THURSDAY, APRIL 2, 1891.

EXTERMINATION.

EXTERMINATION is a process which has attracted far less attention from naturalists than it deserves. Signifying literally a driving out of bounds, it may in some cases indicate that in certain parts of the range of a species, whether plant or animal, that species may have ceased to exist, however abundant it may remain elsewhere; while in other cases, especially if the species have but a limited distribution, it easily becomes equivalent to extirpation. The older school of zoologists seems hardly to have contemplated the possibility of a whole species becoming extinct within the period since man appeared upon earth, or to have supposed that a species could by human efforts be utterly swept away. We have but to look back into the days before Duncan and Broderip and Strickland wrote upon the Dodo to learn that this was so. There were zoologists of established reputation who doubted whether such a creature had lived, because they could not find that it still existed; while there were others of no inferior rank who were prepared to believe in the reality of its scanty remains, but only because, in the profound ignorance of the facts of the geographical distribution of animals that then prevailed, they were always expecting that it would turn up again as the inhabitant of some hitherto undiscovered country. Neither class was prepared to admit that the Dodo, or indeed any other species of animal, had flourished within a comparatively few years, and had somehow or other since become extinct through the direct or indirect agency of man.

We now know better; but far be it from us to blame our predecessors for not knowing what they could not possibly have known. Indeed there was much to be said in support of the view they took. They knew how that by many nations war had been declared for centuries against this, that, or the other animal, which notwithstanding maintained its existence. There was the notorious case of the Wolf. The *caput lupinum* had passed into a proverb in every European language, and yet there was scarcely a country on the Continent in which Wolves did not defy the price set on their heads, and persistently carry on their retaliations, ravaging flocks and herds, and even at times mangling or destroying men, women, and children. This is still true. Though extirpated in the United Kingdom, the Wolf is yet found throughout Europe, except in the Netherlands, an artificial country—

> won from the raging deep
> By diligence amazing,

and liable to be overwhelmed by its waves on any intermission of watchfulness—and Denmark, consisting of a peninsula and several islands, in which effectual methods of destruction could be easily employed. That these methods were rather indirect than direct is pretty plain from what we know of its extirpation in Great Britain and Ireland. It was effected much more by the gradual curtailment of and inroads upon the forests which grew on so much of our soil, than by the doing to death of this or that individual of the species who made its presence known. Direct persecution hardly reached the robbers of the folds.

> With their long gallop, which can tire
> The hound's deep hate, and hunter's fire,

the power they possessed of covering vast distances, so that the marauder of the night was far distant from the scene of its depredations before they were discovered in the morning, was enough to baffle successful pursuit. When its haunts were invaded by the axe and the plough, it was easily traced to its lair, and the use of firearms soon completed the business; but so long as it could lie under the greenwood tree, it could safely snarl in scorn of Acts of Parliament passed for its extirpation.

In nearly the same tones could we write of the Birds-of-prey, and those which are detrimental to crops of grain or fruit. For centuries past war has been almost incessantly waged against Kites and Crows, and *hoc genus omne*. In this country the *Accipitres* have at last pretty well succumbed to persecution, carrying the harmless Owls in their train; but it has taken years to bring about this almost entire destruction, and we may safely say that without the system which places almost every wood under the guardianship of one or more gentlemen in velveteen, just as every shrubbery is under that of a gardener, this extermination would not have been effected. The Crow-kind would have undergone the same fate—indeed, the king of their race has practically met it—but for a divided opinion among farmers as to the agricultural utility of some of the species, aided by the delusion which in some quarters obtains that Rook-shooting (notwithstanding the example set by Mr. Winkle) is a sportsmanlike recreation, and therefore that Rooks should be reserved for slaughter in a peculiar way. The effective cunning of the two lower forms of *Corvidæ*, though not proof against the temptation of a poisoned egg, just enables them to maintain a position as denizens.

Conversely, human efforts often fail signally when they try to continue the existence of a species. Confining ourselves for the present to our own island, there is an ancient Act of Parliament (25 Hen. VIII., cap. 11) well known, and still, we believe, in force in England and Wales, prohibiting, under what was then a heavy penalty, the taking of the eggs of certain Birds, of which six kinds are expressly named. Despite this protecting law, four of these six have long since ceased to breed in this country, and the reason is plain. They have been exterminated by the agricultural improvements that have changed the face, nay, the substance of the land in so many parts—that is to say, they have been exterminated by human means, but means acting indirectly, and without intention of hurting them. To the category of these might easily be added many others that have undergone the same fate, if it were now our object to give a complete account of the effects of extermination in the British Islands, for it ranges from the Bear to a Butterfly; while if we turn to other parts of the world we immediately find such a crowd of examples as would take a volume to enumerate. Nobody in New Zealand wished to destroy its Quail, which was in some places abundant enough to afford a good day's shooting. Yet it is gone—perished by fires which were lighted for other purposes. In many British colonies the baleful effects of what some thirty years ago was highly lauded as "Acclimatization" by half-informed persons calling themselves naturalists have been felt with fearful force. Nearly all the most interesting animals of New Zealand and Australia will disappear before the ravages of the *Mustelidæ*, so foolishly if not wickedly introduced to check the great but obviously transient pest of a superabundant population of Rabbits--themselves an inconsiderate importation. The Mongoose has been set free in Jamaica to kill the Rats that spoil the sugar-canes. Its effect on the Rats is inappreciable. It prefers another kind of country and a different food. The result is the destruction of every Turkey-Buzzard's egg it can find; the Turkey-Buzzard, commonly known as "John Crow"—a species cherished and protected by colonial law for its cathartic character—being a bird that makes its nest upon the ground, and ignorant of the danger it thereby incurs from the interloper. To the Mongoose is also laid the charge of extirpating the peculiar Petrel of the island, *Œstrelata jamaicensis*, which breeds in holes on the mountain sides, and proof of the crime seems likely enough after what Colonel Feilden has told us[1] of the extinction of its congener, *Œ. hæsitata*, the Diablotin in Dominica by a species of Opossum, imported with no evil intention, so far as is known, into that island.

It may surprise some persons who imagine that because Sea-fowls can and do roam almost illimitably they are, therefore, the safer from danger. The fate of the Diablotin is proof to the contrary. No birds possess the "homing" influence more strongly than Petrels, and of many species the home is a narrow one. The Diablotin has been known to wander to Long-Island Sound, the English Channel, the county of Norfolk, and even to Hungary, but its only home was in the Islands of Dominica and Guadeloupe, and in each it has apparently become extinct. In the early days of the colonization of the Bermudas, another species of Petrel, then and there known as the Cahow, which was presumably the *Puffinus auduboni*, was so abundant, upon at least some of the islands, that many of the settlers being at the time (between 1612 and 1615) suffering from famine and sickness, they were set ashore there by the Governor that they might feed on these birds. They did so, and nearly extirpated the species, so that a few years later (1616-1619) a succeeding Governor issued "a Proclamation against the spoile of Cahowes, but it came too late, for they were most destroyed before."[2] They never recovered their numbers, and when in 1849 Sir John Orde landed on the only island where the Cahow was reported still to exist, he found it represented by but two adults, a down-clad nestling, and an egg![3] Whether the species yet maintains itself there is unknown.

We have been led to make these remarks in consequence of the award, announced at the last general meeting of the Zoological Society, as already stated in our columns, of two of its silver medals to as many recipients, in recognition of the long-continued and successful efforts made by members of their respective families for more than one generation to protect from extermination one of the most remarkable species of our native birds. This species, the Great Skua, seems never to have had more than three breeding-stations in the British Islands, and all of them situated in Shetland where it is known familiarly as the "Bonxie." On one of these stations, Roeness Hill (commonly called Rona's Hill), in Mainland, it is said to have been exterminated some fifteen or twenty years ago. On each of the others it has been preserved—in the case of Foula, the most westerly of the group, and in that of Unst, the most northerly—by the exertions of the late Dr. Robert Scott and of the late Dr. Laurence Edmondston respectively. Though it has pleased some ornithological writers to give this bird the name of the "Common" Skua, the fact only shows how little they knew about it and its rarity. Widely ranging at other times of the year, in the breeding season it has but three resorts in the North Atlantic, and, though represented by other species in other parts of the globe, this is strictly a North Atlantic bird. The first of these resorts is in the Shetlands, as just stated; the second is in the Faeroes, where it was once fairly numerous, but latterly has been "minished and brought low"—since in 1872 a good observer computed the number upon all the islands in the group to be about thirty pairs; the third is in Iceland, where it seems to breed sporadically but not numerously along the southern shore, but is more abundant on the Westman Islands. Its existence, therefore, is exposed to much hazard, and if it is to survive in Shetland it will have to be protected. The protection afforded by the gentlemen above-named has saved it hitherto, and the recognition of their service to science thereby will be hailed, we are sure, by all as a graceful act on the part of the Zoological Society. True it is that the recognition is posthumous, and that the reward goes to the successors of the real benefactors, but the good effect of their example should not on that account be the less. Medals are struck and bestowed for preservation by posterity, and we trust that a long line of each of the families of Scott and Edmondston will look back with gratification on these memorials of good action on the part of their predecessors. We also congratulate the Zoological Society, the foremost of its kind in the world, on its discrimination as being the first to reward a service of this kind, and thereby to recognize the importance of attempting to ward off the dire effects of the process of extermination.

[1] Transactions of the Norfolk and Norwich Naturalists' Society, vol. v. p. 24.

[2] See Lefroy's "Memorials of the Discovery and Early Settlement of the Bermudas or Somers Islands," vol. i. pp. 76, 137.

[3] Jones's "The Naturalist in Bermuda," p. 96.

PSYCHOLOGY IN AMERICA.

Outlines of Physiological Psychology: A Text-book of Mental Science for Academies and Colleges. By George Trumbull Ladd, Professor of Philosophy in Yale University. (London: Longmans, 1891.)

The Principles of Psychology. By William James, Professor of Psychology in Harvard University. 2 Vols. (London: Macmillan, 1890.)

WHILE the "Outlines of Physiological Psychology" is not a mere abridgment of Prof. Ladd's larger work, entitled "Elements of Physiological Psychology," it is cast on the same general lines, and has a similar object in view. At the time of its appearance, we noticed at some length (NATURE, vol. xxxvi. p. 290) the longer and more elaborate treatise. This relieves us of any necessity for saying more than a few words of commendation and criticism of the later and shorter work. In it will be found a careful and accurate description of the results which have been reached through the experimental and physiological study of those neural processes which are regarded as the correlates of mental phenomena. What is known of the neural processes, and what is inferable concerning their psychical concomitants, are clearly and fairly presented; and concerning this part of Prof. Ladd's work we have naught to offer but praise of the impartial consideration of the facts and the judicial presentation of the results. But there are higher and more abstract regions of psychology, where exact correlation of mental processes with neural processes is at present impossible. From the point of view of physiological psychology these should either be left alone, or indicated as regions in which the nature of the correlation, if such exist, is at present unknown. Careful and judicious speculation in this region, if duly presented as such, is fairly admissible. But dogmatic assertions, positive or negative, are an occasion of stumbling to the student, and a serious dereliction of duty on the part of the teacher. When Prof. Ladd says that "a thorough analysis of mental life discloses other forms of activity than those properly called sensory-motor," he is well within his province; but when he proceeds to lay it down "that these forms not only do not find their full explanation in the changing state of the brain, but are not even conceivable as correlated with such states," he is building too confidently on his ignorance. It is noteworthy that, where our ignorance concerns a conclusion which he rejects, the conclusion is said to be "inconceivable"; but where it concerns a conclusion which he accepts (as in the case of the causal connection between the mind and the body as distinct entities), it is called a "mystery," and we are gravely admonished that "because of its mystery it is no less to be acknowledged as a fact." What is here spoken of as a "fact" is one solution out of several of one of the most difficult philosophical problems. Just where the scientific spirit, as opposed to that which is the outcome of other considerations, demands increased caution and modesty, since the grounds of perceptual verification are far distant, and since investigators of admitted knowledge, insight, and fairmindedness have reached other conclusions, Prof. Ladd assumes a dogmatic tone, which seems to us a serious error in an otherwise excellent work.

In his "Principles of Psychology," Prof. James presents us with a work of unusual ability and power. His sturdy individuality, his keen insight, his wide range of learning and culture render him especially fitted for the task which he has undertaken. His pages abound in pithy epigrams, and observations which indicate that the author is a keen student of the individualities as well as the generalities of human thought and conduct. His style is at once lucid and flowing. Not a little of the matter which he has embodied in his pages has seen the light before in periodical publications; and this it is, perhaps, that has led to one of the defects we find in his work. It is too long. It many parts it would be rendered better and more effective by wise and judicious condensation. After reading a hundred and fifty pages on the perception of space, or a hundred and ten on the consciousness of self, one wishes that the author had seen fit to give us the essence of his thought in one-third of the space. Some of the longer quotations might go, or be considerably curtailed. In view of future editions, we would counsel Prof. James to make experiments on the following lines: to read ten pages over night, sleep over them, and condense them to five on the following morning. We believe his work will thus gain in power what it loses in bulk.

The method of arrangement of the subject-matter is somewhat peculiar and not altogether satisfactory. Perception, with which so much of our daily life is concerned, should have an earlier place; and the chapters on the stream of thought and the consciousness of self, with those on the automaton theory and the mind-stuff theory, might well have been deferred till towards the close of the treatise. This is, however, a matter of no great importance, since the work is presumably not intended for those who come fresh to the subject of psychology with no previous acquaintance with its methods and results; and every author of an advanced text-book has a perfect right to develop the subject as seemeth to him best.

It is obviously impossible in the space at our command to attempt anything like an analysis or detailed criticism of a work of well-nigh 1400 pages of closely-printed matter. Easy and pleasant would it be to dwell on the many arguments and passages which seem to us wholly satisfactory—which are, that is to say, in conformity with our own views. We think it better, however, to indicate some of the points in which we find Prof. James's treatment less satisfactory.

The description and definition of "perception" is on the whole excellent. It is clearly seen and enunciated that while part of what we perceive comes through the senses from the object before us, another part, and it may be the larger part, always comes, in Lazarus's phrase, out of our own head. The fact, also, that a pure sensation is an abstraction—that it answers to the presentative as isolated from the representative elements which the analysis of percepts discloses—is shown and insisted on. So far good. But the distinction thus satisfactorily laid down is not steadily adhered to. An hallucination is called "as good and true a *sensation* as if there were a real object." In the chapter on the perception of space we read in one place: "When we speak of the relation of direction of two points toward each other, we mean simply the *sensation* of the line that joins the two points together;" and again, "rightness and leftness, upness and downness, are pure *sensations*." But further on we find it stated that "for the distance between the 'here' and the 'there' to be felt, the entire intervening space must be itself an object of *perception*." Again, in the chapter on sensation we find it written: "And though our *sensations* cannot so analyze and talk of themselves, yet at their very first appearance, quite as much as at any later date, are they cognizant of all those qualities which we end by extracting and conceiving under the names 'objectivity,' 'exteriority,' and 'extent.'"

Now we venture to think that there is in all this a good deal of confusion, if not of thought at least of statement; though we must bear in mind Prof. James's view that "the words 'sensation' and 'perception' do not carry very definitely discriminated meanings in popular speech, and in Psychology also their meanings run into each other." A line, as such, is not an object of abstract sensation but of perception. Rightness and leftness are concepts; and the practical right and left or up and down which we observe in daily experience are the outcome of the perceptual process, are part of the perceived relations between sensations, and are not themselves sensations. This Prof. James himself practically admits when he tells us that "the fuller of relations an object is, the more it is something classed, *located*, measured, compared, assigned to a function, &c., the more unreservedly do we call the state of mind a perception." The title of the chapter on "The Perception of Space" is a misnomer. Space is a concept, not a percept: what we perceive is extension and distance, or rather that objects are extended and distant, out of which by abstraction thought reaches the conception of space. The same is true of time: what we perceive is duration, or rather that phenomena are enduring, and from this our thought rises to the conception of time. His confusion of terms makes Prof. James's discussion of space (in which he strives to establish an original *sensation of space*), and those parts of his chapter on sensation, in which he runs a tilt against the doctrine of "eccentric projection," rather difficult reading. With regard to the former question his contention, unless we misunderstand its essence, seems to come to this—that in our given perception of external things there are special relations of extent, direction, and distance, which are *sui generis*, and behind or beyond which we cannot get in the process of analysis. If this represent the core of his argument, we consider it sound; while, at the same time, we deny the sensation of space. With regard to "projection," again, the whole question seems to us to depend very much upon our definition of terms. Few will be likely to deny the fact that, in the immediate perception of adult life, the objects which we see are given to consciousness as external to us. We then proceed to analyze. And in our analysis we reach sensations, and the relations in which sensations lie to one another. The sensations are pure abstractions; they have been abstracted from their normal relationships; their outness is part of their relationship; therefore *quâ* sensations they have no outness. Moreover, analysis discloses, or seems to disclose, that consciousness, whether sensational or relational, is a concomitant of certain neural vibrations in the cerebral cortex. And the question arises, How does the consciousness accompanying these molecular processes in the brain give us the outness of the object of perception? The answer seems to us to be that certain of these neural processes are directly symbolic of this outness. The question thus shifts to one of origin: How have these neural processes come to be symbolic of outness? Prof. James himself tells us, in italics, that "every perception is an acquired perception." How, then, has the perception of outness been acquired? Whether we believe in use-inheritance, or, with Prof. James, rely on the selection of chance variations, the question is a valid and not an idle one.

Prof. James is insistent, as against the crude Associationalists, that a compounded idea is a new psychic fact to which the separate ideas stand in the relation, not of constituents, but of occasions of production. He denies, and in our opinion rightly denies, that we can explain the constitution of the higher mental states by viewing them as identical with the lower ones merely summed together. No more can we do so than we can explain the constitution of grey nerve-matter as identical with certain material atoms merely summed together. In each case the nature of the summation is determined by the fixed laws of the constitution of thoughts and things. Whether we have any right to say more than this, from the scientific standpoint, is open to question.

An interesting chapter is devoted to instinct. In Prof. James's acceptation of the term, instinct means innate impulse, or what we have elsewhere termed innate capacity for response in action, as opposed to any special and more or less definite manifestation of this impulse performed without intelligence and without practice. We question whether this wide extension of the field of instinct is advisable. In any case, it is in opposition to the tendency of recent thought on the subject.

In the last chapter there is an interesting and able discussion of the range and limits of experience. We propose merely to indicate the nature of the problem, and to refer our readers to Prof. James's work for his ingenious treatment of it. The modes of external noumenal existence are represented in the mind of man in mental symbolism. Whether the products of this mental symbolism in any sense *resemble* the noumena symbolized is wholly immaterial and altogether beside the question. To us it appears that the only phenomenal which can be colorably supposed to resemble the noumenal is that which we designate sequence. By Prof. James both time and space are so regarded. "The time- and space-relations between things," he says, "do stamp copies of themselves within." If we are right in understanding him to contend that our ideas of concrete things in any sense resemble their noumena, we altogether disagree with him. But this, as we said, is immaterial.

Now, experience may be described as the adjustment of the symbolism for the practical purposes of life and thought. And in a series of organic beings this adjustment may be developed and perfected, either by the inheritance of individual increments of adjustment (use-inheritance), or by the elimination of failures in adjustment (natural selection), or by both combined. We may therefore speak of (1) experience as developed through use-inheritance, and (2) experience as developed by natural selection. Some people would have us restrict the term to the former category, which appears to us somewhat unsatisfactory. The more satisfactory plan is to distinguish between experience as a guide, determining what varieties spontaneously arising shall survive, and experience as a source of origin. If use-inheritance be disproved, experience as a source of origin goes by the board. But at the same time we may quite legitimately say that it is by experience that our mental symbolism has been *moulded*. For origin we are then thrown back either upon accidental variation (which is but a phrasal cloak for our ignorance), or upon the view that psychical development has obeyed certain fixed laws of our mental being, just as a crystal is developed in accordance with certain fixed laws of physical being.

When we have to deal, not with perceptual symbolism, where verification is within easy range, but with conceptual symbolism, the moulding by experience is indirect. Hence the divergent theories of things, ethical types, and æsthetic standards. Prof. James stoutly maintains that with these experience has had little or nothing to do. And he is so far right in that they rose to the clouds (or, if it sound better, the empyrean) of conception, from the solid (*i.e.* verifiable) ground of experience, and can only be tested in so far as their conclusions can be brought down to the same practical and mundane sphere. For the rest, as we have elsewhere suggested, our theories of things and interpretations of nature are subject to a process of elimination through incongruity, whereby conceptions incongruous with any individual system of mental symbolism strive in vain to enter therein and become incorporated therewith.

We have presented this question rather as it had formulated itself in our own mind before we had read Prof. James's work, than as it is set forth in his final chapter, to which we must now refer our readers. And we have dwelt upon the point at some length that our biological readers may see the bearing of the use-inheritance question in the mental field. Prof. James is on the side of Weismann, and rejects use-inheritance. Our own position is that of anti-dogmatism. We think use-inheritance is still upon its trial, and we desire to see fair play. Mr. Platt Ball's valuable discussion does not get down to the root of the question, which is really one of the origin of variations. We want more experimental evidence. But crucial experiments are hard to devise.

We regard Prof. James's work as an exceedingly valuable contribution to psychological literature; and trust that in a second edition he will make the index in some degree worthy of the volumes, the treasures of which it at present very inadequately indicates.

C. Lloyd Morgan.

THE ANNUAL REPORT OF THE UNITED STATES GEOLOGICAL SURVEY.

Ninth Annual Report of the United States Geological Survey to the Secretary of the Interior, 1887–88. By J. W. Powell, Director. (Washington, 1889.)

A CONSIDERABLE part of this volume is occupied by the reports of the Director and of his principal assistants on the progress in work for the year 1887–88. Of these reports, several touch upon topics of general interest, but as they are more or less of a preliminary nature, we shall restrict ourselves to a notice, necessarily brief, of the larger and later part of the volume; that containing the papers accompanying the official report. The first of these, drawn up by Captain C. E. Dutton, gives a history of the Charleston earthquake on August 31, 1886, with a discussion of the observations collected in order to determine the position, depth, and velocity of the shock. The memoir begins with three accounts of the earthquake, contributed by eye-witnesses. The first is written in a graphic, perhaps over picturesque style, but as it was originally composed by one of the staff of a daily newspaper for public consumption, the roar of the young lion is to be expected; the others are shorter, simpler, but in some respects more valuable. At Charleston the first and principal shock occurred about 9.50 p.m. At that time the writer of the popular account was at work, as usual, in the office of the *Charleston News and Courier*, in a room on the second floor. His attention was first attracted by a sound, apparently from below, which was accompanied by a perceptible tremor, much as if a heavily-loaded dray were passing by. For two or three seconds the occurrence excited no surprise or comment:—

"Then, by swift degrees, or all at once—it is difficult to say which—the sound deepened in volume, the tremor becoming more decided; the ear caught the rattle of window-sashes, gas-fixtures, and other movable objects.... The long roll deepened and spread into an awful roar that seemed to pervade at once the troubled earth and the still air above and around. The tremor was now a rude, rapid quiver that agitated the whole lofty strong-walled building, as though it were being shaken—shaken by the hand of an immeasurable power; . . . there was no intermission in the vibration of the mighty subterranean engine. From the first to the last it was a continuous jar, adding force with every moment, and as it approached and reached the climax of its manifestation, it seemed for a few seconds that no work of human hands could possibly survive the shocks. . . . For a second or two it seemed that the worst had passed, and that the violent motion was subsiding. It increased again, and became as severe as before. . . . The uproar slowly died away in seeming distance. The earth was still."

The earthquake also was very severe at Summerville, a village 22 miles to the north-west, where some slight preliminary shocks had been felt on August 27 and 28, which, however, were not perceived at Charleston. Other shocks occurred at Charleston during the month following, but fortunately none were severe. But the devastation caused by the great shock was lamentable: 27 persons were killed; at least thrice that number died afterwards from hurts, or the indirect results of the earthquake; not more than half-a-dozen houses escaped injury; many buildings were practically destroyed. The numerous illustrations, taken in many cases from photographs, enable the reader to judge of the devastation. "Hibernian Hall," with two broken columns, projecting from a mass of *débris*, the sole remnants of a portico, might, at the present day, suggest to some minds a political interpretation. The work of the jerry-builder—who evidently exists in Charleston as in London—was revealed by the earthquake, which proved to be a satisfactory, though expensive test of the relative value of different building materials. Fissures and craterlets were formed in the surrounding country, from which water and sand were ejected. The tracks of railways were distorted and otherwise injured; and in one case a train was wrecked by being thrown off the rails.

Captain Dutton's observations have enabled him to construct a map of the isoseismal lines, and to estimate the depth and velocity of the shock. These "isoseisms" are arranged about two "epicentra," which lie on a straight line running rather west of south, about 13 miles apart, and nearly as much west of Charleston. The curves around the northern one are almost circular; around the southern one they are somewhat elongated and rather distorted ovals. To the focus of the former a depth of 12 miles is assigned, with a probable error of less than 2 miles; to that of the latter, a depth of nearly

8 miles, but here the data are less complete. The velocity of the shock is estimated at nearly 5200 metres a second. Large numbers of observations are recorded and discussed in their practical and theoretical bearings, and the memoir is enriched by numerous illustrations, which, as usual, are admirably executed.

The second memoir, by Prof. N. S. Shaler, describes the geology of the district about Cape Ann (Massachusetts). This district consists entirely of igneous rocks, but it affords exceptional advantages for the study of dykes, of the phenomena of joints, and of the superincumbent drift, while on its coasts the relation of wave-work to ice-work can be readily examined. This memoir, as might be anticipated, is not of such exceptional interest as the preceding one; the illustrations, however, are excellent, and in several cases will be valuable to students who cannot travel far afield.

The third memoir, by Mr. W. H. Weed, "On the Formation of Travertine and Siliceous Sinter by the Vegetation of Hot Springs," is one of much general interest. A large number of facts have been collected and co-ordinated by the author, which establish, as he believes, the following conclusions: (*a*) that the plant-life of the calcareous Mammoth Hot Springs waters causes the deposition of travertine, and is a very important agent in the formation of such deposits; (*b*) the vegetation of the hot alkaline waters of the geyser basins eliminates silica from the water by its vital growth, and produces deposits of siliceous sinter; (*c*) the thickness and extent of the deposits produced by the plant-life of thermal waters establishes the importance of such vegetation as a geological agent. The effect of organisms in promoting mineral accumulation, indirectly by decomposition after death, or directly during life, has long been recognized, but it is interesting to find that some algæ, like some corals, may claim to be called "reef-builders."

A short memoir on the geology and physiography of a part of North-Western Colorado, Utah, and Wyoming concludes the volume. As may be inferred from our brief outline of its contents, it is not less interesting than any one of its eight predecessors, and it abounds in illustrations admirably executed. In this respect, unfortunately, geological works published in Great Britain cannot be compared with those of the United States of America.

T. G. Bonney.

OUR BOOK SHELF.

On the Eggs and Larvæ of some Teleosteans. By Ernest W. L. Holt. (Transactions of the Royal Dublin Society, Vol. IV. Part 7, February 1891.)

In 1890 the Royal Dublin Society appointed a Committee for the purpose of investigating the fishing grounds on the west coast of Ireland, and they voted a considerable sum of money for the cost of the expedition. This sum was increased by a vote in aid from the Irish Government, who thus properly assisted the efforts of a private Society in attempting a work of great general utility. Here it is not our intention to refer in any detail to the general work done during the last year by the expedition, which was under the charge of several well-known biologists, and which we are glad to learn is being continued this year, under even more favourable auspices than before; but to call attention to a report by Mr. Ernest Holt on the eggs and larvæ of some Teleosteans met with in the first year's cruise. The ova were collected between June 12 and July 11, 1890, and the observations made thereon, with the series of beautiful drawings now before us, were laid before the Royal Dublin Society in November of the same year, while the memoir itself was published in February last. All the observations and drawings of the living forms were made on board ship, but the bad weather generally encountered during the cruise left much to be desired in the way of facilities for microscopical study. It must also be remembered that the main object of the expedition was an inquiry into the presence of "fish grounds," into the condition of the reproductive organs and food of adult fishes, and that these special researches of Mr. Holt were conducted only in such time as could be spared from these other duties.

Under all these circumstances it is surprising that so much good work was done, the ova of twenty-two forms having been more or less carefully investigated. Most of these were pelagic ova. The methods used for their capture were: (1) towing at the surface small ring-nets of fine cheese cloth at the sides of the vessel whilst trawling; (2) sinking larger ring-nets and a large triangular midwater net, after Prof. McIntosh's pattern, to a fathom or so below the surface, and allowing the ship to drift with them for a short time; (3) trawling from the ship's boats with a small naturalist's trawl with muslin net—owing probably to some defect in the latter, this method was not very successful. The first method proved by far the most productive. The following were the species examined. The ova and larvæ of those with an asterisk are figured in detail in the plates. *Scomber scomber*, **Trachinus vipera*, *Trigla gurnardus*, **Gobius niger*, **Callionymus lyra*, **Cepola rubescens*, **Lepadogaster bimaculatus*, *Labrus maculatus*, *Crenilabrus melops*, *Merluccius vulgaris*, *Macrurus* (?), **Rhombus lævis*, **Pleuronectes microcephalus*, *P. cynoglossus*, *Clupea sprattus*, **Solea* (?), **S. lutea* (?), **A. Motella*-like form, **Ctenolabrus rupestris* (?), **A Coris*-like form, and four unidentified forms which are figured. There is also a table showing the comparative dimensions of the ova.

This memoir forms a very important contribution to the history of the development of our native fish, and is exceedingly well printed and illustrated.

The Hand-book of Games. Vol. II. Card Games. New Edition. (London: George Bell and Sons, 1891.)

The first volume of this work dealt with table games, while the present one treats entirely of card games. Of the twenty-two games here described, eleven are new, or we should rather say did not appear in the first edition, and several of the old games, such as Boston, quinze, lansquenet, &c., which have become obsolete, are omitted.

The game that stands above all others is undoubtedly whist, and to play it well requires no small amount of scientific reasoning. Bearing this in mind Dr. William Pole treats of it in a very able manner, laying before the reader a good account of the game as it is played to-day. In a general view of the modern game, he points out that it was Edmund Hoyle's book which, in 1743, was the first to bring the game to a definite shape. Then followed William Payne's "Maxims," published in 1770, and T. Mathews's "Advice to the Young Whist Player," written in 1800. It is in these three books that the game as then played is fully described. In the later works of "Cavendish" and Clay, the game now known as "modern whist" is expounded, and the author, by making a judicious use of all the above-mentioned authorities, has, in the space of about 100 pages, written both an instructive and refreshing essay on the subject in question.

In regard to the other games treated of, we may note that they are all written by the best authorities. Thus solo whist is dealt with by Robert F. Green, piquet, écarté, euchre, bézique, and cribbage by "Berkeley," and the round games, including napoleon, loo, &c., by Baxter-Wray. Although we have only just mentioned these games by name, it must be understood that each is thoroughly well described, and numerous illustrations for the explanation of difficult hands are inserted. The work will be most popular as a book of reference; and we may add that those who wish only to be instructed in one or two games in particular can obtain different sections in separate volumes.

Object-Lessons from Nature: a First Book of Science. By L. C. Miall, Professor of Biology in the Yorkshire College, Leeds. (London: Cassell and Co., Limited, 1890.)

THIS little volume is a very laudable attempt to form a first guide to the study of Nature for children; the lessons are meant to serve as guides to the intelligent teacher, who should in a half-demonstrative, half-catechetical way, bring the various subjects treated of before the young pupil's mind. Throughout there is no attempt to teach the student that there are a number of different sciences, but the various natural phenomena witnessed in the history of a "summer shower," in the "burning of a candle," or the "growth of a plant or insect," are gradually led up to, the various stages in the story of each being explained as they arise. Teachers must use their personal experience in illustrating their teachings, as based on these lessons, and those who are able to use the chalk and blackboard will find an immense advantage in doing so, as the little student's attention is thereby doubly attracted, and a much greater impression is made upon his mind.

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 International Congress of Hygiene and Demography.

As I notice that you give an excellent account of the meeting of the General Committee of this Congress recently held under the presidency of H.R.H. the Prince of Wales, I have no doubt that your readers will be interested to know how the Congress came to be invited to hold its session in London this year.

In 1884 I was asked by the Organising Committee of the Congress to be held at The Hague that year to give an address at a general *séance* of the Congress; I did so, selecting as my subject "La Science Ennemie de la Maladie." During the meeting of that Congress, one of the most pleasant that I ever attended, I was frequently asked why we had never invited the Congress to London, and I was obliged to answer that our Government would give no pecuniary assistance for such a purpose, and that we had no Hygienic Society strong enough to undertake it; but I determined that anything I might be able to do to bring about that result should be done, as I felt that it was a disgrace to this Country that this Congress, of all others, had not met here.

The next Congress was to be held in Vienna in 1886, but was postponed until 1887, and in that year my opportunity came in the proposed amalgamation of the Sanitary Institute of Great Britain and the Parkes Museum. I urged the importance of the matter on the Councils of both those bodies, and induced them to pass resolutions inviting the Congress to hold its next session in London. Sir Douglas Galton (as Chairman of both Councils) and I were appointed delegates to attend the Congress at Vienna, and present the invitation; the Society of Medical Officers of Health was also asked to co-operate, and appointed Mr. Shirley Murphy as its delegate. We three went to Vienna, and presented the invitation, which was accepted. After our return to this country we set to work to form, with the aid of the other English members of the Vienna Congress, especially Sir Spencer Wells, Prof. (now Sir George) Humphry, Dr. Cameron, M.P., Prof. Frankland, and Dr. Mapother, the large and influential General Committee.

In the early days it was thankless and rather trying work, especially as the money did not come in as readily as we could have wished; but now, thanks to the patronage of Her Majesty the Queen, to the presidency of His Royal Highness the Prince of Wales, and to the support of the Lord Mayor and Corporation of the City of London, it is comparatively plain sailing, and there can be no doubt that the Congress will be a great success, to which no one will have contributed more than Sir Douglas Galton, the indefatigable Chairman of the Organising Committee. To ensure this, however, and to enable us to bear the heavy expense of printing the Transactions, &c., funds are still urgently needed. I may add that all general correspondence relating to the Congress should be addressed to Dr. G. V. Poore, the Hon. General Secretary, at the offices, 20 Hanover Square, W.

W. H. CORFIELD,
Hon. Foreign Secretary.

19 Savile Row, W., March 28.

On the Rôle of Quaternions in the Algebra of Vectors.

THE following passage, which has recently come to my notice, in the preface to the third edition of Prof. Tait's "Quaternions," seems to call for some reply:

"Even Prof. Willard Gibbs must be ranked as one of the retarders of quaternion progress, in virtue of his pamphlet on 'Vector Analysis,' a sort of hermaphrodite monster, compounded of the notations of Hamilton and of Grassmann."

The merits or demerits of a pamphlet printed for private distribution a good many years ago do not constitute a subject of any great importance, but the assumptions implied in the sentence quoted are suggestive of certain reflections and inquiries which are of broader interest, and seem not untimely at a period when the methods and results of the various forms of multiple algebra are attracting so much attention. It seems to be assumed that a departure from quaternionic usage in the treatment of vectors is an enormity. If this assumption is true, it is an important truth; if not, it would be unfortunate if it should remain unchallenged, especially when supported by so high an authority. The criticism relates particularly to notations, but I believe that there is a deeper question of notions underlying that of notations. Indeed, if my offence had been solely in the matter of notation, it would have been less accurate to describe my production as a monstrosity, than to characterize its dress as uncouth.

Now what are the fundamental notions which are germane to a vector analysis? (A vector analysis is of course an algebra for vectors, or something which shall be to vectors what ordinary algebra is to ordinary quantities.) If we pass over those notions which are so simple that they go without saying, geometrical addition (denoted by +) is, perhaps, first to be mentioned. Then comes the product of the lengths of two vectors and the cosine of the angle which they include. This, taken negatively, is denoted in quaternions by $S\alpha\beta$, where α and β are the vectors. Equally important is a vector at right angles to α and β (on a specified side of their plane), and representing in length the product of their lengths and the sine of the angle which they include. This is denoted by $V\alpha\beta$ in quaternions. How these notions are represented in my pamphlet is a question of very subordinate consequence, which need not be considered at present. The importance of these notions, and the importance of a suitable notation for them, is not, I suppose, a matter on which there is any difference of opinion. Another function of α and β, called their product and written $\alpha\beta$, is used in quaternions. In the general case, this is neither a vector, like $V\alpha\beta$, nor a scalar (or ordinary algebraic quantity), like $S\alpha\beta$, but a quaternion—that is, it is part vector and part scalar. It may be defined by the equation—

$$\alpha\beta = V\alpha\beta + S\alpha\beta.$$

The question arises, whether the quaternionic product can claim a prominent and fundamental place in a system of vector analysis. It certainly does not hold any such place among the fundamental geometrical conceptions as the geometrical sum, the scalar product, or the vector product. The geometrical sum $\alpha + \beta$ represents the third side of a triangle as determined by the sides α and β. $V\alpha\beta$ represents in magnitude the area of the parallelogram determined by the sides α and β, and in direction the normal to the plane of the parallelogram. $S\gamma V\alpha\beta$ represents the volume of the parallelopiped determined by the edges α, β, and γ. These conceptions are the very foundations of geometry.

We may arrive at the same conclusion from a somewhat narrower but very practical point of view. It will hardly be denied that sines and cosines play the leading parts in trigonometry. Now the notations $V\alpha\beta$ and $S\alpha\beta$ represent the sine and the cosine of the angle included between α and β, combined in each case with certain other simple notions. But the sine and cosine combined with these auxiliary notions are incomparably more amenable to analytical transformation than the simple sine and cosine of trigonometry, exactly as numerical quantities combined (as in algebra) with the notion of positive or negative quality are incomparably more amenable to analytical transformation than the simple numerical quantities of arithmetic.

I do not know of anything which can be urged in favour of the quaternionic product of two vectors as a *fundamental* notion in vector analysis, which does not appear trivial or artificial in comparison with the above considerations. The same is true of the quaternionic quotient, and of the quaternion in general.

How much more deeply rooted in the nature of things are the functions $S\alpha\beta$ and $V\alpha\beta$ than any which depend on the definition of a quaternion, will appear in a strong light if we try to extend our formulæ to space of four or more dimensions. It will not be claimed that the notions of quaternions will apply to such a space, except indeed in such a limited and artificial manner as to rob them of their value as a system of geometrical algebra. But vectors exist in such a space, and there must be a vector analysis for such a space. The notions of geometrical addition and the scalar product are evidently applicable to such a space. As we cannot define the direction of a vector in space of four or more dimensions by the condition of perpendicularity to two given vectors, the definition of $V\alpha\beta$, as given above, will not apply *totidem verbis* to space of four or more dimensions. But a little change in the definition, which would make no essential difference in three dimensions, would enable us to apply the idea at once to space of any number of dimensions.

These considerations are of a somewhat *a priori* nature. It may be more convincing to consider the use actually made of the quaternion as an instrument for the expression of spatial relations. The principal use seems to be the derivation of the functions expressed by $S\alpha\beta$ and $V\alpha\beta$. Each of these expressions is regarded by quaternionic writers as representing two distinct operations; first, the formation of the product $\alpha\beta$, which is the quaternion, and then the taking out of this quaternion the scalar or the vector part, as the case may be, this second process being represented by the selective symbol, S or V. This is, I suppose, the natural development of the subject in a treatise on quaternions, where the chosen subject seems to require that we should commence with the idea of a quaternion, or get there as soon as possible, and then develop everything from that particular point of view. In a system of vector analysis, in which the principle of development is not thus predetermined, it seems to me contrary to good method that the more simple and elementary notions should be defined by means of those which are less so.

The quaternion affords a convenient notation for rotations. The notation $q(\ )q^{-1}$, where q is a quaternion and the operand is to be written in the parenthesis, produces on all possible vectors just such changes as a (finite) rotation of a solid body. Rotations may also be represented, in a manner which seems to leave nothing to be desired, by linear vector functions. Doubtless each method has advantages in certain cases, or for certain purposes. But since nothing is more simple than the definition of a linear vector function, while the definition of a quaternion is far from simple, and since in any case linear vector functions must be treated in a system of vector analysis, capacity for representing rotations does not seem to me sufficient to entitle the quaternion to a place among the *fundamental* and *necessary* notions of a vector analysis.

Another use of the quaternionic idea is associated with the symbol ∇. The quantities written $S\nabla\omega$ and $V\nabla\omega$, where ω denotes a vector having values which vary in space, are of fundamental importance in physics. In quaternions these are derived from the quaternion $\nabla\omega$ by selecting respectively the scalar or the vector part. But the most simple and elementary definitions of $S\nabla\omega$ and $V\nabla\omega$ are quite independent of the conception of a quaternion, and the quaternion $\nabla\omega$ is scarcely used except in combination with the symbols S and V, expressed or implied. There are a few formulæ in which there is a trifling gain in compactness in the use of the quaternion, but the gain is very trifling so far as I have observed, and generally, it seems to me, at the expense of perspicuity.

These considerations are sufficient, I think, to show that the position of the quaternionist is not the only one from which the subject of vector analysis may be viewed, and that a method which would be monstrous from one point of view, may be normal and inevitable from another.

Let us now pass to the subject of notations. I do not know wherein the notations of my pamphlet have any special resemblance to Grassmann's, although the point of view from which the pamphlet was written is certainly much nearer to his than to Hamilton's. But this is a matter of minor consequence. It is more important to ask, What are the requisites of a good notation for the purposes of vector analysis? There is no difference of opinion about the representation of geometrical addition. When we come to functions having an analogy to multiplication, the product of the lengths of two vectors and the cosine of the angle which they include, from any point of view except that of the quaternionist, seems more simple than the same quantity taken negatively. Therefore we want a notation for what is expressed by $-S\alpha\beta$, rather than $S\alpha\beta$, in quaternions. Shall the symbol denoting this function be a letter or some other sign? and shall it precede the vectors or be placed between them? A little reflection will show, I think, that while we must often have recourse to letters to supplement the number of signs available for the expression of all kinds of operations, it is better that the symbols expressing the most fundamental and frequently recurring operations should not be letters, and that a sign between the vectors, and, as it were, uniting them, is better than a sign before them in a case having a formal analogy with multiplication. The case may be compared with that of addition, for which $\alpha + \beta$ is evidently more convenient than $\Sigma(\alpha, \beta)$ or $\Sigma\alpha\beta$ would be. Similar considerations will apply to the function written in quaternions $V\alpha\beta$. It would seem that we obtain the *ne plus ultra* of simplicity and convenience, if we express the two functions by uniting the vectors in each case with a sign suggestive of multiplication. The particular forms of the signs which we adopt is a matter of minor consequence. In order to keep within the resources of an ordinary printing-office, I have used a dot and a cross, which are already associated with multiplication, but are not needed for ordinary multiplication, which is best denoted by the simple juxtaposition of the factors. I have no especial predilection for these particular signs. The use of the dot is indeed liable to the objection that it interferes with its use as a separatrix, or instead of a parenthesis.

If, then, I have written $\alpha \,.\, \beta$ and $\alpha \times \beta$ for what is expressed in quaternions by $-S\alpha\beta$ and $V\alpha\beta$, and in like manner $\nabla \,.\, \omega$ and $\nabla \times \omega$ for $-S\nabla\omega$ and $V\nabla\omega$ in quaternions, it is because the natural development of a vector analysis seemed to lead logically to some such notations. But I think that I can show that these notations have some substantial advantages over the quaternionic in point of convenience.

Any linear vector function of a variable vector ρ may be expressed in the form—

$$\alpha\lambda.\rho + \beta\mu.\rho + \gamma\nu.\rho = (\alpha\lambda + \beta\mu + \gamma\nu).\rho = \Phi.\rho,$$

where

$$\Phi = \alpha\lambda + \beta\mu + \gamma\nu;$$

or in quaternions

$$-\alpha S\lambda\rho - \beta S\mu\rho - \gamma S\nu\rho = -(\alpha S\lambda + \beta S\mu + \gamma S\nu)\rho = -\phi\rho,$$

where

$$\phi = \alpha S\lambda + \beta S\mu + \gamma S\nu.$$

If we take the scalar product of the vector $\Phi \,.\, \rho$, and another vector σ, we obtain the scalar quantity

$$\sigma.\Phi.\rho = \sigma.(\alpha\lambda + \beta\mu + \gamma\nu).\rho,$$

or in quaternions

$$S\sigma\phi\rho = S\sigma(\alpha S\lambda + \beta S\mu + \gamma S\nu)\rho.$$

This is a function of σ and of ρ, and it is exactly the same kind of function of σ that it is of ρ, a symmetry which is not so clearly exhibited in the quaternionic notation as in the other. Moreover, we can write $\sigma.\Phi$ for $\sigma.(\alpha\lambda + \beta\mu + \gamma\nu)$. This represents a vector which is a function of σ, viz. the function conjugate to $\Phi.\sigma$; and $\sigma.\Phi.\rho$ may be regarded as the product of this vector and ρ. This is not so clearly indicated in the quaternionic notation, where it would be straining things a little to call $S\sigma\phi$ a vector.

The combinations $\alpha\lambda$, $\beta\mu$, &c., used above, are distributive with regard to each of the two vectors, and may be regarded as a kind of product. If we wish to express everything in terms of *i*, *j*, and *k*, Φ will appear as a sum of *ii*, *ij*, *ik*, *ji*, *jj*, *jk*, *ki*, *kj*, *kk*, each with a numerical coefficient. These nine coefficients may be arranged in a square, and constitute a matrix; and the study of the properties of expressions like Φ is identical with the study of ternary matrices. This expression of the matrix as a sum of products (which may be extended to matrices of any order) affords a point of departure from which the properties of matrices may be deduced with the utmost facility. The ordinary matricular product is expressed by a dot, as $\Phi . \Psi$. Other important kinds of multiplication may be defined by the equations—

$$(\alpha\lambda) \overset{\times}{\times} (\beta\mu) = (\alpha \times \beta)(\lambda \times \mu), \qquad (\alpha\lambda) : (\beta\mu) = (\alpha.\beta)(\lambda.\mu).$$

With these definitions $\frac{1}{6}\Phi \overset{\times}{\times} \Phi : \Phi$ will be the determinant of Φ, and $\Phi \overset{\times}{\times} \Phi$ will be the conjugate of the reciprocal of Φ multiplied by twice the determinant. If Φ represents the manner in which vectors are affected by a strain, $\frac{1}{2}\Phi \times \Phi$ will represent the manner in which surfaces are affected, and $\frac{1}{6}\Phi \overset{\times}{\times} \Phi : \Phi$ the manner in which volumes are affected. Considerations of this kind do not attach themselves so naturally to the notation $\phi = \alpha S\lambda + \beta S\mu + \gamma S\nu$, nor does the subject admit so free a development with this notation, principally because the symbol S refers to a special use of the matrix, and is very much in the way when we want to apply the matrix to other uses, or to subject it to various operations.

J. WILLARD GIBBS.

New Haven, Connecticut.

The Meaning of Algebraic Symbols in Applied Mathematics.

PROF. GREENHILL, on p. 462 (March 19), gives a naïve and most instructive description of the straits to which a "practical man" is put when he wishes to interpret the simplest general formula.

I have always held, not in sarcasm but in sorrow, that students brought up on the system of specialized and limited numerical formulæ used by Prof. Greenhill and some other Professors of Engineering in this country, must necessarily go through the tentative trial-and-error sort of process which he so graphically describes, whenever they have to obtain a numerical result from anything not already arithmetical. In other words they are unable to deal with complete algebraic symbols or concrete quantities.

The symbol "*v*" to them does not completely represent a velocity, it only represents a number; and to make it represent a velocity some words, such as "in feet per second," must be added. Whereas, since it is plain that the velocity of light does not vary with its numerical specification, nor the size of a room change according as it is measured in feet or in inches, it is surely better to make a symbol express the essential and unchanging aspect of the thing to be dealt with, *i.e.* the thing itself, and not merely the number of some arbitrary and conventional units which the thing contains.

May I, then, assure Prof. Greenhill very seriously, *and with entire appreciation of and accord with his insistence that all expressions should be complete and capable of immediate practical numerical interpretation*, that the equation $T = \rho v^2$ is perfectly complete, and that it is true and immediately interpretable in every consistent system of units that has been or that can be invented? T is the tenacity, ρ the density, of the material of a ring, and v is its critical or bursting velocity. There is no need to say a word more. And no properly taught student ought to have the slightest difficulty in obtaining a numerical result directly in any system of conventional units that may be offered him.

It is the frequent recurrence of such ghastly parodies of formulæ as—

$$T = \frac{62 \cdot 4}{2240 \times 144} \cdot \frac{\rho v^2}{g}$$

in many engineering treatises which makes them such dismal reading. It is a standing wonder to physicists how a man of Prof. Greenhill's power can fail to see the inadequacy and tediousness of expressions which are only true in one particular system of units, and which to be true even in that require the special statement of every unit employed. For not only is Prof. Greenhill's expression long in itself, but it is incomplete without the tiresome addition, "ρ being measured in so and so, v in &c., T in something else, and g meaning nothing more than the pure number 32·2." All this has been needlessly put into the formula, and so has to be wearisomely taken out again.

If there be any physicist who does *not* contend for the concrete interpretation of algebraic terms (wherein each symbol is taken to represent the quantity *itself*, and not merely a numerical specification of it in some conventional unit—see, for fuller explanation, NATURE, vol. xxxviii. p. 281), I trust he will write and uphold his position on the side of Prof. Greenhill.

I suspect that the cause of Prof. Greenhill's failure at present to recognize the extreme simplicity and reasonableness of the physicist's procedure is to be found, partly in a vague idea that in order to get numerical results in British units from a general expression it is necessary to work it out in C.G.S. units first, and then translate, which I assure him is not the case; and partly in the general difficulty which most people feel in thinking it possible that they can be mistaken.

I would gladly convince Prof. Greenhill if I could, because he would carry with him so many other teachers, and thus a mass of waste labour would be saved annually to several thousands of students. Would it be too much to ask him to consider the matter with care, and, if possible, from our point of view; setting me a sum to do if that would be any assistance towards bringing him to the desired point of view?

OLIVER J. LODGE.

Tension of a "Girdle of the Earth."

IT is perfectly true, as Prof. Lodge has asserted, that a cord or chain running on its own track as an endless band in a frictionless groove *of any form* will not require the sides of the groove to keep it in that form. But whatever velocity it moves with, such a tension will exist all round it as to resist the centrifugal forces of its windings; and to preserve them by virtue of the curvature and constant tension, invariable in shape however the speed of coursing of the belt may be increased, without any external guidance and assistance. If w is the cord's mass per unit of its length, and v the velocity with which it pursues its course, wv^2 is the tension in dynes which will be set up all round it. The speed may of course be so increased as to tear the cord or chain to pieces; and this will occur in steel tires of railway wheels, for example, if the train's velocity on which the wheels are carried is much more than 120 miles an hour. Mr. Bourne long ago pointed out, in his works on the steam-engine, that a very low limit of speed in railway trains is enforced for safety in view of this dynamical condition so as not to approach and exceed working and proof-stresses at least in the material of which steel wheel-tires are formed.

But the truth of the proposition $T = wv^2$ rests entirely on the supposition that the running cord or cable pursues *exactly its own curve* in its motion. For a tension of 30 tons per square inch to be reached in a maritime cable in virtue of its being carried round either at the equator or at any distance of latitude from the equator, it must be presupposed that while buoyed with its own submerged lightness so as to be practically weightless in the water, it must from one end point of support to the other *follow accurately* the equator's circle of curvature, or the circle of curvature of the small circle of latitude along which it is laid, because this is *the line of motion* along which its parts are carried along by the earth's rotation. These circles are practically straight lines for any mile or two of cable, and truly enough, if in the presence of even the weak force acting "centrifugally" on the cable's mass by the earth's rotation, it is attempted (supposing it to be quite weightless otherwise) to pull it as nearly straight as the hardly sensible curvature of the earth requires, mathematics would not yield its point an inch, and a pull of something like 30 *tons per square inch would be required.* If the natural weight of the cable is nearly 400 times as great as the counter-force on it produced "centrifugally," we could imitate this outwards, by supposing the earth to revolve about 20 times faster than it is actually doing; and then the end pulls required to make a telegraph cable relieved from gravitation and acted on only by this outward "centrifugal force" just equal to the cable's weight, to stretch it to as flat a curve as that of the earth's circumference and of its circles of latitude, would be about 20 × 30 tons per square inch, or vastly more, of course, than any cable could withstand! But what telegraph-engineer would ever dream of trying to stretch telegraph wires on poles along the banks of a canal so that even between poles they should everywhere *be absolutely parallel to* the water level surface in the canal? With their natural weight acting outwards against him he would know that the attempt would be sure to result in rupture! And even if the centrifugal force due to the earth's velocity in this latitude of about 1250 feet a second acted alone on the wires instead of the above supposed one of about 5 miles a second to represent an equivalent to gravity, he would surely not be surprised to find his attempt to pull the wires as straight as the surface of still water, end only unsuccessfully in breaking them?

A. S. Herschel.

New Athenæum Club, London, March 20.

Spinning Disks.

Mr. Wehage, of Berlin, has independently obtained Prof. Ewing's results for a solid disk by putting $p_1 = p_2$ for $r = 0$, in Grossmann's equations.

As Prof. Boys points out, Maxwell attacked the problem in 1850, at the age of 19. I have gone over his solution ("Papers," vol. i. p. 61), and find that, in consequence of two slips he has made, his results do not agree with Ewing's. His first equation of (57) should read $\frac{c_1}{2r^2}$, instead of $\frac{c_1}{r^2}$; and his third equation of (57) will then read $-\frac{c_1}{2r^2}$, vice $-\frac{c_1}{r^2}$. His final result (59) is not affected by this alteration, which only changes c_1 and not c_2; but, owing to a wrong sign, that equation should read

$$q = h_1 + \frac{\pi^2 k}{2gt^2}\left[-2(r^2 + a_1^2) + \frac{E}{m}(3r^2 - a_1^2)\right];$$

which, remembering that Maxwell's E and m are E and $E/(1+\mu)$ respectively in Ewing's notation [or Thomson and Tait's $9nk/(3k + n)$ and $2n$, respectively], is seen to be the same as

$$p_1 = h_1 + \frac{\omega^2\rho}{8}\left[(3 + \mu)a^2 - (1 + 3\mu)r^2 \right];$$

except that Maxwell takes tensions negative.

Maxwell's equation (60) is correct; but is not deducible from his (59).

As Prof. Pearson remarks (Todhunter and Pearson's "History of Elasticity," vol. i. p. 827), Maxwell seems to have thought the disk would yield first at the rim.

Cambridge, March 23. J. T. Nicolson.

P.S., March 28.—Since writing the above, I have read Prof. Pearson's letter (Nature, March 26, p. 488), in which he says that "Grossmann's results, such as they are, flow at once from Hopkinson's corrected equations." They flow, however, also at once from Maxwell's equations, if these are corrected in the manner I have shown above. Prof. Pearson is not correct in supposing that Maxwell's and Grossmann's solutions lead to different results for the position on the disk of the maximum hoop-tension. Maxwell probably thought the disk would yield first at the rim from his putting $r = a_1$ in his final result; but his corrected results are identical with Grossmann's for a hollow, and with Ewing's for a solid disk.—J. T. N.

The Stresses in a Whirling Disk.

By an unfortunate accident my letter on this subject was printed (Nature, March 19, p. 462) without correction in proof. There are several *errata*, due to slips in the manuscript.

In column 1, p. 462, line 10 from bottom, the words "hoop" and "radial" are transposed. Read "the hoop tension p_1 and the radial tension p_2."

In column 2, equation (7) should read

$$\frac{u}{r} = \frac{C}{r^2} + C_1 - \frac{(1 - \mu^2)\,\omega^2\rho r^2}{8E} \quad . \; . \; . \; . \; . \; (7)$$

Further down, the expression for the maximum radial tension in a disk with a central hole should read

$$\text{Max. } p_2 = \frac{\omega^2\rho}{8}(3 + \mu)(a_1 - a_2)^2.$$

And in the special case when the hole is very small,

$$\text{Max. } p_1 = 2 \times \text{Max. } p_2 = \frac{\omega^2\rho a_1^2(3 + \mu)}{4}.$$

Cambridge, March 20. J. A. Ewing.

The Poison Apparatus of the Heloderma.

Our largest United States lizard, the *Heloderma suspectum*, is too well known to science to require any special description here. In the Proceedings of the Zoological Society of London, for April 1, 1890, the writer published quite an exhaustive memoir upon the entire morphology of this famous reptile, and among other parts of its anatomy reference was made to its poison apparatus. Fig. 4 of Plate xvi., of the contribution in question, showed a superficial dissection of the under side of the head of a large Heloderm, and upon the left side of the same the submaxillary gland is turned outwards, thus rendering it possible to be seen the four structures leading from (or to) this gland to separate and as many foramina opposite them, which exist upon the external aspect of the ramus of the lower jaw. Heretofore, these four structures have very generally been looked upon as the four poison ducts leading from this gland to the hollow space in the mandible, which latter in turn had its upper outlets in the minute foramina, found one each at the base of the teeth of this jaw. The poison was supposed to pass from the gland through these four ducts into the body of the ramus, then through the above-noted foramina, whence it was conducted into any wound the reptile might inflict with its teeth, along the grooves which have for many years been known to exist upon them. Without making any especial microscopical description, this is practically the view I supported in my memoir in the Proceedings of the Zoological Society, and I added that "Fischer found in his specimen that these ducts branched as they quit the gland; this was not the case in the reptile examined by me. Each duct passes obliquely upwards and inwards through the lower jaw, and its internal opening within the mouth is found at the base of the tooth it supplies, near the termination of the groove of the tooth" (p 207). Dr. Fischer's paper was published in Hamburg in 1882, and in it he also gives a figure showing the ducts I have just mentioned; and he is largely responsible for the view that has been adopted in regard to them As early as 1857, however, John Edward Gray evidently leaned in the same direction, and he speaks of "*Heloderma horrida*, in which all the teeth are uniformly furnished with a basal cavity and foramen,"—structures which he compared with the poison fangs and associated parts of venomous serpents. Dr. C. K. Hoffmann had the same ideas about the poison apparatus of the *Helodermas* (Bronn's "Klassen des Thier-Reichs," Bd. vi., iii. Abth., 30–32 Lief., pp. 890–892), and he republished Fischer's figures; and thus running through the similar views of numerous other leading herpetologists, we find, even as late as 1890, Prof Samuel Garman, of Harvard University, quoting Fischer's description of the poison glands of *Heloderma* without question (author's reprint from Bull. Essex Inst., vol. xxii., Nos. 4–6, 1890, p. 9), and he adds that "no glands have been found on the upper" jaw. With all this in my mind, I was not a little surprised when Prof. C. Stewart, on January 20 last, in a paper read before the Zoological Society of London, claimed that he believed "that he had shown that in both species [*H. horridum* and *suspectum*] the ducts of the gland did not enter the lower jaw, but passed directly to openings situated under a fold of mucous membrane between the lip and the jaw. He thought that the structures previously described as ducts were only the branches of the inferior dental nerve and blood vessels." Upon hearing this, I at once took a large specimen of *Heloderma suspectum* to my friend Dr. E. M. Schaeffer, the well-known microscopist in Washington, D.C., who kindly examined the structures, and found them to be exactly as Prof. Stewart had described. It now remains to be said of what use are the foramina at the base of the teeth in these lizards. Why are the upper teeth grooved when there is no poison gland upon the upper jaw? Would not such a severe bite as a *Heloderma* is enabled to give kill "frogs and insects," even were no poison injected into such wounds? Are the grooves on the teeth there to conduct a poison into the wounds inflicted by the teeth of this lizard? Here in America the evidence would seem to be rapidly leading to the demonstration of the now entertained theory that the saliva of this heretofore much-dreaded reptile is possibly almost entirely innocuous. If it be proven, it is unnecessary to add that it leaves the great order *Lacertilia*, in so far as our present acquaintance goes with it, without a single representative possessed of the power of inflicting a venomous wound by the means of its bite.

R. W. SHUFELDT.

Smithsonian Institution, February 24.

The Recession of the Niagara Gorge.

BEFORE the survey of 1842, the only data for estimating the rate of recession of the Niagara Gorge were the observations of the people of the neighbourhood. Mr. Bakewell, in 1829, "was informed by Mr. Forsyth, the proprietor of the Pavilion Hotel on the Canada side, that during his residence of forty years the Falls had receded forty yards" (*American Journal of Science*, May 1857, p. 85). It is well-known that Sir Chas. Lyell, at the time of his visit to the Falls, came to the conclusion that the rate of recession was not over one foot a year. He based his estimate upon the statement of his guide, that between 1815 and 1841 the American Fall had receded forty feet. Modern investigators, basing their calculations on the surveys of 1842, 1875, and 1883, estimate the recession of the Canadian Fall at from three to five feet per year, and the age of the gorge from seven to ten thousand years (see Mr. Wesson's article in NATURE, vol. xxxii. p. 229; and Prof. Wright's "Ice Age in North America," p. 452 *seq.*). A recent American publication, "The Journal of William Maclay," a member of the Senate in the first Congress, 1789-91, brings to light the result of local observation of the recession of the Falls for the thirty years previous to the beginning of Forsyth's observations, which have generally been cited as the earliest. The passage in Maclay's journal reads:—"February 1st [1790]. . . . . Mr. Ellicott's accounts of Niagara Falls are amazing indeed. I communicated to him my scheme of an attempt to account for the age of the world, or at least to fix the period when the water began to cut the ledge of rock over which it falls. The distance from the present pitch to where the Falls originally were, is now seven miles. For this space a tremendous channel is cut in a solid limestone rock, in all parts one hundred and fifty feet deep, but near two hundred and fifty at the mouth, or part where the attrition began. People who have known the place since Sir William Johnson took possession of it, about thirty years ago, give out that there is an attrition of twenty feet in that time. Now, if 20 feet = 30 years = 7 miles, or 36,960 feet; answer, 55,440 years." In view of the fact that since 1842 the rate of recession has varied widely, this earlier testimony, so far as it can be relied upon, is especially interesting. It is possible that the observation related to the American Falls, as did that of Sir Chas. Lyell's guide; but there is nothing to indicate it, except that this is about the present rate of recession of the American Fall as calculated by Mr. Wesson. The intelligent attitude and fertile scientific suggestion of Senator Maclay are very remarkable in 1790. It will be remembered that Hutton's "Theory of the Earth" was not published separately till 1795. Maclay seems to have anticipated men of science by many years in proposing to use the recession of the gorge as a scale for measuring geological time. Perhaps even more noteworthy is the unconcern with which he sets down his conclusion that the gorge was over 55,000 years old, at a time when geology was still confined by the narrow and seemingly impassable limits of traditional Biblical chronology. Maclay's informant, Ellicott, was a Government surveyor, and, according to the account of his life in Appleton's "Cyclopædia of American Biography," "he was selected by Washington in 1789 to survey the land lying between Pennsylvania and Lake Erie, and during that year he made the first accurate measurement of the Niagara river from lake to lake, with the height of the falls and the descent of the rapids." This early survey seems entirely unknown to geologists, who all refer to Hall's of 1842 as the first. Ellicott was a man of considerable attainments, a professor in West Point from 1808 to 1820, and the correspondent of European learned societies. He left some published writings, and works still in manuscript. Cannot the data of this survey of a century ago be found? If it proved a careful one, it would double the period of scientific observation of the recession of the gorge, and increase the certainty of any generalization. The map of Evershed's survey of 1883 was published in *Science*, vol. v. p. 398. In NATURE, vol. xxxii. p. 244, Mr. Garbett quotes some interesting observations from the *Gentleman's Magazine*, January 1751, from Kalm's description of the Falls as seen shortly before. Mr. Garbett tries to identify an island that Kalm mentions as intersecting the Falls as Luna Islet. This would give a definite point for calculation. It seems to me that an attempt to test Mr. Garbett's conjecture by the Evershed map must show that it is untenable. If I have understood him, and correctly measured in accordance with his suggestions, the recession of the Canadian Falls will have been about half a mile in the 133 years, or about twenty feet a year. Notwithstanding Kalm's small dimensions, I think he meant Goat Island, for he says the island lies parallel with the river, and Luna Islet prolonged would lie almost at right angles to the river at its lower end.

EDWARD G. BOURNE.

Adelbert College, Cleveland, March 5.

Modern Views of Electricity.

I WILL reply to the last paragraph of Dr. Lodge's letter in NATURE of March 19 (p. 463) in his own language. I have no objection to contemplate a piece of isolated zinc surrounded by straining oxygen atoms, each negatively charged. And on the hypothesis that such atoms exist, I have no difficulty in realizing the depression of potential of the zinc, nor do I fail to appreciate the momentary transfer of electricity accompanying the sudden approach of the crowd of oxygen atoms when contact is made between the zinc and copper.

If we assume that atoms of oxygen have a negative charge, and if we further assume that, whether or not actual chemical combination takes place, they are attracted by zinc, and in a less degree by copper, then I admit that we have, on Dr. Lodge's principles, a consistent explanation of certain facts, viz. that, in an oxidizing medium at zero potential, isolated zinc has potential $-1 \cdot 8$ volts, and isolated copper has potential $-0 \cdot 8$ volt, and that, when metallic contact is made between them, both assume a common potential $-1 \cdot 3$ volts. So far I understand, and with only slight modification can accept, the theory developed in "Modern Views."

But I cannot, on this hypothesis, account for the slope of potential assumed (p. 111) to explain the aluminium needle experiment. Suppose the zinc and its contiguous air film to be exactly inclosed by a surface S. There is then within S an excess of negative electricity. Dr. Lodge says it is wholly on the oxygen atoms within the film, and not on the zinc—which statement I accept. Then, at every point in external space we have lines of force converging towards S, as to any negatively charged body—that is, a slope of potential downwards towards S, and at all points not too distant this will be appreciable. When contact is made with the copper, the excess of negative electricity within S is diminished, and the slope of potential at any external point is diminished in the same proportion.

Another, and, I think, quite distinct view which Dr. Lodge seems disposed to entertain is, that the atoms (or molecules) of oxygen within the air film on the surface of the zinc, without having any actual charge, are polarized—that is, have one pole positively and the other negatively charged, and present the negative pole to the zinc. So that the zinc is inclosed in a shell exactly analogous, *mutatis mutandis*, to a closed magnetic shell with its negative face inwards. I admit that such an arrangement, could it exist, would possess the properties attributed by Dr. Lodge to the air film. It would have no influence at any external point, and would give a negative potential to the inclosed zinc. But a difficulty arises *in limine*. To polarize the atoms in this manner, or to arrange them in this manner if independently polarized, requires work to be done. And from what source is the energy derived, no actual combination taking place between the atoms in question and the zinc?

S. H. BURBURY.

Mud Glaciers of Cromer.

BETWEEN Cromer and Trimmingham, it is well known, the cliffs are largely made up of boulder clay and chalky loam, in the contorted drift. The lower part is, generally speaking, composed of more compact boulder clay, while the upper portion, sand and loam, is eroded backwards more rapidly by constant slipping; this slacks by the action of rain and springs, and becomes a feeding-ground for a number of mud-streams, which creep down the gulleys or fall as mud-avalanches over the cliffs of the more resistant boulder clay below. This coalesces, and forms mud glaciers, and flows outwards upon the sands. The ends spread out, and assume the convex form usual with ordinary glaciers. The edges are crevassed longitudinally by being stretched open by the more rapid central currents, while the centre is well crevassed in the process of fanning out. The largest stream measured 60 feet across the fan, where it would be about 12 feet deep as far as exposed above the sands, for it sinks some distance into it, with a front of 6 feet.

Its slow advance causes the sand upon the shore in front to pucker into folds or convolutions, which crack, and large flakes or cakes of sand are pushed forwards in a slanting direction over those in front.

Every tide obliterates these marks and inequalities, but the distance the flanges of these flakes overlap between each tide indicates a movement of about 5 or 6 inches every twenty-four hours.

Here we have no fracture and regelation, or molecular disturbance in the exact sense of ice movement, yet the forms and currents are so similar, that with a slight covering of snow it would be difficult or impossible to detect any difference between mud and ordinary ice streams.

The convolutions and overlapping of the sands in front may not be without its value when studying the forms of the contorted drift, where folding is frequent, and the sliding of one bed over another apparent from the frequent repetition of the same bed in a vertical section. The slanting and concentric laminæ so frequently seen in our boulder clays, inclosing beds of stratified sands and gravels, may also, in some cases, be explained by the running down and overspreading of reconstructed boulder clay in a similar way to the mud-streams of Cromer, where it can be seen now in progress.

Sutton Coldfield. William Sherwood.

On Frozen Fish.

During the last few weeks there have appeared in Nature several letters containing extraordinary statements of the vitality of fish that have been frozen in ice. I have just found in a quaint old book—the "Anatomy of Sleep"—two further statements on this subject, and have therefore extracted them, thinking that they might prove of interest to readers of Nature. The first statement is quoted by the author (Dr. Binns) from Franklin's "Journey to the Polar Seas," p. 248:—"The fish froze as fast as they were taken out of the net, and in a short time became a solid mass of ice; and, by a blow or two of the hatchet, were easily split open, when the intestines might be removed in one lump. If, in the completely frozen state, they were thawed before the fire, they recovered their animation. We have seen a carp recover so far as to hop about with much vigour, after it had been frozen six-and-thirty hours."

The second statement is quoted from Isaak Walton ("Complete Angler," p. 257), who "quotes Gessner for the fact of some large breams being put into a pond, which was frozen the next winter into one mass of ice, so that not one could be found, and all swimming about again when the pond thawed in the spring," which phenomenon seemed to Walton "a thing almost as incredible as the resurrection of an atheist."

Dr. Binns further quotes, from the *Quarterly Review*, a statement which goes to show that *mosquitos* have been frozen on to the surface of a lake in the evening, and thawed again by the morning sun into animation. I should like, therefore, to inquire, firstly, whether there are any records of cold-blooded vertebrates other than fish being thus frozen and recovered to life; secondly, whether any such thing is known of the invertebrates, such as Mollusca, Echinodermata, and Vermes; and thirdly, whether this reported freezing and subsequent recovery of insects can be confirmed? In connection with this last class, I ought perhaps to mention that caterpillars have, I believe, been reported to recover animation after being frozen.

March 27. F. H. Perry Coste.

On the Presence of a Sternum in *Notidanus indicus*.

I have just found that the omosternum of *Notidanus*, described by me in a recent number of Nature (vol. xliii. p. 142), was originally described by my friend Prof. W. A. Haswell, of Sydney, who, in his paper "Studies on the Elasmobranch Skeleton" (Proc. Linn. Soc. N.S.W., vol. ix., 1884), has the following passage:—

"The shoulder-girdle is remarkable for the presence in the middle ventral line of a distinct four-sided lozenge-shaped cartilage let in to the arch, as it were, in front. This is a condition which I have not observed or seen described in any other form; it does not seem to occur either in *Heptanchus cinereus* or in *Henanchus griseus*. The intercepted cartilage is temptingly like a presternal, but the absence of such an element in any group nearer than the Amphibia seems to preclude this explanation."

Dr. Haswell gives no figure of the shoulder-girdle, but from the above description it would appear that the post-omosternum was absent in his specimen. T. Jeffery Parker.

Dunedin, N.Z., January 27.

Cackling of Hens.

I have recently published the following letter in the *Field*; but not having so far received any answer to the question which it presents, I should like to republish it in your columns, in order to ascertain whether any ornithologists to whom I have not already applied may have any information to give upon the subject. George J. Romanes.

Christ Church, Oxford, March 28.

Cackling of the Hens of Jungle Fowl.

"Can any of your readers inform me whether or not the hens of the wild jungle fowl (*Gallus bankiva*) cackle after laying their eggs, in the manner of their domesticated descendants? I cannot find any literature upon the subject; but if wild hens do cackle in the jungle, surely somebody must have heard them. Mr. A. P. Bartlett informs me that, when confined in shrubberies of the Zoological Gardens, they do not cackle; and, therefore, if nobody has ever heard them do so in a state of nature, we may fairly infer that the instinct is a product of domestication. If this should turn out to be the case, it would be a somewhat remarkable fact, and would, moreover, lead to the further question whether there are any parts of the world where domesticated poultry do not cackle."

Wood's Holl Biological Lectures.

A review of some lectures given by American naturalists at the Wood's Holl Laboratory appeared in your columns on March 19 (p. 457), signed with the initials "R. L." As there are statements made in that review for which I should much regret to be held responsible, I beg you to allow me to prevent any mistake which might arise from the similarity of the reviewer's initials to my own, and to state that the review was not written by me. E. Ray Lankester.

Bembridge, I.W., March 30.

New Comet.

At 9 p.m. on Monday, March 30, I picked up a bright, round nebulous object in Andromeda which I could not identify. I soon found it in rapid motion to the south, and its cometary nature was therefore placed beyond a doubt.

The position of the object is approximately R.A. 14°, Decl. 43° north, and its motion is carrying it quickly towards the sun, so that it becomes important it should be immediately observed for place and its orbit computed. It will probably disappear in two or three weeks.

At 4.30 on the morning of Tuesday, March 31, the new comet was very distinctly visible, though the gibbous moon was shining brightly at the time.

At the end of the present week the comet will be near β Andromedæ, and a little sweeping in the region of this star will almost certainly reveal it, for it is too conspicuous to be overlooked even in a comparatively small telescope.

Bristol, March 31. W. F. Denning.

THE ASTRONOMICAL CONGRESS.

Our readers will be glad to know that the invitations issued by Admiral Mouchez to the Directors of the various Observatories interested in prosecuting the photographic chart of the heavens have met with a very ready response. It is confidently expected that this Congress will be the last that will be found necessary, and before the astronomers have separated it is hoped that the scheme will have received its final shape, and that no important detail will have escaped attention.

The English contingent is represented by the Astronomer-Royal, Dr. Gill from the Cape of Good Hope, Mr. Plummer from Oxford, Captain Abney, and Mr.

Knobel. Of German astronomers, while the Congress regrets the absence of Prof. Vogel, it will welcome from the Potsdam Observatory Dr. Scheiner. Prof. Kopteyn of Groningen is also expected. Prof. Bakhuyzen comes from Leyden. Italy is represented by Prof. Tacchini of the Collegio Romano, Padre Denza, the representative of the Vatican, and M. Riccò of Palermo. Russia sends two astronomers, Drs. Donner of Helsingfors and Belopalsky of Pulkova. The Chilian troubles have not prevented the Government of that country accrediting M. Maturana; while Brazil illustrates its interest in the scheme by despatching M. Beuf of Rio de Janeiro.

Of course the French astronomers are strongly represented: M. Rayet of Bordeaux, M. Trépied of Algiers, and M. Baillaud of Marseilles, represent the more distant French Observatories; while of the Paris men of science, in addition to MM. Mouchez and Janssen, we may confidently expect the co-operation of MM. Faye, Wolf, Cornu, and Henri *frères*.

An informal meeting was held on Saturday last at the Observatory to draw up a paper of agenda containing the points which it was thought that the Congress should consider. This does not exclude any other points which may prove of interest in the course of the debate.

The following programme, to be submitted to the General Congress, was drawn up:—

1. Verbal reports on the astrophotographic installations.
2. Exhibition of the photographs obtained, and the discussion of the most desirable point of exposure with regard to the focus.
3. The mode of printing the *réseau*.
4. Report on the various kind of plates and photographic processes and developments.
5. On the formation of a catalogue of guiding stars.
6. (*a*) On the desirability of increasing the distance of the guiding stars from the centre of the plate.
(*b*) On the best form of micrometer for the guiding telescope.
7. On the orientation of the plate.
8. Is it desirable that the plates for the catalogue and the chart be taken on the same evening?
9. To determine the length of the exposure in the catalogue plates, and whether it be desirable to increase it.
10. Is it desirable to make three exposures on the same plate?
11. To fix the minimum diameter which the 11th and 14th magnitude star images shall have on the plate.
12. To discuss the possibility of determining a relation between the diameter and time of exposure.
13. To obtain a definition of the 14th magnitude.
14. The method of measuring the negatives and the employment of the *réseau*.
15. To discuss
(*a*) The number of fundamental stars for each negative.
(*b*) The choice of these stars.
(*c*) The proper plan to assure the meridian observation of these stars.
16. On the method of reproduction of the plates for the chart.
17. (*a*) To discuss the desirability of a Central Bureau for measuring and discussing the plates.
(*b*) Shall Observatories possessing measuring instruments begin to measure at once?
18. To discuss when the work should be begun.
19. Should there be plates of longer exposure in the neighbourhood of the ecliptic?

No. 10 seems to require some explanation. It has been found by MM. Henry that the visibility of minute stars is much assisted if there be three exposures a line; and it is possible that a proposition will be made to do away with the very short exposures and to substitute three separated from each other in length of exposure by an equal or nearly equal interval of time.

PHOTOGRAPHIC PERSPECTIVE AND THE USE OF ENLARGEMENT.

It is not uncommon to hear it remarked that photographs make hills look low, or that they make things look "such a long way off"; and that they do so in a great many cases is perfectly true.

In explanation of the apparent lowness of photographed mountains, I have heard it suggested that the eye judges horizontal and vertical distances by different standards, and this, too, is probably the case; but since there is a horizontal and a vertical in a picture as well as in nature, the eye ought to form similar judgments on both.

The true meaning of the appearances alluded to, though they admit of a most simple explanation, is not as generally understood as might be expected.

The fact is that they depend merely on perspective.

In elementary books on drawing there often appears a diagram in which imaginary threads are supposed to be stretched from every point of an object, through an upright sheet of glass, and to intersect in some point behind it. The trace of these threads on the glass will there form a picture of the object which is in true perspective, when viewed from the intersection of the threads; and if the proper amount of light, shade, and colour be supposed to be added, this picture, to the single eye so placed, would be absolutely undistinguishable from the object itself.

But now suppose the eye is not at the place of intersection of the threads, but a certain distance further off or nearer to the glass. It is evident that the apparent angular magnitude of every object in the picture is altered in the ratio of the distance of the intersection of the threads to the distance of the eye from the glass. But this is exactly what would be the case, if, keeping the eye at the intersection of the threads, a new picture were formed on the glass either by altering the size of the real objects in this ratio, or their distance from the glass in the inverse ratio.

For instance, let the objects forming the picture be two towers, one say half a mile off and the other a mile, and suppose that the intersection of the threads is one foot behind the glass; to the eye placed at that distance the towers in the picture will subtend the same angle as they do in reality; but if the eye be moved a foot further from the glass these angles will be halved, and the same picture will then fall on the retina as would be formed there were the eye one foot from the glass and the towers only half their actual size, or if they were removed to the distances of one mile and two miles respectively.

Thus by viewing the picture from the wrong distance, either the apparent size of the objects represented by it is multiplied by the ratio $\frac{\text{true distance}}{\text{wrong distance}}$, or their apparent distances by $\frac{\text{wrong distance}}{\text{true distance}}$.

Putting this in symbols, for the sake of simplicity and brevity, we have, if D = true distance of an object from the point of view, A = its real linear magnitude, F = distance at which the picture must be viewed in order to convey a correct impression of D and A. Then if d, and a, are the values corresponding to D and A, when the picture is seen from the distance f, we have $d = \frac{f}{F} D$ when A is judged correctly; $a = \frac{F}{f} A$ when D is judged correctly. Of course both A and D may be misjudged, but apparent and true distances and sizes are still connected by the relation

$$ad = AD.$$

In a photograph, F is the focal length of the lens with which it was taken, and f the distance at which it is looked at. Thus, if, as is generally the case with all moderate sized pictures, the focal length of the lens is less than the distance one would naturally hold the picture at for convenient view, the inevitable result is either that the apparent distances of the picture are greater than the real ones in the proportion of $\frac{f}{F}$, or that the apparent sizes of the things represented in it are reduced in the proportion $\frac{F}{f}$, or a combination of both these wrong impressions is produced.

Which of these effects or what combination of them is suggested depends much on the nature of the picture itself.

In interiors taken with a wide-angle, short-focussed lens, distances are enormously exaggerated, while in landscapes it is generally the sizes of things which seem diminished.

As a rule, it may be said that objects which do not themselves suggest any scale will be made to look small, while those which do, such as men, houses, &c., will appear distant.

When $\frac{F}{f}$ is greater than unity, *i.e.* when the picture is viewed too near, the reverse of the above effects is seen; and as far as the perspective is concerned, the scene is being viewed through a telescope.

The magnifying power of a telescope is the focal length of the object-glass divided by the focal length of the eye-piece, or, in other words, the distance from the lens at which the image is formed divided by the distance from which it is viewed.

If the focal length of the eye-piece is the same as that of the object-glass, there is no magnification, and in the field of the telescope will be seen an exact reproduction of the natural view.

When, however, by shortening the focal length of the eye-piece, magnification is obtained, foreshortening of all the distances in the ratio $\frac{F}{f}$ naturally takes place.

This may be practically illustrated in rather a striking way by looking from a railway bridge along a straight piece of line at an approaching train.

Supposing the train to be travelling at forty miles per hour, if the telescopic power be forty, the apparent rate of approach will be only one mile per hour.

From what has been said, it will be clear that just the same laws apply to photographic pictures (or any pictures in true perspective) as to telescopic images, and that there is only one distance at which they will convey a correct impression to the eye.

This being so, it is evident that any photograph taken with a lens of less than about a foot focal length, must exaggerate all the distances, or make objects in the picture look smaller than they should, and the only remedy for this is to enlarge the picture until the right distance to view it from becomes also the convenient distance.

Even if this be done, however, there is still a tendency to view the picture too far off; for few lenses, except those for portraits embrace an angle so small as to be taken in at a single glance, and people are naturally inclined to stand far enough from a picture to see the whole of it at once.

Still, a proper amount of enlargement offers the best means of making a photograph give a true idea of the scene which it represents; and this is especially true of the small pictures taken by so-called "detective" cameras, having lenses varying from four to six inches in focal length; and it is for this end, and not, in general, to enable more detail to be seen, that the enlarging process is most useful.

Of course, negatives for enlargement must be well enough defined to bear being examined from the focal distance of the lens which took them, or less than this (since detail is lost in the enlarging process), and many which would pass muster well enough when held a foot or more off will be found imperfect when looked at from the lesser distance.

In a subsequent article I will, if the Editor permits, enter more fully on the subject of photographic definition and its limits, both as they depend on the nature of the various sensitive films, and on the lenses by which the image is formed.

A. Mallock.

ÉMILE GAUTIER.

COLONEL GAUTIER was born on April 18, in the year 1822, at Geneva, where he made his first studies. When he had concluded his course at the ancient Academy, his tastes and natural talents inclined him to astronomy, the science to which he had been early initiated by his uncle, Alfred Gautier, then Director of the Observatory. In order to perfect himself in his studies, he went to Paris, where, thanks to the recommendations of his uncle, and more especially of Frederic Maurice, a member of the Institute, with whom he was on excellent terms, he entered the Observatory and became the pupil of the celebrated astronomer Leverrier. One of the early recollections that Émile Gautier loved particularly to recall was the time when he worked under the direction of the illustrious *savant* at the calculations of the perturbations of the planets; he went over again in duplicate all the calculations relative to the perturbations of Uranus, calculations which Leverrier had presented to the Academy of Sciences in November 1845.

After a stay of about two years in Paris, he returned to Geneva, and published in December 1846 a thesis prompted by the mathematical works in which he had taken part; it was entitled "An Essay on the Theory of the Perturbations of Comets." In the year 1847, he sojourned for several months in England, where he made the acquaintance of several eminent English astronomers, among whom were Airy, Challis, Adams, and perhaps others.

On his return he spent some months at Paris, where he made various astronomical researches, and determined among others the elements of the planet Metis. He afterwards returned to Geneva, where, in conjunction with Emile Plantamour, then Director, he worked at the Observatory, especially at magnetic observations.

In 1849 he married Mdlle. V. Sarazin Maurice, granddaughter of Frederic Maurice, already mentioned. He leaves two sons: the elder, Lucien, is at present Professor of Hebrew at Lausanne; the younger, Raoul, in the beginning of 1890 was nominated Professor of Astronomy at the University of Geneva, and succeeded his father at the commencement of the same year as the Director of the Observatory.

In the early part of the year 1850, pressed by Colonel Aubert and General Dupour, Gautier turned his attention more especially towards a military career, for which he possessed remarkable qualities. His advancement was rapid, and he was considered one of the best officers in the Swiss army. We need not here follow this part of his life, nor note the various administrative functions in connection with which he introduced useful innovations. This subject would lead us far from the limits of this article. We may say, nevertheless, that in his numerous occupations he never lost sight of astronomy, but constantly made himself acquainted with its current progress.

In 1860 he went to Spain to observe the total eclipse of the sun on July 18 at Taragona (Aragon), and gave an account of his observations in the *Archives des Sciences Physiques et Naturelles*, a publication in which the majority of his works were printed. About this time he had recognized the nature of solar prominences, and defended his opinion strenuously against astronomers who still regarded them as appendages of the moon. Afterwards he occupied himself chiefly with the constitution of the sun, and with the study of spots and prominences. He presented the Observatory with a direct-vision spectroscope by Hoffman, the instrument with which he pursued his researches up to the time when he was confined to his bed in October 1890.

On the death of Émile Plantamour, Gautier was nominated Director of the Observatory of Geneva, and from the outset displayed great activity in endowing this establishment with new instruments. In meteorology the eye observations have been completed by a self-recording barograph by Hipp, reversing thermometers by Negretti and Zambra, self-recording thermometer and hygrometer by the brothers Richard, and during the last year an anemograph by the same makers. The Meteorological Station of Great Saint-Bernard, which is a sub-station to the Observatory of Geneva, had been likewise supplied with a barograph. In chronometry, an important branch of the Observatory, apart from the usual tests installed by É. Plantamour, Gautier instituted two competitions for the studies of errors of compensation. He has presented the Observatory with an electrical pendulum of extreme accuracy made by Hipp.

If we speak of the man, all astronomers with whom he was acquainted, and they were many, will agree in saying that he was the type of a true gentleman. His cordial welcome, his frankness, his good nature, and his readiness to help, gained for him in succession the love of everyone. A day of great happiness for him was that on which the Astronomische Gesellschaft resolved to make Geneva the place of meeting in 1885; those who took part in this Congress were able to enjoy his good nature and frank hospitality, and yet a few days before he experienced a cruel grief.

Émile Gautier died from heart disease, by which he was carried suddenly away on the night of February 24–25.

A. R.

NOTES.

THE "Flora of British India" has reached the seventeenth part—in other words, the first part of the sixth volume has appeared, and Sir Joseph Hooker may be congratulated on having so nearly accomplished his great task. Since his retirement from the Directorship of Kew Gardens, Sir Joseph has worked single-handed, and several large families yet remain to be done, notably the grasses, which are very numerous, and, in some respects, more difficult than the petaloid monocots, and mainly so because the majority of the species have a much wider area of distribution, thus entailing more literary research. The last published part of "The Flora of British India" is of more than ordinary interest, inasmuch as it contains the conclusion of the descriptive account of the orchids of India. About 1400 species are described; they are referred to upwards of a hundred genera, and they constitute about ten per cent. of the flowering plants of that vast country. This is a larger proportion than that recorded for the rich orchid flora of Mexico and Central America. Among epiphytal, or tree orchids, the beautiful genus *Dendrobium* contributes upwards of 150 species, and *Habenaria* among ground orchids numbers 106 species. All lovers of orchids will welcome this masterly synopsis of the Indian species; and all botanists will wish the eminent author health to finish his great work.

THE Camera Club has issued the programme of the fifth annual Photographic Conference, which will be held in the theatre of the Society of Arts on Tuesday and Wednesday, April 7 and 8, under the presidency of Captain Abney. On Tuesday Captain Abney will deliver his presidential address, and papers will be read by Mr. L. Clark, Mr. Joseph Pennell, and the Rev. F. C. Lambert. Major Nott, Mr. C. V. Boys, and Sir H. T. Wood will read papers on Wednesday, and in the evening the members and their friends will dine together at the Criterion Restaurant. All photographers are invited to attend the Conference. The annual exhibition of photographs by members will be on view in the Club-house meeting-room, Charing Cross Road, from 10 a.m. to 4 p.m., and it will remain open for about six weeks.

THE Congress of German Geographers was opened in Vienna yesterday. A Geographical Exhibition in connection with the Congress was on view at the University. The exhibits consisted chiefly of photographs, maps, and charts.

THROUGH the death of Mr. Henry Groves, on March 1, at the age of fifty-six, of paralysis, the English colony at Florence has lost one of its most active scientific members. Mr. Groves had been in business in Florence as a pharmaceutical chemist for about thirty years, and had given the whole of his leisure time to the investigation of the flora, not only of Tuscany, but of nearly the whole of Italy, of which he possessed an exceptional knowledge. His vast stores of information were always at the service of English and other visitors to Florence. His dried collection of Italian plants was probably unrivalled in extent, and has, we are informed, become the property of the Central Botanical Society of Tuscany.

ON Thursday last, about midday, two distinct shocks of earthquake were felt at Boscastle, North Cornwall. The first shock was very severe, shaking the windows and furniture in some of the houses. The second shock followed in about two minutes. It was not quite so severe, but was distinctly felt. Some people state that they felt the ground trembling under their feet for several seconds.

DR. H. WILD read a paper before the Imperial Academy of Sciences of St. Petersburg on January 16 last on the use of the electric light for photographic self-registering instruments. He finds that it is more economical than gas or petroleum, and gives more uniform and sharper curves. Also that it reduces the possibility of loss of continuous registration to a minimum, and completely prevents the disturbance caused by increase of temperature when gas or oil is used.

MR. C. CALLAWAY has submitted to the Shropshire County Council a report on technical education; and now the Staffordshire County Council has invited him to prepare a similar report on technical instruction in the district occupied by the industries of North Staffordshire.

MR. R. L. WEIGHTON has been appointed Professor of Engineering and Naval Architecture at the Durham College of Science, Newcastle-upon-Tyne.

DR. WILLIAM SOMERVILLE, having been appointed to the chair of agriculture and forestry recently founded in the Durham College of Science, Newcastle-upon-Tyne, will begin his duties early in the summer. The College has acquired 15 acres of land at Gosforth for the purposes of an experimental station, and it is hoped that smaller stations will be established in other parts of the district. It is the desire of the College that the members of the staff of its agricultural department should assist in the establishment of a system of agricultural education throughout the adjoining counties, partly by a system of "extension lectures" and partly by conducting special classes for teachers.

THE *Revue Biologique du Nord de la France* publishes a paper by M. J. de Guerne on the new steam yacht recently built in London for the Prince of Monaco. The *Princess Alice* is, in fact, a sailing vessel with an engine and screw-propeller to help it; the proprietor much prefers sailing to steaming. The boat contains three laboratories very well equipped for zoological work, as well as photography and oceanography; and a short trial cruise is to be made in the autumn.

AT the Royal Institution, Mr. J. Scott Keltie will on Tuesday, April 7, begin a course of three lectures on the geography of Africa, with special reference to the exploration, commercial development, and political partition of the continent; Prof. Dewar will on Thursday, April 9, begin a course of six lectures on recent spectroscopic investigations; and Prof. Silvanus P. Thompson will on Saturday, April 11, begin a course of four lectures on the dynamo. The Friday evening meetings will be resumed on April 10, when Sir William Thomson will give a discourse on electric and magnetic screening.

PROF. KARL PEARSON proposes to deliver at Gresham College lectures on the following subjects: April 14, the geometry of motion; April 15 and 16, matter and force; April 17, the classification of the sciences. These lectures begin at 6 o'clock p.m., and are free to the public.

THE following lectures on scientific subjects are to be given at the Royal Victoria Hall:—April 7, "Minute Things in Nature," by Prof. Rupert Jones; April 14, "Extinct Volcanoes of France," by Dr. Crosskey; April 21, "Mountain Exploration in the Caucasus," by Mr. Dent.

LAST August various gentlemen connected with agricultural industry in different parts of the United Kingdom were invited by the High Commissioner for Canada to visit the Dominion, to report upon its agricultural resources, and the advantages the country offers for the settlement of farmers and farm labourers. The reports of these gentlemen have now been issued. If published together, they would have made a rather bulky volume; so it was decided that they should be divided into four parts. They present a great mass of useful and well-arranged information, and are carefully illustrated.

THE *Revue Mensuelle de l'École d'Anthropologie de Paris*, of which three numbers have now been published, is likely to do much good work in extending among the educated classes of France a knowledge of anthropological science. Among the contributors are MM. G. de Mortillet, André Lefèvre, and C. Letourneau.

ACCORDING to a telegram sent through Dalziel's Agency, a magnificent grotto has been discovered near Ajaccio. It is entered with difficulty, owing to the smallness of the aperture; but upon his entrance the explorer finds himself in a vast and lofty hall, the sides of which are some 25 yards in height. From this there are several passages leading to an indefinite number of other chambers. A thorough investigation of the grotto has not yet been made.

THE following are some results of a study of 197 thunderstorms in Russia in 1888, with reference to their speed of travel. The author (Herr Schönrock) obtained as mean velocity about 28·5 miles an hour, with variation from 13 to 50 miles. In the hot season the velocity was less than in the cold (28 m. against 32 m.). It was least in the early morning, then increased, at first slowly, then faster, reaching a maximum between 9 and 10 p.m. Thunderstorms travel most quickly from south-west, west, and north-west. An interesting geographical difference was observed. From west to east the velocity increased at first; but about 30° to 35° east longitude a maximum was reached, and further east the speed declined; the decline showing, however, a secondary maximum between 45° and 50°.

THE Annual Report of the Berlin branch of the German Meteorological Society contains the results of rainfall observations at a number of stations in and near Berlin for the year 1890. This year is among the driest experienced since 1848, when regular observations were begun; the months of February and September, especially, are the driest on record. Dr. Hellmann, the Secretary, has carried on some useful experiments to determine the influence of the height of rain-gauges upon the records of rainfall—a matter of considerable importance in towns, owing to the difficulty of obtaining a good exposure at a low level. He finds that about a quarter of the rainfall is lost in an elevated exposure, such as on the roof of a house, during strong winds; but he arrives at the important conclusion that an elevated exposure is permissible if the gauge can be protected from the disturbing influence of the wind. The Report also contains a list of the severe winters since 1728. The coldest winter was 1788; on December 28 a minimum of $-21°·6$ was recorded.

A KNOWLEDGE of how water behaves with regard to passage of different light-rays through it is important, not only for determining the colour of the substance, but also for ascertaining what rays penetrate to the inhabitants of ocean depths. This matter has been studied lately by Herren Hüfner and Albrecht, with the aid of the new spectro-photometry (*Wied. Ann.*). Above or below the spectrum obtained directly from a beam of sunlight passing to the spectro-photometer, was thrown one from an equally strong beam which had passed through a water column of known length. The intensities of the two spectra were then compared in a series of sections by means of the polarization apparatus, and the coefficients of extinction determined. It was proved that the light-extinction by water is in general greater the longer the wave-length. But the curve of transmitted light has not a regular course; in the region of D and G, where broad absorption bands have been observed, it shows sudden rises.

ABOUT 18° C., according to Herr Kleinstrick (*Beibl.*), Japanese and ordinary wax sink in water, but above 18° they float. This is because the wax has a much greater coefficient of expansion than water, and under 18° it has a higher specific gravity.

THERE is reason to believe that the indications of tromometers are sometimes vitiated by wind. This matter has been lately investigated by M. Carcani, who made inquiry at four Observatories (Rome, Mestre, Florence, and Rocca di Papa), the tromometers of which were quite isolated from the floors or walls of buildings. At the last-named station, the instrument is in a cavern several metres under the surface. M. Carcani finds, in many cases, synchronism between the maxima of wind and of tremors; on an average, in 84 per cent. of the observations. As it is difficult to suppose this coincidence fortuitous, it is inferred that wind-pressure enters as an important factor into the movements of the tromometer. It is pointed out that, in the case of a tromometer isolated in a building, the oscillations caused in the latter by wind exert pressure on the ground, and so affect the instrument. In a case like that of Rocca di Papa, the direct pressure of the wind on sloping ground has to be considered; the vibrations so produced may be transmitted to some distance from their place of origin, like tremors of earthquake nature. Mr. Carcani finds, in this particular case, many instances of rise and fall in the tromometric intensity, corresponding with rise and fall of wind. In view of these facts, tromometric observations, he considers, should be made only when the wind is not very high.

WITH reference to observed changes in the earth's axis of rotation, it has been pointed out that through changes in distribution of air-pressure and movement of water masses, considerable differences of level in the ocean may be produced. Herr Lamp notes (*Astr. Nachr.*) the displacement northwards of the maxima of air-pressure in the trade-wind region, and of ocean currents, as the sun rises in summer. Thus a certain quantity of water passes over in summer from the southern to the northern hemisphere; and it is improbable that compensation takes place by means of under-currents. As the year advances, water passes back to the southern hemisphere, reaching there a maximum in our winter. This periodical transference of mass is supposed to cause periodical variation in the earth's axis. Herr Lamp calculates that to cause a change of latitude of $0''\cdot5$, it would be sufficient that at 180° longitude from Berlin, a water-mass of 2500 cubic metres should move in a meridional direction from 30° S. lat. to 35° N. lat.; and that with reference to the oceanic area concerned, we need only suppose a mean elevation of 10 cm. (or 4 inches) in the sea-level.

THE Director of the Agricultural Experiment Station of the Agricultural and Mechanical College for the State of Alabama has issued his Bulletin No. 13, which is devoted to an exhaustive account of the different varieties of cotton grown in the State, by Mr. P. H. Mell, the botanist and meteorologist to the station. According to Mr. Mell, only three species of *Gossypium* are of special commercial importance, viz. (1) *G. Bahma*, or Egyptian cotton; (2) *G. barbadense* or *nigrum*, Sea-Island cotton, or long staple, or black-seed cotton; (3) *G. herbaceum* or *album*, short staple, or upland, or green-seed cotton. These three species have been multiplied into 20 or 30 so-called varieties, by certain kinds of cultivation and careful selection. *G. Bahma* is supposed to be originally a hybrid between the native Egyptian cotton-plant and a species of *Hibiscus*. The "Sea-Island" cotton requires a salt atmosphere, and is mainly used in the manufacture of lace. Mr. Mell gives the microscopic characteristics of 25 varieties of cotton, and his descriptions are accompanied by photographic illustrations made with a photo-micro camera and micrometer. The Bulletins are supplied free on application to any citizen of the State.

ACCORDING to the *Japan Mail*, the Japanese Government has made an award of $1000 to Dr. Shohei Tanaka, a graduate in science of the Tokio University and of the Berlin University, for the invention of a new musical instrument, as to which the following information is given. In Germany, Dr. Tanaka devoted himself specially to the study of sound and of music, and is no doubt the first Japanese who has obtained an intimate knowledge of Western music on its practical, theoretical, scientific, and historic sides. On the purely scientific side he has added to our knowledge of the laws of vibrations of plates, and has also communicated to musical literature several papers of interest. One of these contains an account of a harmonium which he has devised, and which is tuned in practically pure intonation. From a cursory glance at the contents of this pamphlet, it is difficult to pick out the really original matter. Judging from the references and foot-notes, the author has read widely, and appears to be warranted in claiming to be the first who has constructed a keyed instrument capable of giving practically pure chords in all the usual keys, and of being played almost exactly as a piano or an organ is played. The manual is, to a first glance, very similar in appearance to the ordinary organ or piano manual. But a closer inspection shows that a short black note is introduced between E and F, and that the other black notes are divided into two or even three. In all, there are twenty distinct notes within the compass of an octave, instead of the usual twelve in our instruments of equal temperament. Dr. Tanaka's white notes are tuned to the perfect major scale of C, the E being therefore considerably flatter than the note of the same name on the piano. If it is desired to play on the scale of D, this E, the true major third to C, must not be used. A slightly but appreciably sharper note must be used, and this is inserted between D and E, in front of the ordinary black note known as D sharp or E flat. Strictly speaking, as on Bosanquet's organ for instance, D sharp and E flat are really different notes, but the difference is too slight to be practically appreciable. In Dr. Tanaka's scheme, however, the requirements of modern transposition in music necessitate a C sharp distinct from D flat.

THE additions to the Zoological Society's Gardens during the past week include a Purple-faced Monkey (*Semnopithecus leucoprymnus*) from Ceylon, presented by Mrs. Sutton Sams; a Sooty Mangabey (*Cercocebus fuliginosus* ♂) from West Africa, presented by Miss Kathleen Hill; an Indian Civet (*Viverricula malaccensis*), two Malabar Squirrels (*Sciurus maximus*) from India, presented by Colonel A. Bloomfield; a Two-spotted Paradoxure (*Nandinia binotata*) from West Africa, presented by Dr. J. Galbraith Westlake; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. Charles C. Barton; five Summer Ducks (*Æx sponsa* 5 ♀) from North America, four Gadwalls (*Chaulelasmus streperus* ♂ ♂ ♀ ♀), European, purchased; a Black-headed Lemur (*Lemur brunneus*), born in the Gardens.

SCIENCE IN NEW ZEALAND.

THE following is the Presidential address delivered by Sir James Hector, at the recent meeting of the Australasian Association for the Advancement of Science:—

When I rashly replied in the affirmative to the cablegram which I received from our Secretary in Melbourne, asking me to undertake the honourable and responsible duties which I have to commence this evening, I fear I did not fully realize the difficulties of the position, but since then the sense of my unfitness for the task has become very oppressive. To address an assembly of this kind on general science must involve unusual difficulties, owing to the audience being largely composed of those who, only taking a casual interest in scientific discussions, look chiefly to the results; while, at the same time, there are present professional specialists in almost every branch of knowledge. I feel that on this occasion I must be ruled by the interest of the majority, and claim the forbearance of my fellow-workers in science if I have to refer in a sketchy way to subjects in which they are deeply interested, and far more learned than I profess to be.

Seeing that I am addressing a Christchurch audience, I hope I may be permitted, in the first place, to say a word concerning one whose scientific services should, without doubt, have obtained for him the position of first President in New Zealand of the Australasian Association. We naturally recall the name of Sir Julius Von Haast on this occasion, and mourn for the loss the colony has sustained of one who for thirty years occupied a most prominent position. His early researches in the North Island in company with Von Hochstetter, were followed by the exploration of the remote districts on the west coast of Nelson, after which Canterbury secured his distinguished services, and enabled him to leave that monument of his varied scientific knowledge, shrewd capacity, and indefatigable industry which is to be found in the Canterbury Museum. There are others of our fellow-colonists whose wide range of experience would have peculiarly fitted them to act as your President, and I am able to say that had our veteran colonist and explorer Sir George Grey felt more assured in health and strength, it would have been your pleasure this evening to listen to a flood of eloquence on all scientific topics that relate to the future development of Australasia. There is another name I feel must be mentioned as one who should have been in this position had his health permitted. I refer to the Rev. William Colenso, who is not only the greatest authority on the folk-lore of the Maoris, on whom he was among the first to confer a printed literature in their own language. His long-continued work as a field naturalist, and especially as a botanist, is exceedingly interesting, seeing that it forms a connecting link that has continued the early spirit of natural history research in New Zealand, that commenced with Banks and Solander, and was continued by Menzies, Lesson, the two Cunninghams, and Sir Joseph Hooker, prior to the arrival of colonists. Thus we still have in my esteemed friend, Mr. Colenso, an active veteran naturalist of what we may call the old school of explorers.

It is wonderful to reflect that little more than fifty years ago this European colony was represented by a few fishing hamlets on the seaboard of a country occupied by a considerable native population. To the early explorers, and even down to a much later date, the obstacles that beset their path were very different from those of the present time: often obstructive natives, no roads, no steamers, no railways. Had an Association then existed and desired to promote science by giving our visitors an opportunity of visiting the remote parts of the islands, the same excursions which have on this occasion been planned to occupy a few days, would have occupied as many months, and then been accomplished only with great hardship and difficulty. I must ask the young and rising generation of colonial naturalists to bear this in mind when they have to criticize and add to the work of their predecessors. Such names of early colonists as Bidwill, Sinclair, Monro, Mantell, Travers, and many others should ever be held in esteem as those who, amidst all the arduous trials of early colonization, never lost sight of their duty towards the advancement of science in New Zealand. I will not attempt to particularize other names from amongst our existing, and, though small in number, very active corps of scientific workers. They are here, or should be, to speak for themselves in the sectional work; and I have no doubt some of those who did me the great honour of placing me in my present position are secretly congratulating themselves that they have secured for themselves the position of free lances on this occasion.

This is now the third annual gathering of this Association, and New Zealand should feel honoured that it has at so early a date in the Association's history been selected to the turn in rotation as the place of meeting among so many divisions of the great colony of Australasia. The two volumes of the Transactions of the Association, already in the hands of members, are quite sufficient to prove that the hopes of its founders, or rather, I may almost say, the founder—Prof. Liversidge, of Sydney—have been amply fulfilled. The papers read before the different Sections, and the addresses delivered, have, in my opinion, to a most remarkable extent embodied information and discussions which were not likely to have been produced as the result of any of our local scientific organizations. The authors seemed to have felt it incumbent on them to place their subjects in the environment of Australasia, and not in relation to the colony they happened to represent. This, I take it, is the first truly effective step towards Federation which has yet been achieved, and I trust that all our members will continue to be imbued with this spirit. Politicians should take this well to heart. Let them continue to aid all efforts that will tend to bring scientific accumulations in these colonies into a common store, so that each may discover for what purpose it has been best adapted by nature, and by paying proper political respect in fiscal policy to one another, each may prosper to the full extent of its natural advantages.

But it is not alone in the value of the papers communicated the Association contributes to advance true civilization in the colonies. The face to face conference, the personal contact of the active workers in different lines of scientific work, must greatly facilitate the more thorough understanding of the work which has been done and which is still undone. A vague idea, simmering in the brain of one man of science, who thinks light of it because it has no special application in his particular environment, may, by personal converse, flash into important results in the mind of another who has had the difficulties facing him, but without the happy thought occurring. It would be rather interesting for someone with leisure to endeavour to recount how many great discoveries have eventuated in this manner.

In casting my thoughts for a particular subject on which to address the Association I felt perplexed. Presidents of similar Associations in the Old World, who are in constant contact with the actual progress in scientific thought, feel that a mere recital of the achievements during their previous term is sufficient to command interest; but in the colonies most of us are cut off from personal converse with the leading minds by whom the scientific afflatus is communicated; and in our suspense for the tardy arrival of the official publications of the Societies, we have to feed our minds with science from periodical literature. But even in this respect my own current education is very defective, as I reside in the capital city of New Zealand, which has no College with a professorial staff whose duty, pleasure, and interest it is to maintain themselves on a level with the different branches of knowledge they represent. I therefore decided that, instead of endeavouring to review what had been done in the way of scientific progress, even in Australasia, it would be better to confine my remarks to New Zealand—the more so that this is the first occasion that there has been a gathering of what must, to some extent, be considered to be an outside audience for the colony. To endeavour to describe, even briefly, the progress made in the science of a new country, is, however, almost like writing its minute history. Every step in its reclamation from a wild state of nature has depended on the application of scientific knowledge, and the reason for the rapid advance made in these colonies is chiefly to be attributed to their having had the advantage of all modern resources ready to hand.

As in most other matters in New Zealand, there is a sharp line dividing the progress into two distinct periods, the first before and the second after the formation of the colony in 1840. With reference to the former period it is not requisite that much should be said on this occasion. From the time of Captain Cook's voyages, owing to his attractive narrative, New Zealand acquired intense interest for naturalists. His descriptions of the country and its productions, seeing that he only gathered them from a few places where he landed on the coast, are singularly accurate. But I think rather too much is sometimes endeavoured to be proved from the negative evidence of his not having observed certain objects. As an instance, it has been asserted that if any of the many forms of the moa still survived, Captain Cook must have been informed of the fact. Yet we find that he lay for weeks in Queen Charlotte Sound, and in Dusky Sound, where all night long the cry of the kiwi must have been heard just as now, and that he also obtained and took home mats and other articles of native manufacture, trimmed with kiwis' skins; and that most likely the mouse-coloured quadruped which was seen at Dusky Sound by his men when clearing the bush was only a gray kiwi; and yet the discovery of this interesting bird was not made till forty years after Cook's visit. As a scientific geographer Captain Cook stands unrivalled, considering the appliances at his disposal. His longitudes of New Zealand are wonderfully accurate, especially those computed from what he called his "rated watches," the first type of the modern marine chronometer, which he was almost the first navigator to use. The result of a recent measurement of the meridian difference from Greenwich by magnetic signals is only two geographical miles east of Captain Cook's longitude. He also observed the variation and dip of the magnetic needle, and from his record it would appear that during the hundred years which elapsed up to the time of the *Challenger's* visit, the south-seeking end of the needle has changed its position $2\frac{1}{2}°$ westward, and inclines $1\frac{1}{2}°$ more towards the south magnetic pole. Captain Cook also recorded an interesting fact, which, so far as I am aware, has not been since repeated or verified in New Zealand. He found that the pendulum of his astronomical clock, the length of which had been adjusted to swing true seconds at Greenwich, lost at the rate of 40 seconds daily at Ship Cove in Queen Charlotte Sound. This is, I believe, an indication of a greater loss of the attraction of gravity than would occur in a corresponding north latitude.

The additions to our scientific knowledge of New Zealand acquired through the visits of the other exploring ships of early navigators, the settlement of sealers and whalers on the coast, and of Pakeha Maoris in the interior, were all useful, but of too slight a character to require special mention. The greatest additions to science were made by the missionaries, who, in the work of spreading Christianity among the natives, had the services of able and zealous men, who mastered the native dialects, reduced them to a written language, collected and placed on record the traditional knowledge of the interesting Maori, and had among their numbers some industrious naturalists who never lost an opportunity of collecting natural objects. The history of how the country, under the mixed influences for good and for evil which prevailed almost without Government control until 1840, gradually was ripened for the colonist, is familiar to all.

The new era may be said to have begun with Dieffenbach, a naturalist who was employed by the New Zealand Company. He travelled and obtained much information, but did not collect to any great extent, and, in fact, appears not to have anticipated that much remained to be discovered. For his conclusion is that the smallness of the number of the species of animals and plants then known—about one-tenth of our present lists—was not due to want of acquaintance with the country, but to paucity of life forms. The chief scientific value of his published work is in the appendix, giving the first systematic list of the fauna and flora of the country, the former being compiled by the late Dr. Gray, of the British Museum. The next great scientific work done for New Zealand was the Admiralty survey of the coast-line, which is a perfect marvel of accurate topography, and one of the greatest boons the colony has received from the mother country. The enormous labour and expense which was incurred on this survey at an early date in the history of the colony is a substantial evidence of the confidence in its future development and commercial requirements which animated the Home Government. On the visit of the Austrian exploring ship *Novara* to Auckland in 1859, Von Hochstetter was left behind, at the request of the Government, to make a prolonged excursion to the North Island and in Nelson; and he it was who laid the foundation of our knowledge of the stratigraphical geology of New Zealand. Since then the work of scientific research has been chiefly the result of State surveys, aided materially by the zeal of members of the New Zealand Institute, and of late years by an increasing band of young students, who are fast coming to the front under the careful science training that is afforded by our University Colleges.

In the epoch of their development the Australasian colonies have been singularly fortunate. The period that applies to New Zealand is contemporaneous with the reign of Her Majesty, which has been signalized by enormous strides in science. It has been a period of gathering into working form immense stores of previously acquired observation and experiment, and of an escape of the scientific mind from the trammels of superstition and hazy speculation regarding what may be termed common things. Laborious work had been done, and many grand generalizations had been formerly arrived at in physical science; but still, in the work of bringing things to the test of actual experiment, investigators were still bound by imperfect and feeble hypotheses and supposed natural barriers among the sciences. But science is one and indivisible, and its subdivisions, such as physics, chemistry, biology, are only matters of convenience for study. The methods are the same in all, and their common object is the discovery of the great laws of order under which this universe has been evoked by the great Supreme Power.

The great fundamental advance during the last fifty years has been the achievement of far-reaching generalizations, which have provided the scientific worker with powerful weapons of research. Thus the modern "atomic theory," with its new and clearer conceptions of the intimate nature of the elements and their compounds that constitute the earth and all that it supports, has given rise to a new chemistry, in which the synthetical or building-up method of proof is already working marvels in its application to manufactures. It is, moreover, creating a growing belief that all matter is one, and reviving the old idea that the inorganic elementary units are merely centres of motion specialized in a homogeneous medium, and that these units have been continued on through time, but with such individual variations as give rise to derivative groups, just as we find has been the case in the field of organic creations. The idea embodied in this speculation likens the molecule to the vortex rings which Helmholtz found must continue to exist for ever, if in a perfect fluid free from all friction they are once generated as a result of impacting motion. There is something very attractive in the simplicity of the theory of the constitution of matter which has been advocated by Sir William Thomson. He illustrates it by likening the form of atoms to smoke rings in the atmosphere, which, were they only formed under circumstances such as above described, such vortex atoms must continue to move without changing form, distinguished only from the surrounding medium by their motion. As long as the original conditions of the liquid exist they must continue to revolve. Nothing can separate, divide, or destroy them, and no new units can be formed in the liquid without a fresh application of creative impact. The doctrine of the conservation of energy is a second powerful instrument of research that has developed within our own times. How it has cleared away all the old cobwebs that formerly encrusted our ideas about the simplest agencies that are at work around us; how it has so simplified the teaching of the laws that order the conversion of internal motions of bodies into phases which represent light, heat, electricity, is abundantly proved by the facility with which the mechanicians are every day snatching the protean forms of energy for the service of man with increasing economy.

These great strides which have been made in physical science have not as yet incited much original work in this colony. But now that physical laboratories are established in some degree at the various College centres, we shall be expected, ere long, to contribute our mite to the vast store. In practical works of physical research, we miss in New Zealand the stimulus the sister colonies receive from their first-class Observatories, supplied with all the most modern instruments of research, wielded by such distinguished astronomers as Ellery, Russell, and Todd, whose discoveries secure renown for their separate colonies. I am quite prepared to admit that the reduplication of Observatories in about the same latitude, merely for the study of the heavenly bodies, would be rather a matter of scientific luxury. The few degrees of additional elevation of the South Polar region which would be gained by an Observatory situated even in the extreme south of New Zealand could hardly be expected to disclose phenomena that would escape the vigilance of the Melbourne Observatory. But star-gazing is only one branch of the routine work of an Observatory. It is true that we have a moderate but efficient Observatory establishment in New Zealand sufficient for distributing correct mean time, and that our meridian distance from Greenwich has been satisfactorily determined by telegraph; also, thanks to the energy and skill of the Survey Department, despite most formidable natural obstructions, the major triangulation and meridian circuits have established the basis of our land survey maps on a satisfactory footing, so that subdivision of the land for settlement, and the adoption and blending of the excellent work done by the provincial Governments of the colony, is being rapidly overtaken. Further, I have already recalled how much the colony is indebted to the mother country for the completeness and detail of the coastal and harbour charts.

But there is much work that should be controlled by a physical Observatory that is really urgently required. I may give a few illustrations. The tidal movements round the coast are still imperfectly ascertained, and the causes of their irregular variations can never be understood until we have a synchronous system of tide-meters, and a more widely extended series of deep-sea soundings. Excepting the *Challenger* soundings on the line of the Sydney cable, and a few casts taken by the United States ship *Enterprise*, the depth of the ocean surrounding New Zealand has not been ascertained with that accuracy which many interesting problems in physical geography and geology demand. It is supposed to be the culmination of a great submarine plateau; but how far that plateau extends, connecting the southern islands towards the great Antarctic land, and how far to the eastward, is still an unsolved question. Then, again, the direction and intensity of the magnetic currents in and around New Zealand require further close investigation, which can only be controlled from an Observatory. Even in the matter of secular changes in the variation of the compass we find that the marine charts instruct that an allowance of increased easterly variation of 2′ per annum must be made, and as this has now accumulated since 1850, it involves a very sensible correction to be adopted by a shipmaster in making the land or standing along the coast; but we find from the recently published work of the *Challenger* that this tendency to change has for some time back ceased to affect the New Zealand area, and as the deduction appears only to have been founded on a single triplet observation of the dip taken at Wellington and one azimuth observation taken off Cape Palliser, it would be well to have this fact verified. With regard to the local variation in the magnetic currents on land and close in shore, the requirement for exact survey is even more imperative. Captain Creak, in his splendid essay, quotes the observations made by the late Surveyor-General, Mr. J. T. Thomson, at the Bluff Hill, which indicate that a compass on the north side was deflected more than 9° to the west, while on the east side of the hill the deflection is 46° to the east of the average deviation in Foveaux Strait. He adds that if a similar island-like hill happened to occur on the coast, but submerged beneath the sea to a sufficient depth for navigation, serious accidents might take place, and he instances a case near Cossack, on the north coast of Australia, when H.M.S. *Medea*, sailing on a straight course in eight fathoms of water, experienced a compass deflection of 30° for the distance of a mile. A glance at the variation entered on the meridian circuit maps of New Zealand shows that on land we have extraordinary differences between different trigonometrical stations at short distances apart. For instance, in our close vicinity, at Mount Pleasant, behind Godley Head lighthouse, at the entrance to Lyttelton Harbour, the variation is only 9° 3′ east, or 6° less than the normal; while at Rolleston it is 15° 33′, and at Lake Coleridge 14° 2′. In Otago we have still greater differences recorded, for we find on Flagstaff Hill, which is an igneous formation, 14° 34′, while at Nenthorn, thirty miles to the north, in a schist formation, we find an entry of 35° 41′.

In view of the fact that attention has been recently directed to the marked effects on the direction and intensity of the terrestial magnetic currents of great lines of fault along which movements have taken place, such as those which bring widely different geological formations into discordant contact, with the probable production of mineral veins, this subject of special magnetic surveys is deserving of being undertaken in New Zealand. In Japan and in the United States of America the results have already proved highly suggestive. A comparison between this country and Japan by such observations, especially if combined with systematic and synchronous records by modern seismographic instruments, would be of great service to the physical geologist. There are many features in common, and many quite reversed in the orographic and other physical features of these two countries. Both are formed by the crests of great earth waves lying north-east and south-west, and parallel to, but distant from, continental areas, and both are traversed by great longitudinal faults and fissures, and each by one great transverse fault. Dr. Naumann, in a recent paper, alludes to this in Japan as the *Fossa Magna*, and it corresponds in position in relation to Japan with Cook Strait in relation to New Zealand. But the *Fossa Magna* of Japan has been filled up with volcanic products, and is the seat of the loftiest active volcano in Japan. In Cook Strait and its vicinity, as you are aware, there are no volcanic rocks, but there and southward, through the Kaikouras, evidence of fault movements on a larger scale is apparent, and it would be most interesting to ascertain if the remarkable deviations from the normal, in direction and force, of the magnetic currents, which are experienced in Japan, are also found in New Zealand. For it is evident that, if they are in any way related to the strain of cross fractures in the earth's crust, the observation would tend to eliminate the local influence of the volcanic rocks which are present in one case and absent in the other. With reference to earthquakes, also, few, if any, but very local shocks experienced in New Zealand have originated from any volcanic focus we are acquainted with, while a westerly propagation of the ordinary vibrations rarely passes the great fault that marks the line of active volcanic disturbance. In Japan, also, out of about 480 shocks which are felt each year in that country, each of which, on an average, shakes about 1000 square miles, there are many that cannot be ascribed to volcanic origin.

There are many other problems of practical importance that can only be studied from the base-line of a properly equipped Observatory. These will readily occur to physical students, who are better acquainted with the subject than I am. I can only express the hope that the improved circumstances of the colony will soon permit some steps to be taken. Already in this city, I understand, some funds have been subscribed. As an educational institution, to give practical application to our students in physical science, geodesy and navigation, it would clearly have a specific value that would greatly benefit the colony.

Another great branch of physical science, chemistry, should be of intense interest to colonists in a new country. Much useful work has been done, though not by many workers. The chief application of this science has been naturally to promote the development of mineral wealth, to assist agriculture, and for the regulation of mercantile contracts. I cannot refrain from mentioning the name of William Skey, analyst to the Geological Survey, as the chemist whose researches during the last twenty-eight years have far surpassed any others in New Zealand. Outside his laborious official duties he has found time to make about sixty original contributions to chemical science, such as into the electrical properties of metallic sulphides—the discovery of the ferro-nickel alloy *awaruite* in the ultra-basic rocks of West Otago, which is highly interesting as it is the first recognition of this meteoric-like iron as native to our planet—the discovery that the hydrocarbon in torbasic and the gas shales is chemically and not merely mechanically combined with the clay base—of a remarkable colour test for the presence of magnesia, and the isolation of the poisonous principle in many of our native shrubs. His recent discovery, that the fatty oils treated with aniline form alkaloids, also hints at an important new departure in organic chemistry. His suggestion of the hot-air blow-pipe, and of the application of cyanide of potassium to the saving of gold, and many other practical applications of his chemical knowledge, are distinguished services to science, of which New Zealand should be proud. In connection with the subject of chemistry, there is a point of vast importance to the future of the pastoral and agricultural interests of New Zealand, to which attention was directed some years ago by Mr. Pond, of Auckland. That is, the rapid deterioration which the soil must be undergoing by the steady export of the constituents on which plant and animal life must depend for nourishment. He calculated that in 1883 the intrinsic value of the fixed nitrogen and phosphoric acid and potash sent out annually was £592,000, taking into account the wool and wheat alone. Now that we have to add to that the exported carcasses of beef and mutton, bones and all, the annual loss must be immensely greater. The proper cure would, of course, be to bring back return cargoes of artificial manure, but even then its application to most of our pastoral lands would be out of the question. I sincerely hope that the problem will be taken in hand by the Agricultural College at Lincoln as a matter deserving of practical study and investigation.

I have already referred to several great generalizations which have exercised a powerful influence in advancing science during the period I marked out for review, but so far as influencing the general current of thought, and almost entirely revolutionizing the prevalent notions of scientific workers in every department of knowledge, the most potent factor of the period has been the establishment of what has been termed the doctrine of evolution. The simple conception of the relation of all created things by the bond of continuous inheritance has given life to the dead bones of an accumulated mass of observed facts, each valuable in itself, but, as a whole, breaking down by its own weight. Before this master-key was provided by the lucid instruction of Darwin and Wallace, it was beyond the power of the human mind to grasp and use in biological research the great wealth of minute anatomical and physiological details. The previous ideas of the independent creation of each species of animal and plant in a little Garden of Eden of its own must appear puerile and absurd to the young naturalists of the present day; but in my own College days, to have expressed any doubt on the subject would have involved a sure and certain pluck from the examiner. I remember well that I first obtained a copy of Darwin's "Origin of Species" in San Francisco when on my way home from a three years' sojourn among the Red Indians in the Rocky Mountains. Having heard nothing of the controversies, I received the teaching with enthusiasm, and felt very much surprised on returning to my *alma mater* to find that I was treated as a heretic and a backslider. Nowadays it is difficult to realize what all the fuss and fierce controversy was about, and the rising school of naturalists have much cause for congratulation that they can start fair on a well-assured logical basis of thought, and steer clear of the many complicated and purely ideal systems which were formerly in vogue for explaining the intentions of the Creator and for torturing the unfortunate students. The doctrine of evolution was the simple-minded acceptance of the invariability of cause and effect in the organic world as in the inorganic; and to understand his subject in any branch of natural science, the learner has now only to apply himself to trace in minutest detail the successive steps in the development of the phenomena he desires to study.

With energetic leaders educated in such views, and who, after their arrival in the colony, felt less controversial restraint, it is not wonderful that natural history, and especially biology, should have attracted so many ardent workers, and that the results should have been so good. A rough test may be applied by comparing the number of species of animals and plants which had been described before the foundation of the colony and those up to the present time. In 1840, Dr. Gray's list in Dieffenbach's work gives the number of described species of animals as 594. The number now recognized and described is 5498. The number of Mammalia has been doubled, through the more accurate study of our seals, whales, and dolphins. Then the list of birds has been increased from 84 to 195, chiefly through the exertions of Sir Walter Buller, whose great standard work on our avifauna has gained credit and renown for the whole colony. The number of fishes and Mollusca has been more than trebled, almost wholly by the indefatigable work of our Secretary, Prof. Hutton. But the greatest increase is in the group which Dr. Gray placed as Annulosa, which, chiefly through the discovery of new forms of insect life, has risen from 156 in 1840 to 4295, of which over 2000 are new beetles described by Captain Broun, of Auckland.

When we turn to botany, we find that Dieffenbach, who appears to have carefully collected all the references to date in 1840, states the flora to comprise 632 plants of all kinds, and, as I have already mentioned, did not expect that many more would be found. But by the time of the publication of Hooker's "Flora of New Zealand" (1863), a work which has been of inestimable value to our colonists, we find the number of indigenous plants described had been increased to 2456. Armed with the invaluable guidance afforded by Hooker's "Handbook," our colonial botanists have renewed the search, and have since then discovered 1469 new species, so that our plant census at the present date gives a total of 3355 species. It would be impossible to make mention of all who have contributed to this result as collectors, and hardly even to indicate more than a few of those to whom science is indebted for the description of the plants. The literature of our post-Hookerian botany is scattered about in scientific periodical literature, and, as Hooker's "Hand-book," is now quite out of print, it is obvious that, as the new discoveries constitute more than one-third of the total flora, it is most important that our young botanists should be fully equipped with all that has been ascertained by those who have preceded them. I am glad to be able to announce that such a work, in the form of a new edition of the "Hand-book of the Flora of New Zealand," approved by Sir Joseph Hooker, is now in an advanced state of preparation by Prof. Thomas Kirk, who has already distinguished himself as the author of our "Forest Flora." Mr. Kirk's long experience as a systematic botanist, and his personal knowledge of the flora of every part of the colony, acquired during the exercise of his duties as Conservator of Forests, point to him as the fitting man to undertake the task.

But quite apart from the work of increasing the local collections which bear on biological studies, New Zealand stands out prominently in all discussions on the subject of geographical biology. It stands as a lone zoological area, minute in area, but on equal terms as far as regards the antiquity and peculiar features of its fauna, with nearly all the larger continents in the aggregate. In consequence of this, many philosophical essays—such, for instance, as Hooker's introductory essay to the early folio edition of the "Flora," the essays by Hutton, Travers, and others, and also the New Zealand references in Wallace's works, have all contributed essentially to the vital question of the causes which have brought about the distribution and geographical affinities of plants and animals, and have thus been of use in hastening the adoption of the doctrine of evolution. But much still remains to be done. Both as regards its fauna and its flora, New Zealand has always been treated too much as a whole quantity, and in consequence percentage schedules prepared for comparing with the fauna and flora of other areas fail from this cause. It is absolutely necessary to discriminate not only localities, but also to study more carefully the relative abundance of individuals as well as of species before instituting comparisons. The facility and rapidity with which change is effected at the present time should put us against rashly accepting species which may have been accidental intruders, though wafted by natural causes, as belonging to the original endemic fauna or flora. Further close and extended study, especially of our marine fauna, is urgently required. We have little knowledge beyond the littoral zone, except when a great storm heaves up a gathering of nondescript or rare treasure from the deep. Of dredging we have had but little done, and only in shallow waters, with the exception of a few casts of the deep-sea trawl from the *Challenger*. When funds permit, a zoological station for the study of the habits of our sea fishes, and for the propagation of such introductions as the lobster and crab, would be advantageous. I observe that lately such an establishment has been placed on the Island of Mull, in Scotland, at a cost of £400, and that it is expected to be nearly self-supporting, With respect to food-fishes, and still more with respect to some terrestrial forms of life, we, in common with all the Australian colonies, require a more scientific and a less casual system of acclimatization than we have had in the past. One must talk with bated breath of the injuries that have been inflicted on these colonies by the rash disturbance of the balance of nature. Had our enthusiasm been properly controlled by foresight, our settlers would probably not have to grieve over the losses they now suffer through many insect pests, through small birds and rabbits, and which they will in the future suffer through the vermin that are now being spread in all directions.

Sir James Hector then went on to say that there were many other points that he had intended to touch upon, but all had been forestalled by the remarks of His Excellency the Governor and Mr. Goodale. He was the better pleased that these gentlemen had spoken upon them, as they were remarks relating to the advantages of this Association. He felt, however, that he would like to have given a description of what had been ascertained relating to the geology of New Zealand. He might state that the early explorers appeared to have had only most vague ideas of the geologies of the countries they explored. Indeed, the whole science of geology seemed to have been almost brought into existence during the last fifty or fifty-five years. It existed only as drawing its knowledge from other branches of science; it barely existed as a science until these branches had become established. In New Zealand our geological explorations had been since the matters he had referred to had been settled, and the result had been that we had attained more rapidly, competent and tolerable, more complete knowledge of the structure of the country. New Zealand was probably the outcrop of a great earth-wave, the hollow of which formed the submarine plateau lying to the east. New Zealand appeared to have first originated as dry land in the Palæozoic times, merely as volcanic islands rising in a sea of moderate depth. After the Palæozoic period there appeared to have been a great blank in the geological formation. It was a period during which no deposits took place, and it was probable that all which had been deposited was removed. He went on to trace the various formations, referred to the first traces found of the moa at Timaru, and then leaving that subject stated that at the sectional meeting on ethnology there would be presented to the Association the first proof-sheets of a great lexicon of the languages spoken in the Pacific Islands, especially by the natives of the Sandwich Islands, of Tonga, and of New Zealand. It was being prepared by Mr. Tregear, one of the most profound workers in New Zealand in Maori mythology.

There was another subject on which he would like to touch. It was concerning the great Antarctic continent, but as he understood that Baron von Mueller wished the discussion of it should be deferred for Saturday forenoon, he would say no more upon it. He had to apologize for the very feeble manner in which he had attempted to discharge his duties, though he had the most perfect confidence in the success of the Association. He thought it was about twenty-four years since Mr. Travers got the Act passed which established the New Zealand Institute. In a small way it was an association of scientific men, and it was founded to absorb and render permanent the active endeavours in all parts of the colony to advance science. How well it had succeeded was known by all. Baron Von Mueller had kindly attributed its success to him, but he must really disclaim that, and say its success was due to the wise framer of the special Act. He hoped to see the colonies united together as one whole in this matter; the whole of the Australasian colonies were not too large to combine for the purpose, and he hoped that the inclusion of New Zealand in the magic circle would come about in time. In conclusion he expressed a wish that the visitors might have a pleasant sojourn in New Zealand; and trusted that he had succeeded in proving the claims of New Zealand as a place for the meeting of the Association, and that he had shown there was enough scientific work to merit such recognition as they had received; and he thought he had shown that New Zealand had great capabilities for scientific research, and that there was still a great deal to be done.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 12.—"On the Bisulphite Compounds of Alizarin-blue and Cœrulin as Sensitizers for Rays of Low Refrangibility." By George Higgs. Communicated by Lord Rayleigh, Sec.R.S.

The determination of the relative wave-lengths of the Fraunhofer lines, by photographing all the orders of spectra given by any particular grating, includes certain subjects which present more or less difficulty, and that of selecting or producing a dyebath adapted to the requirements of the two or more orders comprising the subject is intimately connected with that of the choice of absorbing media.

Having been engaged for some time in investigations of this nature, I had occasion, during the summer of 1889, to require an impression of the second order, about w.l. 3300, contiguous with that of the red end of the first order; and finding that the ordinate of an actinic curve for a plate immersed in a very dilute alcoholic ammoniacal solution of cyanin (1 : 30,000), reduced to about one-fourth of that for an unprepared plate, I abandoned its use for this purpose. The results appeared to be unaffected by the addition of quinine.

Subsequently, induline, cœrulin, alizarin-blue, and the bisulphite compounds of the two latter were used, and when obtained in a state of sufficient purity the alizarin-blue S leaves little or nothing to be desired, for, whilst possessing, in a high degree, sensitizing properties for rays throughout the region comprised between w.l. 6200 and 8000, it does not, like cyanin, lower the sensitiveness to the violet and ultra-violet.

The following is one of the processes I employed in the preparation of the dye-stuff in a pure state :—

To a saturated solution of sodium bisulphite in a mortar is added alizarin-blue paste, this is disintegrated with a pestle, and poured into a glass vessel capable of holding an additional quantity of sodium bisulphite, in all 10 parts of the paste to 20 parts of bisulphite, and another 10 parts of water. The vessel is well stoppered, set aside in a cool place for five or six weeks, and shaken daily, but left undisturbed during the last eight or ten days.

The solution is decanted, filtered, and treated with alcohol, to precipitate the greater portion of the remaining sodium bisulphite. 50 parts of water are now added with a sufficiency of sodium chloride to form a concentrated solution. Again set aside in an open-mouthed glass jar, covered with bibulous paper, for seven or eight days, a deposition of the dye in a crystalline state, together with sulphite of calcium, will take place, which latter, owing to its insolubility in water, may be removed by filtration.

The alizarin-blue S is separated from any unaltered substance left in the original stoppered vessel by solution, and added to the brine, now purified from lime salts, and once more set aside to crystallize, the final purification being effected in a beaker containing alcohol and a small percentage of water to remove the last traces of sodium chloride, collecting the crystals on a filter-paper and drying at ordinary temperatures.

The needle-shaped crystals are of a deep-red. Dilute solutions are of a pale-sherry colour, changing, with the addition of a few drops of ammonia, to a green, which immediately gives way to magenta and every shade of purple, till oxidation is complete, when it assumes a blue colour, the absorption spectrum of which is continuous and strongest in the least refrangible end, presenting the appearance of extending into the infra-red.

Plates immersed in a solution containing 1 : 10,000 and 1 per cent. of ammonia give the most perfect results the day after preparation, but rapidly deteriorate unless kept quite dry.

With a slit 1/1000 inch in width, and an exposure of forty minutes, results have been obtained in the region of great A of the second order which possess all the detail and definition usually so characteristic of the violet end. Numerous lines are sharply depicted which were previously not known to exist. W.l. 8400 has been reached, giving almost equal detail.

The process for the preparation of pure cœrulin S is a slight modification of the preceding. The results obtained, as well as the actinic curve, are almost identical. The pure substance is almost white ; dilute solutions pass rapidly from pale yellow to a bright green ; a trace of ammonia produces an olive-green.

For several samples of paste I am indebted to the kindness of Messrs. Schott, Segner, and Co., of Manchester, agents to the Badische Anilin- und Soda-Fabrik, Ludwigshafen, who hold the patent rights for the manufacture of alizarin-blue S. It is hoped this company may be induced to manufacture this substance free from the minute crystallizable impurities which render it unsuitable for use in investigations of such delicate nature.

Geological Society, March 11.—Dr. A. Geikie, F.R.S., President, in the chair.—The following communications were read :—Manod and the Moelwyns, by A. V. Jennings and G. J. Williams. The area described by the authors is on the north side of the Merionethshire anticlinal of Lower Cambrian rocks, and contains Lingula Flags, Tremadoc, and Arenig rocks. The authors correct what they think is an inaccuracy of some importance in the correlation of beds in different parts of the range, as interpreted in the map and memoir of the Geological Survey, and trace with greater completeness the position and constancy of the beds of slate in the Arenig series—a point of considerable local and practical importance to those engaged in slate-quarrying. They offer also what seems to them to be conclusive evidence to show the intrusive nature of the great crystalline mass known as the syenite of Tan-y-Grisiau, and to its intrusion are due, in their opinion, the peculiar physical characteristics of the surrounding country. Though in the immediate neighbourhood of Festiniog there is no direct evidence of unconformity between the Tremadoc and Arenig series, it seems probable that an unconformity does exist ; for when traced toward the west the Tremadoc beds thin out and the Lingula Flags are overlain by graptolite-bearing slates of Arenig age, while eastward, near Llyn Serw, the grit comes close upon Upper Lingula Flags. The division of the Arenig volcanic rocks into Lower Ashes, Felstone, and Upper Ashes, while true of some districts and useful as a generalization, conveys an idea of uniformity of strata all round the anticlinal which more detailed examination of different districts does not support. After the reading of this paper there was a discussion, in which Prof. Hughes, Dr. Hicks, Mr. Sherborn, and the President took part.—The Tudor specimen of *Eozoon*, by J. W. Gregory.

Zoological Society, March 17.—Prof. G. B. Howes in the chair.—Mr. Sclater exhibited and made remarks on some horns with scalps attached of an Antelope sent to him from Somali-Land by Captain H. G. C. Swayne, R.E., which he referred to the lately described *Cervicapra clarkii* of Mr. Oldfield Thomas.—Mr. Sclater also exhibited two skins of the Ounce (*Felis uncia*) in reference to the specimen of this Cat lately acquired by the Society, and made some remarks on the geographical range of the Ounce in Central Asia.—Mr. A. Smith Woodward gave an account of some dermal plates of *Homosteus* from the Old Red Sandstone of Caithness, lately sent to him by Mr. Donald Calder, of Thurso, the examination of which had enabled him to advance our knowledge of some points in the structure of this remarkable form of extinct fishes.—Mr. G. A. Boulenger gave a detailed description of Simony's Lizard (*Lacerta simonyi*) from the large specimen lately living in the Society's Gardens, which had been brought from the rock of Zalmo, Canaries, by Canon Tristram.—Mr. W. F. Kirby gave an account of a small collection of Dragon-flies made by Mr. E. E. Green in Ceylon. The series contained examples of sixteen species, of which three appeared to be new to science.—Mr. Oldfield Thomas read some notes on the specimens of Antelopes procured by Mr. T. W. H. Clarke in Somali-Land, which had been submitted to his examination by Messrs. Rowland Ward and Co. The specimens were referred to eight species. One of these, already preliminarily described as *Cervicapra clarkii*, was now regarded as constituting a new generic form allied to the gazelles, and proposed to be called *Ammodorcas clarkii*.

Royal Microscopical Society, March 18.—Dr. R. Braithwaite, President, in the chair.—A letter from Colonel O'Hara, dealing with sundry points connected with photomicrography, was read.—Mr. E. M. Nelson exhibited and described a new design of student's microscope recently brought out by Mr. Baker, the idea of which was to provide a microscope of this class, fitted with some of the more important accessories usually only applied to instruments of an expensive character. It had a good coarse adjustment, a differential fine adjustment, a centring sub-stage with rack-work, and a Wright's finder. The stage was of the horseshoe shape, and solid and well made, so that the instrument answered to the description of it as a cheap microscope, capable of doing all ordinary microscopic work.—Mr. T. Charters White read a paper on a new method of demonstrating cavities in dental and osseous tissues, and exhibited specimens in illustration.—Mr. E. M. Nelson exhibited an enlargement of a photomicrograph.—Mr. E. M. Nelson read a paper on the optical principles of microscope bull's-eyes, illustrating the subject by drawings on the black-board.—Mr. Mayall read a communication from the authorities of the Antwerp Microscopical Exhibition to be held during August and September next.—Prof. Bell announced that the President and the Rev. Dr. Dallinger had been appointed delegates to represent the Society at the forthcoming International Congress of Hygiene.—The President announced that the next *conversazione* would be held on Thursday, April 30.

Linnean Society, March 19.—Special General Meeting.—Prof. Stewart, President, in the chair.—The Secretary having read the minutes of the last meeting, the President announced that the sense of the meeting would be taken by ballot on the proposed alteration of certain bye-laws, of which due notice had been given as prescribed by the charter of the Society ; and after explaining the nature and object of such alterations, he invited those present to express their opinions. A discussion followed, in which twenty-two of the Fellows took part, and on the votes being counted it was found that a portion only of the proposed alterations were assented to, the remainder being negatived by 40 to 29.—The following papers were then read :—Researches on earthworms belonging to the genus *Lumbricus*, by the Rev. H. Friend.—The Hemiptera and Heteroptera of Ceylon, by W. F. Kirby.

CAMBRIDGE.

Philosophical Society, March 9.—Dr. Lea in the chair.—The following communications were made:—Observations upon the cerebral heat centres, by Mr. J. G. Adami.—On the nature of supernumerary appendages in insects, by Mr. W. Bateson. The author exhibited a number of specimens in illustration of this subject. The evidence related to about 220 recorded cases of extra legs, antennæ, palpi, or wings, and particulars were given as to the mode of occurrence of these structures. Speaking of cases in which the nature of the extra parts could be correctly determined, it was found that the following principles were followed (amongst others):—I. Extra appendages arising from a normal appendage usually contain all parts found in the normal appendage peripherally to the point from which they arise, and never contain parts central to this point. II. A. Extra appendages of double structure are the commonest. (1) Whether separate or in part compound, they consist of a pair of complementary parts, one being right and the other left. (2) Of the two extra appendages, that which is adjacent to the limb from which they arise, is a limb of the other side of the body. (3) If the pair of extra appendages arise from the anterior surface of the normal appendage, the surfaces which they present to each other are structurally posterior; if they arise posteriorly, the adjacent surfaces are anterior; if they arise ventrally, the adjacent surfaces are dorsal, &c. II. B. A single extra appendage is rarely perfect. (1) If it arises from the body, it is formed as an appendange of the side on which it is placed. (2) If it arises by peripheral division of an appendage, the parts central to the point of division are commonly right or left as the case may be, while the peripheral part may be a symmetrical and complementary pair. It was pointed out that these phenomena are important as an indication of the physical nature of bodily symmetry, and in their bearing upon current views of the character of germinal processes. The author expressed his indebtedness for information, or the loan of specimens, to Messrs. H. Gadeau de Kerville, Pennetier, Giard, Kraatz, L. von Heyden, Dale, Mason, Westwood, Waterhouse, N. M. Richardson, Janson, Reitter, &c., and especially to Dr. Sharp for much help and advice in examining the specimens.—On the orientation of *Sacculina*, by T. T. Groom.—Some experiments on blood-clotting, by Albert S. Grünbaum. The author described the results of six experiments in which the cœliac and mesenteric arteries were ligatured. He finds that the effects of the ligature on the clotting of blood removed (after an interval of four hours) from the body is less pronounced than has been stated by Bohr (*Centralb. f. Physiol.*, 1888, s. 263). The clotting was slightly delayed, in one case by twenty minutes, but in the others by a few (three to four) minutes only. The clots, when formed, were less firm than normally. Bohr has stated that, in two experiments conducted as above, the clotting of blood was delayed for about two hours.

EDINBURGH.

Royal Society, March 2.—Sir Douglas Maclagan, President, in the chair.—Dr. Alexander Buchan read a paper on the relation of high winds to barometric pressure at Ben Nevis Observatory. The Observatory at the top of Ben Nevis is exposed to winds from all quarters, while the Observatory at the foot of Ben Nevis is well sheltered from all winds. It is found that the variations of barometric pressure at the high-level Observatory are practically proportional to the speed of the wind throughout a range varying from zero speed to a speed of 110 miles per hour. The difference is practically proportional to the square of the speed of the wind, which agrees with the well-known result regarding the reduction of pressure in the interior of a moving fluid.—Dr. A. Bruce read notes on a case of cyclopia in a child. There was a single median lozenge-shaped ocular cavity furnished with two upper and two lower eyelids. The nose was represented by a short pedunculated process of skin and subcutaneous tissue attached to the skin of the forehead above the median eye. On microscopic section, two rudimentary eyes were found, the two retinæ of which evidently arose from a single optic vesicle. The brain was nearly normal below the cerebrum, which was imperfectly divided into two membranes, and contained only a single ventricular cavity. The two optic thalami were fused together, apparently in consequence of the pressure of thickened membranes around them, which was considered to be the probable cause of the deformity. The premaxillary and ethmoid bones, the vomer and turbinated bones, and the pre-sphenoid were absent.—Dr. Byron Bramwell read a paper on cases illustrating the pos tion of the visual centre in man. He first referred to the experimental investigations of Ferrier, Munk, Schäfer, and other observers, and emphasized the difficulty of determining the exact position of the visual centre by experiments on the lower animals. He then quoted Ferrier's analysis of the recorded cases of lesion of the visual centre in man; and finally detailed some cases which had come under his own observation which have important bearings on the subject. In the first case the patient was seized, two and a half years before her death, with symptoms indicative of cerebral embolism. From the presence of right-sided homonymous hemianopsia, the author diagnosed an embolic enfarction of the left posterior cerebral artery, with resulting softening of the left occipital lobe—probably the cuneus and adjacent white matter. The hemianopsia, which was complete, which passed just outside the fixing-point, and which affected light, form, and colour, persisted absolutely unchanged until the patient's death from a second embolism of the right internal carotid artery, two and a half years after the first attack. A sharply-defined softening of the posterior and inferior part of the cuneus and of the inferior occipital convolution was found after death. The angular gyrus was absolutely unaffected. The white matter of the occipital lobe was only involved to a limited extent from before backwards. Except in the top of the occipital lobe, the optic radiations were in no way directly implicated by the lesion. The author claimed that the case conclusively proved the presence of a half-vision centre in the top of the occipital lobe. In the second case a localized irritative lesion in the surface of the left occipital lobe produced flashes of light referred by the patient to the right eye, but in reality projected to the right side of the middle line (visual area corresponding to the left side of each retina). The third case was a typical example of sensory Jacksonian epilepsy, the left half of each retina being completely blind (right-sided homogonous hemianopsia). In the fourth case a localized softening beneath the left angular gyrus had produced temporary word-blindness and hemianopsia, the latter symptom being apparently due to arrested function in the optic radiations. A fifth case, still under consideration, was briefly referred to. In it complete right-sided hemianopsia, complete word-blindness, and temporary mind-blindness, without any other symptom whatever, had resulted from a sudden cerebral lesion, which was thought to be an embolism of the left middle cerebral artery, with resulting softening of the angular gyrus.—Prof. Crum Brown communicated a paper by Mr. F. Beddard on the anatomy of Ocnerodrilus (Eisen).

March 16.—Rev. Prof. Flint, Vice-President, in the chair.—Emeritus-Professor Blackie read a paper on bistratification in the living Greek language. The author asserts that modern Greek has been but slightly altered, since the time of Coraes, from classical Greek. The first thirty-one verses of the Gospel of St. John, as published in Athens in 1855, contain only nine departures from the classical type; while the corresponding portion of the Romaic version, published 200 years ago, contains twenty-eight. In the higher walks of Greek literature this purity of literary style is very marked. In thirty-one pages of Tricoupis' "History of the Greek War of Independence" (published in London in 1853), only fifteen deviations from the standard of ancient Greek appear; and in two chapters of Paspati's "History of the Capture of Constantinople by the Turks" (published in Athens in 1890), only ten deviations appear. The standard to which the author appeals is the Greek, not only of Plato and Xenophon, but of Diodorus, Lucian, Polybius, and Chrysostom. In the lower colloquial Greek of common life, very great divergence from classical literary style is evident. Thus, in the first twenty-six lines of the dialogues in a primer of colloquial Greek, published this year in Leipzig, thirty-three deviations from classical style occur. But, in even this lower form of Greek, very few words borrowed from other languages are found; and the accented syllable still remains as it was fixed by the Alexandrian grammarians.—Dr. John Murray read a paper, written by Mr. Robert Irvine and himself, on silica and siliceous formations in modern seas. There is great difficulty in accounting for the number of organisms which secrete silicic acid, and for the remains of such organisms which occur in the ocean and on the bed of the ocean. The amount of silicic acid which exists in solution in sea-water is far too small to account for the immense development of such organisms in various parts of the ocean. The authors have proved that clay and mud, carried down by rivers to the sea, are to be found in even the least disturbed parts of the ocean. And the diatoms can extract from these clays sufficient material for the formation of their siliceous sheaths. The authors have also proved that the suspending power of sea-water for such clays diminishes very markedly as the temperature rises. This appears to account for the great abundance of diatoms in the colder seas. —A communication, by Dr. W. G. Aitchison Robertson, on the relation of nerves to odontoblasts, and on the growth of dentine, was read.—Mr. A. Silva White read a paper, illustrated by a map, on the comparative value of African lands. The chief points dealt with in this paper are the relative value of African lands to the European Powers which control them; the progressive value of these lands from strategic bases on the coasts; and the lines of least resistance, and tracts of highest resistance, to European domination.

PARIS.

Academy of Sciences, March 23.—M. Duchartre in the chair.—The President announced the loss sustained by the Academy in the death of M. Cahours on March 17, and gave a short account of the life and works of this eminent chemist.—Action of heat on carbonic oxide, by M. Berthelot. It is known that carbonic oxide shows indications of decomposition at a red heat, with production of traces of carbon and carbon dioxide. A discussion of the facts leads M. Berthelot to the conviction that the phenomena are not due to the direct dissociation of carbonic oxide, but to molecular condensation, the condensed product separating into carbon dioxide and suboxides.—On a reaction of carbonic oxide, by the same author. In the course of researches on the preceding subject, M. Berthelot observed a characteristic reaction of carbonic oxide, due to its reducing action on an ammoniacal solution of silver nitrate. If bubbles of carbonic oxide, or an aqueous solution of it, be added to the nitrate solution, an abundant black precipitate appears upon boiling.—On the proper odour of earth, by MM. Berthelot and G. André. The authors have endeavoured to determine the origin of the odour which is so marked an emanation from vegetable mould after a fall of rain. They find that the essential principle resides in an organic compound of the aromatic family. Its odour is very penetrating, and analogous to that of camphors; its proportion in mould is only a few millionths, but one three-millionth of a gram is sufficient to produce a sensible smell. The new principle is neither acid, alkali, nor a normal aldehyde; its concentrated aqueous solutions may be precipitated by potassium carbonate, with the production of a resinous ring. When heated with potash, an acrid odour, analogous to that of the resin of aldehyde, is developed. It does not reduce ammoniacal silver nitrate. Under certain conditions—that is, by the employment of potash and iodine—iodoform is produced. This property is common, however, to many other substances, but the authors have not found furfurol, acetone, nor ordinary alcohol, although M. Muntz has stated that these bodies existed in some vegetable mould he examined.—Contribution to the biology of parasitic plants, by M. A. Chatin. The author contests the idea that parasitic plants nourish the hosts.—On the glycolytic power of the blood of man, by MM. R. Lépine and Barral. The glycolytic power is defined as the percentage loss of sugar which occurs when blood is kept for an hour in a water-bath at 38°–39° C. Determinations have been made of the glycolytic power of the blood of persons suffering from diabetes, pneumonia, and uræmia.—Observations of Millosevich's asteroid (304) made at Paris Observatory with the East Tower equatorial, by Mdlle. D. Klumpke. Observations for position were made on March 13 and 17.—On the theory of applicable surfaces, by M. J. Weingarten.—On the changes which are presented after imbibition in a system formed by the superposition of two thin, homogeneous, hygroscopic laminæ having different properties, by M. J. Verschaffelt.—On the action of hydroiodic acid on silicon chloride, by M. A. Besson. A part of the chlorine of silicon chloride has been replaced by iodine; the products obtained are described.—On the transformation of pyrophosphite of soda into phosphite, by M. L. Amat.—On bromo-nitrite salts of platinum, by M. M. Vèzes.—On the disaggregation of neutral amine salts by water, by M. Albert Colson.—New combinations of pyridine, by M. Raoul Varet.—On the theory of dyeing phenomena, by M. Léo Vignon.—On a method of simultaneously measuring the time of electrical excitation, and the resulting muscular contraction, by M. A. d'Arsonval.—On the action of phenic acid on animals, by MM. Simon Duplay and Maurice Cazin.—Actinometric observations made at the Observatory of the Petrowsky Academy, near Moscow, by MM. R. Colley, N. Michkine, and M. Kazine.—Some remarks on these observations, by M. Crova.

AMSTERDAM.

Royal Academy of Sciences, February 28.—Prof. van de Sande Bakhuyzen in the chair.—Mr. D. T. Korteweg discussed the spinodal and connodal curves of a surface having a slightly deformed conical point.—Mr. van Bemmelen showed two new compounds of mercuric oxide—a crystalline sulphate of mercury with one molecule of water, $HgO.SO_3.H_2O$, and a colourless basic sulphate, $(HgO)_3.(SO_3)_2.(H_2O)_2$, both obtained by Mr. C. Hensgen in the inorganic chemical laboratory of Leyden, during his researches on the chemical equilibrium between mercuric oxide and diluted sulphuric acid. He shows also that a homogeneous solution of chloride of antimonium, S_6Cl_3, in diluted sulphuric acid is separated into two layers of liquid by heating it to a certain temperature. The two layers disappear again by cooling. The temperature of the separation depends on the composition of the mixture. Mr. Hensgen is carrying on his researches on the subject.—Mr. L. M. J. Stoel, on the influence of temperature on the viscosity of fluid methyl chloride (communicated by Mr. Lorentz). Mr. Stoel has measured the transpiration times for a fixed volume of the liquid, the capillary tube being always the same. The pressure at one end was equal to that of the saturated vapour; that at the other end was greater by an amount which is measured by a column of mercury of a fixed height. The temperatures range from $-28°$ C. to $+123°$ C., the boiling-point being $-23°$, and the critical temperature $+143°$. The transpiration times (in seconds) may be calculated with a fair approximation by the formula

$$\log D = \frac{896 - T}{250},$$

T being the absolute temperature. The experiments were executed in the laboratory of Mr. Kamerlingh Onnes, at Leyden.

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