character. Consequently, we thought of crystallization; but this would necessitate the recognition of ice-crystals of very large dimensions—a conclusion obviously at variance with the existing notions concerning the crystalline characters of ice. We thought, however, that the matter might be worthy of investigation, and, on returning to town, were pleased to find that large crystals of lake ice had been found at St. Moritz by Messrs. McConnel and Kidd, which, however, did not attain the size of those we noticed on Llyn Creigenen. But the fact that on the St. Moritz Lake the ice attained a thickness of over one foot shows that the temperature must have been lower, and the conditions more rigorous, than in North Wales at the time of our visit.

If it is possible at all to obtain large crystals of ice, I should say the conditions for such on Llyn Creigenen were of the most favourable character. For three days previously, the temperature varied very little from zero C., and, from the slight wind that prevailed at the time, the lake was well sheltered by the hills which rise abruptly around; indeed, the lake was unusually free from disturbing influences of any kind.

If these sheets of ice were gigantic crystals, it is in the highest degree probable that the surface of the ice coincided with the basal plane, as was the case with the columnar crystals observed by Prof. Heim in the lake ice of the Swiss lowlands. For want of a polariscope we were prevented from investigating the matter further in the field; but in some small well-sheltered pools on Tyrau Mawr we found it easy to produce the same phenomena of percussion-figures, whilst the ice in the marshy places amongst the grass gave fractures of a most irregular kind. We found, in several places, skeleton-crystals like ornamented equilateral triangles, measuring some inches across.

THOMAS H. HOLLAND.

Normal School of Science, South Kensington.

Use of the *Remora* in Fishing.

WITH reference to Mr. A. C. Haddon's interesting account of the use of the *Remora* or sucker-fish by the natives of Torres Straits in fishing for turtles (NATURE, January 17, p. 285), I may call attention to the paper on this subject read by our corresponding member, Mr. Frederick Holmwood, C.B., late H.B.M. Consul at Zanzibar, before the Zoological Society of London on June 17, 1881 (see P.Z.S., 1881, p. 411), which Mr. Haddon does not seem to be acquainted with. Mr. Holmwood has fully described the mode of the use of the *Remora* by the native fishermen of Zanzibar in catching turtles and fishes. It is curious to find a somewhat similar method of employing the *Remora* practised by the islanders of Torres Straits.

P. L. SCLATER.

3 Hanover Square, London, W., January 19.

A Remarkable Rime.

UNDER this heading a letter appears from Lutterworth (p. 270), but no mention is made of the colour of the water obtained on melting some of the rime collected from the trees. In the neighbourhood, far removed from any large town, the rime crystals, on melting, gave water tasting very sooty, and looking as though the liquid had been used to wash Indian ink brushes in, it being quite black with sooty particles.

M. H. MAW.

Barrow-on-Humber, Hull, January 22.

HUMAN VARIETY.[1]

IT would have been a pleasure to me in this address, given at the conclusion of my office as your President, to have cast a retrospect over the proceedings of our Institute during the four years that I have had the honour to hold it. But the subjects that have come before us are so varied that it seemed difficult to briefly summarize them in a manner that should not be too desultory.

On the whole, I thought it might be more useful if I kept to a branch of anthropometry with which many inquiries have made me familiar, and took the opportunity of urging certain views that seem to be worthy the attention of anthropologists.

Before entering upon these more solid topics, let me mention that the laboratory of which I spoke in my last address has been in work during the past year, and that about 1200 persons have been already measured at it in many ways, some more than once. I lay on the table a duplicate of one of the forms of application to be measured, and of one of the filled-up schedules. It will be observed that I now have the impressions made in printers' ink of the two thumbs of each person who is measured, being desirous of investigating at leisure the possibilities of employing that method for the purpose of identification, not forgetting the success that attended Sir W. Herschel's use of it in India, but conscious at the same time of practical difficulties. There is no doubt that the thumb or finger marks vary so much that a glance suffices to distinguish half a dozen varieties, while a minute investigation shows an extraordinary difference in small, though perfectly distinct, peculiarities. Neither is there any room for doubt that these peculiarities are persistent throughout life; nor, again, that so satisfactory a method of raising a very strong presumption of identity would be valuable in many cases. It will suffice to quote the following. A newspaper was lately sent me from the distant British settlement of North Borneo, where, owing to the wide and rapid spread of information nowadays, attention had been drawn to an account of a lecture I gave on one of the Friday evenings last spring, at the Royal Institution. It was on "Personal Description and Identification," and a writer in the *British North Borneo Herald* commented upon the remarks there made on finger imprints. He spoke of the great difficulty of identifying coolies either by their photographs or measurements, and that the question how this could best be done would probably become important in the early future of that country. I also am assured that the difficulty of identifying pensioners and annuitants has led to frequent fraud from personation, involving in the aggregate a very large sum of money annually, as there is good reason to believe. If finger imprints could be practically brought into use, such frauds would be extremely difficult. I am still unable to speak positively as to the best way of making them, but the plan adopted at the laboratory is as follows. A copper plate is smoothly covered with a very thin layer of printers' ink, a printers' roller being used, and the plate being cleaned every day. When the layer is thin, no ink penetrates into the delicate furrows of the skin, but the ridges only are inked, and these leave their impression when the inked thumb is pressed on paper. In this way a permanent mark is registered. A little turpentine cleans the fingers effectually afterwards. But for purposes of identification a simpler process is necessary, one by which a person suspected of personation could furnish an imprint for comparison with the registered mark without having recourse to the troublesome paraphernalia of the printer. Such a process is afforded by slightly smoking a piece of smooth metal or glass over the candle, pressing the finger on it, and then making the imprint on a bit of gummed paper that is slightly damped. The impression is a particularly good one, and is sufficiently durable for the purpose. The iron used for the ironing of clothes is excellent for the purpose; even a smooth penny can be used. As for the gummed paper, luggage labels can be used; even the fringe to sheets of postage stamps is broad enough to include as much of the impression as is especially wanted—namely, where the whorl of ridges takes its origin.

I hope at some future time to recur to this subject.

Correlation.—The measurements made at the laboratory have already afforded data for determining the general form of the relation that connects the measures of the different bodily parts of the same person. We know in a general way that a long arm or a long foot implies on the whole a tall stature—*ex pede Herculem;* and conversely that a tall stature implies a long foot. But the question was as to whether that reciprocal relation, or correlation as it is commonly called, admitted of being precisely expressed. Correlation is a very wide subject indeed. It exists wherever the variations of two objects are in part due to common causes; but on this occasion I must only speak of such correlations as have an anthropological interest. The particular problem I first had in view was to ascertain the practical limitations of the ingenious method of anthropometric identification due to M. A. Bertillon, and now in habitual use in the criminal administration of France. As the lengths of the various limbs in the same person are to some degree related together, it was of interest to ascertain the extent to which they still admit of being treated as independent. The first results of the inquiry, which is not yet completed, have been to myself a grateful surprise. Not only did it turn out that the expression and the measure of correlation between any two variables are exceedingly simple and definite, but it became evident almost from the first that I had unconsciously explored the very same ground before. No sooner had I begun to tabulate the data than I saw that they ran in just the same form as those that referred to family likeness in stature, and which were submitted to you two years ago. A very little reflection made it clear that family likeness was nothing more than a particular case of the wide subject of correlation, and that the whole of the reasoning already bestowed upon the special case of family likeness was equally applicable to correlation in its most general aspect.

It may be recollected that family likeness in any given degree of kinship—say that between father and son—was expressed by the fact that any peculiarity in the father appears in the son, reduced on the average to just one-third of its amount. Conversely, however paradoxical it might at first sight appear, any peculiarity in a son appears in the father, also reduced on the average to one-third of its amount. The regression, as I called it, from the stature of the known father to the average son, or from the known son to the average father, was here from 1 to $\frac{1}{3}$; from the known brother to the unknown brother it was $\frac{2}{3}$; from uncle to nephew, or from nephew to uncle, it was $\frac{2}{9}$; and in kinship so distant as to have insensible influence, it was from 1 to 0. Whether the peculiarity was large or small, these ratios remained unaltered. The reason of all this was thoroughly explained, and need not be repeated here. Now the relation of head-length to head-breadth, whose variations are on much the same scale, is of the same kind as the above. They are akin to each other in the same sense as kinsmen are. So it would be in the closer relation between the lengths of the corresponding limbs, left arm to right arm, left leg to right leg. The regression would be strictly reciprocal in these cases. When, however, we compare limbs whose variations are on different scales, these differences of scale have to be allowed for before the regression can assume a reciprocal form. The plan of making the requisite allowance is perfectly

[1] Address delivered at the anniversary meeting of the Anthropological Institute, on Tuesday, January 22, by Mr. Francis Galton, F.R.S., President.

simple, but I cannot explain it without using technical terms. In some cases this allowance is large; thus the length of the middle finger varies at so very different a rate from that of the stature that 1 inch of difference of middle-finger length is associated on the average with 8·4 inches of stature. On the other hand, 10 inches of stature is associated on the average with 0·6 inch of middle-finger length. There is no reciprocity in these numerals; yet, for all that, when the scale of their respective variabilities is taken into account as above mentioned, the values at once become strictly reciprocal. I shall be able to explain this better later on.

In every pair of correlated variables the conditions that were shown to characterize kinship will necessarily be present—namely, that variation in one of the pair is on the average associated with a proportionate variation in the other, the proportion being the same whatever may be the amount of the variation. Again, when allowance is made for their respective scales of variability, the proportion is strictly reciprocal, and it is always from 1 to something less than 1. In other words, there is always regression.

Variety.—The principal topic of my further remarks will be the claims of variety to more consideration from anthropologists than it has hitherto received. They commonly devote their inquiries to the mean values of different groups, while the variety of the individuals who constitute those groups is too often passed over with contented neglect. It seems to me a great loss of opportunity when, after observations have been laboriously collected, and been subsequently discussed in order to obtain mean values from them, the very little extra trouble is not taken that would determine other values whereby to express the variety of the individuals in those groups. Much experience some years back, and much new experience during the past year, has proved to me the ease with which variety may be adequately expressed, and the high importance of taking it into account. There are numerous problems of especial interest to anthropologists that deal solely with variety.

There can be little doubt that most persons fail to have an adequate conception of the orderliness of variability, and think it useless to pay scientific attention to variety, as being, in their view, a subject wholly beyond the powers of definition. They forget that what is confessedly undefined in the individual may be definite in the group, and that uncertainty as regards the one is in no way incompatible with statistical assurance as regards the other. Almost everybody is familiar nowadays with the constancy of the average in different samples of the same large group, but they do not often realize the way in which the same statistical constancy permeates the whole of the group. The Mean or the Average is practically nothing more than the middlemost value in a marshaled series. A constancy analogous to that of the Mean characterizes the values that occupy any other fractional position that we please to name, such as the 10th per cent., or the 20th per cent.; it is not peculiar to the 50th per cent., or middlemost.

Greater interest is usually attached to individuals who occupy positions towards either of the ends of a marshaled series, than to those who stand about its middle. For example, an average man is morally and intellectually a very uninteresting being. The class to which he belongs is bulky, and no doubt serves to keep the course of social life in action. It also affords, by its inertia, a regulator that, like the fly-wheel to the steam-engine, resists sudden and irregular changes. But the average man is of no direct help towards evolution, which appears to our dim vision to be the primary purpose, so to speak, of all living existence. Evolution is an unresting progression; the nature of the average individual is essentially unprogressive. His children tend to resemble him exactly, whereas the children of exceptional persons tend to regress towards mediocrity. Consider the interest attached to variation in the moral and intellectual nature of man, and the value of variability in those respects. For example, in the Hebrew race, whose average worth shows little that is worthy of note, but which is mainly of interest on account of its variety. Its variability in ancient and modern times seems to have been extraordinarily great. It has been able to supply men, time after time, who have towered high above their fellows, and left enduring marks on the history of the world.

Some thoroughgoing democrats may look with complacency on a mob of mediocrities, but to most other persons they are the reverse of attractive. The absence of heroic gifts is a heavy set-off against the freedom from a corresponding number of very degraded forms. The general standard of thought and morals in a mob of mediocrities must be mediocre, and, what is worse, contentedly so. The lack of living men to afford lofty examples, and to educate the virtue of reverence, would leave an irremediable blank. All men would find themselves at nearly the same dead average level, each as meanly endowed as his neighbour.

These remarks apply with obvious modifications to variety in the physical faculties. Peculiar gifts, moreover, afford an especial justification for division of labour, each man doing that which he can do best.

The method I have myself usually adopted for expressing and dealing with the variety of the individuals in a group, has been already explained on more than one occasion. I should not have again alluded to it had I not had much occasion of late to test and develop it, also to devise an unpretentious little table of figures, that I call a "table of normal distribution," which has been of singular assistance to myself. I trust it may be equally useful to other anthropologists. It is appended to these remarks, and I should like after a short necessary preface to say something about it. The table and its origin, and several uses to which it has been applied, will be found in a book by myself, that will be published in a few days, called "Natural Inheritance" (Macmillan and Co.). All the data to which I shall refer will be found in that book also, except such as concern correlation. These accompanied a memoir read by me only a month ago before the Royal Society, and will be published in due course in their Proceedings.[1]

It has already been said that the first step in the problem of expressing variety among the individual members of any sample, is to marshal their measures in order, into a class. We begin with the smallest measure and end with the greatest. The object of the next step is to free ourselves from the embarrasment due to the different numbers of individuals in different classes. This is effected by dividing the class, whatever its size may be, into 100 equal portions, calling the lines that divide the portions by the name of grades. The first of these portions will therefore lie between grades 0° and 1°, and the hundredth and last portion between grades 99° and 100°. We have next to find by interpolation the values that correspond to as many of these grades as we care to deal with. It is of no consequence whether or no the number in the class is evenly divisible by 100, because we can interpolate and get the values we want, all the same. This having been done, the value that corresponds with the 50th grade will be the middlemost. It is practically the same for ordinary purposes as the mean value or the average value; but as it may not be strictly the same, it is right to call it by a distinctive name, and none simpler or more convenient occurs than the letter M. So I will henceforth use M to denote the middlemost or median value, or, in other words, that which corresponds to the 50th (centesimal) grade.

The difference between the extreme ends of a marshaled

[1] For abstract, see NATURE, January 3, p. 238. For tables of percentiles, see vol. xxxi. p. 223. For hereditary stature, see vol. xxxiii. p. 295.

series is no proper measure of the variety of the men who compose it. However few in number the objects in the series may be, it is always possible that a giant or a dwarf, so to speak, may be among them. The presence of either would mislead as to the range of variety likely to be found in another sample taken from the same group. The values in a marshaled series run with regularity only about its broad and middle part; they never do so in the parts near to either of its extremities. In a series that consists of a few hundreds of individuals, the regularity usually begins at about grade 5°, and continues up to about grade 95°. Therefore it is out of this middle part, between 5° and 95°, or better, in a still more central portion of it, that points should be adopted between which variety may be measured. Such points are conveniently found at the 25th and the 75th grades. Just as the grade 50° divides the class into two equal parts, so the grades 25° and 75° subdivide it into quarters, and the difference between those values affords an irreproachable basis for the unit of variety. The actual unit is half the value of that difference, because the value at 25° tends to be just as much below that at 50°, as the value at 75° is above it. Therefore the average of these two values is a better measure than their sum. Briefly, if we distinguish the measure at 25° by the letter Q_1, and that at 75° by Q_3, then the unit of variety is $\frac{1}{2}(Q_3 - Q_1)$, and this unit we will henceforth call Q. As M measures the average, so Q measures the variety, and they are independent of one another. In strength, for example, the relation of Q to M in the particular group of adult males on which I worked was as 1 to 10; in the statures of the same group it was as 1 to 40; in breathing capacity as 1 to 9; in weight as 1 to 14.

The arithmetic mean or average is a muddle of all the values in the series; it is by no means so clear an idea as the middlemost value M. Therefore, although the peculiarities of an individual are commonly considered in the light of deviations from the average value, I prefer to reckon them as deviations from M. Practically the two methods are identical, but I find the latter more convenient to work with, and believe it to be the better of the two in every way.

Deviation is identical with variation, and the well-known law of frequency of error gives data whence the *relative* values of the deviations at the several grades may be calculated for any normal series. If we know the deviation at any one grade, then the *absolute* value of those at every other grade can be calculated; consequently the variety of the whole series is thereby expressed.

The small table of distribution, of which I spoke, gives the values at each grade when Q is equal to 1. Then the value at 25° is − 1, and that at 75° is + 1. If we desire to determine Q in any such series, the only required datum is the deviation at some one known grade, since, by dividing that deviation by the tabular value, we get Q at once. Or, conversely, if we know the Q of the series, and wish to calculate the deviation at any given grade, we multiply Q by the tabular deviation. Thus, in stature, which varies in an approximately normal manner, the value of Q is about 1·7 inch, therefore to find the deviation in stature at any grade, we multiply 1·7 inch by the tabular value.

If we know the *measures* at any two grades of a normal series, we are easily able to calculate both Q and M, and can thence derive the measures at any other desired grades. I have long since pointed out the possibility of a traveller availing himself of this method; but, for the want of a table of distribution, the calculation would probably puzzle him. With the aid of this table the calculation is made most readily. Let us suppose that the traveller is among savages who use the bow, and that he desires to learn as much as he can about their strengths. He selects two bows; the one somewhat easy to draw, and the other somewhat difficult, and at leisure, either before or after the experiment, he ascertains exactly how many pounds weight they severally require to draw them to the full. Then by exciting emulation, and by offering small prizes, he induces a great many of the natives to try their strengths upon them. He notes how many make the attempt, and how many of them fail in either test. This is all the observation requisite, though common-sense would suggest the use of three and not two bows, in order that the data from the third bow might correct or confirm the results derived from the other two. Let us work out a case, not an imaginary one, but derived from tables I have already published, and of which I will speak directly. Let the problem be as follows:—

30 per cent. of the men failed to exert a pulling strength of 68 pounds; 60 per cent. failed to pull 77 pounds. What is the Q and the M of the group?

Consider this 30 per cent. to be the exact equivalent of grade 30°, and the 60 per cent. of grade 60°. The reason why the percentage of failure, and the number of the grade are always equivalent will be found in a footnote to the table, and I need not stop to speak of it. Now, the tabular value at grade 30° is − 0·78; that at 60° is + 0·38; the difference between them being 1·16. On the other hand, the difference between the two test values of 68 pounds and 77 pounds is 9 pounds. Therefore Q is equal to 9 pounds divided by 1·16; that is, to 7·8 pounds. M may be obtained by either of two ways, which will always give the same answer. We may subtract 0·38 × 7·8 pounds from 77 pounds, or we may add 0·78 × 7·8 pounds to 68 pounds. Each gives 74 pounds. Observation gave precisely these values both for Q and for M. The data were published in the Journal of this Institute as a table of "percentiles," and were derived from measures made at the International Health Exhibition. The value of M is given directly in the table, but that of Q happens not to be given there; it may easily be found by interpolation. That table affords excellent material for experimental calculations on the principle of this test, and for estimating its trustworthiness in practice.

It contains a variety of measures referring to eighteen different series, all corresponding to the same grades—namely, to 5°, 10°, 20°, and onwards for every tenth grade up to 90° and ending with 95°. The measures refer to stature, height sitting above seat of chair, span, weight, breathing capacity, strength of pull, strength of squeeze, swiftness of blow, keenness of eyesight, in each case of adult males and of adult females separately. I have since found that when the deviations are all reduced in terms of their respective Q values, by dividing each of them by its Q, that the average value of all the deviations at each of the grades in the eighteen series closely corresponds to the normal series, though individually they differ more or less from it, some in one way, some in another. On the whole, the error of treating an unknown series as if it were a normal one can rarely be very large, always supposing that we do not meddle with grades lower than 5° or higher than 95°.

It will be of interest to put the comparison on record. It is as follows:—

Grades	5°	10°	20°	30°	40°	50°
Observed	− 2·44	− 1·87	− 1·24	− 0·77	− 0·40	0
Normal − below 50° + above 50° ...	2·44	1·90	1·25	0·78	0·38	0
Observed	+ 2·47	+ 1·92	+ 1·21	+ 0·75	+ 0·38	0
Grades	95°	90°	80°	70°	60°	50°

The "observed" are the mean values, made as above described, of the eighteen series; the "normal" are taken from the table of distribution given further on.

An ingenious traveller might obtain a great number of approximate and interesting data by the method just described, measuring various faculties of the natives, such as their delicacy of eyesight and hearing, their swiftness in running, their accuracy of aim with spear, arrow, boomerang, sling, gun, and so forth, either laterally or else vertically, distance of throw, stature, and much else. But he should certainly use three test objects, and not two only.

It should be remarked that, if the distribution of deviation was constant throughout any large class of faculties, though the Q might differ in different sub-classes of it, then, even though the distribution of that faculty was very far indeed from being normal, an appropriate table of distribution could be drawn up to solve such problems as those mentioned above. I have as yet no accurate data to put this idea to a practical test.

There are three convenient stages of expressing the variety of the various measures in a series, each reaching considerably nearer to precision than the one before. The first is to give only Q and M; the second is to record the measures at the grades 10°, 25°, 50°, 75°, and 90°; the third is the more minute method, adopted in the table of percentiles—viz. to give the measures at 5°, 10°, 20°, &c., 80°, 90°, and 95°. It may in some cases be found worth while to go further, say to 1° and 99°, or even also to 0° and 99°·9. So much for the expression of variety.

The use of Q is by no means limited to the objects just named. It is a necessary datum wherever the law of frequency of error has to be applied, and the properties of this law are applicable to a very large number of anthropological problems, with more accuracy of result than might have been anticipated when the series are only approximatively normal. This has been practically shown by the agreement among themselves of several inquiries to which I will shortly allude, and it is theoretically defensible by two considerations. The one is that the law of frequency supposes the amount of error or of deviation to be the same in symmetrically disposed grades on either side of 50°, their signs being alone different, *minus* on the one side of 50° and *plus* on the other. Now, in an observed series there may be, and often is, a want of symmetry, but if the deviate, say at 70°, is as much greater than the normal as the deviate at 30° is less than the normal, then the effects of these two upon the final result will be much the same as if there had been exact symmetry at those points. The other consideration is that any nonconformity of the observed deviates with the theoretical ones towards the end of the series has but a small and perhaps insensible effect on the broad general conclusions. We need care little for any vagaries outside of the grades 5° and 95°, if the intervening portion gives fairly good results. The latter portion forms nine-tenths of the whole series, and even considerable irregularities in the remaining tenth are of small relative importance.

One great use of Q is to enable us to estimate the trustworthiness of our average results. We require to know both Q and the number of observations before we can estimate the degree of dependence to be placed on M. If there was only one observation, then the degree of dependence would be equal to Q; in other words, the error of M would be just as likely as not to exceed Q. If there were two, two hundred, two thousand, or any other number of observations, the error of M would then be reduced, but not in simple proportion. It would be as likely as not to exceed a value equal to Q divided by the square roots of those numbers. When we desire to ascertain the trustworthiness of the difference between the M values of two series, as between the mean statures of the professional and artisan class as derived from certain observations, the properties of the law of frequency of error must again be appealed to. Anthropologists are much engaged in studying such differences as these; but from their disregard of the simple datum Q, and from not being familiar with its employment, there is usually a lamentable and quite unnecessary vagueness in the value to be attached to their results. This is especially the case in comparisons between the average dimensions of the skulls of various races, which often depend upon the measurement of only a few specimens. An almost solitary exception to this needless laxity will be found in a brief but admirably-expressed memoir by Dr. Venn, the well-known author of the "Logic of Chance." It is upon Cambridge anthropometry, and was published in the last number of the Journal of this Institute. It deserves to be a model to those who are engaged in similar inquiries.

Another class of investigations in which a knowledge of Q is essential was spoken of some time back—namely, questions of correlation in the widest sense of the word. These problems have nothing to do with the relations of the M value, but are solely concerned with those of the deviations from M at the various grades. It is true that a knowledge of M is requisite in order to subtract it from the measures, and so to get at the deviations. But after this is done, M is put aside. It has no part in the work of the problem; it is only after the results have been arrived at without its use that it is again brought forward and added to them. Numerous properties of the law of frequency of error in which Q is the datum were utilized in my inquiries into family likeness in stature, and in all cases they brought out consistent results. An excellent example of this was seen in the success of the methods employed to determine the variety in families of brothers. Four different properties of the law had to be applied to partly different samples of the same group in order to determine the value of the Q of stature in fraternities, and they respectively gave 1·07, 0·98, 1·10, and 1·10 inch, which, statistically speaking, are much alike. Certain properties of the law of frequency of error were also applied to family likeness in eye colour, with results that gave by calculation the total number of light-eyed children in families differently grouped according to their parentage and grandparentage, and according to three different sets of data, as 623, 601, and 614 respectively, the observed number being 629. Other properties of the same law have been applied to determine the ratio of artistic to non-artistic children in families whose parentages were known to be either both artistic, one artistic, one not, or neither artistic. They gave to 1507 children the ratios of 64, 39, and 21, respectively, as against the observed values of 60, 39, and 17.

Lastly, as regards the correlation of lengths of the different limbs. It has already been shown that the correlation lies between the deviations, and has nothing to do with the values of M. Now, to express this relation truly, so that it shall be reciprocal, the scale of deviation of the correlated limbs, say, for example, of the cubit and of the stature, must be reduced to a common standard. We therefore reduce them severally to scales in each of which their own Q is the unit. The Q of the cubit is 0·56 inch, therefore we divide each of its deviations by 0·56. The Q of the stature is 1·75 inch, so we divide each of its deviations by 1·75. When this is done the correlation is perfect. The value of regression is found to be 0·8, whether the cubit be taken as the "subject" and the mean of the corresponding statures as the "relative," or *vice versâ*.

The value of the regression has been ascertained for each of many pairs of the following elements, and a comparison was made in each case between the correlated values as observed and those calculated from the ratio of regression. The coincidence was close throughout, quite as much so as the small number of cases under examination, 350 in all, could lead us to hope. The elements were nine in all, viz. head-length, breadth of head, length of right leg below the knee, of left cubit, of left middle finger, of the height sitting above the chair, of stature, of the differences between the two foregoing (which indicate the total length of the lower limbs), and of the span. Anthropologists seem to have little idea of the wide fields of inquiry open to them as soon as they are prepared to deal with individual variety and cease to narrow their view to the consideration of the average.

Enough has now been said to justify the claims with which I started, and which take this final form. First, wherever it is likely to be of use, that, in series of which the M is calculated, the measures at a certain number of selected grades should also be calculated and given, sufficient to enable the rest of the series to be found with adequate accuracy by interpolation. Secondly, that the value of Q should always be given, as well as that for M, for two reasons. The one is, that they suffice between them to give an approximate determination of the whole series, more closely approximate as the series is more closely of the normal type; and, secondly, because Q is an essential datum before any application can be made of the law of frequency of error. The properties of this law are, as we have seen, largely available in anthropological inquiry. They enable us to define the trustworthiness of our results, and to deal with such interesting problems as those of correlation and family resemblance, which cannot be solved without its help.

Table of ordinates to normal curve of distribution, in which the unit = the probable error, and the grades, which are the abscissæ, run from 0° *to* 100°.

Grades.	0	1	2	3	4	5	6	7	8	9
0	∞	−3·45	−3·05	−2·79	−2·60	−2·44	−2·31	−2·19	−2·08	−1·99
10	−1·90	−1·82	−1·74	−1·67	−1·60	−1·54	−1·47	−1·42	−1·36	−1·30
20	−1·25	−1·20	−1·15	−1·10	−1·05	−1·00	−0·95	−0·91	−0·86	−0·82
30	−0·78	−0·74	−0·69	−0·65	−0·61	−0·57	−0·53	−0·49	−0·45	−0·41
40	−0·38	−0·34	−0·30	−0·26	−0·22	−0·19	−0·15	−0·11	−0·07	−0·04
50	0·00	+0·04	+0·07	+0·11	+0·15	+0·19	+0·22	+0·26	+0·30	+0·34
60	+0·38	+0·41	+0·45	+0·49	+0·53	+0·57	+0·61	+0·65	+0·69	+0·74
70	+0·78	+0·82	+0·86	+0·91	+0·95	+1·00	+1·05	+1·10	+1·15	+1·20
80	+1·25	+1·30	+1·36	+1·42	+1·47	+1·54	+1·60	+1·67	+1·74	+1·82
90	+1·99	+1·99	+2·08	+2·19	+2·31	+2·44	+2·60	+2·79	+3·05	+3·45

This table is an inverse rendering of the values derived by interpolation from the ordinary table of the probability integral, but its unit is changed from that of the modulus to that of the probable error, Q. and the (centesimal) grades are reckoned from 0° to 100°. In the usual way of reckoning, the 50th grade should have been reckoned as 0°, and the deviations should have run on the one side down to − 50°, and on the other up to + 50°.

Referring to what was said some way back, that if 30 per cent. fail to pull 60 pounds, then 60 pounds must be taken as the measure corresponding to grade 30°, the reason is as follows. The 30th grade separates the man who ranks 30th in a class of 100 men from his neighbour who ranks 31st. It does so for the same reason that grade 1° separates the man who ranks 1st from the man who ranks 2nd. Now, the 30th man failed in the test, and the 31st succeeded. Therefore the grade corresponding to bare success lies between them, and is the same as grade 30°.

SUPPOSED FOSSILS FROM THE SOUTHERN HIGHLANDS.

On Monday, the 14th instant, the Royal Society of Edinburgh held a special meeting for the purpose of hearing a discussion on the crystalline rocks of the Scottish Highlands. The subject was brought forward by the Duke of Argyll, who had found in the quartzite beds which cross Loch Fyne near Inveraray certain markings which he believed to be of organic origin. His attention was first called to some ferruginous stalk-like incrustations on the surfaces of fragments of quartzite, his impression being that these markings were the remains of plants, and were embedded in the rock. The importance of the discovery of organic remains in any of the rocks that form the Central and Southern Highlands of Scotland will at once be recognized by geologists. Since the recent work of the Geological Survey in Sutherland and Ross, and the demonstration thereby afforded that the apparent upward succession on which Murchison relied, from the base of the lowest quartzite up into the upper or eastern or younger gneisses, is deceptive, there has been, perhaps, a tendency to assume that the extraordinarily complicated structure that supervenes to the east of the quartzites and limestones of Sutherland extends across the whole of the rest of the Highlands, and that the crystalline schists of these regions are made up of all kinds of crushed and sheared igneous or sedimentary masses, out of which it may be impossible to make anything like intelligible order. But those observers who have themselves examined the schists of the central and southern counties of the Highlands are tolerably confident that such assumptions have no warrant in the actual structure of the ground. On the contrary, they regard the greater proportion of the schistose and altered rocks of these districts as unquestionably of sedimentary origin. They feel persuaded that sooner or later they will be found to yield fossils, and that any day may bring to light a series of corals, shells, graptolites, or trilobites, which will furnish a palæontological basis for settling the geological age of the rocks, and placing them in their true position with regard to the Palæozoic formations of the rest of the country.

The announcement that the Duke of Argyll had found what seemed to be organic remains in the Inveraray quartzites awakened accordingly much interest among geologists. His Grace soon discovered, however, that what he had at first believed to be fossils were only external markings due to the precipitation of hydrous peroxide of iron round the decaying stems of mosses, heaths, or other plants. These markings occurred indifferently on pieces of quartzite, mica-schist, gneiss, &c., and in no instance were found within the stone, but always on the surface. But in turning over the exposed blocks of quartzite the Duke found numerous ferruginous markings which undoubtedly occurred all through the interior of the rock. After quarrying away portions of the solid rock, collecting a large series of specimens, and comparing them with others obtained from the quartzite of Sutherland, he deemed himself in a position to announce the probably organic nature of these markings; and the paper which he communicated last week to the Royal Society of Edinburgh gave the results of his inquiries. The bodies which he regards as fossils are compared by him to the "annelid burrows" which form so prominent a feature in the quartzites of Sutherland and Ross. He recognizes in the Inveraray rock similar ovate sections, the position and form of each tube being marked by a ferruginous ring, which is well defined along its inner margin, but fades outward into a general discoloration of the stone. He points out that in the Inveraray rock, as in that of the North-West Highlands, there is a general tendency of these ovate bodies to lie in one prevalent direction; and though he admits that the rocks have been considerably disturbed and crushed, he cannot trace among them any evidence of such stupendous movements as have been described from Sutherland. Accordingly he is disposed to look upon the parallelism of the stripes into which he thinks the original tubes have been flattened as evidence of the direction in which the worms burrowed through the still soft sand.

Perhaps the most original and valuable part of the Duke's paper was the account which he gave of his own experiments on the habits of the common lob-worms of our present shores. He had watched the operations of these creatures on the beach of dark silt at Inveraray; had cut out portions of the silt with the burrows and mounds intact, and had these removed to his own drawing-room to enable him to watch them more attentively. He had likewise injected plaster of Paris into the vertical or winding passages made by the worms in the silt, and had thereby obtained casts of the interior of these tunnels. He exhibited a very interesting and valuable collection of specimens illustrating these researches.

Mr. Geikie, the Director-General of the Geological

Survey, opened the discussion, and regretted that, though he had enjoyed the advantage of seeing the large collection of specimens made by the Duke of Argyll from the Inveraray quartzite, and also of examining the rock *in situ*, he was still unconvinced that the markings were really of organic origin. It was possible, he thought, to trace a series of stages from single crystals or irregular groups of crystals of pyrites through variously shaped aggregates into the "ovate bodies" of the Duke. In the more solid, massive, and uncrushed portions of the quartzite, these aggregates could be seen quite fresh, and probably not far from their original shape. But wherever the rock had undergone shearing (and this was the case throughout most of its mass), its component particles had been drawn out in the direction of movement, the original irregular, rounded or egg-shaped aggregates of sulphide of iron had been flattened and elongated, becoming eventually mere strips that run parallel to each other. The trend of these strips exactly coincided with that of the long axes of the foliation-minerals in the surrounding rocks, and were regarded by Mr. Geikie as pointing to the results of shearing in the rock-mass and not to the burrowings of worms. The ferruginous rings seemed to him to be due to an oxidation and leaching out of the pyritous matter of the little mineral aggregates, as so often happens among the Carboniferous and Jurassic sandstones that contain ferruginous concretions. While he could not admit that the markings in the quartzite of Inveraray had yet been shown to be of organic origin, he thought it quite possible that the precipitation of the iron-disulphide had originally taken place in presence of decomposing organic matter, as in many blue muds of the present day, and that portions of such pyritous mud had been drifted into the sandy deposit which is now quartzite.

Mr. B. N. Peach, of the Geological Survey, was also unable to recognize organic forms among the Inveraray markings. He thought that the parallelism of these markings where they were most elongated, and their coincidence with the general line of shearing movement in the rock, cast doubt upon their having any connection with worm-burrows.

Mr. Murray, of the *Challenger* Expedition, who thinks that sandstone deposits generally are sub-aërial formations, was disposed to refer the so-called annelid tubes of the Sutherland quartzite not to the borings of marine worms, but to the remains of terrestrial plants that grew upon sand-dunes.

In a paper which followed this discussion, Mr. Geikie gave an account of the evidence supplied by the rocks of the Highlands of remarkable deformation by mechanical movements. Illustrating his remarks by a large series of specimens, he showed how the Cambrian conglomerate of Assynt had its pebbles of quartz drawn out and its original sandy mud converted into a fine micaceous schist; how the conglomerates of the Central Highlands had their quartz-pebbles flattened like buttons and drawn out in the direction of movement, while their envelope of original sand and mud had been changed into a quartz-schist; how the granular quartzite of Sutherland had been crushed and rolled out into a thoroughly schistose mass; how the coarse Archæan pegmatites had been likewise crushed down until their material had, as it were, flowed onwards so as now to show a close parallel to the "fluxion-structure" of many porphyries, and even to assume a finely laminated or shaly structure, and lastly, how the highly crystalline basic dykes of the most ancient gneiss of the north-west had been sheared and rearranged until they passed into the most perfect forms of sericite-schist. He adverted to the obviously sedimentary origin of the great mass of the rocks constituting the Highlands east of the line of the Great Glen, and mentioned that the recent work of the Geological Survey in tracing the great belt of limestones from the coast of Banffshire through the Grampians into Argyllshire afforded now a good horizon, from which it might be hoped the general structure of the Highlands might be worked out. He exhibited specimens of quartzite from Perthshire and other districts containing various markings, some of which there could be little doubt were of organic origin. He also showed a singularly interesting series of specimens which he had recently received from Dr. Reusch, of the Geological Survey of Norway, displaying recognizable trilobites and corals embedded in a finely crumpled micaceous schist, exactly similar in character to much of the schist that constitutes wide regions in the Scottish Highlands. These specimens afforded much encouragement to search for fossils in the calcareous and ferruginous layers and concretions that occur so frequently among our finer mica-schists and phyllites.

THE LAW OF STORMS IN CHINA.

THE law of storms in Hong Kong was investigated by aid of the lithographed paths of the typhoons in 1884 and 1885, published in "Observations and Researches made in 1886" and in those of 1886 and 1887 now in course of publication. Only those within 300 miles of the Observatory were considered in this connection. The angles between the wind and the radius vector, *i.e.* the line joining the Observatory with the centre of the typhoon, were measured and mean values derived, and the same was done for Victoria Peak (1816 feet above the sea) and for the lower clouds.

No connection could in any case be traced between the distance from the centre and the direction of the wind, but the latter depends upon the bearing of the centre. As pointed out in "The Law of Storms in the Eastern Seas" (NATURE, vol. xxxv. p. 136), and elsewhere, the wind has a tendency to blow along the southern coast of China when a typhoon is raging in the China Sea, so that the wind in such cases veers only about half as much while the typhoon moves westward as in other cases, and this is the reason why the angle between the wind and the radius vector is larger than usual when the centre is situated to the south of Hong Kong.

When there is a typhoon anywhere between north and east within 300 miles of the colony—which, however, is not common—the wind at the Peak (about north-north-west) blows away from the centre—much more so than the clouds, which in fact describe almost a circle round the centre in that case; and this remarkable feature or something very like it has been found to obtain also at Ben Nevis with the centre of a depression in the north-east.

The angle between the wind and the radius vector is, at the Observatory, 81° north of the centre, 55° to the west, 56° to the south, and 58° to the east. At the Peak, it is 91° to the north, 87° to the west, 81° to the south, and 78° to the east. At the level of the lower clouds, it is 92° to the north, 85° to the west, 67° to the south, and 86° to the east.

The observations made at South Cape (Formosa) were treated similarly, with the following result: to the north of the centre the angle was 50°, to the west 56°, to the south 64°, and to the east 47°.

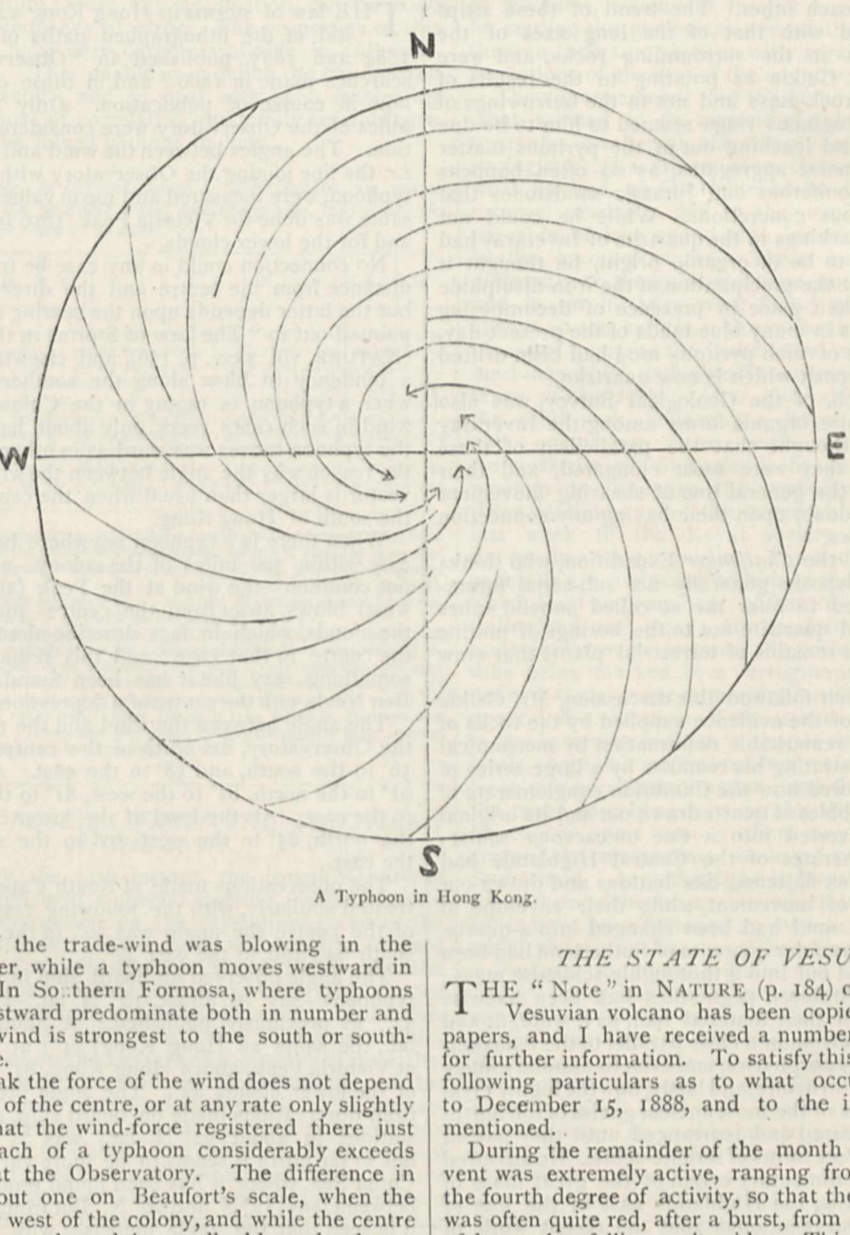
The angle observed on board ship in the China Sea in typhoons—say in about 16° N. lat.—is on an average 47°, as previously published; at South Cape (22° N. lat., the same as Hong Kong) 54°; at Hong Kong 62°; at Victoria Peak, above Hong Kong, 84°; and at the level of the lower clouds 82°. The angle previously obtained from observations made on board ship, and also at coast stations in about 32° N. lat., was 75° on an average, but so far north it seems to be more variable than in the China Sea, where it has been found remarkably constant, between (say) 12° N. and 20° N. But from the figures given it is seen that the angle increases with the latitude and with elevation above sea-level. The woodcut represents a typhoon in the neighbourhood of Hong Kong, or rather a mean of those observed during the four years. The diameter of the circle is 600 miles.

The average force of the wind according to Beaufort's scale (0–12), at various distances from the centre, expressed in nautical miles, is shown in the following table, but owing to the typhoons differing so much in size the figures representing the mean values are often widely different from the values obtained from observations made in a particular typhoon. That is not the case with the direction of the wind, which depends upon the bearing but not upon the distance from the centre or the size of a typhoon.

Distance.	Observatory.	Peak.	South Cape.	China Sea.
60	8	9	9	11
160	5	6	6	—
250	4	5	4	6

This table proves the wind to be strongest over the open sea, and also, though to a less extent, at some height above sea-level. The force of the wind increases at a greater rate on approaching the centre at sea, and that is to a certain extent likewise the case at South Cape, which is far from the mainland and soon reached by typhoons arriving fresh from the Pacific Ocean, where most typhoons originate, although some of them are formed to the westward of the Southern Philippines.

The force of the wind is much greater behind the centre than in the anterior semicircle both at sea and on shore, and the consequence is that the strongest blow is not experienced till the barometer begins to rise. For instance, at an average distance of about 160 miles, the mean force in Hong Kong is 7 to the north, 3 to the west, 6 to the south, and 5½ to the east. At South Cape (Formosa) it is 6 to the north, 5 to the west, 7 to the south, and 6 to the east. To the north of a typhoon the wind is remarkably fresh along the southern coast of China, even when the centre is over 300 miles away. It makes an



A Typhoon in Hong Kong.

impression as if the trade-wind was blowing in the middle of summer, while a typhoon moves westward in the China Sea. In Southern Formosa, where typhoons moving north-westward predominate both in number and in intensity, the wind is strongest to the south or south-east of the centre.

At Victoria Peak the force of the wind does not depend upon the bearing of the centre, or at any rate only slightly so. It follows that the wind-force registered there just before the approach of a typhoon considerably exceeds that registered at the Observatory. The difference in force is only about one on Beaufort's scale, when the centre is north or west of the colony, and while the centre is situated to the southward it usually blows harder at sea-level than on top of the Peak. W. DOBERCK.

THE STATE OF VESUVIUS.

THE "Note" in NATURE (p. 184) on the state of the Vesuvian volcano has been copied by many newspapers, and I have received a number of letters asking for further information. To satisfy this desire, I give the following particulars as to what occurred subsequent to December 15, 1888, and to the information above mentioned.

During the remainder of the month of December, the vent was extremely active, ranging from the second to the fourth degree of activity, so that the cone of eruption was often quite red, after a burst, from the large number of lava cakes falling on its sides. This constant ejection of fragments of red-hot pasty lava rapidly increased the height and size of the eruptive cone, making its slope exceedingly steep. So rapid was its growth, that the most casual observers noticed it from Naples, and discussed it. In fact, from November 1 to January 6 at least 20 metres was added to the height of Vesuvius, whilst the size of the base of the cone of eruption was proportionally increased.

On January 1, 1889, the eruptive cone burst on the north side, allowing the lava to issue and flow down, turning east and west so as to fill up, in part, the crescentic depression between the annulus or crater ring of July and August 1886, and the cone of eruption which is situated eccentrically to the former. As I have shown elsewhere, outflows of lava from the cone of eruption are always very limited, the violence of the outburst being generally proportional to the distance of the lateral opening below the summit.

After this relief the activity fell to the first degree, but soon again rose to the third. On Sunday, the 6th, I was standing at the summit of the mountain on the 1872 crater plain, preparing my apparatus for a photograph of the cone of eruption, when suddenly (about 3 p.m.), at about half-way down the side of the eruptive cone, and facing me, a slight puff of dust occurred, followed by the oozing forth of some lava. This rapidly increased in quantity as it carried forward the fragments forming the sides of the aperture. I immediately changed my lens to an instantaneous one, and took two negatives. The explosive activity increased, so that I was standing in a constant hail of red-hot lava fragments. These it required constant vigilance to avoid, and my face and hands were scalded by the radiant heat from the rapidly advancing lava, and tormented by the whirlwinds that always occur under such conditions. My two porters abandoned me, so that I had just time to remove my apparatus two minutes before lava flowed over the spot where I stood. In consequence of these unfavourable conditions, I lost some of my coolness, and allowed my camera-cloth to partly hide the lens. I was therefore greatly disappointed to find only part of this splendid scene registered for the eyes of others who had not had the good fortune to see that interesting spectacle.

The point of rupture was a few degrees east of south, and nearly opposite the cleft of January 1. The opening at beginning could not have been more than 10 metres from the top of the vent, showing the great height of the lava in the volcanic chimney. The outflow was very rapid, for, half an hour after, the place where I took the photographs could not be approached by 40 or 50 metres, having been all inundated with lava. Part of the fluid rock rapidly reached the edge of the 1872 crater plain, and flowed some distance down the slope of the great cone in the direction of Torre Annunziata, and another portion flowed out by another gap a little farther east in the remaining edge of the 1872 crater ring. The supply, however, soon stopped, and late in the evening had already become consolidated. After this, the activity, as seen from Naples, slightly diminished, but the next evening it was again at the third degree. Cloud-cap somewhat interrupted the view up till last night (January 12), when it was again observed to be at the third degree, and the light emanating from the lava was very white, showing the high temperature.

So far, the great cone has resisted fracture, but the south-west fissure, to which I have already drawn attention, is more active, and from this side of the crater plain there is very great fumarolic activity. When, therefore, the hydrostatic pressure overcomes the resistance, it will probably be in this direction that a lateral outburst will take place.

H. J. JOHNSTON-LAVIS.

Naples, January 13.

VOLCANIC SEA WAVE.

THE following account from the Berlin *Annalen der Hydrographie*, 1888, p. 518, with reference to the wave observed in the regions about the north-east of New Guinea, already briefly noticed in NATURE (vol. xxxviii. p. 491), is of interest.

The data given are too vague to permit of definite conclusions as to the probabilities of the disturbances felt at Sydney and Arica having originated in a volcanic eruption in New Guinea, but it may be observed that, assuming that the volcanic centre was from 200 to 400 miles north of Hatzfeldt Harbour, in which direction sounds were heard at 6 a.m. on the 13th, followed in forty minutes by a wave, the disturbance recorded at Arica at 5 p.m. on the 14th would have travelled the intervening distance of 10,000 geographical miles at a speed of 416 miles an hour, a velocity which agrees very fairly with the probable mean depth of ocean traversed.

To Sydney, on the other hand, assuming the first disturbance to have occurred at 6 a.m. on the 15th, the speed would only have been about 60 miles an hour, which is much too low a velocity for the depth.

It will be observed that the waves both at New Guinea and Arica were of short period, and in this respect quite unlike the long-distance waves emanating from Krakatão in 1883.

W. J. L. WHARTON.

"With regard to the extraordinary tidal wave that was observed in the Bismarck Archipelago, and on the coast of New Guinea, on the 13th of March, Heft iii. of the 'Notices of Kaiser Wilhelm's Land and the Bismarck Archipelago' relates as follows:—

"After the Expedition which had been undertaken for the discovery of Herren von Below and Hunstein, who had attempted an exploration to the west coast of New Pomerania (New Britain), had returned without finding any trace of them, a second Expedition, consisting of seven officers and fourteen Miocese, under command of the surveyor, V. Brixen, was despatched on the 17th of March from Finsch Hafen to the west coast of the above-mentioned island. This discovered, on the 18th of March, the spot where Below's Expedition had landed, which was easily recognized by the objects lying there partly covered with sand—a tent, torn pieces of clothing, and bent bits of metal. A part of the Expedition then repaired to a ruined village near the place where the missing persons (according to the account of the two Miocese who had been saved) had encamped during the night of the 12th-13th of March on the shore. At this place the land falls very steeply, about 25 metres, to the sea, and there is only a narrow strip of flat coast between the declivity and the sea. The tidal wave had even occasioned a landslip, large stones and trees being torn away from the slope, so that here escape could have been scarcely possible, and, according to the two Miocese, the catastrophe happened before daybreak. With the exception of a few bamboos cut by a knife, no trace of an encampment was perceptible. An excavation, attempted on the 19th of March, led to no result. Sea-sand, stones, and things washed up by the sea, covered the former level of the shore for more than 4 feet. On the 20th of March parties were despatched into the interior in a north-easterly and southerly direction, who came upon the encampment of the natives who had escaped from the above-named village. As these confirmed, by gestures and signs, the accounts of the Miocese, hardly any doubt can remain that Below and Hunstein had fallen victims to the tidal wave. On the 21st of March a large cross, therefore, was erected at the place of the misfortune, and, to provide for necessity, two boxes with provisions and drink were buried under a large and marked tree near the landing-place. The tidal wave on this portion of New Pomerania had rendered completely desolate a coast formerly covered with dense forest for a breadth of about 1 kilometre. Large spaces had been reduced to a swamp covered with trees heaped above one another, broken coral rocks, sea-sand, and a quantity of putrid fish. Measurements made at the declivities here give a height for the tidal wave of 12 metres (39 feet).

"As was to be expected, the tidal wave had also left its mark on other coasts of the German Protectorate, without having, however, caused any serious damage. In Hatzfeldt Hafen, on the coast of New Guinea, a noise like firing was heard on March 13, shortly after 6 a.m., from a north-north-easterly direction, and at 6.40 a.m. came an astonishingly high tidal wave from the north that rose 2 metres (6½ feet) above the highest flood-mark, and then receded with such violence that half the port was dry. The sea now rose and fell at intervals of three to four minutes, which lasted until 9 a.m.

"At 8 a.m. the height of the tidal wave stood at 7 to 8 metres (23 to 26 feet), so that the station was in imminent danger. In the course of the forenoon the movement gradually subsided, although the water still continued to rise and fall with steady intervals until 6 p.m., when it resumed its normal condition.

"In Kelana, the newly-established plantation near Cape King William, the phenomenon occurred from north-east at 6.30 a.m. The first wave forced itself 25 feet on the land the fourth, however, 35 feet; this was the greatest of the twenty waves observed, which came about every three minutes. The phenomenon was not observed here to be longer than an hour in occurring. No other circumstance of a striking nature was perceptible. The weather was calm and dull. On the morning of the 14th of March the whole coast to some distance was strewn with small pumice stones.

"From Matupi it was reported that from 8.15 until near 11 a.m. the sea receded at times from the island 12 to 15 feet below the lowest water mark, and then rose in several waves to the same height above high water mark. The phenomenon appeared chiefly on the south-east and north side of the island, the west side remaining untouched. The waves came partly from south, partly from west-north-west. The water appeared disturbed in its depths; it had a dark appearance, and carried discoloured foam. Neither earthquakes nor any subterranean rumblings were noticed. The weather was clear, with a gentle south-east breeze. On the south side of Gazelle Peninsula the phenomenon was also noticed by a ship ying at anchor.

"So far the report in the 'Notices of Kaiser Wilhelm's Land and Bismarck Archipelago.' Of the further movement of the tidal wave, nothing is as yet known, although it is not improbable that it spread further.

"In Sydney (Australia) and Arica (South America), extraordinary commotions of the sea were observed between the 14th and 17th of March, which may possibly have been in connection with this tidal wave. In Arica, as appeared in the *Mercurio* of Valparaiso of the 23rd of March, an immense wave was observed on the 14th of March towards 5 p.m., in the distance, which, increasing as it drew nearer, broke with great force near the pier. Three great waves followed quickly, one after another. Of the vessels busied in taking in cargo, several were shattered, and others capsized. The sea was for some time so agitated that the shipping of merchandize was attended with difficulty. On the island in front of the port the sea broke for a still longer period with great violence.

"According to the English journal NATURE (vol. xxxviii. p. 491), the tidal curves on the self-registering water-gauge in Sydney, showed on the 15th, 16th, and 17th of March, deviations from their customary form which may have been caused by the waves of an earthquake."

NOTES.

THE fifteenth general meeting of the Association for the Improvement of Geometrical Teaching was held on Saturday last at University College, London. After the reading of the Report of the Council, Mr. R. B. Hayward, who had been President for eleven years, resigned the presidency, and the post was conferred on Mr. G. M. Minchin, Professor of Applied Mathematics in the Royal Indian Engineering College at Cooper's Hill. In the place of Mr. Moulton, Q.C., Mr. Hayward was elected a Vice-President; while the other Vice-Presidents—the Rev. G. Richardson, Mr. R. Levett, and Mr. R. Tucker—retain their posts. In the course of his valedictory address, the retiring President remarked that, though they had not quite attained the expectations of some of their more ardent reformers, still they had met with a fair measure of success. Their influence was rather indirect than direct, and it must be expected that their advance would be, while steady, yet comparatively slow. The new President (Prof. Minchin) read a paper on "the vices of our scientific education."

IN the correspondence of Mrs. Austin, just published by her granddaughter, in the work entitled, "Three Generations of Englishwomen: Memoirs and Correspondence of Mrs. John Taylor, Mrs. Sarah Austin, and Lady Duff Gordon," there is an interesting and whimsical letter from Humboldt, dated Sans Souci, 1844. Mrs. Austin had written to him, suggesting a translation of his work, "Ansichten der Natur," into English. He jokes about the defects of his book from the translator's point of view, with its notes larger than the text, its "Teutonic sentimentality," and the impossibility of finding a title in English for it. He then proceeds:—"Alas! you have got someone in England whom you do not read, young Darwin, who went with the Expedition to the Straits of Magellan. He has succeeded far better than myself with the subject I took up. There are admirable descriptions of tropical nature in his journal, which you do not read because the author is a zoologist, which you imagine to be synonymous with 'bore.' Mr. Darwin has another merit —a very rare one in your country—he has praised me."

ON Tuesday, January 15, a new wing of the Leeds Mechanics' Institute, comprising the School of Science and Technology and Boys' Modern School, was opened by Sir James Kitson. The Leeds Mechanics' Institute was founded sixty-four years ago, and, like some other establishments of the same kind in various parts of the country, it has always recognized the need, not only from an intellectual but from an industrial point of view, of scientific education. Thanks, in part, to the wholesome influence of the Yorkshire College, Leeds, there has been lately in all the manufacturing centres of Yorkshire a remarkable growth of opinion as to the importance of this question; and the action taken by the Mechanics' Institute in adding to its buildings a set of rooms for scientific training must be regarded as a characteristic and eminently satisfactory sign of the times. The extension has involved a total cost of nearly £7000. The building is three stories high. The class rooms on the ground-floor will be used for the Boys' Modern School. On the first floor a series of rooms will be used jointly by the Boys' Modern School and the School of Science and Technology. On this floor there is a physics lecture theatre, with a sloping gallery capable of accommodating about fifty students. The metallurgical laboratory, the balance room, and the chemical laboratory occupy the second floor, which is reached by a wide stone staircase. In the former twenty-two students will be able to work at the same time.

EVERYONE who is in the habit of using Whitaker's Almanac is now familiar with the issue for 1889. It may not, however, be too late to say that the work as a whole is more valuable than ever, and that especial credit is due to the editor for the excellent sections on subjects relating to astronomy. We may call his attention to the fact that for some mysterious reason the office of NATURE is not included in the list of newspaper offices in London.

THE prevention of smoke formed the subject of an interesting discussion at a recent meeting of the Institution of Engineers and Shipbuilders in Scotland. Mr. G. C. Thomson, who had read a paper on the subject, said, in summing up the debate, that he would like to add to his paper the effect of a day's fog in London. For the twenty-four hours ending Thursday morning, November 17, 1887, the gas sent out by the Gas light and Coke Company was 103,664,000 cubic feet, or 35,000,000 excess over the same day in 1886. The gas was sold at 3*s*. per 1000 feet, and was equal to £15,500, so that the value of the excess in money equalled £5250—a sum that would go a great way in putting many of the faulty furnaces in London into good working order, so that they would give no smoke. Mr. Thomson also called attention to the loss of health and life which a foggy day always entailed on a community.

AT the meeting of the Royal Geological Society of Ireland on January 9, Mr. Kinahan communicated a paper, a general supplement to his previous articles on the "Economic Geology of Ireland." In this the suggestion as to the pre-Cambrian age of some of the Irish rocks is of interest. The author pointed out that it is highly improbable that any Irish rocks are equivalents of the American Laurentians or Huronians; but in reference to the "Gap rocks" of the epoch between the Huronian and Primordial, called by Chamberlain the Agnotozoic epoch, he suggested that possibly rocks of this age might be found. He mentioned the Bray Head series, which he would provisionally call "Oldhamians," as rocks that seem to be evidently older than the Welsh Cambrians; and as the Welsh Cambrians by their fossils seem to be the equivalents of the American Primordials, any rocks older than the Welsh Cambrians ought to belong to strata of Agnotozoic age. The suggestion that the Oldhamians are older than the Welsh Cambrians was founded on the profound break between them and the Irish equivalents of the Llandriloes; while in Wales the Cambrians pass conformably upwards into the Welsh Llandriloes. The author then pointed out that if the Oldhamians are of pre-Cambrian age (Agnotozoic) it is probable the rocks of North-West Mayo (Mullet) are similarly Agnotozoic, while it is possible, if not probable, that Griffith's older rocks in Ulster (Donegal, Tyrone, and Antrim?) may also be Agnotozoic.

THE Quarterly Record of the Royal Botanic Society for April-June 1888 contains the report of a lecture, by Mr. G. J. Symons, on sunshine. The subject is discussed chiefly from an instrumental point of view, under the heads of thermometric solar radiation, sunshine-recorders, and sunlight-recorders. Mr. Symons points out that Newton, in the seventeenth century, compared the readings of two thermometers, one in the sun and the other in the shade. De Saussure, in 1774, was the first to make an apparatus for direct observations upon the heat of sunshine, and, in 1837, the subject was taken up by Sir John Herschel, M. Pouillet and others. Their researches led to the use of the black-bulb thermometer *in vacuo*, while bright and black-bulb thermometers were used by Arago in 1844. This class of instruments was further improved by the Rev. F. W. Stow, in 1869. The first direct sunshine-recorder was designed by the late Mr. J. F. Campbell, and erected by him in Whitehall, in December 1854; it consisted of a mahogany bowl, with a hollow sphere of glass, nearly filled with acidulated water, to form a lens. In December 1857, a solid glass sphere was substituted; the observations were discussed by Profs. Roscoe and Stewart (Proc. Roy. Soc., June 1875) Finally, towards the end of 1879, after various experiments at Greenwich and Kew Observatories, Prof. Stokes designed the card supporter which is now used by the Meteorological Office and other institutions. The observations have been discussed by Mr. Scott for the years 1880–85 (Quart. Journ. Roy. Met. Soc., July 1885). Mr. Blanford stated, some years ago, that this instrument would give better results than the thermometric method, which has now been practically discontinued in India. Of the photographic sunlight-recorders, the principal are those by Mr. J. P. Jordan and Prof. McLeod. Another pattern has been designed by Dr. Maurer, and illustrated in *La Nature* for May 19, 1888, in which it is stated that the paper can be left unchanged for twenty days. Mr. Symons concludes his interesting lecture by remarks upon the action of light upon vegetation.

REPORTS of earthquakes have lately been received from many different parts of the world. At Flekberö, in the Torrisdal, in South Norway, there was an earthquake on December 27, 1888. Shocks were felt at Herisau, Zug, Frauenfeld, and Zürich, and at Wyl (Aargau), on January 1, at 5 a.m. A severe shock occurred at Constance on January 7, at three minutes to 12 noon. It continued for two seconds, and seemed to move from west to east. It was also noticed in several parts of North Switzerland; and at Wattwyl the shock was so severe that the inhabitants rushed out of their houses in terror. At St. Gallen, the pictures, curtains, &c., swung about on the walls, and the woodwork creaked. According to a telegram sent through Reuter's agency from Smyrna on January 21, a disastrous earthquake occurred last Thursday at Sparta (?), in Asia Minor, in which 300 houses were destroyed. A shock was felt at Athens on January 22, as well as at Megara and Arachova. It was accompanied by heavy rains and a violent gale.

ON Friday morning last, a shock of earthquake was felt in the county of Midlothian. Of this earthquake we may have something to say on a future occasion.

REFERRING to the Calcutta earthquake of December 23 last, which is said to have been the most severe felt since 1885, the *Englishman* says that it took place at 10.50 p.m., and lasted for about a minute and a half. It was severe enough to try the stability of substantial houses. Sleepers were awakened by the loud rattling of doors and windows, and pemka frames and lamps swung about in a curious fashion. So far as could be judged, the direction of the wave was from east to west. The disturbance was wide-spread, but appears to have varied in intensity in different places.

THE papers on "Modern Views of Electricity," by Prof. Oliver Lodge, which have been appearing in NATURE, will soon be published as a volume of the "Nature Series."

A WORK on "The Principles of Inductive or Empirical Logic," by Dr. John Venn, is about to be issued by Messrs. Macmillan. It contains the substance of lectures delivered in Caius College for a number of years past. The general treatment of the subject is somewhat more in accord with that adopted by J. S. Mill than with that of the majority of recent English works on logic.

MESSRS. MACMILLAN AND CO. have in the press Part I. of "A Graduated Course of Natural Science for Elementary and Technical Schools and Colleges," by M. B. Loewy. The author's object is to place the teaching of natural science in schools upon an exclusively experimental basis, and to make it at the same time thoroughly methodical and systematic, the scholar being led from known and easily-understood facts to less-known and more difficult results. In this way, it is thought instruction in science may be brought into close harmony with educational methods employed in other subjects of school teaching.

MESSRS. MACMILLAN AND CO. will also issue soon "Hydrostatics for Beginners," by F. W. Sanderson. The work is based on the author's experience in teaching physics to large classes of boys varying from 12 to 19, and in arranging and conducting each class in laboratory work.

A NEW compound, containing aluminium in a lower state of oxidation corresponding to ferrous iron, has been obtained by Prof. Hampe-Clausthal. It is a double fluoride of sodium and aluminium of the composition $2NaF \cdot AlF_2$. In the earlier experiments which resulted in the production of this interesting substance, cryolite, the natural fluoride of sodium and aluminic aluminium, $6NaF \cdot Al_2F_6$, was fused for several hours in a gas-carbon crucible along with a quantity of metallic aluminium. Air was rigidly excluded during the fusion, a current of hydrogen being led through the crucible by means of tubes inserted through the air-tight cover. The carbon crucible was protected from the direct flame of the furnace by means of an outer one o platinum. Under these circumstances the metal dissolved either wholly or in part, depending upon the amount present, in the melted cryolite; a little carbide of aluminium was at the same time formed and disseminated throughout the mass, especially aggregating near the surface, in minute particles, accompanied by small globules of the metal itself. After separation of these particles by various means as completely as possible, it was found that about half as much aluminium had entered into combination as that originally contained in the cryolite, indicating the probability of the course of the reaction being as follows: $6NaF \cdot Al_2F_6 + Al = 3(2NaF \cdot AlF_2)$. It was afterwards found, however, that by substituting a polished wrought-iron crucible for the carbon one, a product was obtained perfectly free from these particles, carbide of aluminium being no longer a possible product of the reaction, and the excess of metallic aluminium forming an alloy with the iron upon the walls of the crucible above the fused fluoride. Hydrogen was led through as before in order to exclude oxygen. 58 grammes of cryolite were fused for 5½ hours with 29·5 grammes of the metal; at the end of the operation the homogeneous white substance formed at the bottom of the crucible on cooling was submitted to analysis, with results which entirely confirm the above supposition, the numbers corresponding to the formula $2NaF \cdot AlF_2$. The new compound, which in outward appearance very much resembles cryolite, must therefore be considered as sodium aluminious fluoride. It may be remarked that all three elements were estimated, the sodium and aluminium in the ordinary manner, as chloride and hydrate, while the fluorine, which was found most difficult to determine satisfactorily and required prolonged treatment, was eventually weighed in the form of calcium fluoride. Now that an aluminious salt has at last been obtained, it is to be hoped that further attempts may follow, having for their object the formation of other compounds corresponding to the well-known salts of ferrous iron.

REFERRING to the discovery, last year, at Sandnaes, near Stavanger, in Norway, of enormous deposits of infusorial earth, at the time communicated by Lord Salisbury to the Royal Society, Prof. P. Waage, the well-known Norwegian chemist, is of opinion that this earth should be suitable for the preservation of food, after having been subjected to a process of intense heating, whereby all organic matter should be destroyed. Prof. Waage thinks that sterilized infusorial earth would be very much better as a means of preservati n than boric acid, &c., now used in the preservation of fish.

LAST autumn, a family of the hazel-mouse, *Myoxus avellanarius*, was discovered in a wood at Slagelse, in Denmark. It is said that the animal had never before been found in that country. The mice had made their nest—ball-shaped in appearance—of grass and leaves between the branches of a tree, 6 feet above the ground. It had a circular entrance-hole at the side. Two of the animals are now hibernating in the possession of a farmer.

THE Museum of the Christiania University has just been enriched with a Runic stone, hitherto unknown, found in the Romsdal. The writing is in older Runic characters, and very clear, but part of the stone is missing. It is believed to have been a memorial stone. Some years ago, a similar stone was found in the same locality, but the Runes are illegible.

THANKS to strict preservation, and to the fact that the inhabitants are realizing the value of the bird, the eider has greatly increased in number in Iceland during recent years. The people do all in their power to attract the bird to their property. Among these attractions are bells worked by the wind or by water, the hanging up of dress material of a glaring colour, and the keeping of brightly-coloured fowls. A Society has been formed for the granting of premiums for the killing of animals preying upon the eider, and, last year, 1155 such prizes were awarded.

THERE has been a very marked increase in the number of visitors to the South Kensington Museum during the last year, the numbers rising from 788,412 in 1887 to 897,225 in 1888. But this increase of 108,813 is quite put in the shade by that of 500,582 at the Bethnal Green Museum, which, in its total of 910,511 for the past year, has, as will be seen, distanced the parent institution. This great influx of visitors, more than double that of the previous year, was, no doubt, in great measure due to the exhibition there of Her Majesty's Jubilee presents after they had been shown to the West End at St. James's Palace. But some part of the increase must be attributed to the fine collection lent by the Hon. W. F. B. Massey Mainwaring. The increase in numbers at South Kensington was not confined to the main Museum, but extended to the separate collections—the Science Museum and the India Museum—which are in the galleries at the west side of Prince's Gate, and which are not open in the evening, as are, on three evenings in the week, the collections in the main building on the east of Prince's Gate, and the Bethnal Green Museum. The numbers visiting the Science collections increased from 177,465 in 1887 to 258,796 in 1888, notwithstanding the fact that the galleries have been severed by the new road cut across the Horticultural Gardens, while the visitors to the India Museum increased from 116,574 to 152,911. The numbers of visitors are taken in all cases by turnstiles.

WE have received several letters on "Hares Swimming." Mr. G. H. Kinahan writes that he believes the phenomenon to be not uncommon. "When I was stopping at Inver Lodge, Co. Galway," he says, "the keeper told me a hare had a nest with three young ones on a small island in the lake, and that it left them most of the day-time. I went with him to the island, which was about 30 yards in diameter, and about 100 yards from the shore, and after searching it most carefully, we could only find the three leverets. Sheep are animals that in general keep out of water, yet in Connemara I have seen them quite naturally swimming a river or even a lake." "E. H." writes:—"I was by the little River Arun below the old mill at Pulborough one day, when I saw a hare quietly cantering down the opposite field towards the river. A bank hid the actual crossing of the river from me; but when the hare emerged from the water into the field in which I was standing, I was amused to see the dog-like fashion in which it stood and shook off the moisture, scattering the spray far and wide, before resuming its leisurely canter. The act had the air of being habitual." Mr. G. Plarr, writing from Tunbridge, tells us of a hare which he saw many years ago while he was walking along a mill-stream in El-ass. The hare was being chased by some boys in a meadow on the opposite side of the stream. It disappeared in the water, and emerged on the side on which Mr. Plarr was walking. Without stopping to shake the water away, it made off with great speed. The creature presented a strange appearance, its head seeming to be large beyond all proportion to its body. This was, of course, due to the fact that the head had been kept dry above water, while the rest of the body had been immersed.

MESSRS. DULAU AND CO. have sent us a catalogue of zoological and palæontological works, including works on Echinodermata, Vermes, and Crustacea.

IN the letter on "Alpine Haze," by Antoine d'Abbadie (p. 247, lines 13 and 17 from the top), *for* "earth-haze" *read* "earth-ashes," and *for* "Ventouk" *read* "Ventoux."

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mrs. Henderson; a Rhesus Monkey (*Macacus rhesus* ♂) from India, a Brown Capuchin (*Cebus fatuellus*) from Brazil, deposited; two White Ibises (*Eudocimus*, sp. inc.) from Central America, purchased; a Rufous-necked Wallaby (*Halmaturus ruficollis* ♂) from New South Wales, received in exchange.

OUR ASTRONOMICAL COLUMN.

DISCOVERY OF A NEW COMET.—A faint comet was discovered on January 14, at 18h. 4·7m., by Mr. W. R. Brooks, of Geneva, New York. Its position at the time of discovery was R.A. 18h. 4m. 0s., Decl. 21° 20′ S. The comet was moving rapidly towards the west.

MINOR PLANETS.—Herr Palisa at Vienna discovered a minor planet on January 4, which may possibly be Siwa, No. 140. Should it be a new planet, it will be No. 282, and Herr Palisa's sixty-ninth discovery. Three minor planets, all discovered by Herr Palisa, have recently been named. No. 278 has been called Paulina; No. 279, Thule; and No. 280, Philia.

THE OBSERVATORY OF TOKIO.—An Astronomical Observatory has just been instituted at Tokio, Japan, by the combination of the astronomical portions of the old Naval Observatory and of the Home Office, together with the Astronomical Observatory of the Imperial University. The site of the old Naval Observatory has been selected for the new institution, the meteorological portion of the former having been transferred to the Central Meteorological Observatory of the Home Office. The principal instruments of the new Observatory are a Repsold meridian instrument of 5¼ inches aperture; a transit-circle, by Merz and Repsold, of 5 inches aperture; and two equatorials, the one by Troughton, of 8 inches, and the other by Merz, of 6½ inches aperture. The staff of the institution has not yet been fully organized, but Prof. H. Terao has been appointed Director, and has commenced regular observation. The approximate position of the Observatory is—longitude E. of Greenwich, 139° 44′ 30″·3; N. latitude 35° 39′ 17″·5.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1889 *JANUARY* 27—*FEBRUARY* 2.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on January 27

Sun rises, 7h. 47m.; souths, 12h. 13m. 5·4s.; sets, 16h. 39m.: right asc. on meridian, 20h. 40·9m.; decl. 18° 19′ S. Sidereal Time at Sunset, 1h. 8m.

Moon (New on January 31, 9h.) rises, 3h. 54m.; souths, 8h. 22m.; sets, 12h. 43m: right asc. on meridian, 16h. 48·9m.; decl. 19° 10′ S.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	8 30	13 24	18 18	21 52·5	13 19 S.
Venus ...	9 18	15 7	20 56	23 35·8	2 57 S.
Mars ...	9 6	14 34	20 2	23 2·4	7 2 S.
Jupiter ...	5 29	9 24	13 19	17 51·9	23 5 S.
Saturn ...	17 24*	0 56	8 28	9 21·9	16 37 N.
Uranus ...	23 33*	4 56	10 19	13 22·3	7 59 S.
Neptune..	11 39	19 22	3 5*	3 50·9	18 25 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Jan.	h.	
28	9	Jupiter in conjunction with and 1° 42′ south of the Moon.
30	14	Mercury at greatest elongation from the Sun 18° east.
Feb.		
1	15	Mercury in conjunction with and 4° 24′ north of the Moon.
2	22	Mercury at least distance from the Sun.
2	23	Mars in conjunction with and 3° 57′ north of the Moon.

Variable Stars.

Star.	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·5	81 17 N.	Jan. 28,	20 31	*m*
			Feb. 2,	20 11	*m*
λ Tauri	3 54·6	12 11 N.	Jan. 29,	0 11	*m*
			Feb. 1,	23 3	*m*
η Geminorum	6 8·2	22 32 N.	Jan. 30,		*m*
ζ Geminorum	6 57·5	20 44 N.	,, 28,	3 0	*m*
			Feb. 2,	3 0	*M*
R Canis Majoris	7 14·5	16 11 N.	Jan. 28,	19 8	*m*
		and at intervals of		27 16	
U Monocerotis	7 25·5	9 33 S.	Feb. 2,		*m*
S Cancri	8 37·6	19 26 N.	,, 1,	20 7	*m*
T Vulpeculæ	20 46·8	27 50 N.	Jan. 29,	22 0	*m*
			,, 31,	0 0	*M*
Y Cygni	20 47·6	34 14 N.	,, 28,	17 40	*m*
		and at intervals of		36 0	
W Cygni	21 31·9	44 53 N.	Feb. 2,		*M*
δ Cephei	22 25·0	57 51 N.	,, 2,	22 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near τ Leonis	169°	4° N.	Very swift.
,, α Coronæ	235	26 N.	Very swift; streaks.

GEOGRAPHICAL NOTES.

THE International Geographical Congress will be held at Paris from August 5 to August 10 next. The Geographical Societies of London, Berlin, Leipzig, Manchester, Edinburgh, New York, Melbourne, Lisbon, Antwerp, and Milan, and many French Societies will be represented at the meeting.

M. EUGENE MARKOW sends to the French Geographical Society an interesting account of his recent ascent of Mount Ararat. He and his companion, after passing the night among the rocks at a height of 13,000 feet, began their ascent on August 13, at 5 a.m. Soon they passed a conical rock which rises on the south-east face of Ararat, and here M. Markow places the limit of perpetual snow on the mountain. From the base of the rock extends in an opposite direction a broad plain of *névé*, which reaches the summit at a slope of 35°. Underneath this *névé* was heard the sound of a stream formed of the melted ice. At the height of 14,800 feet, M. Markow found among the rocks a *Coccinella septempunctata* of a very bright red. At 15,500 feet, some flowers were found on a small sandy eminence. At 2 p.m. the party reached the foot of the sacred summit of the mountain. This summit presents a vast extent of snow, separated into two by a precipice commencing on the north-east side, and getting broader and deeper as it reaches the south-east. Part of the right summit is almost entirely free from snow, and is covered with small stones. The left summit, on the north-west, presents a plateau covered with snow, but having a small elevation in the middle. It is much larger than the right summit.

A pyramid is being raised on the summit of the mountain, which it is hoped will dissipate the superstition among the natives below that no one can ever reach the top.

CAPTAIN TRIVIER, a French naval officer, has left France to undertake an expedition across Africa. One of his chief objects will be the exploration of the Lualaba and its tributaries, and more especially the Lukuga, the outlet of Lake Tanganyika, about the real nature of which there has been so much controversy. Captain Trivier will follow the Lukuga to the lake, and make a careful report on its actual condition. Captain Hore, who has been more than ten years on the lake, has just returned home. He states that during the whole period of his stay on the lake it has steadily decreased in size; its level has fallen quite 15 feet, but the Lukuga still flows out with a rapid current. This it will continue to do until its muddy bed is worn down to the rock, when it will cease to be an outlet of Tanganyika. Captain Hore seems to think the lake will go on decreasing in size. Captain Trivier will cross to Ujiji and endeavour to make his way to the east coast at Bagamoyo.

THE first number is to hand of the *National Geographic Magazine*, the organ of the recently founded "National Geographic Society" of the United States. The work of the Society, of which all the leading officers of the U.S. Survey are members, promises to be of much higher scientific value than that of the American Geographical Society of New York. The Society has 200 members. Among the articles in this number are "Geographic Methods in Geologic Investigations," by Mr. W. M. Davis; "Classification of Geographic Terms by Genesis," by Mr. W. J. McGee; "The Survey of the Coast," by Mr. H. G. Ogden; and the "Survey and Map of Massachusetts," by Mr. Henry Gannett.

CAPTAIN WAHAB reports as follows regarding the survey work accomplished by himself and one sub-surveyor while with the Hazara field force:—"Up to the end of the Ahazai country we have a complete survey extending a good way west of the Indus, and a certain amount of reconnaissance work in the Chagarzai country up to about Judbai. North-east of the Black Mountains we have surveyed from Nandihar (the limit of this survey in the 1868 expedition), north to the range beyond Allahi, and west to the hills overlooking the Indus. We have fixed the course of the river up to say 15 miles north of Thakot, and I have sketched, on the ¼-inch scale, as much as possible of the country between the Indus and the Swat Valleys, what I could see from the Chel Mountain and the Ghorapher Pass. I have made three stations, and fixed a number of points in the lower ranges between the snowy peaks fixed by the Great Trigonometrical Survey and our frontier, which I hope may be useful on future occasions. While I was triangulating on the top of Chel, Imam Sharif went down the hill to Pokal, for the day, and got in most of the Allahi Valley. There is a gap in the survey of the Indus Valley from the bend west of Thakot down to Judbai, which cannot be seen without going into the Chagarzai country, but even if we do not go, I have got the course of the river practically fixed within about half a mile one way or the other."

ELECTRICAL NOTES.

F. KOHLRAUSCH has just made a fresh determination of the ohm. He makes it equal to 106·32 centimetres of mercury 1 square millimetre in section.

PROF. ROWLAND has made preparations to repeat his classical Berlin experiment by which he demonstrated the fact that a static electrical charge in motion acted like a current. He is going to use higher speeds and higher electrification, and it is therefore hoped that he will get accurate quantitative determinations.

AT Paisley, an electric discharge, which seriously damaged a chimney and its defective lightning-conductor, also killed a quantity of fish that were in a pond close by in which the conductor was earthed. When will people take the precaution to examine their lightning protectors?

DUBS (*Centralblatt für Electrotechnic*, 1888, No. 28) has shown that a strong blowpipe or oxyhydrogen flame from one carbon to another sets up an E.M.F. which would fully account for the opposing E.M.F. of the arc.

A NEW mode of regulating dynamos for constant current and constant potential has been devised by the Waterhouse Company in the United States by means of a *third brush* slightly in advance of the positive brush, and an external variable shunt circuit which can be adjusted automatically or by hand. The desire to evade patents has at least one merit—it exercises the faculty of invention and stimulates design. The *Electrical Engineer* of New York (December 1888) has an excellent paper by Mr. Caldwell on this third brush.

IT is sometimes asserted that the unit of work—the *erg*—is too small to be of any use, but Prof. Langley has shown that the perception of the colour crimson is produced by an expenditure of energy upon the retina, which can be represented by 10^{-13} horse-power, or 0·001 of an erg; while the sensation of green is due to 0·00000001 of an erg.

HELMHOLTZ has shown that if an invisible jet of steam be electrified or heated it becomes visible with bright tints of different colours according to the potential or the temperature.

DR. GORE, F.R.S., has submitted to the Royal Society a new instrument of research, which he thus describes:—"Take two small glass cups containing known volumes of distilled water. Form two voltaic cells of them by means of strips of stout wires of unamalgamated zinc cut from the same piece, and two small sheets of platinum, also cut from the same piece. Connect them together in series to a sufficiently sensitive galvanometer, so that the currents from the two cells oppose each other, and produce no visible deflection of the needles. This arrangement constitutes a 'voltaic balance,' and is extremely sensitive to change of chemical composition of the liquid in one of the vessels. Make an aqueous solution of known strength of the substance, and add it in sufficiently small quantities at a time to the water in one of the cups until the needle of the galvanometer visibly commences to move, and note the proportion of the substance and of water then contained in that vessel. As the amount of energy required to move the needle is the same in all cases, the different numbers thus obtained with different substances represent the relative amounts of voltaic energy of those substances. And as each substance and mixture of substances gives a different number, it is possible by this method to detect substances, to ascertain the degrees of strength or concentration of liquids, to ascertain whether a substance contains a soluble impurity, &c. The method also is in many cases an extremely sensitive one."

PROF. J. J. THOMSON (R.S., January 17, 1889) has examined the screening influence of conducting plates upon alternating currents of great frequency, and has deduced thereby the resistance of electrolytes and of graphite. He shows that the screening effect depends on the conductivity and thickness of the plate and upon the frequency of the alternations. The secondary induced currents are confined to the skin of the plate next to the primary, the thickness of this skin varying inversely with the conductivity of the plate and the frequency of the currents. Thus a thin plate of badly conducting material will be efficient with currents of great frequency, such as those of the rate 10^8 per second; while a thick plate of the best conducting material will not be sufficient to screen off currents of low frequency, such as those with a rate below 10^2 per second. Thus to measure the resistance of electrolytes it is necessary to have vibrating electrical systems such as those examined by Hertz, whose frequency is of the former class; and if two different plates produce the same screening effect, their thickness must be proportional to their specific resistances. He supports Maxwell's theory that the rate of propagation of electrostatic potential is practically infinite, a point called in question by Hertz; and he agrees with Hertz that the rate of propagation of electro-dynamic action is finite and measurable. He shows that the rate of propagation of an electro-magnetic disturbance through a metallic conductor and through the surrounding dielectric is the same, and this differs from one of Hertz's conclusions. But he also shows that this is not so when the conductor is a dilute electrolyte or a rarefied gas. In such a case there would be interferences and standing vibrations. Hence the striæ in so-called vacuum-tubes. He also concludes that the relative resistance of electrolytes is the same when the current is reversed a hundred million times a second as for steady currents.

SOMEONE in the United States has proposed to call static electricity "amberism." It is a good analogue to "galvanism" and to "magnetism." It would be well to introduce some term to relieve the word "electricity" from the dreadful abuse to which it is now subjected. The Board of Trade in their draft Provisional Orders are using it in three distinct and different senses: (1) for electrical energy, which is measured in *watts*; (2) for electric currents, which are measured in *amperes*; and (3) for electrical quantity, which is measured in *coulombs*.

JOÉ has discovered that the resistance of cobalt in a magnetic field is *increased* in the direction of lines of force, and *diminished* in directions at right angles to them.

STAR NAMES AMONGST THE ANCIENT CHINESE.

IN two recent numbers of the *Chinese Review* (vol. xvi. Nos. 5-6) the well-known scholar, Dr. Joseph Edkins, writes on the subject of star naming amongst the ancient Chinese. He says that there are two great periods of star naming in ancient China, the first being about B.C. 2300, and the second during the Chow dynasty from B.C. 1120 to B.C. 220. The real beginning of Chinese astronomy is, in Dr. Edkins's opinion, to be found in the period preceding B.C. 2300, about which date, by command of the Emperor Yan, the observation of the meridian stars was made. Amongst primitive Chinese observers our Scorpion was a dragon, Aquarius a serpent or tortoise, Taurus a tiger, and Leo a bird. These figures were, however, larger than our zodiacal signs; for instance, the chief portion of Virgo, Leo, and Cancer would form the Red Bird. At that remote period we find that Chinese astronomers divided the heavens into four large sections, and twenty-eight small groups or constellations. The former, the large ones, are all animals, and are arranged from east to west, while the constellations are arranged from west to east. There were seven eastern constellations forming the Green Dragon—which comprised the stars in Libra, Scorpio, and Sagittarius; seven southern constellations, the Red Bird, or *Feng-hwang*—comprising Cancer, Leo, and Virgo; seven western constellations, the White Tiger—made up of Aries, Taurus, and Gemini; and the seven northern constellations, the Dark Warriors—or the Serpent or Tortoise. Each group, whether large or small, had its Chinese name. The Red Bird or Pheasant is the constellation of summer or the south; the Dragon, of spring or the east; the Tiger, of autumn or the west; and the Serpent or Tortoise, of winter or the north. Since the Great Bear points to Spica Virginis, the Chinese astronomers made the group led by Spica the group of spring. Another reason for thus making Spica the gate of the year is, perhaps, to be found in the fact that the Babylonians, from whom the Chinese probably got their astronomy, for a long time regarded Scorpio as the first of the signs. This is, of course, a mere guess, for we cannot, after this lapse of time, tell how much of the astronomical knowledge of the Chinese is derived from external sources. On the probable Babylonish origin of some of the astronomical knowledge of the Chinese, Dr. Edkins says:—"The contests of the early Buddhists with the worshippers of fire show that the Persian religion was propagated in India during and after the sixth century before Christ, and the eagerness with which the Hindus adopted the Greek astronomy after Alexander's invasion of India, as well as our knowledge of the fondness of the Buddhists for astrology, make it probable that Babylonian ideas on the stars were familiarly known in ancient India, during the period when they became popular in China. The resemblance of the cosmogony of the laws of Manu to that of the Babylonians seems to support strongly the correctness of the statement that Babylonian astrology was accepted at the same time in ancient India and in ancient China." With regard to the names of the four zodiacal signs, they are, as we have seen, those of animals, and it is peculiar that they are all Chinese animals but the Dragon, and it is not known that any species of dragon ever existed in China. In the naming of the constellations a wider field is included. Thus, the following are found: parts of the body, as heart, stomach, lips; buildings, a house, a wall, a well, a tower; articles of daily use, a peck measure, a net, a carriage; animals, K'wei K'ien (a humped boar leading a cow to sacrifice); adjectives and numerical groups, &c. From these names it appears that the origin of the appellations was popular rather than Imperial. In B.C. 1144, Wen Wang began to write the treatise called "Yi King." The adoption by Wen Wang of red as the Court colour of the Chow dynasty, and the fact that his son introduced five colours into the sacrifices, show that the Babylonish doctrine of the five colours and the five planets was known in China at that time. There are, however, variations in the colours. Thus, Mars is red in both China and Babylon; Jupiter, orange in Babylon, blue or purple in China; Venus, yellow in Babylon, white in China; Mercury, blue in Babylon, black in China; Saturn, black in Babylon, yellow in China. The "Yi King" shows that the stars were divided into four groups from the earliest times, for the Dragon and the Tortoise lie at the root of all the divination of that work; and the Tiger and Red Bird are respectively assigned to the west and the south. Shortly after the date of "Yi King" we find the following points mentioned: the cycle of twelve years, dependent on a revolution of Jupiter; the twelve hours into which the horizon is divided by the pointing of the Bear; the cycle of ten days; the cycle of twenty-eight constellations; the four seasons; the sun, moon, and planets. Astrology was, of course, implicitly believed in; in fact, the end and aim of all ancient Chinese astronomy was astrology. The conjunction of the sun and moon controlled the good and bad luck of the Empire, and particular stars foretold the fortunes of the various portions of the Empire, for each province had its presiding star. During the Chow dynasty—that is, after B.C. 1120—many constellations are named. Thus the fifth Emperor ordered a group of stars in Cepheus to be called Tsau-fu, after his favourite charioteer. Wang liang was also a charioteer about B.C. 470, and his name was given to a number of stars in Cassiopeia. The virtues of a duke of the Tsi kingdom who died in B.C. 488 were so great that a star was called after him. Unlike the old names, all of which seemed to denote a popular origin, those named during the Chow dynasty show their Imperial origin. Thus several stars in Leo were styled Wu-ti-tso—that is, "throne of the five emperors." During the second century before the Christian era, Chinese astronomers pointed out the five emperors. The chief ruler of Heaven is the ancient pole, the star Tai-yi, 22° from our present pole. The seven stars of the Great Bear are the Government—rulers of the sun, moon, and five planets. The palace of the heavenly emperor is bounded by the oval formed of the fifteen stars of Draco, amongst which is Tai-yi. At the back of the bear is the group Wen Ch'ang Kung, "the palace of literature brilliantly spread abroad," the favourite object of the adoration of the *literati*. The abode of the eastern emperor is in Scorpio. The group containing Antares is Ming-t'ang, the council-hall of the emperor, where he give laws to his subjects. The adjoining stars are the sons of the emperor. The palace of the emperor is Arcturus, and the two large stars in Centaur to the south of Sagittarius form the south gate of his dominions. In Cancer and Leo lies the residence of the southern emperor. One group is the palace of the sun, moon, and planets, and surrounding this group is a guard of twelve feudal barons who keep the throne of the five emperors. Between Procyon and Regulus, and between the ecliptic and equator, there is a group in Hydra called the willow-branch, which rules over planets, and forms the beak of the Red Bird. The constellations of the Seven Stars adjoin this, and form the neck of the Red Bird: its crop is the kitchen of the palace; Hydra forms the bird's wings; the constellation Yi is the imperial hotel where visitors at the palace are accommodated; the constellation Corvus finishes the shape of the Red Bird, and is the last in the zodiac. The seven western constellations—that is, those made up of Aries, Taurus, and Gemini—are "the lake of fulness," "the five reservoirs of heaven," "the home of the five emperors." Hyades is "the announcer of invasion on the border." Later on—that is, probably about the second century—the stars are grouped into three principal sections, the first section containing the circum-polar stars, the second stars in Leo and Virgo, the third twenty-two stars in Serpens, Hercules, and Ophiuchus, the latter being said to be feudal rulers paying homage to the Emperor. The whole history, in fact, of Chinese astronomy is full of this comparison of the state of the kingdoms on the earth with the heavenly bodies. Thus, under the Tsin dynasty, the pole star is the abode of the supreme ruler. "The circum-polar stars form his court. Their name as a whole is the 'purple subtle inclosure.' The stars selected to represent the emperors of the five colonies" (*i.e.* blue, red, yellow, white, and black) "were Denebola and four others in Leo. They are surrounded by twelve groups, which have received names of office and rank representing together the court of an earthly emperor. This inclosure is the court, especially, it is said, of the yellow emperor, whose essence is called Han-shu-nieu. The four remaining colours are near him. The blue emperor is Ling-wei-yang. The red or south emperor is Chi-piau-nu. The white emperor of the west is Pe-chan kü, 'the white beckoning mason's rule.' The north or black emperor is

Hie-kwang-ki, 'mark of combining light.' Besides this palace in Leo and Virgo, there is another, Tien-shï-yuen, 'inclosure of the heavenly market.' It is not far to the north-east of Scorpio. It is the serpent in our astronomy. Within the brilliant circle of the serpent is a star called 'court of the western heaven.' There is also a bright star, α Herculis, which is called 'emperor's throne.' The twenty-two stars in the Serpent are named after the States into which China was formerly divided."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Among the numerous lectures on physics and chemistry this term, we note those of Prof. Liveing, on spectroscopic chemistry; Prof. Dewar, on physical chemistry; Mr. Pattison Muir, on chemical affinity; Mr. Robinson, on agricultural chemistry; Mr. Heycock, on chemical philosophy; Prof. Thomson, on electricity and magnetism, and on the kinetic theory of gases; Mr. Shaw, on thermodynamics and radiation; and Mr. Wilberforce, on dynamo electric machines (continuous current generators and motors). Prof. Stuart lectures on theory of structures.

Prof. Foster continues his elementary course of physiology; Dr. Lea his chemical physiology; and Mr. Langley his advanced histology and physiology.

In zoology, Prof. Newton lectures on the geographical distribution of Vertebrates. Mr. Sedgwick and Mr. Darwin conduct the large class of elementary biology. Mr. Gadow's course is on the morphology of the Amniota (recent and extinct). Mr. Sedgwick, Mr. Harmer, and Mr. Weldon continue their classes on the Invertebrata.

Mr. Darwin lectures on the physiology of plants (advanced), Mr. Gardiner has a general elementary course, Mr. Vaizey lectures on the morphology and classification of Cryptogams, and Dr. Hicks on elementary botany.

The lectures on geology are divided thus: Prof. Hughes, geology of a district to be visited at Easter; Mr. Marr, principles, and geology and scenery; Mr. Harker, petrology; Mr. Roberts, palæontology; Mr. Seward, palæobotany.

The principal mathematical lectures are the following: Prof. Stokes, semi-convergent series involving powers of a complex variable; Prof. Cayley, analytical geometry; Prof. Adams, lunar theory; Mr. Pendlebury, projective geometry; Mr. Glazebrook, hydrodynamics (waves and sound); Mr. Hobson, spherical and cylindrical harmonics; Mr. Larmor, geometrical optics and electro-magnetism; Mr. Forsyth, modern algebra (binary forms); Dr. Ferrers, elliptic functions; Dr. Besant, analysis; Mr. H. M. Taylor, higher plane curves; Mr. Webb, dynamics (elasticity and viscosity); Mr. Stearn, hydrodynamics (multiply-connected velocity-potentials and vortices); Mr. Herman, hydrodynamics (viscous and gravitating fluids).

An examination will be held at Gonville and Caius College on March 15 for one Shuttleworth Scholarship, value £60 per annum for three years. Candidates must be medical students of the University of not less than eight terms standing. In the case of candidates not already scholars of the College, the examiners may recommend at the same time for a foundation scholarship. Further particulars may be obtained from the tutors.

SCIENTIFIC SERIALS.

American Journal of Science, January.—The history of a doctrine, by S. P. Langley. This is the address delivered last year to the American Association for the Advancement of Science, here published complete with the notes that have not hitherto appeared. Its object is to show the steady progress of scientific truth, as illustrated by the history of the undulatory and corpuscular theories of light from the time of Descartes, Boyle, and other precursors of Newton down to the present day, when the identity of radiant light and heat as forms of motion, or as different effects of radiant energy, has been finally established.—Description of the new mineral beryllonite, by Edward S. Dana and Horace L. Wells. This is a new phosphate of sodium and beryllium discovered in 1886 by Mr. Sumner Andrews near Stoneham, Maine, the same district that has already yielded fine specimens of phenacite, herderite, and other rare minerals. It occurs mostly as a crystal in a fragmentary state, of small size and seldom well formed, but remarkable for the number of planes they present, eight or more distinct planes being frequently presented in each zone on a single crystal. Twins are common, leading to many curious variations of form. The crystals are colourless, or slightly yellowish, and transparent, with specific gravity 2·845, and hardness 5·5.—The iron ores of the Penokee-Gogebic series of Michigan and Wisconsin, by C. R. Van Hise. The author's recent explorations of this region confirm Prof. Irving's conclusion that the original rock of the iron-bearing formation is a cherty iron carbonate from which the various phases of rock and the ore found in it have been produced by a complex series of alterations. The iron ore is a soft, red, somewhat hydrated hæmatite, more or less manganiferous, and mostly very friable.—A quartz-keratophyre from Pigeon Point and Irving's augite-syenites, by W. S. Bayley. The remarkable bright red rock of Pigeon Point, Minnesota, is here studied in its various phases, with the general result that the sections described by Irving as augite-syenites are partly identical with the typical red rock itself, and partly the same in all essentials as the formations which have been called its intermediate varieties. The space between the fresh olivine-gabbro and the typical quartz-keratophyre is occupied by a series of rocks exhibiting a gradual transition between the heavy dark basic rock and the light red keratophyre.—On the occurrence of hanksite in California, by Henry G. Hanks. This anhydrous sulphate of soda has hitherto been found in limited quantities amongst the various borax fields of California. But the author's researches tend to show that it exists in great abundance, and that it plays an important part in the metamorphoses that produce gay-lussite, thinolite, and perhaps borax.—Further papers on Mount Loa are contributed by James D. Dana and the Rev. E. P. Baker, bringing its history down to July 1888.—H. L. Wells and S. L. Penfield contribute notes on the new mineral sperrylite.

American Journal of Mathematics, vol. xi. No. 2 (Baltimore, January 1889).—The number opens with an instalment of a memoir entitled "Remarque au sujet du théorème d'Euclide sur l'infinité du nombre des nombres premiers," by J. Perott (pp. 99–138). A footnote supplies bibliographical information as to previous memoirs on the same subject.—Next, Prof. Cayley writes on "The Theory of Groups" (pp. 139–57), a subject he has pretty largely written upon before, and to which his attention has been recalled by the section, in Mr. Kempe's Philosophical Transactions memoir "On the Theory of Mathematical Form," entitled "Groups containing from One to Twelve Units." The paper is largely illustrated by what the author styles "colour groups."—Mr. A. E. H. Love discusses "Vortex Motion in certain Triangles" (pp. 158–71), by a method explained by Dr. Routh in a paper in vol. xii. of the London Mathematical Society's Proceedings.—Another hydrodynamical paper follows, by Mr. Basset, "On the Steady Motion of an Annular Mass of Rotating Liquid" (pp. 172–81), wherein he follows up previous work in the line of Poincaré's and Prof. G. H. Darwin's recent investigations of the figures of equilibrium of rotating masses of liquid. The case considered is for an approximately circular cross-section and for rotation under the influence of its own attraction about an axis through its centre of inertia, which is perpendicular to the plane of its central line.—A paper, by Sophus Lie, "Die begriffe Gruppe und invariante" (pp. 182–86), is reprinted from the *Berichte der k. Sächs Gesellschaft der Wissenschaften*, August 1887.—A short note, by E. Picard, "Sur les formes quadratiques binaires à indéterminées conjuguées et les fonctions fuchsiennes" (pp. 187–94), closes the number. The method employed is that used by Poincaré in his memoir on fuchsian functions (*Journal de Jordan*, 1887).

SOCIETIES AND ACADEMIES.

LONDON.

Royal Meteorological Society, January 16.—Dr. W. Marcet, F.R.S., President, in the chair.—The Report of the Council showed that a large amount of work had been done during the past year, and that considerable progress had been made in the investigation of one of the most interesting and hitherto neglected branches of meteorology, viz. thunderstorms. Forty-nine new Fellows were elected last year, the total number on the books now being 525.—After the Report had been adopted, the President delivered an address on "Fogs," which he illustrated by a number of interesting lantern slides. Fogs and clouds are one and the same thing. A cloud is a fog when entered into, and a fog seen from a distance, suspended in the air, becomes a cloud. After describing the various kinds of fog—*e.g.* river, sea, Newfoundland, radiation, town, &c., fogs—Dr. Marcet referred to London fogs. Dr. Tyndall has accounted for them by assuming each particle of condensed vapour to be covered by coal-smoke. These fogs usually accompany a high barometer, and are frequently dry in their character. It is a well-known fact that cold air on the tops of hills, being heavier than the air below, slides down the slopes, so that the lower parts of the hill-sides are actually colder than the plains at some distance from the hills. Now, London, in the Thames Valley, is surrounded by hills—to the north, Highgate, Hampstead, and Harrow; in a westerly direction, Putney and Wimbledon; and in a more southerly direction, Clapham and Sydenham. The air is colder on these hills than in London with its millions of inhabitants, its coal-fires and factories, hence it is heavier, and will have a great tendency to slide down the hills towards the town and the river. Should the air in town be on the point of saturation, and the cold air from above saturated with vapour, it is obvious that the increased cold from above will produce a precipitation of moisture, and it will come to pass that a fog is produced. If the hill-tops be not only colder than the air below, but enveloped in a fog, it stands to reason that the fog below will be all the denser, and especially in the neighbourhood of water, such as the River Thames and the ornamental waters in the parks.—The following gentlemen were elected the officers and Council for the ensuing year:—President: Dr. Wm. Marcet, F.R.S. Vice-Presidents: Francis Campbell Bayard, Henry Francis Blanford, F.R.S., William Ellis, Richard Inwards. Treasurer: Henry Perigal. Trustees: Hon. Francis Albert Rollo Russell, Stephen William Silver. Secretaries: George James Symons, F.R.S., Dr. John William Tripe. Foreign Secretary: Robert Henry Scott, F.R.S. Council: Edmund Douglas Archibald, William Morris Beaufort, Arthur Brewin, George Chatterton, William Henry Dines, Frederic Bernard Edmonds, Charles Harding, Baldwin Latham, Capt. John Pearse Maclear, R.N., Edward Mawley, Henry Southall, Dr. Charles Theodore Williams.

Zoological Society, January 15.—Prof. Flower, F.R.S., 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 18 8, and called attention to the young Chimpanzee purchased of Mr. Cross, of Liverpool, on December 6 (see NATURE of January 10, p. 254).—A letter was read from Heer F. E. Blaauw, of Amsterdam, containing an account of the development of the horns of the White-tailed Gnu as observed in specimens bred in his menagerie.—Prof. Newton exhibited a specimen of *Pennula millsi*, Dole, brought from the Sandwich Islands by Mr. S. B. Wilson, remarking that it seemed to be identical specifically with *Rallus obscurus*, Gmelin, a species which has not been lately recognized.—Prof. Bell made some remarks on the question of the food of *Bipalium*.—Canon Tristram made some remarks on a specimen of *Emberiza cioides*, a Bunting of Siberia, of which a specimen was believed to have been obtained in this country at Flamborough in October 1887.—Prof. F. Jeffrey Bell read a note on the Echinoderm fauna of the Bay of Bengal.—Mr. F. E. Beddard and Mr. Frederick Treves gave an account of the anatomy of the Sumatran Rhinoceros as observed in two specimens of this animal that had lately died in the Society's Gardens. The muscular anatomy of the limbs of this Rhinoceros was especially treated of.—Prof. Newton read a paper on the breeding of the Seriema (*Cariama cristata*) in the Society's Gardens.

Entomological Society, January 16.—Fifty-sixth Anniversary Meeting.—Dr. D. Sharp, President, in the chair.—An abstract of the Treasurer's accounts, showing a balance in the Society's favour, was read by Mr. Osbert Salvin, F.R.S., one of the auditors; and Mr. H. Goss read the Report of the Council. It was announced that the following gentlemen had been elected as officers and Council for 1889:—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. Henry W. Bates, F.R.S., Capt. H. J. Elwes, Mr. William H. B. Fletcher, Mr. F. DuCane Godman, F.R.S., Prof. Raphael Meldola, F.R.S., Dr. P. B. Mason, Mr. Osbert Salvin, F.R.S., and Dr. D. Sharp.—Dr. Sharp, the outgoing President, then delivered an address, for which a vote of thanks to him was moved by Capt. Elwes, seconded by Mr. Salvin, and carried. A vote of thanks to the Treasurer, Secretaries, and Librarian was moved by Mr. J. W. Dunning, seconded by Lord Walsingham, and carried. Mr. Saunders, Mr. Goss, and Mr. Grut severally replied.

PARIS.

Academy of Sciences, January 7.—M. Janssen in the chair.—On the Lindstedt series, by M. H. Poincaré. Although these series are not convergent in the strict sense of the word, they are often very useful in astronomical calculations owing to the rapidity with which the terms decrease. Here M. Lindstedt's method is presented from a fresh standpoint, and brought into connection with the principles expounded in Jacobi's "Vorlesungen über Dynamik."—On the reactions between chromic acid and oxygenated water, by M. Berthelot. This peculiar reaction has acquired a fresh interest since M. Ad. Carnot's recent discovery of an ingenious method for effecting the quantitative analysis of chromic acid by means of oxygenated water, and reciprocally, with simultaneous reduction of both compounds. The analogy of the reaction with that of permanganic acid has induced M. Berthelot to repeat the experiments, with the result that this, like other reactions of oxygenated water, may now be interpreted by the laws of thermo-chemistry.—On an hydraulic machine constructed in England, by M. Anatole de Caligny. The reference is to Mr. Pearsall's apparatus, in which M. de Caligny's open tube is replaced by a chamber allowing the air to escape freely.—Observations made on the summit of Mont Ventoux on the calorific intensity of solar radiation, by MM. A. Crova and Houdaille. The object of these observations has been to ascertain whether, at an altitude of 1900 metres, solar radiation undergoes daily variations analogous to those recorded at Montpellier, and whether from the curves registered at the higher elevation a value may be deduced for the solar constant more exact than those obtained from the calculation of the curves traced at sea-level. It results from these researches that at the altitude of 1900 metres the solar constant may acquire a value very close to the 3 calories obtained by Mr. Langley from his observations on Mount Whitney. The polarization of the blue light of the sky was also studied by means of M. Cornu's photopolarimeter, and its spectrum analysis made with M. Crova's spectrophotometer modified for the purpose. The polarization appears in general to increase with the solar constant, thus furnishing useful data for determining the degree of calorific transparency in the atmosphere.—On the true and mixed butylic ethers, by M. E. Reboul. Theory anticipates ten of these compounds; but two only are known, the normal diprimary butylic ether of Lieben and Rossi, and Kessel's di-secondary. Williamson's general method having mostly failed, or yielded only doubtful results, M. Reboul has attempted by the process here described to complete the whole series. He has so far obtained five, not yet described, which with the two already known leave three to be still procured under other conditions.—On M. Hirn's new work, entitled "Constitution de l'Espace céleste," by M. Faye. This important work deals with the question of an ether or subtle medium filling all space, as postulated by physicists to explain the phenomena of light, heat, and electricity, but the presence of which astronomers have failed to detect as a resisting medium in the movements of the heavenly bodies. With a view to putting this seeming contradiction in a clear light, M. Hirn has worked out some delicate calculations which have led to several remarkable and at times wholly unexpected conclusions. Thus in estimating the density of a new medium capable by its resistance of causing a secular acceleration of half a second in the mean velocity of the moon, he finds that such a density would correspond with a kilogramme of matter uniformly diffused throughout a space of about 390,000 square miles. This is a density one million times rarer than that of the air reduced to one millionth of its normal density in Mr. Crookes's ingenious apparatus.—On the perturbations of the planet Hestia (46), according to the theory of M. Gyldén, by M. Brendel. The application of M. Gyldén's theory of perturbations to this planet shows that it is subject to some very considerable disturbances, its mean motion being about three times that of Jupiter.—On a process by which diurnal nutation may be demonstrated, and its constants determined, by M. Folie. This extremely simple process consists in observing at intervals of six hours two stars distant not more than 3′ from the Pole.—On the quantitative analysis of organic nitrogen by the Kjeldahl method, by M. L. L'Hote. This new method is here subjected to a thorough test, and is found to yield results greatly inferior in accuracy to those obtained from the old process by means of sodaic lime.—On the early and late varieties of beet-root, by MM. C. Viollette and F. Desprez. The early maturing plant, although yielding the largest proportion of sugar, is in other respects subject to many drawbacks preventing its general adoption by growers. A series of experiments here described show that a late variety may be obtained, which, while free from these disadvantages, yields an abundant supply of sugar.—M. Hermite has been unanimously elected Vice-President, and MM. Becquerel and Fremy members of the Central Administrative Committee, for the current year. In the present number is also published the list of the members of the Academy on January 1, 1889. The following are the English and American corresponding members:—*Geometry:* James Joseph Sylvester, George Salmon. *Astronomy:* John Russell Hind, J. C. Adams, Arthur Cayley, Joseph Norman Lockyer, William Huggins, Simon Newcomb, Asaph Hall, Warren De La Rue, Benjamin Althorp Gould, Samuel Langley. *Geography and Navigation:* Admiral G. H. Richards. *General Physics:* James Prescott Joule, George Gabriel Stokes. *Chemistry:* Edward Frankland, Alexander William Williamson. *Mineralogy:* James Hall, Joseph Prestwich. *Botany:* Joseph Dalton Hooker, Maxwell Tylden Masters. *Rural Economy:* Sir John Bennet Lawes, Joseph Henry Gilbert. *Anatomy and Zoology:* James Dwight Dana, Thomas Henry Huxley. *Medicine and Surgery:* Sir James Paget. Foreign Associates: Sir Richard Owen, Sir George Biddell Airy, Sir William Thomson.

January 14.—M. Des Cloizeaux in the chair.—On the solar statistics of the year 1888, by M. R. Wolf. From the various solar and magnetic observations made at the Observatories of Zurich and Milan, M. Wolf has by his well-known method deduced and tabulated for last year the mean monthly values for the relative number r, for the variations in declination v, and for the increase Δr and Δv that these quantities have received since the corresponding epochs of the year 1887. It results from these tables that both the relative number and the magnetic variation have continued to diminish, and that it is probable the minimum has now been nearly reached. It also appears that the slight anomalies recorded during the previous year have disappeared, and that the parallelism between these two series has been almost completely re-established.—Mode of diffusion of the voltaic currents in the human organism, by M. L. Danion. From the series of experiments here described it appears that, excepting the skin and bones, the various tissues and substances constituting the organism have practically the same electric conductibility. The skin is in general highly resisting, while the conductibility of the bones, which alone affect the diffusion of the current, is perceptibly less than two-fifths of that of the other hypodermic tissues. Under like conditions the diffused intra-polar and extra-polar intensities have the same value. Contrary to the universal opinion, the choice and combination of electrodes of various dimensions does not perceptibly modify the effects of hypodermic electrization. The experiments made on animals and on man confirm those carried out on homogeneous liquid masses, while at the same time showing the extreme diffusion of the voltaic currents, hence the deductions drawn from the latter order of experiments are applicable to the electrization of the animal organism.—Observations of Faye's comet, made at the Observatory of Algiers with the 0·50 m. telescope, by MM. Trépied, Rambaud, and Sy. These observations cover the period from December 28 to January 5.—On the influence of the shock on the permanent magnetism of nickel, by M. G. Berson. These experiments form a supplement to those lately made by the author with a bar of steel. The various phenomena are in both cases strictly analogous, tending to show that with a field of feeble intensity a bar of either metal may be permanently magnetized, provided the shocks be given while the bar is within the field. The vibrations of the apparatus furnished with permanent magnets should also be carefully avoided, as they tend rapidly to diminish the force of the magnetic momentum.—On the oxidation and scouring of tin, by M. Léo Vignon. In a previous communication (*Comptes rendus*, November 5, 1888) the author showed that crystallized tin, deposited by the action of zinc and of the chemically neutral solutions of the stannous or stannic chlorides, is capable of high oxidation, and also when heated in contact with the air presents the curious property of combining with oxygen without melting, but burning like tinder (*amadou*). His further experiments with this partially oxidized tin have disclosed several facts, which explain the phenomena already described, and at the same time supply the elements of the theory on which depend the common industrial operations known as tinning and tin-soldering. In general it may be concluded that tin is capable of considerable oxidation in a dry or moist atmosphere, a conclusion which agrees with the comparative data already obtained on the heats of formation of the metallic oxides.—On ergosterine, a new immediate principle of the ergot (spur) of rye, by M. C. Tanret. The ergot of rye contains a crystallized substance, which closely resembles, and may readily be confounded with, cholesterine. But the careful study made by M. Tanret of this fungus shows that it differs in its composition both from animal cholesterine and its isomerous vegetable substances. This new principle is accordingly here described and analyzed under the name of ergosterine. Its composition may be represented by the formula—

$$C_{52}H_{40}O_2, H_2O_2.$$

It crystallizes in alcohol in the form of little pearly pellets, and in ether in that of sharp needles, and is quite insoluble in water. Like cholesterine, it is a monatomic alcohol, as appears from the analysis of its formic, acetic, and butyric ethers.—Papers are contributed by M. Hugo Gyldén, on the elementary terms in the co-ordinates of a planet; by M. Maquenne, on the heptine of a perseite; by MM. Ed. Heckel and Fr. Schlagdenhauffen, on the chemical constitution and industrial value of the gutta yielded by *Bassia latifolia*; and by M. Hueppe, on the virulence of cholera parasites.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Annuaire de l'Académie de Belgique, 1889 (Bruxelles).—Longmans' School Arithmetic with Answers: F. E. Marshall and J. W. Welsford (Longmans).—Biologia Centrali-Americana (Botany): W. B. Hemsley and Sir J. D. Hooker.—Hulfstabellen zur Mikroscopischen Mineralbestimmung in Gesteinen: H. Rosenbusch (Stuttgart, Koch).—Les Minéraux des Roches: A. M. Lévy and A. Lacroix (Paris, Baudry).—A Course of Easy Arithmetical Examples for Beginners: J. G. Bradshaw (Macmillan).—Solutions of the Examples in a Treatise on Algebra: C. Smith (Macmillan).—Calendar and General Directory of the Department of Science and Art for the Year 1889 (Eyre and Spottiswoode).—A Monograph of the British Uredineæ and Ustilagineæ: C. B. Plowright (Kegan Paul).—Graphics, or the Art of Calculation by Drawing Lines, Part 1: R. H. Smith (Longmans).

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