THURSDAY, APRIL 5, 1883

FIRE-FOUNTAINS

Fire-Fountains; the Kingdom of Hawaii, its Volcanoes, and the History of its Missions. By C. F. Gordon Cumming. In two vols. 8vo. (Edinburgh and London: William Blackwood and Sons, 1883.)

MISS GORDON CUMMING has, in the work before us, given a most lively and interesting account of the Sandwich Islands. The large amount of experience which she has gained during five years of almost continual travel among the islands of the Pacific has enabled her to make careful comparisons between the physical features, the productions, and the populations of the different groups. In her two previous works, "A Lady's Cruise in a French Man-of-War," and "At Home in Fiji," our authoress has given us her impressions of Tahiti and the Fiji Islands respectively.

It is evident that Miss Gordon Cumming's first sentiments on arriving in the islands were those of disappointment. In productiveness, in the picturesque character of their scenery, in the beauty of their coral reefs, and in richness of flora, the Hawaiian Islands must certainly yield the palm to the Archipelagos of the Pacific. Even Kilauea itself failed to satisfy the traveller's expectation, for at the time of her first visit the fires of Halemaumau seemed to be almost extinct. Fortunately these first feelings of disappointment were to some extent removed by what the authoress subsequently witnessed during her long sojourn in the country.

The title of "Fire-Fountains" may perhaps lead a geologist to anticipate a more than usually exact account of the volcanic phenomena of these interesting islands. The extreme liquidity of the Hawaiian lavas enables them—as Dana, Brigham, Coan, and others have so well shown—to be thrown up into actual "fountains," and such jets have been witnessed both in Kilauea and Mauna Loa, rising to the height of several hundred feet. Any expectations of scientific accuracy in the account of the volcanic phenomena are, however, dispelled when we turn to the work itself. Miss Gordon Cumming's descriptions are wonderfully graphic, and a small amount of geological training would have enabled her to avoid popular errors, and employ accurate instead of misleading terms, thus making them valuable records of the phenomena she witnessed. Unfortunately, as in so many similar cases, this small amount of previous training was wanting.

The first part of the work consists of descriptions of the physical features of the group and of the characteristics of the inhabitants, and here the authoress largely relies upon her own observation, and furnishes us with many instructive comparisons with Tahiti and Fiji.

The second part of the book, which contains a history of the islands and of missionary enterprise in them, is of course compiled from published works, the information thus acquired being supplemented by facts derived from independent sources, such as letters and conversations.

The visit to Kilauea has been so often described that it may seem difficult to understand how any ordinary traveller can find anything new to say on the subject. But Miss Gordon Cumming had the good fortune (though she does not seem to have appreciated it at the time) to see the crater under somewhat exceptional conditions, as the following account will show (vol. i. pp. 164, 165):—

"After traversing three miles of this strangely varied lava-bed we reached the base of that inner circle of crags which within the last few months have been thrown up all round the central crater—*i.e.* the Halemaumau. So rapidly have they been upheaved, that they now form a ring 600 feet in height; and up this steep ascent we had to climb in order to look into the Lake of Fire.

"It was a toilsome ascent over very brittle lava; but Roback kept cheering me by telling me what a grand sight awaited me, and that he had never seen the lake in finer action than last week. So we climbed over coils of huge hollow vitreous lava-pipes, which constantly broke beneath our weight, and over ridges which looked to me like gigantic sugarsticks pulled out and twisted—and at last we gained the summit, and looked eagerly for the much-described Lake of Fire.

"THERE WAS NONE! at least nothing worth speaking of, in the first instance. I turned to look at my guide, and he stood staring in stupefied, bewildered amazement. He could not believe his own eyes. Only a few days had elapsed since he had led a party of Americans to the very spot where he now stood beside me in speechless wonder at the change.

"They had watched the blood-red waves dashing in scarlet spray against the cliffs on the farther side of the lake of molten fire, then rushing back to form a mad whirlpool in its centre, and thence, as if with a new impulse, flinging themselves headlong into a great cavern which undermined the lava-terrace just below the spot where I was now standing."

This was written on October 29, 1879, but three days afterwards the authoress has a very different state of things to chronicle (vol. i. pp. 186-189):—

"*November 1st.*

"Last night was Hallowe'en—the great fire-festival of our ancestors—and here it has been celebrated in right royal style, for the fire-spirits have broken loose and are holding high revel.

"The flow is increasing rapidly and is magnificent. The fire has burst out at so many points together that it has formed a new lake in the outer crater, in which fire-jets are spouting and molten lava thrown high in mid-air, great masses of red-hot solid lava being tossed to a height of from forty to fifty feet, while from the overflowing rim, or from weak points in the sides of the lake-basin, flow rivers of lava, forming a network of living, rushing fire, covering fully two square miles of the very ground over which I was walking only two days ago. It is a scene of marvellous beauty and is inexpressibly fascinating.

"From the edge of the crater-wall I have watched each stage in the growth of this strange new lake. I have seen it gradually rise higher and higher, till at last it overflowed in glowing streams, like rivers of golden syrup, but brighter far—an indescribable colour. The centre of the lake is oftenest of a silvery grey, only crossed by zigzag lines of flame colour and deep rosy red; but all round its shores it is continually surging and upheaving great crested billows, which break in fiery surf and toss up clouds of fire-spray. Sometimes the whole lake appears to be in a tremendous commotion—heaving and trembling as if acting obedient to some pressure from the furnace below.

"About a dozen cones have formed in and around the lake, each a distinct fire-fountain, yet all flameless—only merrily flinging about the molten metal: a bouquet of rare fireworks.

"These cones are miniature volcanoes—spouting liquid lava in the most sportive manner, playing gracefully like true fountains—spouting like intermittent geysers, and falling in showers of red hail—sometimes silently, sometimes with puffing and spluttering, v ried with a roar like an angry bull; then a hush, followed by low moaning sobs.

"Some of these explosive forces have not built themselves chimneys, or, if they have, the lake has melted them, for they only betray their existence by suddenly bursting beneath the surface, like torpedoes, and tossing up red rockets.

"From the crag above I looked down upon a heaving, restless expanse of dull red almost entirely coated over with a silvery-grey scum, intersected by flowing rivers of red gold. The ceaseless movement beneath the surface kept up a glancing, gleaming play of white and red light, glistering like quicksilver in motion. Sometimes there came a swirling eddy, like the rush of a Highland stream.

"Then, again, the lava seemed to writhe and twist as if in agonised contortions, and then commenced a violent boiling and bubbling preparatory to its bursting into active fire-fountains. These play sometimes singly, sometimes alternately, sometimes a dozen burst into simultaneous action—like some marvellous display of rockets, flinging their fiery rain on every side, then dying away altogether, till the silvery coating spreads so evenly over the surface of the lake, that, but for the sulphureous exhalations and columns of smoke, it might almost be mistaken for some cool refreshing pool. In truth, the white vapours which play so eerily among those black rock-masses, might well be morning mists floating upward from a quiet mountain-tarn.

"This, however, is a delusion not to be cherished for long, especially towards sunset; for then the lake appears in its true glory, and all the wonderful chemical colours which were lost in the full light of day reveal themselves, the difference of the scene before and after sundown being that of any huge smelting works, as seen by day or by night, only magnified ten thousand times. Then the scale of colour varies from deepest chocolate, crimson, and scarlet, to orange, yellow, and primrose tints, and the silvery grey becomes tinged with pink and violet, while the solid rocks become ever more intense in their blackness; and the many-tinted sea plays around them, and dashes over them, and from time to time detaches some huge fragment, which falls with thunderous crash, reverberating from crag to crag.

"As the twilight faded away, my kind landlord rigged up blankets and lanterns to make me a snug sketching-point on the hill above this house, whence I could watch the glory undisturbed, and attempt to preserve notes in colour, which may give you and others an idea, however faint, of the amazing scene before me. A full moon added its cool, pure light to the lurid crimson glow, which was reflected on all the overhanging clouds, as well as on the column of white steam which for ever rises from the Halemaumau itself; and these clouds, being visible at a distance of many miles, must have declared plainly to our friends in Hilo that there was unusual activity at Kilauea."

The authoress of this work did not reach the summit crater of Mauna Loa, but at the end of her book she has collected from various sources a tolerably complete account of the great outbursts of 1880 and 1881.

The details given in this volume concerning the aboriginal inhabitants and their manners and customs—or rather, we should say, of the total want of the former and the utter "beastliness" of the latter—is interesting to the anthropologist. The judgments of the authoress upon historical questions are by no means unfair, and if she does not follow American writers in treating Capt. Cook's visit as an act of piracy and his fate as a just retribution, she clearly points out that the death of the great navigator followed as a natural consequence of the sad misunderstanding between the English and the natives. From the traditions of the natives we can now fill in many details of the story, and explain certain matters which Cook, in his total ignorance of the language of the people, could scarcely guess at. In this and in the subsequent transactions between the English under Capt. Vancouver, and the Hawaiians, it must be confessed that the natives were treated with but scant justice at the best, and in too many instances with wanton cruelty and tyranny.

The admirable illustrations of this work constitute one of its most valuable features. They are reproduced by the autotype process from the sketches of the authoress. The frontispiece, showing the low rounded dome of Mauna Loa, with Kilauea on its flanks, is one of the best representations of this most wonderful district which we remember to have met with. The indefatigable traveller who has now become an acknowledged favourite with the public may be heartily congratulated upon the success of this latest production of her busy pen and pencil.

OUR BOOK SHELF

Africana, or the Heart of Heathen Africa. By the Rev. Duff Macdonald. 2 vols. (London: Simpkin, Marshall, and Co., 1882.)

NOTWITHSTANDING a large amount of professional commonplace, this work rises considerably above the level of ordinary missionary productions. The author, who administered the Church of Scotland Mission at Blantyre, south of Lake Nyassa, during the years 1878-81, applied himself diligently to the study of his dusky flock, and has embodied his experiences chiefly in the first volume, devoted to the "native customs and beliefs." The second is occupied more specially with "mission life," and with the inevitable difficulties and troubles entailed upon the writer in consequence of his accepting a position which from the first he felt to be untenable.

Since his enforced retirement from active work, Mr. Macdonald has usefully occupied his time in arranging for publication some of the rich materials collected during his stormy missionary life. Most of these materials, being the result of original observation in a new field not yet disturbed by contact with Europeans, possess great scientific value. The descriptions of the native manners, customs, beliefs, superstitions, and traditions are as interesting as they are trustworthy, and they are supplemented by two appendixes, which may be specially commended to the attention of all lovers of folk-lore. These comprise numerous selections of original "native tales" and "cosmical tales," literally translated from the author's manuscript collection of tales, songs, enigmas, &c., the whole of which it is to be hoped he will be induced to publish. Some of the tales accounting for natural phenomena have at least the merit of brevity, as, for instance, that about the wind: "A great man had a daughter, and she said, 'Father, in this country I am hot, I sweat.' Then her father said, 'Come here, my child, I have pity, I will blow with my breath.' So he blew, and thence came wind" (i. 283).

It is sad to learn that trial by ordeal and torture is still as universally practised as it was in Europe during mediæval times. "When a Magololo suspects his wives, he places a stone in a jar of boiling water or oil, and orders them to fetch it up with their bare arms. He then judges of their guilt by the amount of injury they sustain. When a woman is thus convicted, he makes her confess who seduced her. In vain does the helpless creature protest that she is innocent. Notwithstanding that her arm is severely scalded, she is subjected to the most cruel torture by a kind of thumbscrew (*mbanilo*), which is applied to her head. A small tree is partly divided along the middle, the skull of the poor woman is inserted as if it were a wedge for splitting the tree still farther. Great pressure is exerted by forcing the halves of the tree together with the aid of pulleys" (i. 201). This of course has the wished-for effect, and as in the "processus inquisitorii," the wretched victims "dum propria sua confessione contra se pugnare coguntur sui ipsius proditores tortæ constituuntur." A. H. K.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

I READ with interest, in NATURE, vol. xxvii. p. 362, the reply made by Dr. Romanes to a letter of mine which, although not originally addressed to a scientific organ, found hospitable reception in your columns. It was not much out of place there, for it was essentially an inquiry whether certain inferences may or may not *scientifically* be drawn from certain premisses. I am not wholly without hope of making it clear that the criticisms which I ventured to bring forward are grounded in reason; and confining my rejoinder strictly to the issue joined, I may hope not to be long nor very tedious. Let me trust that no curtness of statement will imply any want of the great respect which I entertain for an able investigator and writer, whose view may be imperfectly apprehended, or may bear an interpretation I should accede to.

The issue is a narrow one, and there is no need to widen it. Dr. Romanes is understood to derive from scientific premisses the conclusion that evidence of design is not legitimately derivable from the structure and adaptations of plants and animals, and, more particularly, that the theory of natural selection has destroyed the evidence of special design in organic nature, so that now the facts of organic nature furnish no other and no better evidence of design than do the facts of inorganic nature.

The first of these conclusions was derived from the proposition that there is no point of logical contact between natural science and the idea of design, wherefore no inference can legitimately be carried from the facts of the one to the conceptions of the other. I suggested that the maintainer of that position could not consistently argue that a particular scientific theory has annihilated an inference admittedly beyond its logical range. The reply is that, "If a man believes that there is no logical connection between one thing and another, I do not understand why he should be deemed inconsistent because he endeavours to show the fictitious character of the logical connection which has been erroneously supposed to exist." But the point of the objection was that, while insisting that any inference from the one to the other was invalid from the nature of the case, he actually inferred that certain scientific facts and theories completely overthrow and destroy the theory of particular design in organic nature. This may be. Only one would think that whatever may be legitimately overthrown may be as legitimately supported.

Moreover, if I rightly understand, there was not long ago a legitimate ground of inference (whether scientific in the narrower sense or philosophical need not here be inquired) from organic nature to design. "For it would be proof positive of intelligent design if it could be shown that all species of plants and animals were created"; and therefore proof presumptive while the theory of special creation was accepted and probable. At least—and this is the point—the argument from structure and adaptation to design was then admissible and even cogent.

Now, from the scientific side, upon which we are standing, special creation means only that the forms were scientifically inexplicable, and to be taken as original; their adaptations to their surroundings and their relations of means to ends in themselves equally as primary endowments. And whatever evidences of intellectual origination these manifested, *were seen in the things themselves*, and we suppose are to be seen there still. The inference was not one from an intellectual originator to design in the organic world, but from marks and operations in the latter which indicated design to an intellectual originator. The inference to most minds was convincing; at least it was legitimate. The recognised laws and operations of nature—a better knowledge of which has destroyed so many crude notions—were not thought to interfere with it.

It used to be so, but we have changed all that. How?

First, by the declaration of the principle that the facts of organic nature, in all their multiplicity and variety, yield no other and no better indications of design than do any of the facts of inorganic nature. That is to say, a stratum no more than a structure, a crystal than a chrysalis, living things and their responses than lifeless things simply acted upon, things which are intelligible only when contemplated as means and ends, no more than things of which ends are predicable, if at all, only by remote implication. Not only is the one as good as the other, but any one is said to be as good as all. Because of "the universal prevalence of laws and sequences of cause and effect, . . . they are not really or logically strengthened by a mere enumeration of particular instances. . . . The so-called law of causation as a whole being known, and its universality recognised, its true argumentative value to the theory of theism is not influenced by the explicit formulation of any number of its specific cases."

Here "law of causation," or the way how something comes to pass, is mixed up with "evidence of design," or what it was for. And we are to conclude that the immense variety and multiplicity of adaptations of particular means which accomplish particular ends in organic nature bring no contributory and cumulative evidence as to there being any design in them. In palliation of the charge of "damnable iteration," to which the teleologists are thus exposed, it may be pleaded that, although possibly one good witness or one good observation may be as convincing as many for certifying a fact, surely the more and the more varied the better for proving an underlying intervention—of which the evidence must always be circumstantial, and the conclusion a judgment or belief.

The old belief that adaptation of means to ends in plants and animals gives evidence of intellectual origination, had not been seriously unsettled by the scientific belief of the universality of the law of causation. It remains to be seen whether it will survive the establishment of the belief that the forms in which these adaptations are recognised have themselves been slowly evolved and diversified in a way that is partly explained by the doctrine of natural selection; and this is the gist of the question.

Dr. Romanes thinks that we have, in natural selection, "a cause other than intelligence competent to produce the adaptations," one which supersedes intelligence by working gradually. For, "if the adaptations have been effected gradually, and by *the successive elimination of the more favourable variations by a process of natural causation*, we clearly have a totally different case to contemplate, and one which is destitute of any evidence of special design." "The progressive adaptations of structures to functions by such a purely physical cause as natural selection, when once clearly revealed, must destroy all special or particular *evidence* of design, even supposing such design to exist." This phrase, "such a purely physical cause as natural selection," and the preceding phrase italicised by its author as specially significant and as being its equivalent, show that the term is used in its strict sense. So the substitute for intelligence, that which is said to account for all the adaptations in living nature, is the successive destruction of the less favourable variations by natural causes, leaving the most favourable to survive! Here "we clearly have a totally different case to contemplate, and one which is destitute of any evidence of special design,"—equally destitute, one would say, of any pretensions to act as its substitute until it is explained how the physical destruction of a part should have set the rest into varying at all, into varying advantageously, and into varying into the very special ways they have done. Not till this, or something like it, is done, can natural selection pure and simple claim to give scientific explanation of the adaptations and the forms at whose birth it has assisted.

When I before insisted that "to make the purely physical explanation tenable it must be shown that natural selection scientifically accounts for the adaptation," and that it has not done this, that no reasons have been given why the organisms must have responded in the ways they do, or have responded at all to the environment, I meant only that the theory ought to fulfil the conditions which other physical theories are bound to satisfy, *i.e.* to account for the principal facts of the case. I had no reference to any subsidiary hypothesis which might help the matter. Dr. Romanes rightly says that it lies not with the evolutionist to show that variations may not have been intellectually planned or guided. But when he assigns the whole results to known physical causes and discards the factor of intelligence, he is bound to render their adequacy at the least conceivable.

It may now be seen, I trust (and the context might have made it clear), that, in asking Dr. Romanes if he was quite sure that any other cause than intelligence could adapt organisms to their environment gradually, I was not inviting him to guesses "about the possibilities of supernatural creation," but to a reconsideration of his antithesis between special (and as he will have it, sudden) creation, requiring intelligence, and gradual evolution, which might dispense with it; and I was intimating that he had not shown how the latter could dispense with it. The problem was: Given plants and animals with certain structures and certain adaptations to their environment, to be changed into other forms with other structures equally well adapted to a more or less changed environment, how to do this solely by the action of said environment. Answer: By the killing out of all which have not somehow or other acquired the particular structure and adaptation they needed.

But now comes an important qualification: "The evolutionist may freely admit that natural selection has probably not been the only physical cause at work, and even that the variations supplied to natural selection may not have been wholly fortuitous, but may have occurred along favourable lines as responses of the organisms to their physical surroundings"; and Dr. Romanes calls my attention to a statement of his that it may be so in an essay which I regret that I have not read. He continues, however: "But such admissions would make no change in the logical aspect of the case; for, however many supplementary causes of this kind we may choose to imagine as possible, the evolutionist is bound to regard them as all alike in this: that they are of a physical or natural kind."

"Physical or natural kind." The agency which explained away all implication of design was in the strict sense physical, being the action of the environment on the organisms. It is now extended to whatever is *natural*, that is, to whatever occurs in the course of nature, presumably under established laws; and it is assumed that whatever so occurs is thereby void of all *evidence* of intellectual intention (we need not regard the difference—if any there be in such relations—between general and special design, the question being wholly one about the grounds of any *evidence* of design in nature). To me it is wholly probable that existing species and their special adaptations became what they are in the course of nature. And my argument is that, if "such a purely physical cause as natural selection" leaves these adaptations still unaccounted for, whatever implication of designed origination there formerly was still holds, and may hold, although the series of natural causes be practically endless.

Then as to such causes being all of a piece, so that pure physics may explain all biology. Doubtless in a certain sense all nature is of a piece. But in another sense—the very one we are concerned with—it is of at least two pieces; no matter how it came to be so. One of them is pervaded by an element of its own—that of *direction of action to ends*—which is more and more manifested as we rise in the scale of being, but is characteristic of all organisms. That seems to lay a foundation for a difference in the quality of the "inference which can be drawn by the human mind [*quoad* design] from the province of natural science" This difference might have made Dr. Romanes hesitate to draw, from scientific premisses, the downright conclusion that "the facts of organic nature present no evidence of design of a quality other or better than any of the facts of inorganic nature."

Here lies our whole contention. We agree that natural science leaves aside the question whether evolution and design in nature are compatible or not, this being only a phase of the enigma which was as puzzling before evolution was dominant as it is now. We suppose, too, that the difficulty of conceiving how design can coexist with the natural evolution of organisms is fairly balanced by the difficulty of conceiving how the phenomena of organic nature can be accounted for without it. The point which we have laboured over is that, if science has no call to settle the question, it has none to prejudge it. It was only because Dr. Romanes seemed to me unwittingly to have done so, that I ventured the criticisms which opened this discussion.

Cambridge, Mass., U.S. ASA GRAY

P.S.—A brief note upon Mr. Hannay's letter, NATURE, vol. xxvii. p. 364, referring to my supposition of successive generations slowly changing, "*yet always so as to be in compatible relations to the environment.*" He remarks, this "is just such a statement as 'Design' would require, but cannot be held by scientific evolutionists, otherwise why are there so many extinct species?" Surely it could be held by the soundest of evolutionists, for it is of the very essence of Darwinism. Are not the individuals which compose the present fauna and flora in compatible relations to the environment, and is not the extinction of species going on? In human society do we consider that the unmarried and the childless members of the community are not in compatible relations to their surroundings? Is there any reason to suppose that the individuals of a flora of earlier times—say of the Miocene—were not on the whole in as orderly and compatible relations as the existing flora is? It is not *chaos* but *cosmos* that true Darwinism has in mind, common though the contrary impression be. A. G.

PROF. ASA GRAY is kind enough to remark that he has read my reply to his previous communication with interest. I should like to say, *in limine*, that I have read his reply to me not only with interest but with profit; for it is not often that one meets with an argument so carefully thought out and so clearly presented. Therefore, if I seek to meet his further criticisms, it is not in any spirit of controversy that I do so, but solely for the sake of endeavouring to help, so far as I am able, in determining the true logical position of an important question.

This question, as Prof. Gray observes, is a narrow one, and I shall keep to it. Without therefore trespassing upon the wider question of Theism as a whole, our discussion is confined to "an inquiry whether certain inferences may or may not *scientifically* be drawn from certain premisses."

First, I have to meet the dilemma which is put to me when I am told that, having said there is no point of logical contact between natural science and natural theology, I ought not forthwith to say that natural science is competent to destroy an inference belonging to natural theology. But in stating it as my opinion that natural science had shown the inference previously drawn to be invalid, I did not myself, as my critic asserts, draw any inference (even of a negative kind) from natural science to natural theology; I merely endeavoured to point out that an inference previously drawn from the one to the other was illegitimate, that inasmuch as the inference proceeded from natural science it was liable at any time to be overturned by natural science, and that it had now actually been overturned. Whether or not, therefore, I was right in saying that there is no point of logical contact between natural science and natural theology, at least I did not myself endeavour to institute such contact.

But I am told, you admit that long ago the inference in question was valid, and even cogent. Well, I answer in one sense it was, but in another and a truer sense it was not. For its cogency arose from the hypothesis of special and sudden creation on which it rested; grant this hypothesis, and the inference from organic adaptation to intelligent design becomes not only cogent but inevitable. The hypothesis, however, was not one that really belonged to natural science, and it was just this hypothesis that constituted the "fictitious logical connection" alluded to in the passage which Prof. Gray quotes from my previous letter. The facts presented by science remain, of course, very much the same as they were; but it does not follow that, in the absence of the special creation hypothesis, "whatever evidences of intellectual origination these manifested were *seen in the things themselves*, and we suppose are to be seen there still." Let us take an illustration. In the last issue of NATURE there is a letter from Prof. Darwin describing the formation of mudballs by a suitable and rare combination of natural causes. He and his brother did not see these balls in process of formation, and therefore he says, "On seeing the first one or two, they looked to us like the handiwork of some boy with an enthusiasm for mud pies"; but their number and the constancy of their situation on the slopes of hills—*i.e.* further knowledge of the inferred conditions of their origin—afterwards disposed of the teleological hypothesis in favour of a physical one. Now here it is equally true that "whatever evidences of intellectual origination these manifested were *seen in the things themselves*," and after the hypothesis of their physical origin had been arrived at, were "to be seen there still." Yet we should have deemed the brothers Darwin very unworthy representatives of their family if, after having arrived at the physical hypothesis, they had continued to argue in favour of a teleological enthusiasm for mud pies, on the ground that "the inference was not one from an intelligent originator to design in the (in-)organic world, but from marks . . . in the latter which indicated design to an intelligent originator." In other words, a change in the hypothesis concerning the *origination* of the mudballs entirely changed the logical cogency of the teleological inference.

Now I have purposely chosen this illustration because it is of so simple a character, and therefore serves in a clear manner to show how greatly a teleological inference may be modified by a change of hypothesis concerning the mode of origin of a structure, even though the structure remains the same; if there had been no evidence of a purely physical mode of origin in this case, it might truly have been said of the teleological interpretation, "the inference to most minds was convincing; at least it was legitimate." Of course in organic nature the apparent marks of design "in the things themselves" are much more numerous, varied, and complex than any that we meet with in inorganic nature; but no matter how numerous, varied, and complex such marks of design may be, if we see good reason to conclude that they have *all been produced by physical causes*, they are no more available as *evidences* of special design than are the mudballs—although both they and the mudballs, being alike formed under an orderly system of causation, may be due to a general design pervading the cosmos. And here I understand that Prof. Gray is in agreement with me, for he says that when I assign the whole results to known [or unknown] physical causes and discard the factor of intelligence, I am bound to render their adequacy at least conceivable. This appears to show that Prof. Gray is at one with me in holding that physical causes as such do not constitute other or better evidence of design in the organic than in the inorganic world; and it is only because he cannot conceive how such causes are adequate to produce the results observed in the former that he deems these results unique as evidence of "the factor of intelligence." In other words, supposing for the sake of argument that all these results have been due to purely physical causes, and supposing further that all these causes were as perfectly well known as the less complicated physical causes of the inorganic world, then I take it Prof. Gray would agree with me in saying that under such circumstances the former would constitute no other or better evidence of design than the latter.

If so, our only difference resolves itself into a difference in the estimate which we respectively form of the probable adequacy of purely physical causes to produce all the results which are observable in organic nature. To me the probability appears overwhelming that in respect of method "all nature is of a piece," and therefore that the terms "physical" and "natural," when applied to causation, are logically, as well as etymologically, convertible. To Prof. Gray, on the other hand, the probability appears to be that such is not the case, but that, when we meet with the "*direction of action to ends*," we have special evidence of "the factor of intelligence," which therefore makes nature "of at least two pieces," and so makes the term "natural" to mean more than the term "physical."

Supposing that I am right in understanding this as the only difference between us, I may point out that if, while following my ideas of probability, I have erred on the side of rashness in drawing "the downright conclusion" that the facts of organic nature present no other or better evidence of design than the facts of inorganic, Prof. Gray, in following his ideas of probability, can scarcely be able to shut out the suspicion (more especially in view of abundant historical analogies) that, in resorting to "the factor of intelligence" as a hypothesis wherever physical causation is found to be complex or obscure, he may be merely supplementing our present ignorance of such causation by an inference which is at least as rash as my statement.[1] And here I should like to observe, with special reference to the natural or physical causes summed up in the term "natural selection," that although I speak with all the respect which I sincerely feel for so distinguished a naturalist and so able a dialectician, I am not able to follow Prof. Gray in his understanding of this subject. For he says of the theory of natural selection that it is destitute of any pretensions to act as the substitute of the theory of special design, "until it is explained how the physical destruction of a part should have set the rest into varying at all, into varying advantageously, and into varying into the very special ways they have done." But surely it is no part of the theory of natural selection to suppose that the *physical destruction* of unfit organisms is, or has any need to be, the *cause* of advantageous variations arising in other and allied organisms. The theory merely supposes that variations *of all kinds and in all directions* are constantly taking place, and that natural selection seizes upon the more advantageous. Therefore, so far as this theory is concerned, there is no call to explain why promiscuous variation occurs; it is simply a fact that it does occur, though not necessarily *made to occur* by the destruction of other organisms. Neither is there any call to explain why the variations occur in special and advantageous ways, for they are not supposed to occur in special and advantageous ways, but only to appear to do so on account of all other variations being eliminated, while those which happen to occur in the specially advantageous ways are preserved. Again, Prof. Gray says in his postscript that the theory of natural selection supposes successive generations to be slowly changing, "yet always so as to be in compatible relations to the environment." Now it is true that where the changes in the environment are gradual, and the variations of specific type are being slowly accommodated to them, each generation is, on the whole, in compatible relations with its environment. But it is not true that such continuous compatibility in itself points to design; it only points to the plasticity of the varying type, which, if not sufficiently plastic to meet the new demands upon it in this respect, simply becomes extinct.

In conclusion, I agree that "natural science leaves aside the question whether evolution and design in nature are compatible or not," and I agree that, "if science has no call to settle the question, it has none to prejudge it." But I do not agree that I have prejudged this question by saying that in my opinion the theory of evolution, in supplanting the theory of special creation, has necessarily removed the special evidence of design in organic nature, by showing that in respect of causation organic nature and inorganic nature are one. GEORGE J. ROMANES

[1] I suppose it will be admitted that the validity of an inference depends upon the number, the importance, and the definiteness of the things or ratios known, as compared with the number, importance, and definiteness of the things or ratios unknown, but inferred. If so, we should be logically cautious in drawing inferences from the natural to the supernatural; for although we have the entire sphere of experience from which to draw an inference, we are unable to gauge the probability of the inference when drawn—the unknown ratios being confessedly of unknown number, importance, and degree of indefiniteness: the whole orbit of human knowledge is insufficient to obtain a parallax whereby to institute the required measurement or proportion between the terms known and the terms unknown. Or, otherwise phrased, we may say—As our knowledge of a part is to our knowledge of a whole, so is our inference from that part to the reality of that whole. Who, therefore, can say, even upon the supposition of Theism, that our inferences or "idea of design" would have any meaning if applied to the "All-Upholder," whose thoughts are not as our thoughts?

The High Springs of 1883

THE high springs of the present year, consequent upon the excessive rainfall of the past winter, are an event that ought not to pass unrecorded in the pages of NATURE. I can speak only of phenomena which I have observed upon my native chalk hills of Hampshire, but I doubt not that similar facts have attracted attention elsewhere.

The Candover, a confluent of the Itchen from the north, burst forth this year in a field near Preston Candover, where it has not been known to rise for the last fifty years, and has flooded the road between Preston Candover and Chilton Candover. The Itchen itself rose in the valley above Cheriton beyond its recognised source, and has flooded fields on the road to Kilmeston, where no one recollects to have seen water before.

The Hampshire tributaries of the Thames have acted in exactly the same manner. The Whitewater has issued forth in the valley just below Upton Grey, far above its usual origin even in the highest springs, and has flooded the whole road between Bidden and Greywell. Another branch of the same stream has risen in the fields on the left of the main road from Odiham to South Warnborough, where spring water has never been known within the memory of the oldest inhabitant. In like manner the Wey, which, in wet seasons, takes its rise in the meadows adjoining Chawton House, has issued forth this year at a much higher level in the fields below Farringdon.

These facts are the more worthy of notice because it has been generally believed that, in the Hampshire hills at least, owing to more efficient drainage and other causes, the springs were getting lower every year, and would never again attain the level that they once had according to the traditions of past generations. It should be added that the springs were at their highest about the commencement of this month, and are now gradually falling.

P. L. SCLATER

Hoddington House, Odiham, March 31

Scorpion Suicide

I AM sorry that my experiments on scorpion suicide has given pain to some of your correspondents. Allow me to explain in a few words the object of my investigation. It is commonly believed in this colony and elsewhere that scorpions commit suicide; Dr. Allen Thomson, in a letter to NATURE, lent the weight of his scientific name to this view; and Dr. G. J. Romanes, in his "Animal Intelligence," treats it as an open question. Now if this habit of committing suicide be an established fact, we have in scorpions a highly persistent type of creature that inherits a habit detrimental alike to the individual and the species. *Scorpion suicide, therefore, if a fact, is one of the strongest individual cases against the Theory of Evolution by Natural Selection that is presented to us in the animal kingdom.* It seemed to me that the only way of settling this question was by the direct appeal to experiment. But is the Theory of Natural Selection of sufficient importance in its bearing upon human life and human progress to justify the infliction of pain upon, say, sixty scorpions? I am one of those who believe that it is. I am one of those who believe that the theory of evolution has enormously influenced human thought and action, and is destined to influence it in a constantly increasing degree. I believe that much of the moral and intellectual progress of our race is indissolubly associated with this theory of evolution. I may be wrong in that opinion, but that is the opinion I hold. And holding that opinion it became to me a duty to do something towards settling a question which seemed to me to be of great importance in its bearing on the evolution theory. And it was my object to do the work, as far as I could, thoroughly and once for all. I believed that if I could show that even under torture scorpions do not commit suicide, the view that they do so when irritated by the bright light of a candle-flare became highly improbable. To establish a negative in the face of positive assertions is, however, difficult, and I considered it necessary to experiment upon a number of individuals. *Hinc illæ lachrymæ!* One of my friends, however, protested as follows: "The theory of evolution," he said, "is now so strongly established, that scorpion suicide is *a priori* impossible." But I hold it to be dangerous in the extreme, in the present position of science, to set up the theory of evolution as a doctrine from which to draw deductions, *unchecked by an appeal to nature where such appeal is possible.*

C. LLOYD MORGAN

Rondibosch, March 12

Nesting Habits of the Emu

I AM able fully to confirm Prof. Moseley's statement of the habits of the emu in nesting at Blenheim. Some years ago my father was very successful in rearing these birds at his place at Brockham Lodge, near Dorking. The first egg was usually laid shortly after Christmas; the total number of a brood being from fifteen to twenty, laid usually at intervals of about forty-eight hours. Some time before the full number was laid the cock bird would commence the incubation by carefully drawing them under him. When the hen bird was ready to add to their number she would sit down by his side, produce the egg, and her mate would then carefully draw it under him with his foot. As soon as the number was completed, it became necessary to seclude the hen bird, as she was from this time "vicious" towards her mate and towards her own eggs; and the seclusion continued until the young birds had attained a considerable size, as she showed every disposition to destroy them. The number of eggs laid was often too large for the cock bird to get comfortably under him. Still during several years that my father kept the birds a considerable number of eggs were annually hatched, and the young birds reared to the breeding age. No brood from native birds was, however, obtained. They showed no disposition to change the breeding season from January to July. In captivity the birds strikingly exhibited their singular inquisitive propensities. They were not usually vicious, except during the breeding season, but were very easily frightened.

London, March 31 ALFRED W. BENNETT

The Recent Cold Weather

THE excessively severe and prolonged cold weather of the month of March has hardly a parallel in this century. It appears to have been felt throughout Europe, and has even reached the shores of Africa. Frost, snow, and wintry gales we expect at a season proverbial for its fitful severity, but the scarcely interrupted sweep of the frigid atmospheric waves which have overwhelmed us for three successive weeks is an experience of weather so remarkable that I conceive the record will probably interest some of your readers.

In position, altitude, and in its freedom from the sheltering influence of large towns, this station may be accepted as favourable for giving an accurate account of the weather in the centre of England. Our instruments are on a proper meteorological stand, and are by Negretti and Zambra. I may add that, in its blighting influence on vegetation stimulated into activity by a mild and moist period in February, this weather has proved more destructive to early fruit blossoms, certain shrubs and plants accepted as hardy, than from any weather previously experienced in March in other years; but apart from vegetation, and acting on the upturned fallows and soddened clods of clay, the penetrating winds, frequent frosts and falls of snow have pulverised the land, so that it falls before the plough or harrow like calcined limestone, and in respect to the preparation of land the weather has had a beneficial action.

Record of Weather, March, 1883, *at Belvoir Castle, Leicestershire*

March.	Min.	Max.	Grass.	Wind.	Rain.	Snow.
4	27	50	27	S. to N.	—	—
5	27	51	20	N.	—	—
6	33	52	29	N.	—	0″·2
7	26	40	22	N.	—	0″·2
8	24	41	24	N.	—	0″·25
9	20	35	14	N.	—	0″·12
10	9	37	4	N.	—	0″·5
11	20	38	10	N.	—	—
12	25	39	23	N.W.	—	0″·2
13	25	39	20	W.	—	0″·1
14	29	40	22	W.	—	—
15	27	39	20	N.	—	0″·5
16	26	38	19	W.S.W.	—	—
17	28	38	24	S.W.	—	0″·9
18	25	40	20	S.	—	0″·1
19	28	42	21	N.	—	0″·1
20	31	40	31	E.N.E.	—	0″·31
21	32	37	31	N.E.	—	—
22	28	35	27	E.	—	—
23	28	35	26	N.E.	—	—
24	18	42	5	W.	—	—
25	26	45	16	N.W.	0″·4	—
26	26	41	19	N.W.	0″·5	—
27	27	40	18	N.	—	—
28	26	43	16	N.W.	—	—
29	24	41	12	S.	—	—
30	35	48	35	S.	0″·3	—
31	30	55	24	S.W.	0″·11	—

Belvoir Castle Gardens WILLIAM INGRAM

Sap-Flow

A REMARKABLE instance of the strong up-rush of sap in trees at this time of the year occurred here during the late severe weather. The boughs of a sycamore overhanging a road were trimmed on the 21st of this month during a very keen frost, and next day icicles of frozen sap, varying in length from a couple of inches to a foot, were hanging from the severed ends. The icicles were semi-opaque in appearance and slightly iridescent, like the sheen on the moonstone, and, when put in a bottle and melted, the product was pure sap.

The sycamore, being one of the earliest trees to develop leaves, had its sap rising, notwithstanding the intense cold and late season; while a beech, which is much later in coming out, and an ash, which is usually latest of all, whose boughs had also been lopped, showed no signs of bleeding, and the cuts remained dry and bare.

The icicles have been melted, reformed, and melted again since the 21st, and still the sap is dropping from the cuts.

Highfield, Gainsborough, March 28 F. M. BURTON

Foamballs

To artificial snowballs and mudballs will you permit me to add an experience of foamballs. We were staying at Biarritz in early spring, and one morning on going down to the beach we found it covered with such balls. A strong wind was blowing off the bay, which caught the wave-crests, and threw off little masses of foam. These, though quite small at first, accumulated, and, in some cases, conglomerated as they rolled inland, until they gradually attained a size of two to three feet in diameter; and as many of these balls of various sizes were drifted along by the wind, they presented a most singular appearance. This was made more curious by some of the town dogs catching sight of the objects, and taking to chevying them along the sand, until a sort of steeplechase was established. Every now and then a dog would overtake and dash into a flying sphere, only to find it, to his manifest disappointment, of a very unsubstantial character. The beach was covered far and wide with the debris of the broken balls.

Guildown, March 31 J. RAND CAPRON

Meteor; the Transit; the Comet

As you have on previous occasions deemed it of sufficient interest to record notices of striking meteors observed, I send you an account of a singularly brilliant and unusual form which appeared here about half-past 8 p.m. on the 29th inst.

I happened to be looking at a portion of the sky a little below the constellation "Orion," that is to the southward and eastward, when suddenly a brilliant meteor became apparent. Unlike ordinary meteors, it did not move, at least to my vision; it simply increased in size and brilliancy, till it appeared like a fine "Roman candle" or "blue light," intensely blue, and emitting rays at about two hundred yards' distance. It appeared to illuminate the country with a pale blue light.

It disappeared as suddenly as it came. Could its stationary appearance and increasing brightness have been caused by its *approaching* me in a direct line? I have thought so.

I saw the transit of Venus splendidly from my hilltop, through my binocular, an ordinary hand-telescope, and even with the naked eye, protected of course in each instance by coloured glass.

The comet also was a glorious object for several weeks. It was first seen here on September 23. I noticed very plainly the dark line near the right edge of the tail, as if there had been a *fold* in a luminous substance; that was the idea that the appearance gave me. Fig. 3, p. 610, vol. xxvi. of NATURE, most resembles what we saw here, but the shadow, or dark part, from the V-like incision at the end, should be longer and darker.

Not being a scientific observer, I did not trouble you with any notices of either, feeling sure you would have plenty.

British Consulate, Noumea, January 31 E. L. LAYARD

Ticks

CAN none of your readers be prevailed on to take up the study of the Ixodes (Ticks), of which there are several British species? I feel sure their life-history, if fully worked out, would prove both interesting and instructive, and might throw some light on a mysterious and deadly disease amongst cattle and sheep, which prevails extensively in Scotland, and in some districts in England. It is a curious fact that Ixodes are almost invariably, if not always found infesting sheep where this disease prevails, and it becomes an important question whether their presence is merely a coincidence, from the rough coarse natural grasses forming a congenial habitat, or whether they are not the carriers or inoculators of vegetable or other poison. I should be very glad to give further information to any one disposed to take up the study. W. E. L.

Ignition by Sunlight

"M." MAY like to have the following case:—I went once at sunrise (at Kishnagar, Bengal) into my coachhouse, which opened east. I saw smoke ascending from the tops of the two carriage lamps. I jumped hastily to the conclusion that my syce (groom) had been using the carriage candles illegitimately, and taxed him. His defence obliged me to examine closer, and to see that the two wicks had been ignited to smouldering point by the horizontal rays of the sun condensed by the parabolic reflectors at the backs of the lamps. A notable enough example of Indian heat, was it not? W. J. HERSCHEL

Collingwood, March 31

WHEN driving along the Beaumaris Road on Tuesday last at half-past three, I observed smoke issuing from the top of one of the carriage lamps. I stopped to examine the cause, and found that the reflector had concentrated the sun's rays on the wick of the candle lamp and caused it to smoulder.

Rhianva, Bangor, April 2 EDMUND H. VERNEY

Mimicry

REFERRING to Mr. Stokoe's letter in NATURE, vol. xxvii., p. 508, and to his remarks on the defective vision of the Teleostei as proved by the very poor imitations of insects which are sufficient to entrap them, have not bats and swallows—animals of certainly more than normal acuteness of vision—been hooked on several occasions by the flyfisher? H. J. MORGAN

Exeter, March 31

Braces or Waistband?

CAN you or any of your readers answer the following:—Which method of suspending the trousers is the least interference with *nature*—their suspension from the *hips* or from the *shoulders*, the wearing of braces, or a tight waistband? R. M.

March 16

SINGING, SPEAKING, AND STAMMERING[1]

II.—SPEAKING

IN the first lecture the musical and emotional side of human utterance; in the second, the colloquial and intellectual aspect of speech was adverted to. Speaking in modern times, and in England especially, is a more neglected art than singing. Even in Shakespeare's days there must have been a state of things not very dissimilar; for he makes Dogberry, who always manages to state the wrong proposition, say, "Readin' and writin' come by nature," and there is a quaintly satirical passage in that graceful and ethereal play, the "Midsummer Night's Dream," which goes straight to the point. Theseus, in commenting on the Clown's blunders of diction, says:—

> "Where I have come, great clerks have purposed
> To greet me with premeditated welcomes;
> Where I have seen them shiver and look pale,
> Make periods in the midst of sentences,
> Throttle their practised accents in their fears,
> And in conclusion dumbly have broke off,
> Not paying me a welcome."

It cannot be too often reiterated that speech is essentially an acquirement, and that it must be learned. At first, indeed, it is picked up by imitation in early childhood, and later on in life is commonly neglected and left to take its chance; though much can be done with little labour to correct defects both of this and of the handwriting, the two first things by which a man's intellectual status is judged of. It is unlike singing, in that pleasant and articulate speaking does not require the gift of a musical organ, but is open to all alike. There exists, however, in some quarters a prejudice against fluent speaking. Ineffableness is held to indicate grasp of thought; taciturnity to be the cloak of profundity. This would be correct if fluency were to supersede accuracy; but such an antagonism is by no means necessary, or it would reach its natural limit in the case of the sailor's parrot, which "could not talk, but thought the more."

Some other hindrances to correct speech require passing comment. In the first place its acquirement is too much mixed up with recitation and dramatic representation. Neither exaggeration nor servile imitation produce good speaking, the one salient feature of which is natural-

[1] Abstract by the Author of three Lectures at the Royal Institution, by W. H. Stone, M.B., F.R.C.P. Continued from p. 510.

ness and spontaneity. Elocutionary teaching has also been hindered by an over-cultivation of poetical rhythm, which tends to reduce speech to a kind of singsong. The same may be said of punctuation, which is not elocutionary but grammatical; though the absurd rule has been formulated to "pause one for the comma, two for the semicolon, three for the colon, and four for the full stop." It is sufficient to test this pedantic error by reading any piece of nervous or pathetic English on the system, and thus to show its full absurdity.

It has been said above that whereas in singing the musical note is predominant, in speaking it is secondary and subsidiary to the words; but it still exists, and its function is well described by Cicero in his treatise, "De Oratore." He says, "Est in dicendo etiam quidam cantus obscurior." An appreciation of this fact is of the greatest value to the public speaker, since the imperfect regulation of the laryngeal element often renders the voice indistinct and even inaudible. Many speakers drop their voices with a descending inflection, and from want of musical ear fail to raise it again: others err from excess of noise, and in their anxiety to be audible, shout and labour, with the result of enveloping the significant sound in an overwhelming mass of heterogeneous and meaningless vibration.

It has several times been attempted to reduce speech to a definite musical notation like that of singing. To a certain extent this was done in the Ecclesiastical Plainsong; but it was carried to its extreme limit in a work of the last century, the "Prosodia Rationalis" of Joshua Steele. It is sufficient to glance at the vague and complicated symbols there employed to realise its practical uselessness.[1] Indeed, so far from being an advance, it is really a step of retrograde character. Mr. Deacon, in "Grove's Dictionary of Music," gives very clearly the four chief differences between song and speech:—1. The isochronism of vibration is never present long enough to make a musical note. 2. Little more than the lower third of the singing voice comes into play in speech. 3. In singing short syllables do not exist. 4. Singing tends to preserve intact purity of language; speaking, to split it up into dialects and idiosyncracies.

A common defect in speaking in large buildings is inability to catch the keynote or resonance vibration of the inclosed space. All large areas have such resonance notes, and in some it is very marked: Westminster Abbey, for instance, consonates to G sharp, and intoning on this note is much more audible than on one a semitone above or below it. Personally the lecturer prefers the use of an open chest-voice as little vocalised as may be. It is less laborious, less liable to accidents, less liable to develop the affection commonly known as "clergyman's throat," and, by removing the sensation of effort, more easy and sympathetic.

He then proceeded to analyse the constituents of a good delivery; and first, pauses. Haste is one of the commonest faults in speech. It has two defects; the one in overtaxing the complex muscular mechanism of the speaker; the other in adding to the intellectual labour of the listener. The former would be considered in the third lecture; the latter needed a few words. The rapidity of reception of ideas through the ear differs materially in different persons, even excluding those distinctly "hard of hearing." It is not great among the uneducated, whence it had been paradoxically said that all illiterate persons are deaf. But they do require a longer time to arouse them to a state of attention than the more cultivated. Naval officers had defended the practice of swearing, or as it was euphemistically termed, "shotting their speech," with sailors; the expletive rousing attention and preparing the mind for the succeeding command. Mr. Hullah had on a similar ground explained the refrains or fal-lal-las of the older music, in that they dilute the too concentrated sense of the words, and give time for the perception of the music.

[1] "King's College Lectures on Elocution," Plumptre p. 112.

When the great actor Salvini was in this country in 1875, the lecturer made some experiments on this point. Salvini's voice was one of the most remarkable ever heard for its power of travelling; even suppressed phrases coming up to the distant gallery with perfect clearness. He spoke on a note about D in the bass, from the chest, and in a sort of recitative; there were distinct periods from accent to accent, and the inflections were very large, running over an interval of more than a fifth. The individual words came about one a second, and the pauses were astonishingly long. They frequently amounted to four, several times to five, and at the two great crises of the play to seven continuous seconds. And yet there was no sense of delay or of interruption, but quite the reverse. The lecturer incidentally noted another thing, which the recent development of Wagner's musical theories had invested with additional interest. In the play "Il Gladiatore," the four principal characters, a young Christian virgin, a Roman matron, the hero a Roman officer, and the gladiator, formed an unintentional though perfect vocal quartett of soprano, contralto, tenor, and bass. At times the alternations of dialogue produced a distinctly musical effect, an observation which to his mind strongly corroborated the views of the great musician lately deceased, that dramatic music, instead of being conventional, should be the outflow of passion and emotion, and that this result could be attained as well from the elocutionary as from the strictly melodic side.

Pronunciation, under which is included respiration as well as vocalisation, was then spoken of, schemes of the vowels and consonants by Dr. Bristowe and Melville Bell being distributed among the audience. The latter being unfamiliar in this country, may be reproduced in this abstract.

GENERAL VOWEL SCHEME. MELVILLE BELL.

Lingual.	Labio-Lingual.	Labial.
1. Eel	Ü (German)	Ooze
2. In	U (French)	O (Provincial)
3. Ale	Û (French)	Old
4. Ill (Scotch)	Zur (Provincial)	Ore
5. Ell	Eu (French)	Awe
6. An	Er Ir (English)	Urge (Scotch)
7. Ask	Er Ir (variety)	Urge
8. ,,	Ah	,,

ARTICULATIONS OR CONSONANTS.

			Oral.		Nasal.
Obstructive. Complete contact.		... ...	P	B	M
			T	D	N
			K	G	Ng
Continuous	Approximation	Firm ...	Ph	Bh	
			Rh	R (smooth)	
			Ch	Gh	
			Wh	W	
			S	Zh	
			Sh	Z	
			Yh	Y	
		Relaxed	KRh	Gr (burr)	
			Rh	R (rough)	
	Partial contact	... ...	F	V	
			Th	Th	
			Ll (Welsh)	L	
			L (Gaelic)		

The aspirate was briefly described as being no fixed articulation, but simply a vowel sound first whispered and then pronounced aloud. Accent has for its object to make one syllable or several more prominent than those around. The English language tends to throw it as far back in a word as is practicable. A long word may have one strong, and one or even two weaker accents in it.

Inflexion is either rising, falling, or a compound of these. As a rule, rising tones appeal, falling tones assert, compound tones suggest; a complete balance of the two is the antithesis, which can be heard in such a remark as "It was not so much what you said—as your manner of saying it, which struck me." The contrasted effect of the two accents may be reproduced by reading this sentence aloud and intelligently.

When inflexion is applied in this way to sentences, three cases occur: the sentence either asserts, asks, or orders, and the nature of the inflexion depends on the relative circumstances of the speaker and listener.

Delivery and modulation are combinations of pausing and of pitch. The conversational pitch being taken as a medium, all below this denotes sadness or solemnity; all above it joy or levity. Force, expression, and sentiment, thus developed, are infinite in their variety.

Emphasis can only be attained and regulated by a full perception of the point to be brought out; as a rule it marks the predicate of a logical expression. False emphasis is the foundation of many quaint stories in common currency. Speaking generally, new, contrasted, or antithetical ideas are marked by emphasis.

In conclusion, the lecturer gave three general rules by which any one can speak. The first, in the words of Horace: "Dicendi rectè principium est sapere, et fons;" that is, "Know exactly what you are going to say." The second, "Endeavour to forget yourself." This frame of mind had been formulated by old elocutionists as "Have a contempt for your audience." He preferred to state it in a less obnoxious way as "Consider yourself one of your audience." The third, "Be natural and unaffected."

By bearing in mind these simple injunctions any man free of congenital or acquired defects, though he might not be a brilliant, could hardly fail in being an agreeable and sympathetic speaker.

PROFESSOR SCHIAPARELLI ON THE GREAT COMET OF 1882

READERS of NATURE will be glad to have a full report of the interesting popular lecture which Prof. Schiaparelli, the well-known Italian astronomer, gave in Milan on February 4, on the great comet of 1882. Referring to the national misfortune which had given origin to his and other lectures, he began by showing that while a connection between the comet and the inundations which wasted, in October, 1882, many Venetian provinces, was not absolutely impossible, it was at least very improbable, both because the comet was yet a great distance from the earth when the floods rose, and from the difficulty of understanding why the supposed influence of the comet should have acted only on that little part of the globe. After this preamble M. Schiaparelli gave the public a rapid and elementary account of our planetary system, and of the comet's trajectory during its passage near the sun and planets. The orbit of the comet, in the position which could be subjected to astronomical measurement, is parabolic, in a plane inclined 30° or 40° to planes of the solar system. The greater portion of the orbit is in the southern regions; for in the austral hemisphere the comet was sooner and better observed than in the boreal, where it never was very high above the horizon. The vertex of the parabola is very near the sun, and only when the comet was approaching to this position with an extraordinary rapidity, astronomers could perceive it,—at Auckland (September 2), at the Cape of Good Hope, in Australia, the Argentine Republic, and Brazil. The direction of its movement was perhaps towards the sun; but the inconceivable rate which the comet acquired in its falling towards the sun (480 km. in a second, sixteen times the mean velocity of the earth in its orbit), and the lateral rush coming from it, were enough at that time to overcome the attractive power of the sun, and to hinder the great luminary from swallowing it. The attraction of the sun failed not to produce its effect, slackening successively its flight; but being animated by this great velocity, the comet could escape in security to where the sun's action is very feeble, and whence it will not return for many years.

The Cape astronomer had the opportunity of witnessing this rare spectacle of a heavenly body which, rushing headlong from extraplanetary depths, went directly on the sun, as if it would fall in, and notwithstanding, in a few hours delivered itself, changing completely its direction of motion. At that time the earth was placed very obliquely in respect to the arc described by the comet about the sun, so that astronomers observed it with a great foreshortening of perspective. In those hours the comet, being exposed to an extraordinary heat, swelled and became so luminous, that the Cape astronomers, and afterwards some in Europe, could see it near the sun. They could make the absolutely new observation of a comet's transit before the solar disk, thus satisfying an ancient desire of astronomers, who have wished to know if in those bodies' head, which often appears as a very bright star, is hidden an obscure perceptible nucleus, and to judge of the density of the shining atmosphere whose splendour produces the star's appearance. In this case it was not possible to be deceived by an illusion, as happened in 1819. Messrs. Finlay and Elkin, at the Cape, saw the comet gradually approach the sun's limb, touch it, and disappear; so that their searches to find the comet in the place where it obviously was were vain. The comet then was so thin and clear, that the most slender cloud would more obscure the sun: its solid nucleus (if it had a nucleus, as was very likely) was so small that the observer's telescope could not perceive any spot or shade. After it left the neighbourhood of the sun, the effects of the enormous heat began to appear in the development of that splendid tail, which everybody could see in the morning hours of October and November.

The orbit of the comet (continued the Professor) is not easily deducible from the very little portion which we know. Both because to assign a trajectory observed in a small branch is very difficult, and sometimes impossible, and because exact and definitive calculations will not be undertaken before the vanishing of the comet; the notes which at present can be given are only approximative. On observations of last September, October, and November, it was stated that the period of the comet is included between eight and nine centuries, and the aphelion is nearly six times farther than Neptune from the sun (175 times the earth's mean vector radius), the rate of velocity in aphelion and perihelion being as 1 to 23,000.

On the brightness of the comet, M. Schiaparelli observed that it could be attributed to three causes: the strong illumination of the sun, its own light, and electrical discharges, which take place continually in similar bodies, in the opinion of expert physicists. Those causes united to make that very splendid appearance of a matter clearer and less dense than the rarefied air of our best pneumatic engines. The density of the tail was so small that an astronomer estimated it at no more than a few kilogrammes, while its dimensions were larger than were ever before observed in comets. It is true that other comets (that of 1861, for example) showed an apparently longer tail, their position in respect to the earth being more favourable to observation; but in the annals of astronomy we have never found a comet's tail really as long as this.

I leave out the detailed description of the comet's aspect, because NATURE has given full accounts and sketches, and I come to the most interesting part of M. Schiaparelli's lecture, on the production of those magnificent phenomena. I translate literally from Signor Schiaparelli's manuscript.

The proper nucleus of the comet is a solid or liquid body so small as rarely to be seen : in the greater number of comets, as in this, it seems to be not large enough to be visible even in powerful telescopes. It seems also that in some comets there are several nuclei, very small and close, whose particular atmospheres in their development at last unite in one. As long as such a body (or system of bodies) remains far from the sun, in extraplanetary regions, where temperature is less than – 140° C. (according to the most moderate estimates), and where the sun has perhaps no power to heat it, the matter must be wholly solid or at least liquid ; and, if a small quantity is gaseous or vaporous, it must have a great density and a small volume. The progressive approach to the sun by its descending orbit will obviously swell the enveloping atmosphere, or give rise to one if it does not yet exist, with materials generated by the surface. Shortly the nucleus begins to appear surrounded by a blaze of light, feeble at first, but afterwards more and more brilliant, which is the star or head of the comet. Many comets do not go beyond this first phase, both because they have not matter enough to make an atmosphere, and because they do not come near enough to the sun to be subject to a great heat. Some comets do not enter the earth's orbit, others cannot reach that of Mars, and we know that the comet of 1729 got only a little way into the orbit of Jupiter. The most part of those comets, being exposed very moderately to the solar influence, cannot increase, and remain telescopic ; and it is very probable that a large number stop at Jupiter or Saturn's orbit (or even further) in their descent upon the sun : none of those are seen, and we can speak of them only by conjecture.

When a comet, however, as the present, pierces through the interior part of the planetary system, it is in the best condition to develop its atmosphere if it contain matter enough to do so. But the sun, while attracting to himself the nucleus, has the property to repel some of the matter of the atmosphere. It is not well known how and why this matter is repelled, and to expound the various hypotheses on this point would take too long a time. The effect of repulsion is nevertheless undoubted, and manifested by the fact that those parts of the cometary atmosphere, under the sun's impulse, almost as if under a gale blowing from it, leave the nucleus and fly in an opposite direction away from the sun, producing the tail, which, nourished successively by incessant evaporations of the nucleus, more and more increases in length, till the atmosphere of the nucleus, wholly repelled, overflows into the tail, and thus exhausts itself. This happens usually when the nucleus, after the perihelion, receding from the sun and being then exposed again to the cosmical cold, is no longer able to supply with new evaporations the part of the atmosphere which the tail absorbs. Deprived thus of its former envelopes, and unable to engender others, the nucleus is reduced again to itself, and the comet disappears.

The tail of the comet consists then of matters repelled by the sun with a mysterious power. But, during the above described period of conflagration, other interesting events occur in the comet. It is so much swollen and convulsed by solar heat that the little nucleus is not able sometimes to keep together the fragments by its own very feeble attraction. Violent eruptions take place at the surface, so that pieces of nucleus are raised and thrown out of the principal body's attraction. Those fragments then pass through the heavens as independent bodies, and their orbits are not very different from the orbit of the nucleus. Sometimes one of the broken pieces is great enough to engender another separate comet : that is, the several times observed phenomenon of a divided comet. But most generally it seems that separated pieces are very small and numerous, like the sparks of a piece of salt thrown on the fire ; and extend along the trajectory of the nucleus like a current or projection of corpuscles, which gradually invade all the orbit of the comet. Many comets (probably all) engender in their course a similar retinue ; and the planetary intervals are peopled by those corpuscles produced by a comet's partial disintegration. When the earth in its yearly revolution passes through one of these processions it meets with several pieces, which get inflamed by contact with the terrestrial atmosphere, and burn in a very short time, producing a falling star.

An example of such a process of separation was given by the present comet. In effect, a little before October 15 M. Schmidt, the astronomer at Athens, observed an irregular and very feebly shining thin cloud leaving the comet, withdrawing, and finally disappearing. It was more dense and luminous in some places than in others, but it looked not like a comet, having rather the aspect of a mass of corpuscles exploded by the principal nucleus. The atmosphere also enveloping the principal nucleus offered analogous phenomena, being not round and symmetrical, but lengthened spindle-fashion, with several more luminous centres of different intensity spread in an oval cloud. We have, besides, reason to believe that another little comet, which was observed in the beginning of 1880 in the austral regions of the earth, running in an orbit very similar, was previously separated from our great comet.

M. Schiaparelli passed afterwards to another question, on the chemical constitution of comets, explaining the principle of spectrum analysis and its application to celestial chemistry. He remarked that the present and Wells's comet only, by their coming so near the sun, could present the lines of sodium, whilst all the comets before observed gave only lines of hydrocarbons in the spectroscope ; and it is very probable, according to modern theories, that comets contain also some matters which are made apparent in falling stars and in aërolites, as iron, nickel, silicium, magnesium, aluminium, and others. This confirms the induction as to the similarity of their chemical composition to that of the earth ; and the common origin of comets in the planetary system is evidently proved by their accompanying the sun in its progressive movement towards the constellation of Hercules. It seems that comets belonging to the solar system would have the function of continually dissipating matter in space, as a compensation to the attractive power of the greatest centre, the sun.

Pressure of space obliges me to leave out the very eloquent conclusion of this lecture, in which the lecturer refuted the apprehensions as to the shock of a comet with the earth, and its probable consequences, discussing the great moral importance of these studies as an antidote to the fears and superstitions of ignorant people. Referring to Anaxagoras and Galileo, he concluded with these words: "A science which suffered such noble condemnations, and is able to awake such noble hopes, cannot be considered as futile and idle ; it will always be dear to the friends of truth ; dear to every one who is convinced that man lives not by bread alone." FRANCIS PORRO

R. Observatory of Brera in Milan

THE SOARING OF BIRDS

THE recent correspondence in NATURE upon this subject ought not to close without some reference to a possible explanation of *soaring* which does not appear to have been yet suggested.

I premise that if we know anything about mechanics it is certain that a bird *without working his wings* cannot, either in still air or in a uniform horizontal wind, maintain his level indefinitely. For a short time such maintenance is possible at the expense of an initial relative velocity, but this must soon be exhausted. Whenever therefore a bird pursues his course for some time without working his wings, we must conclude either (1) that the course is not horizontal, (2) that the wind is not horizontal, or (3) that the wind is not uniform. It is probable that the truth is usually represented by (1) or (2); but the question I wish to raise is whether the cause suggested by (3) may not sometimes come into operation.

In NATURE, vol. xxiii. p. 10, Mr. S. E. Peal makes very distinct statements as to the soaring of pelicans and other large birds in Assam. The course is in large and nearly circular sweeps, and at each lap some 10 or 20 feet of elevation is gained. *When there is a wind*, the birds may in this way "without once flapping the wings" rise from a height of 200 to a height of 8000 feet.

That birds do not soar when there is no wind is what we might suppose, but it is not evident how the existence of a wind helps the matter. If the wind were horizontal and uniform it certainly could not do so. As it does not seem probable that at a moderate distance from the ground there could be a sufficient vertical motion of the air to maintain the birds, we are led to inquire whether anything can be made of the difference of horizontal velocities which we know to exist at different levels.

In a uniform wind the available energy at the disposal of the bird depends upon his velocity *relatively* to the air about him. With only a moderate waste this energy can at any moment be applied to gain elevation, the gain of elevation being proportional to the loss of relative velocity squared. It will be convenient for the moment to ignore the waste referred to, and to suppose that the whole energy available remains constant, so that however the bird may ascend or descend, the relative velocity is that due to a fall from a certain level to the actual position, the certain level being of course that to which the bird might just rise by the complete sacrifice of relative velocity.

For distinctness of conception let us now suppose that above and below a certain plane there is a uniform horizontal wind, but that in ascending through this plane the velocity increases, and let us consider how a bird sailing somewhat above the plane of separation, and endowed with an initial relative velocity, might take advantage of the position in which he finds himself.

The first step is, if necessary, to turn round until the relative motion is to leeward, and then to drop gradually down through the plane of separation. In falling down to the level of the plane there is a gain of relative velocity, but this is of no significance for the present purpose, as it is purchased by the loss of elevation; but in passing through the plane there is a really effective gain. In entering the lower stratum the actual velocity is indeed unaltered, but the velocity relatively to the surrounding air is *increased*. The bird must now wheel round in the lower stratum until the direction of motion is to windward, and then return to the upper stratum, in entering which there is a second increment of relative velocity. This process may evidently be repeated indefinitely; and if the successive increments of relative velocity squared are large enough to outweigh the inevitable waste which is in progress all the while, the bird may maintain his level, and even increase his available energy, without doing a stroke of work.

In nature there is of course no such abrupt transition as we have just now supposed, but there is usually a continuous increase of velocity with height. If this be sufficient, the bird may still take advantage of it to maintain or improve his position without doing work, on the principle that has been explained. For this purpose it is only necessary for him to descend while moving to leeward, and to ascend while moving to windward, the simplest mode of doing which is to describe circles on a plane which inclines downwards to leeward. If in a complete lap the advantage thus obtained compensates the waste, the mean level will be maintained without expenditure of work; if there be a margin, there will be an outstanding gain of level susceptible of indefinite repetition.

A priori, I should not have supposed the variation of velocity with height to be adequate for the purpose; but if the facts are correct, some explanation is badly wanted. Mr. Peal makes no mention of the circular sweeps being inclined to the horizon, a feature which is essential to the view suggested. It is just possible, however, that the point might escape attention not specially directed to it.

However the feat may be accomplished, if it be true that large birds can maintain and improve their levels without doing work, the prospect for human flight becomes less discouraging. Experimenters upon this subject would do well to limit their efforts for the present to the problem of gliding or sailing through the air. When a man can launch himself from an elevation and glide long distances before reaching the ground, an important step will have been gained, and until this can be done, it is very improbable that any attempt to maintain the level by expenditure of work can be successful. Large birds cannot maintain their levels in still air without a rapid horizontal motion, and it is easy to show that the utmost muscular work of a man is utterly inadequate with any possible wings to allow of his maintenance in a fixed position relatively to surrounding air. With a rapid horizontal motion, the thing may perhaps be possible, but for further information bearing upon this subject, I must refer to a paper on the resistance of fluids published in the *Philosophical Magazine* for December, 1876.

March 22 RAYLEIGH

PHILIP CHRISTOPH ZELLER

ENTOMOLOGY has just sustained an irreparable loss by the death of Prof. Zeller, which took place at Grünhof, near Stettin, on March 27, suddenly, from heart disease. Zeller was born on April 9, 1808, at Steinheim, in Württemburg. For many years he was attached to official educational establishments in Germany, especially at Glogau in Silesia, and Meseritz in Posen. While at the former place the honorary title of Professor was bestowed upon him by the Government on account of his eminent scientific researches, and some time afterwards he retired from official duties, and settled near Stettin, where much of his leisure was devoted to the Entomological Society that has its headquarters in that town, of which he was acting secretary, and of which Dr. C. A. Dohrn is president. Zeller's fame as an entomologist is more especially based upon his publications on *Lepidoptera*, more particularly of Europe, and chiefly on the smaller moths. His first recorded paper appeared in Oken's *Isis* for 1838, and consisted of a critical determination of the *Lepidoptera* in Réaumur's "Memoirs," a prize essay, in which the author took first place. From that time a continuous stream of valuable papers by him appeared, and on the day of his death he was engaged in scientific work. It is utterly impossible to give here even the titles of his more important works. It is with regret that we are obliged to admit that the title of "entomologist" does not always enable us to take for granted that the entomologist is also a naturalist. Zeller was both, in the fullest acceptation of the terms. While his purely descriptive work is of the highest character, his investigations into the natural history of his subject were persistent, and he never ceased to deprecate the "slop-work" so painfully evident in the writings of some entomologists. For many years he made almost annual excursions in pursuit of his favourite science, especially in the Alps of Central

Europe, and so long ago as 1844 a more extended tour in South Italy and Sicily. In this country he was so well known that British entomologists will feel that in his death they have lost one of themselves; it is nearly thirty-five years since he was elected an Honorary Member of the Entomological Society of London, and he was one of the editors of Mr. Stainton's magnificent "Natural History of the Tineina." There are those amongst us in this country who in Zeller's death have lost one of their dearest friends. Scientific entomology has lost one of its most shining lights. R. McLACHLAN

THE GREAT INTERNATIONAL FISHERIES EXHIBITION

HER MAJESTY THE QUEEN has recently appointed the 12th of May for the opening of the International Fisheries Exhibition, which an influential and energetic committee, under the active presidency of the Prince of Wales, has developed to a magnitude undreamt of by those concerned in its early beginnings. This magnitude is perhaps as great a matter of agreeable surprise to Mr. Birkbeck and its other Norwich founders as it will be to those who have very naturally become accustomed to class all specific exhibitions together upon a standard formed by the unfortunate annual exhibitions of which the public has, not without reason, grown weary.

The idea of an *international* Fisheries Exhibition arose out of the success of the show of British fishery held at Norwich a short time ago; and the president and executive of the latter formed the nucleus of the far more powerful body by whom the present enterprise has been brought about.

The buildings are well advanced towards completion, and will be finished long before the opening day; the exhibitors will, it is hoped, support the executive by sending in their goods in time, and thus all will be ready for the 12th proximo.

The plan of the buildings embraces the whole of the twenty-two acres of the Horticultural Gardens: the upper half, left in its usual state of cultivation, will form a pleasant lounge and resting-place for visitors in the intervals of their study of the collections. This element of garden accommodation was one of the most attractive features at the Paris Exhibition of 1878.

As the plan of the buildings is straggling and extended, and widely separates the classes, the most convenient mode of seeing the show will probably be found in going through the surrounding buildings first, and then taking the annexes as they occur.

On entering the main doors in the Exhibition Road, we pass through the Vestibule to the Council Room of the Royal Horticultural Society, which has been decorated for the reception of marine paintings, river subjects, and fish pictures of all sorts, by modern artists.

Leaving the Fine Arts behind, the principal building of the Exhibition is before us—that devoted to the deep sea fisheries of Great Britain. It is a handsome wooden structure 750 ft. in length, 50 ft. wide, and 30 ft. at its greatest height. The model of this, as well as of the other temporary wooden buildings, is the same as that of the annexes of the great Exhibition of 1862.

On our left are the Dining Rooms with the Kitchens in the rear. The third room, set apart for cheap fish dinners (one of the features of the Exhibition), is to be decorated at the expense of the Baroness Burdett Coutts, and its walls are to be hung with pictures lent by the Fishmongers' Company, who have also furnished the requisite chairs and tables, and have made arrangements for a daily supply of cheap fish, while almost everything necessary to its maintenance (forks, spoons, table-linen, &c.), will be lent by various firms.

The apsidal building attached is to be devoted to lectures on the cooking of fish.

Having crossed the British Section, and turning to the right and passing by another entrance, we come upon what will be to all one of the most interesting features of the Exhibition, and to the scientific student of ichthyology a collection of paramount importance. We allude to the Western Arcade, in which are placed the Aquaria, which have in their construction given rise to more thoughtful care and deliberation than any other part of the works. On the right, in the bays, are the twenty large asphalt tanks, about 12 ft. long, 3 ft. wide, and 3 ft. deep. These are the largest dimensions that the space at command will allow, but it is feared by some that they will be found somewhat confined for fast going fish. Along the wall on the left are ranged twenty smaller or table-tanks of slate, which vary somewhat in size; the ten largest are about 5 ft. 8 in. long, 2 ft. 9 in. wide, and 1 ft. 9 in. deep.

In this Western Arcade will be found all the new inventions in fish culture—models of hatching, breeding and rearing establishments, apparatus for the transporting of fish, ova, models, and drawings of fish-passes and ladders, and representations of the development and growth of fish. The chief exhibitors are specialists, and are already well known to our readers. Sir James Gibson Maitland has taken an active part in the arrangement of this branch, and is himself one of the principal contributors.

In the north of the Arcade where it curves towards the Conservatory, will be shown an enormous collection of examples of stuffed fish, contributed by many of the prominent angling societies. In front of these on the counter will be ranged microscopic preparations of parasites, &c., and a stand from the Norwich Exhibition of a fauna of fish and fish-eating birds.

Passing behind the Conservatory and down the Eastern Arcade—in which will be arranged Algæ, Sponges, Mollusca, Star-fish, worms used for bait, insects which destroy spawn or which serve as food for fish, &c.—on turning to the left, we find ourselves in the Fish Market, which will probably vie with the Aquaria on the other side in attracting popular attention. This model Billingsgate is to be divided into two parts, the one for the sale of fresh, the other of dried and cured fish.

Next in order come the two long iron sheds appropriated respectively to Life-boats and Machinery in motion. Then past the Royal Pavilion (the idea of which was doubtless taken from its prototype at the Paris Exhibition) to the southern end of the central block, which is shared by the Netherlands and Newfoundland; just to the north of the former Belgium has a place.

While the Committee of the Netherlands was one of the earliest formed, Belgium only came in at the eleventh hour; she will, however, owing to the zealous activity of Mr. Lenders, the Consul in London, send an important contribution worthy of her interest in the North Sea fisheries. We ought also to mention that Newfoundland is among those colonies which have shown great energy, and she may be expected to send a large collection.

Passing northward we come to Sweden and Norway, with Chili between them. These two countries were, like the Netherlands, early in preparing to participate in the Exhibition. Each has had its own Committee, which has been working hard since early in 1882.

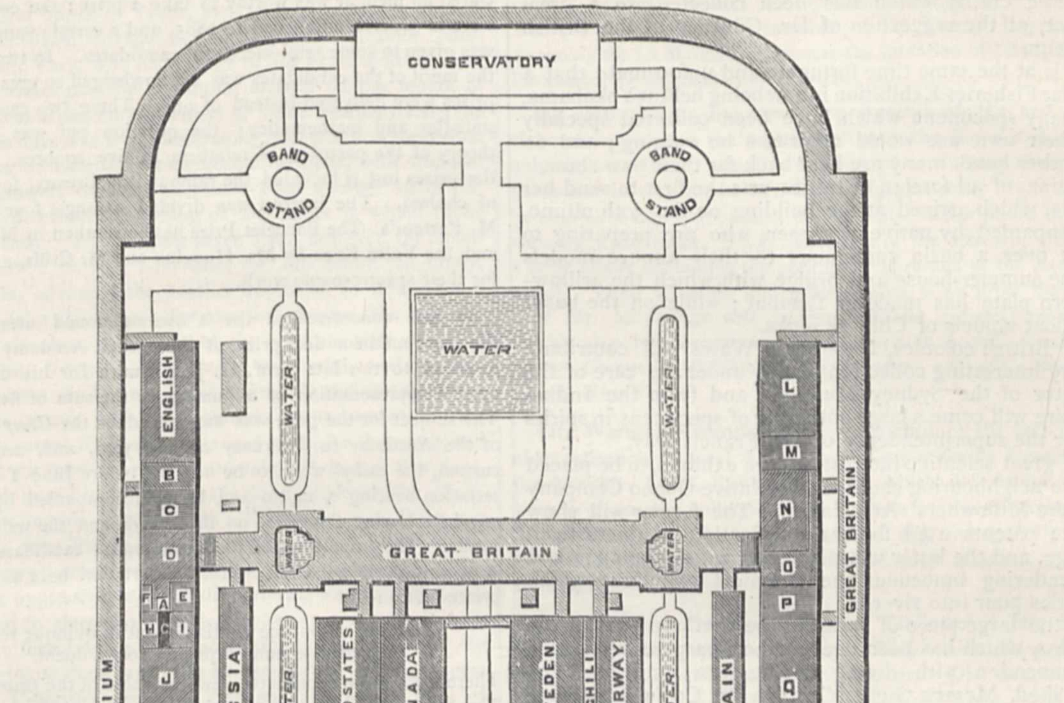
Parallel to the Scandinavian section is that devoted to Canada and the United States. While the American Government has freighted a ship with specimens expected daily, the former has entered heart and soul into the friendly rivalry, and will occupy an equal space—ten thousand square feet.

In the Northern Transept will be placed the inland fisheries of the United Kingdom. At each end of the building is aptly inclosed a basin formerly standing in the gardens: and over the eastern one will be erected the daïs from which the Queen will formally declare the Exhibition open.

Shooting out at right angles are the Spanish annexe, and the building shared by India and Ceylon, China and Japan, and New South Wales: while corresponding to these at the western end are the Russian annexe and a shed allotted to several countries and colonies. The Isle of Man, the Bahamas, Switzerland, Germany, Hawaii, Italy, and Greece—all find their space under its roof.

After all the buildings were planned, the Governments of Russia and Spain declared their intention of participating; and accordingly for each of these countries a commodious iron building has been specially erected.

The Spanish collection will be of peculiar interest; it



Block Plan.—A. Switzerland; B. Isle of Man; C. Bahama and W. I. Islands; D. Hawaii; E. Poland; F. Portugal; G. Austria; H. Germany; I. France; J. Italy; K. Greece; L. China; M. India and Ceylon; N. Straits Settlements; O. Japan; P. Tasmania; Q. New South Wales.—Scale, 200 feet to the inch.

has been gathered together by a Government vessel ordered round the coast for the purpose, and taking up contributions at all the seaports as it passed.

Of the countries whose Governments for inscrutable reasons of state show disfavour and lack of sympathy, Germany is prominent; although by the active initiative of the London Committee some important contributions have been secured from private individuals: among them, we are happy to say, is Mr. Max von dem Borne, who will send his celebrated incubators, which the English Committee have arranged to exhibit in operation at their own expense.

Although the Italian Government, like that of Germany, holds aloof, individuals, especially Dr. Dohrn of the

Naples Zoological Station, will send contributions of great scientific value.

France, the other day only, consented to the official appointment of her Consul to look after the interests of the oyster cultivators who are contributing an important feature.

In the Chinese and Japanese annexe, on the east, will be seen a large collection of specimens (including the gigantic crabs) which has been collected, to a great extent, at the suggestion of Dr. Günther of the British Museum.

It is at the same time fortunate and unfortunate that a similar Fisheries Exhibition is now being held at Yokohama, as many specimens which have been collected specially for their own use would otherwise be wanting; and on the other hand, many are held back for their own show.

China, of all foreign countries, was the first to send her goods, which arrived at the building on the 30th ultimo, accompanied by native workmen, who are preparing to erect over a basin contiguous to their annexe models of the summer-house and bridge with which the willow-pattern plate has made us familiar; while on the basin will float models of Chinese junks.

Of British colonies, New South Wales will contribute a very interesting collection placed under the care of the Curator of the Sydney Museum; and from the Indian Empire will come a large gathering of specimens in spirits under the superintendence of Dr. Francis Day.

Of great scientific interest are the exhibits, to be placed in two neighbouring sheds, of the Native Guano Company and the Millowners' Association. The former will show all the patents used for the purification of rivers from sewage, and the latter will display in action their method of rendering innocuous the chemical pollutions which factories pour into rivers.

In the large piece of water in the northern part of the gardens, which has been deepened on purpose, apparatus in connection with diving will be seen; and hard by, in a shed, Messrs. Siebe, Gorman & Co. will show a selection of beautiful minute shells dredged from the bottom of the Mediterranean.

In the open basins in the gardens will be seen beavers, seals, sea-lions, waders, and other aquatic birds.

From this preliminary walk round enough has, we think, been seen to show that the Great International Fisheries Exhibition will prove of interest alike to the ordinary visitor, to those anxious for the well-being of fishermen, to fishermen themselves of every degree, and to the scientific student of ichthyology in all its branches.

The economic question of the undertaking we have left untouched.

NOTES

It will be seen from a communication in another column that the Council of the British Association have virtually decided that that body is bound to hold its meeting in Canada in 1884. From Sir A. T. Galt's letter it is evident that our Canadian fellow-subjects have already arranged to give the Association a hearty and generous welcome; and now that Canada seems inevitable, we hope that as many members as possible will make up their minds to be present. The expenses for visitors will be reduced to a minimum, and the travelling expenses of officials, to the number of fifty, to *nil*. A magnificent programme for three weeks' excursions has been sketched, and the expenses connected with them will be confined to hotel charges, carriages, &c., the railway companies having handsomely offered to convey members free of charge.

The Academy of Sciences held its Annual Meeting on April 2, M. Jamin in the chair. He pronounced the *éloge* of the three Academicians who died last year, viz., MM. Liouville, Bussy, and Decaisne. M. Blanchard, filling the room of M. Dumas, who, although present, was unable to deliver any speech, read the list of laureates. The number of prizes offered for public competition is yearly enlarging; not less than three of them—Monti, Machedo, and Francœur—were delivered for the first time. The number of verdicts which the commission had to render was thirty-three. In nine cases the commission declared no memoir was worthy to take a prize; the competitions were in general adjourned to 1885, and a certain sum of money was given to some semi-successful candidates. In two instances the merit of the candidates was acknowledged so great that two prizes were delivered instead of one. These two cases were in statistics and mathematics; the question put was to give a theory of the partition of numbers in five squares. Amongst the prizes lost is included the famous Prix Breaux, for the cure of cholera. The interest was divided amongst four pupils of M. Pasteur's. The Poncelet Prize has been taken by M. Clausius, and the Voltz Prize by Mr. Huggins and M. Crüls, a Brazilian, for their spectroscopic work.

It was announced at the above-mentioned meeting that the great mathematical prize of the French Academy had been awarded to the late Prof. H. J. S. Smith for his dissertation on the representation of a number as the sum of five squares. The subject for the prize was announced in the *Comptes Rendus* of the Academy in February of last year, and, according to custom, the essays were to be sent in before June 1—each dissertation bearing a motto and being accompanied by a sealed envelope having the motto on the outside and the writer's name inside. The envelopes of the unsuccessful candidates are destroyed unopened. Prof. Smith's dissertation bore as its appropriate motto:—

> "Quotque, quibusque modis possint in quinque resolvi
> Quadratos numeri pagina nostra docet."

There were three candidates, and the value of the prize is 3000f. The theory of numbers, to which the prize subject related, is one to which Prof. Smith had devoted the greater part of his life, and in which he occupied an almost unique position; with the exception of Prof. Kummer of Berlin, there is no one whose contributions to the science could be compared to his, and this posthumous mark of the appreciation on the Continent of the value of his work is all the more satisfactory as the great prize has never before, we believe, been awarded to an English mathematician. The complete solution of the important problem proposed by the French Academy had been obtained by Prof. Smith sixteen years ago as part of a far more general investigation, and the results were published by him in the *Proceedings of the Royal Society* in 1868, but without demonstration. These researches seem, however, to have escaped the notice of the French mathematicians. When the subject of the prize was announced last year, Prof. Smith extracted from his manuscript books the demonstrations of the propositions relating to the five-square problem, and it is to the dissertation so formed that the prize has been awarded. No more striking instance of the extent to which Prof. Smith had carried his researches, or of his great mathematical genius, could be given than is afforded by the fact that a question considered by the French Academicians of so much importance to the advancement of mathematical science as to be chosen for the subject of the "Grand Prix" should have been completely solved by him as only a particular case in the treatment of a general and even more intricate problem. In 1868 Prof. Smith won the Steiner Prize of the Berlin Academy, so that had he but lived till now he would have been "laureate" of the Academies of both Paris and Berlin.

The removal of the natural history collection from Great Russell Street to its new quarters at South Kensington, on the site of the Great Exhibition of 1862, has been proceeding gradually during the last two years, and is now rapidly approaching completion. Several of the rooms, formerly stocked with birds, fishes, &c., have been already emptied.

LIEUT. SAMUEL W. VERY, U.S.N., and Dr. Orlando B. Wheeler, the two principal members of the expedition sent by the United States Government to Santa Cruz, Patagonia, to observe the recent transit of Venus, arrived in Liverpool on Friday, by the Pacific Steam Navigation Company's mail steamer *Patagonia*. Lieut. Very, who had charge of the expedition as chief astronomer, states that the expedition arrived off the mouth of the Santa Cruz River on November 2. The weather during the first fourteen days was very encouraging, but this was succeeded by nine days of overcast, disagreeable weather, with frequent and sharp showers of hail and rain. Fine weather again followed until the eventful morning of December 6, which broke cloudy and hazy. By half-past seven a.m., however, the clouds began to weaken, half an hour later the sun shone out dimly, and as the day advanced the weather improved, so that when it was time to take up stations for the first contact, the sun was almost entirely clear. All four of the contacts were observed both by Lieut. Very, with the large equatorial, and by Mr. Wheeler, with a smaller one; and during the transit 224 photographs were taken, with a continuous improvement in the results. By sunset the weather changed again for the worse, and the sun was not seen, except at intervals, for four or five days, during which time Lieut. Very was looking anxiously for observations for rating his chronometers. While the expedition was in camp the temperature changed to the extent of 19° in the course of every twelve hours. In the daytime the heat occasionally was oppressive, while at night the air was very cold, and the party had to sleep with double blankets and heavy clothing upon them. The Lieutenant speaks in the highest terms of the kindness and consideration shown to him by the Pacific Steam Navigation Company and the Customs authorities, both of whom, when they were informed of his business, put all possible facilities in his power.

THE next ordinary General Meeting of the Institution of Mechanical Engineers will be held on Wednesday, April 11, and Thursday, April 12, at 25, Great George Street, Westminster. The chair will be taken by the president, Percy G. B. Westmacott, at three o'clock on Wednesday afternoon, and at ten o'clock on Thursday morning. The following papers will be read and discussed:—On the strength of shafting when exposed both to torsion and end thrust, by Prof. A. G. Greenhill, of Woolwich; On modern methods of cutting metals, by Mr. W. Ford Smith, of Salford; On improvements in the manufacture of coke, by Mr. John Jameson, of Newcastle-on-Tyne; On the application of electricity to coal mines, by Mr. Alan C. Bagot, of London.

THE 21st meeting of the delegates of the French Learned Societies took place last week at the Sorbonne. M. Ferry, the French Premier, presided over the final meeting on March 31, and delivered, as is customary, an address. The Minister dwelt much on the circumstance that he had added to the four sections in existence a fifth, devoted to political economy, so that the meeting of the Learned Societies included every subject in human knowledge. He praised the Trustees of the British Museum for their fair dealing towards France in the matter of the Ashburnham manuscripts, and eulogised the French Government for their zeal in the promotion of knowledge, declaring that 60 millions of francs had been already spent for the rebuilding of French universities, and that 40 millions were to be spent shortly for the same purpose. The presidents of the several sections omitted to deliver their reports, and the proceedings terminated somewhat abruptly. The address was well received, but the unexpected silence of the presidents has taken the public by surprise, and has been unexplained as yet.

M. HERVÉ MANGON, President of the Bureau Central of French Meteorology, opened the Session of the Congress of Meteorologists on March 29 by reading a report on the working of the institution. This document states that, from a comparison made by the Bureau, its forecasts have been acknowledged good 83 times in each 100; that for the warning of tempests 207 had been sent to the seaports, out of which 100 had been fulfilled entirely, 65 partly, and 42 had not been warranted by the event. The president, who is a member of the French Legislative Assembly for La Manche, announces the intention of asking from Parliament an augmentation of credit.

MR. MUYBRIDGE has issued a prospectus of "a new and elaborate work upon the attitudes of man, the horse, and other animals in motion." As the expense of conducting these experiments is very great, Mr. Muybridge naturally waits until he obtains a sufficient number of 100-dollar subscriptions before entering upon them. Each subscriber of the sum will receive a large album containing the photographic results of the experiments. Their scientific and artistic value is so great that we trust Mr. Muybridge will receive sufficient encouragement to put his plan into execution. His address is Scovill Manufacturing Company, Publishing Department, 419–421, Broome Street, New York.

THE Warwick Museum has been enriched by the very valuable collection of local Liassic and Keuper fossils formed by the late Mr. J. W. Kirshaw, F.G.S., which it is intended to keep as a separate collection. The whole of the collection in the Museum has lately been classified and arranged by Mr. R. Bullen Newton, of the Natural History Museum, South Kensington.

HARTLEBEN'S "Elektrotechnische Bibliothek" has been further augmented by three volumes. They consist of two little books by Dr. Alfred von Urbanitzky, viz. "Die elektrischen Beleuchtungs Anlagen" and "Das elektrische Licht," and one by Herr W. P. Hauck, "Die galvanischen Batterien, Accumulatoren, und Thermosäulen."

ACCORDING to latest accounts, the eruption of Mount Etna is resuming activity. Enormous quantities of gas are thrown out, and slight shocks are again felt in the neighbourhood of Nicolosi.

THE second number of *Timehri*, the journal of the British Guiana Agricultural and Commercial Society is to hand; it completes the first volume. Among the contents we note the following:—Forest Conservancy in British Guiana, by M. McTurk, G. M. Pearce, and the Hon. W. Russell; Mount Russell in Guiana, by the Editor, Mr. Im Thurn; The Aspect and Flora of the Kaieteur Savannah, by G. S. Jenman; Notes on West Indian Stone Implements, by the Editor, with several coloured illustrations; British Guiana Cave-Soils and Artificial Manures, by E. E. H. Francis. There are also several interesting notes, and the reports of the Society's meetings. Among the notes is a letter from Dr. R. Schomburgk, of Adelaide, giving some interesting autobiographical details. Stanford is the London agent.

WE have received the first number of the new American monthly, *Science*, to which we heartily wish all success.

WHILE Western Europe enjoyed a mild autumn, very severe weather was experienced on the Ural. At Ekaterinburg the average temperature of October was four degrees lower than the average for forty five years, that is, $-3^{\circ}\cdot9$, instead of $+0^{\circ}\cdot9$, the lowest temperatures in October witnessed since 1836 having been but $-2^{\circ}\cdot4$ and $-3^{\circ}\cdot2$. For nineteen days the thermometer did not rise above zero, and it fell as low as $-19^{\circ}\cdot2$ and $-17^{\circ}\cdot9$.

ENTOMOLOGISTS generally, as well as those more particularly interested from their geographical position, will be pleased to learn that the long-expected Yorkshire List of Lepidoptera—on which Mr. Geo. T. Porritt, F.L.S., of Huddersfield, has for some time past been engaged—is now completed, and that the MS. is now being set up for the *Transactions of the Yorkshire Naturalists' Union*. Mr. Porritt, who has been assisted by the leading entomologists of the county, and who has also paid attention to the literature of the subject, has written what will probably be regarded as one of the best county catalogues of Lepidoptera extant. The diversity of soil and climate, geological and physical conformation, for which Yorkshire is famous, is once more illustrated by the richness in species which the lepidopterous fauna shows, 1344 out of the 2031 species recognised as British finding places in Mr. Porritt's catalogue.

THE following occurrence is worth notice:—The Weymouth and Channel Islands Steam Packet Company's mail steamer *Aquila* left Weymouth at midnight on Friday for Guernsey and Jersey on her passage across Channel. The weather was calm and clear, and the sea was smooth. When about one hour out the steamer was struck violently by mountainous seas, which sent her on her beam ends and swept her decks from stem to stern. The water immediately flooded the cabins and engine-room, entering through the skylights, the thick glass of which was smashed. As the decks became clear of water, the bulwarks were found to be broken in several places, one of the paddle-boxes was considerably damaged, the iron rail on the bridge was completely twisted, the pump was broken and rendered useless, the skylight of the ladies' cabin was completely gone, and the saloon skylight was smashed to atoms. The cabins were baled out with buckets, while tarpaulins were placed over the skylights for protection. Fives minutes after the waves had struck the steamer the sea became perfectly calm. Several of the crew were knocked about, but none were seriously injured.

AT 10 p.m. on March 27 an earthquake occurred in and around the town of Miskolcz, Hungary. There were two separate shocks, and so distinctly were they felt that in the theatre, where the performance was going on, a panic ensued, the entire audience rising and rushing in terror towards the outlets. Many persons were injured, but, happily, no lives were lost. An earthquake was observed on March 12 in various parts of Italy. Reports now state that it was principally noticed in the Pellice valley, in the Po district, at Gessi, Varcita, Stura, and Coni. The direction of the shocks was from N.E. to S.W. In the plains the shocks were far less severe than in the mountains, where the foundations of the houses were shaken. Nobody, however, was hurt.

AN interesting discovery has been made at St. Pierre Quiberon (department of Morbihan). It consists of a new dolmen, one of those stone monuments of grey antiquity. It contained several entire human skeletons, besides a number of skulls, stone axes, a bronze pin, and some fragments of vessels.

THE large gold Cothenius medal, which the Imperial "Leopoldinisch-Carolinische" German Academy of Naturalists at Halle awards every year, has this time been given to Prof. F. Eilhard Schulze of Graz.

THE Berlin Mining Academy has purchased for the Mineralogical Museum of this Institution a so-called lightning tube or fulgurite, which was recently found near Warmbrunn. It measures nearly 2 metres in length. It is specially interesting, inasmuch as it shows a branch formation, about 30 centimetres from its end, measuring half a metre in length. The fulgurite was found after a severe thunderstorm in a sandhill and in a vertical position.

A BRILLIANT meteor was observed at Carlsruhe on March 5 at 8.9 p.m. It was about twice as bright as Venus at her greatest briliiancy. Its direction was S.S.W. to N.N.E.; it left a trail of yellowish red colour and of several degrees in length. The phenomenon finally disappeared in the constellation of Cassiopeia, developing little cloudlets at its disappearance.

AT Salez (canton of St. Gallen) some sixty bronze hatchets have been found imbedded in the ground only one metre deep. Their age is stated to be at least 2500 years.

THE additions to the Zoological Society's Gardens during the past week include an Arabian Baboon (*Cynocephalus hamadryas* ♀) from Arabia, presented by Mr. F. E. Goodner; a Sharp-tailed Grouse (*Tetrao phasianellus*) from North America, presented by Mr. Henry Nash; two Sea Mice (*Aphrodite aculeata*) from British Seas, presented by Mrs. A. Browning; an Olive Weaver Bird (*Hyphantornis capensis*) from South Africa, presented by Mr. Edward Ling; two Bonnet Monkeys (*Macacus radiatus* ♂ ♀) from India, deposited; a Red-vented Parrot (*Pionus menstruus*) from South America, a Sordid Parrot (*Pionus sordidus*) from Venezuela, purchased; a Long-eared Fox (*Otocyon lalandii*) from South Africa, received on approval; a Sambur Deer (*Cervus aristotelis* ♀), an Axis Deer (*Cervus axis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1882.—Herr Stechert has continued his ephemeris of this comet from the elliptical elements by Dr. Kreutz, which still agree pretty nearly with observations. We extract as follows:—

At Berlin, Midnight

	R.A. h. m. s.	Decl. ° ′	Log. distance from Earth.	Sun.
April 9	5 59 32	−8 43·1	0·5973	0·5787
11	6 0 30	8 29 5		
13	– 1 30	8 16·5	0·6084	0·5843
15	– 2 33	8 3·9		
17	– 3 37	7 51·9	0·6191	0·5898
19	– 4 43	7 40·4		
21	– 5 50	7 29·3	0·6294	0·5952
23	– 6 59	7 18 8		
25	– 8 10	7 8·7	0·6393	0·6005
27	– 9 22	6 59·1		
29	– 10 35	6 50·0	0·6487	0·6057
May 1	6 11 50	−6 41·3		

Assuming the intensity of light = 1, on February 8, when Prof. Schmidt last saw the comet with the naked eye at Athens, the intensity on April 9 will be 0·234, and on April 29, 0·163.

From September 8, the date of the first accurate observation at the Royal Observatory, Cape of Good Hope, to the middle of last month, the comet had described a heliocentric or orbital arc of 339½°; no other comet since the celebrated one of 1680 has passed over so large an arc of its orbit while under observation. Between Kirch's first observation on the morning of November 14, 1680, and the last observation by Sir Isaac Newton on March 19, 1681, that body traversed a heliocentric arc of 345°.

VARIABLE STARS.—Mr. G. Knott has observed three more minima of Ceraski's variable U Cephei. The resulting times of minima are—

	h. m.		
1883, March 12,	11 49 G.M.T.	...	Magnitude 9·4
,, 22,	11 10 ,,	...	,, 9·45
April 1,	10 29 ,,	...	,, 9·45

Mr. Knott remarks that the star is not a very easy one to observe, and it is not therefore an easy matter to disentangle errors of observation from real irregularities in the light curve.

On March 31 and April 1 he found the variable star R Coronæ Borealis very visible to the naked eye, and nearly equal to π Coronæ. "It has presumably brightened up further since Schmidt's observations towards the end of last year" (*Ast. Nach.* No. 2491). π Coronæ is a sixth magnitude according to Argelander and Heis. The variability of this star was established by Pigott in 1795, but its fluctuations are exceedingly irregular.

Schönfeld in his last catalogue gives, as the limits of variation, 5·8m. and 13m. The actual position is in R.A. 15h. 43m. 45s., Decl. + 28° 31′·0. Schmidt found that a star which precedes R Coronæ by 2 seconds, and 7½ minutes N. varies from 11m. to 13·12m. in a period of perhaps 1½–2 months (see *Ast. Nach.* No. 1915).

Bradley 396 has been so discordantly rated in our catalogues that variability appears highly probable, and the period may not be a long one. The estimates are from 4·5m. to 7m. It is Groombridge 580, Fedorenko 473, and B.A.C. 906. The position for 1883·0 is in R.A. 2h. 53m. 40s., Decl. + 81° 1′·0.

Prof. Pickering reports that a careful study of the fluctuations of Sawyer's variable by Mr. Chandler shows that it belongs to the Algol class, and has a period of little over 20 hours. A long series of observations of the light curve and successive minima gives 20h. 7m. 41·6s. ± 1·3s.

THE LATE TRANSIT OF VENUS.—Prof. Pickering has published the results of contact observations in the transit of Venus made at the observatory of Harvard College; the times are as follow:—

	h.	m.	s.		
First external contact ...	2	4	32	G.M.T.	by 3 observers.
,, internal ,, ...	2	24	43	,,	by 4 ,,
Last internal ,, ...	7	47	40	,,	by 6 ,,
,, external ,, ...	8	7	52	,,	by 6 ,,

These differ from the times given by the equations of reduction inserted in this column by + 58s., + 11s., + 22s., and − 25s. respectively, a very close accordance, considering that observations of the first external contact are less certain than the others.

GEOGRAPHICAL NOTES

AT a recent meeting of the Geographical Society of Copenhagen, Capt. Irminger in the chair, Dr. Oscar Dickson was present to give an account of the proposed Swedish expedition to Greenland. The chairman referred to Dr. Dickson as the Mæcenas who enabled Nordenskjöld to carry out his ideas, while both had an ardent supporter in King Oscar. Of the Arctic expeditions, which wholly or partly owed their origin to Oscar Dickson, he mentioned the following:—The expedition of 1868 to Spitzbergen was almost entirely paid for by him; the expedition of 1870 to Greenland was entirely paid for by him; the expedition of 1872–73, which wintered at Spitzbergen, was partly paid by him, while the great deficiency subsequently arising was covered by him; the expedition of 1875 to the Yenissei was entirely paid by him; the expedition of 1876 to the same river, by sea and by land, was chiefly paid by him; the *Vega* expedition of 1879–80 was paid to the extent of one-third by him, and if the vessel had not succeeded in rounding Asia he would have borne the entire cost of this expedition; and eventually the cost of the Swedish expedition of 1883 would be borne by him. It should also not be forgotten that, at the time when the despatch of the *Dijmphna* expedition was nearly frustrated for the want of 20,000 kr. (1150*l*.), Oscar Dickson came forward to supply the deficiency, and although it was most generously contributed by Mr. Gamél, every lover of geographical discovery ought to appreciate his noble action. Dr. Oscar Dickson next addressed the meeting. He began by stating that the King of Denmark had sanctioned the new expedition. Nordenskjöld had not desired that the programme of the expedition should be made public too soon, as he was much occupied with preparations for his journey and his duties as a senator, and if his plans should be questioned by *savants*, he would have no time for discussing them. He next referred to the oldest accounts of Greenland, its colonisation from Iceland, and "Esquimauxising" from America. After this, Greenland was for a time forgotten, until the west coast was rediscovered. The speaker then mentioned the achievements of Hans Egede, and the founding of a commerce. The west coast was one of the best known Arctic countries, both geographically and ethnologically; but not so the east coast. In spite of several expeditions and researches, only the southern portion was known. The interior was a *terra incognita*. These tracts were, however, too important to remain unknown. He then referred to the wanderings of Nordenskjöld and Lieut. Jensen on the inland ice. From these expeditions it was impossible to infer that the interior of Greenland was entirely covered with ice, while in the constant advance of the glaciers and their melting off he (the speaker) found a corroboration of this theory. By the geographical appearance of Greenland, and more especially by the circumstance that the country gradually rose in the interior, it was more than probable that the interior was not entirely covered with ice. Even in the temperature and moistness of the air there seemed a proof that the country would answer to its name. In any case the exploration of the interior of this country was most important, and it was for this purpose that the expedition would make its researches. To these belonged the ascertaining of the extent of the drift ice between Cape Farewell and Iceland, the study of the inland ice, the fossil remains, and the cosmic dust in the island. Eventually it was hoped that, while Nordenskjöld made his expedition across the ice, another party of the members would pay a visit to the west coast, where there were some very peculiar blocks of ironstone. The expedition would possess a complete staff of scientific specialists. The expedition had also one more object in view, viz. to settle the question as to where the Österbygd had been. Every one who read without prejudice the oldest accounts could but come to the conclusion that its remains must be found on the east coast. After excursions on the inland ice, it was the intention to attempt to penetrate northwards along the east coast. In May next the expedition would start in a well-equipped steamer, and, if the state of the ice would permit, first land on the east coast; but as this was not expected to be the case the party would land on the west coast, and when the researches here were at an end they would proceed along the east coast in a channel between the land and the drift ice. In September next the expedition would return.

THE changes of level of the Caspian are still a puzzling problem for Caucasian geographers. It is known that the late M. Khanikoff was of opinion that the level of this sea has been rapidly falling during our century. After having been, in 1780, 13 feet above the level of 1852, and 10 feet in 1820, he said, it was only 3·3 feet higher in 1830, and has almost regularly decreased since. Sokoloff maintained that it had risen and fallen at irregular intervals since 1744, but was 10 feet lower in 1830 than in 1780. Lenz admitted that it had fallen about 10 feet during the years 1816 to 1830. In any case, for the benefit of subsequent measurements he made permanent marks at Baku showing the level of the sea in 1830, and since that time measurements of level were carried on at Baku. But their results were unsatisfactory—as it appears from M. Filipoff's paper in the last number of the Caucasian *Izvestia*—and the only sure result is that on May 30, 1853, the level of the Caspian was 2 feet 1·3 inches lower than in March, 1830. In September, 1854, at high water it already had risen 1 foot 6 inches *above* the mark of May 30, 1853. On June 4, 1882, that is, at high water, it was also higher than in 1830 by 10·5 inches, so that it may be admitted, according to M. Filipoff, that since 1830 the level of the Caspian, although subject to fluctuations (such as a rapid rise after 1847), has not sensibly fallen during the last fifty years.

ACCORDING to the recent explorations of M. Yadrintseff, the situation of the aborigines throughout Northern and Middle Siberia is very precarious. The Bakaharians and Tartars, who were formerly a privileged class of merchants, and number at present 43,670 souls in Middle Siberia, are decreasing, and belong to the poorest population of the country. The Voguls in the Government of Tobolsk number 6070, and their increase is insignificant. As to the 23,070 Ostyaks and Samoyedes, they are in the worst imaginable position; the rate of increase is very low, while in other parts they obviously decreasing. They are accustomed now to eat bread, but have no means to provide it in necessary quantities owing to its high price. As to the southern Tartars, who have maintained their pasture lands, they are in a better position; those of Barnaoul and Biysk, who are agriculturists, and those of Kuznetsk, living on trade, are on the increase; and M. Yadrintseff quotes an instance of ten families who have maintained their land and occupy now seven villages, making a total of 1270 souls. The dying out of these aborigines is the more regrettable, as M. Yadrintseff proved by numerous instances that they display a high degree of intelligence, and might adapt themselves to new conditions.

IN the April number of *Petermann's Mittheilungen* there is a full account, by Dr. Rink, of recent Danish researches in Greenland,—on the geography of the interior, the ice-formation and glaciers, geology and mineralogy, botany and archæology; accompanying the paper is a map of the west coast between Godhavn and Pröven, coloured geologically. Baron von Richthofen discusses the value of the copy of "Marco Polo" recently discovered in the royal library of Stockholm.

It is reported that Dr. Emil Holub is at last about to start again for the dark continent. As before, so will Dr. Holub now go to Africa without one penny State assistance; and the only support he could obtain is that a special train will carry his cases to the Austrian frontier, and, if the German Government permit, to Hamburg, where they will be embarked for South Africa. The money for his expedition he acquired himself by lecturing in Vienna, Berlin, London, &c. He will leave Austria after he hears that his cases have arrived in Africa, in about two months, and he contemplates remaining on the African continent about four years. The 150 cases and about 100 other packages which he takes contain all that is necessary for a scientific expedition, including scientific instruments which the Austrian War Ministry lends him. He has also a transportable iron cart, which can be taken to pieces, and an iron boat on Stanley's celebrated models, both gifts of Austrian manufacturers or private persons. The remainder of the cases are filled with utensils, arms, stuffs, cotton goods, &c., for the natives, and all other necessaries.

The Museum for Commercial Geography was opened at Berlin in the Architekten House on April 1.

The Imperial Geographical Society of St. Petersburg has awarded its highest distinction, viz. the Constantine medal, to Dr. Hermann Abich of Vienna, for his work, "Geological Researches in the Caucasus."

FACTS AND CONSIDERATIONS RELATING TO THE PRACTICE OF SCIENTIFIC EXPERIMENTS ON LIVING ANIMALS, COMMONLY CALLED VIVISECTION[1]

[*Issued by the Association for the Advancement of Medicine by Research*]

§ 1. MEDICINE, as the art of preventing and curing disease, depends first, upon Anatomy and Physiology, or knowledge of the structure and working of the human body in health; secondly, upon Pathology, or knowledge of the origin, course, and results of disease; and thirdly, upon knowledge of the effects of various mechanical, physical, or chemical means which prevent or modify diseased processes, and are thus available for preventive or curative Treatment.

As in every other practical art, the application of scientific (that is to say, exact and general) knowledge to particular cases must be checked and controlled by practical experience. But the history of medicine abundantly proves that experience is productive only in so far as it is guided by the habit of scientific inquiry and quickened by physiological knowledge. The foundation of efficient medicine was laid by the discoveries of the sixteenth century in anatomy, and of the seventeenth century in physiology, and its rapid progress in modern times has been chiefly the result of discoveries in physics, in chemistry, and in general biology.[2]

[1] The term "Vivisection" is open to objection. As a question-begging epithet, it produces an unfounded prejudice against experiments, of which the majority are painless, and of which the object is to relieve the sufferings of both man and brutes. Moreover, the term is at once too narrow and too wide: too narrow, since it excludes painful experiments which do not involve cutting, such as exposure to disease; and too wide, since it includes painful procedures upon animals for other than scientific or humane objects, for food, as in preparation for the table, for convenience, as in horse and cattle breeding, or for amusement, as in certain sports. The same operation which, if performed for the acquirement of knowledge, is called a vivisection, is not called a vivisection when performed for a less worthy object.

[2] Some otherwise well-informed persons have expressed doubt as to the reality of the great progress of medicine durlng the present century. This doubt arises partly from an arbitrary separation of what is called internal medicine from surgery (la médicine opératoire) and from preventive medicine. The world fully appreciates such triumphs of medicine as the cure of Aneurism and prevention of Small-pox, the discovery of Anæsthetics and the success of Ovariotomy, the results of Antiseptic surgery, the vastly decreased mortality after operations, and the protection of cattle from pestilence by inoculation. But in the treatment of fevers, inflammations, and other internal diseases, conventionally called medical, progress is less striking, because, being more obscure, these maladies have not yet been brought under the complete influence of scientific investigation.

In proof, however, that the scientific spirit of modern medicine has not failed to advance the treatment of even the more obscure diseases, and that practical advance in medical treatment has not been limited to operative surgery, may be adduced as instances: the greatly lessened mortality in Fevers, owing to physiological observations and scientific treatment, the improved diagnosis and more successful results in cases of paralysis and other diseases of the Nervous System; the far shorter and less painful course of acute Rheumatism; the advance in treatment of Diabetes, Consumption, Dropsies, and affections of the Heart, and the successful cure of numerous forms of disease now proved to be due to animal or vegetable parasites.

"Looking back over the improvements of practical medicine and surgery during my own observation of them in nearly fifty years (writes Sir James Paget) I see great numbers of means effectual for the saving of lives and for the detection, prevention, or quicker remedy of diseases and physical disabilities, all obtained by means of knowledge, to the acquirement or safe use of which experiments on animals have contributed. There is scarcely an operation in surgery of which the mortality is now more than half as great as it was forty years ago; scarcely a serious injury of which the consequences are more than half as serious; several diseases are remediable which used to be nearly always fatal; potent medicines have been introduced and safely used; altogether, such a quantity of life and working power has been saved by lately-acquired knowledge as is truly past counting."

Medicine then, including Hygiene, or preventive medicine, and Therapeutics, or curative medicine, whether it acts by operative and mechanical measures,[1] by the administration of drugs, or by other means, does not depend upon arbitrary dogmas, or upon the theories of one or another school; it depends upon accurate knowledge of the structure and functions of the body in health and disease, and of the effects of various agents upon it, applied in each case by the aid of bedside experience—καθ' ἕκαστον γὰρ ἰατρεύει.

The relation of medicine to physiology and pathology is the same as that of navigation to astrometry and meteorology, or of engineering to applied mathematics, or of dyeing and other manufactures to chemistry. A seaman may safely direct a vessel who is ignorant of the construction of a quadrant; a bridge may be built without knowledge of theoretical mechanics, and a watch may be "cured" or a musical instrument "tuned" by a workman who is unacquainted with mathematics or acoustics. In the same way many men are useful practitioners of medicine who are imperfectly acquainted with the scientific basis of their practice. But it is only the most ignorant of sailors who sneer at natural science, and the most presumptuous of watchmakers who rail at mathematics.

§ 2. The knowledge of the functions of the body in health, or Physiology; the knowledge of the origin and course of diseases, or Pathology; and the knowledge of the action of remedies, or Pharmacology, like other branches of natural science, depend entirely upon observation and experiment. Mere observation at its best is but careful noting of such experiments as natural laws or accident may present; experiment, or observation of events under intentionally varied conditions, is absolutely necessary in addition.[2] Indeed, it would be as unreasonable to expect the "Institute of Medicine" (as physiology and pathology are rightly called) to advance without laboratories and experiments on animals, as to hope for progress in chemistry or physics by allowing only observation upon metals and gases and forbidding the performance of experiments.

It is true that there are special difficulties in the study of the natural laws of living bodies. The conditions are far more complicated than those of the inorganic world, and observations and experiments must be proportionately numerous, well-devised, and cautiously interpreted. Fallacies of observation and deduction are difficult to avoid, and often results are seemingly contradictory until their true meaning is perceived by help of fresh experiments and more careful reasoning. But the great and assured results which have been already obtained prove that these difficulties are far from insurmountable. All our present knowledge has been achieved in spite of them, and thereby the path to future discoveries has been cleared. No reasonable person would disparage experimental inquiry into the functions of plants and the cultivation of crops, because the laws of vegetable life are more complicated and obscure than those of mineralogy: or would call the experiments of the botanist useless because they are difficult.

That experiments on living creatures, like all other experiments made by fallible persons, have sometimes misled, is an obvious truth. Many errors attended the first application of the stethoscope, of the microscope, and of chemical analysis to medicine, so that impatience and ignorance pronounced that each of these valuable methods of investigation was useless.

§ 3. The future progress of medicine, in the widest sense of the word, of the art which prevents disease, promotes health, relieves sickness and prolongs life, depends upon the same cause which has led to its present position—upon more complete acquaintance with the laws of health and disease. These laws have been, and can only be, successfully investigated by observations and experiments.

This conclusion is not only the inevitable result of reasoning,

[1] "Forasmuch as the Science of Physick doth comprehend, include, and contain the knowledge of Chirurgery as a special member and part of the same."—Statute 32 Hen. VIII. c. 40.

[2] "L'observateur écoute la nature, l'expérimentateur l'interroge."—Cuvier.

but is also enforced by the unwavering testimony of those best qualified to judge,—not only of scientific workers themselves, but of the medical profession in all civilised countries. There is not the smallest danger that the ninety-nine hundredths of the medical profession who are engaged in the daily effort to prevent or relieve disease will undervalue practical medicine in comparison with the more abstruse branches of experimental physiology and pathology; the danger is the other way. With few exceptions physicians and surgeons are not themselves experimenters in physiology or pathology. Their business is to prevent disease and to relieve their patient's sufferings: but they know the benefits which their art has derived from the work of the laboratory, and understand the nature and value of experiments. They are thus at once the most disinterested and the most competent witnesses, and their constant and unanimous testimony ought to be conclusive.[1]

The International Medical Congress of 1881, where upwards of 3,000 physicians and surgeons assembled in London, among whom were the ablest and most respected leaders of the profession in the three kingdoms, in America, and in foreign countries, passed, without a dissentient voice, the following resolution:—

"That this congress records its conviction that experiments on living animals have proved of the utmost service to medicine in the past, and are indispensable to its future progress. That, accordingly, while strongly deprecating the infliction of unnecessary pain, it is of opinion, alike in the interest of man and of animals, that it is not desirable to restrict competent persons in the performance of such experiments."

§ 4. A moral question, however, arises, from the fact referred to in the resolution just quoted, that some of the necessary experiments of physiology and pathology involve the infliction of pain or of death upon certain of the lower animals.

The better informed opponents of experimental medicine do not dispute its scientific and practical value, but assert that no probable benefit to man or animals justifies the infliction of pain.

No one would succeed in closing the laboratories of the chemist, or the observatories of the astronomer, however strong his disbelief in the experimental method of inquiry might be, however cordially he disliked or dreaded the advance of science, or however obstinately he persisted that the useful arts do not depend on scientific data.[2] It is obvious, however, that the fact of pain or death being inflicted in the course of experiments cannot alter their scientific importance and necessity; it only imposes on us the duty of making a comparison between the injury to a sentient creature and the probable benefit to mankind, or to others of its own species. This comparison we will attempt to make.

Happily, the amount of pain inflicted in the course of scientific experiments need only be small, and the destruction of life insignificant. That, from carelessness or want of forethought, experiments have been performed which were "cruel," because the pain produced was excessive and unnecessary, may be admitted. In many countries consideration for the brute creation is still little developed, and the vice of cruelty lightly regarded;

[1] It would be invidious to dwell upon the very few exceptions to this almost universal testimony. One only deserves special mention. Sir William Fergusson was one of the most skilful and successful operators, but he had no authoritative claim to give an opinion upon the sources or the methods of surgical science, and even he in his evidence before the Royal Commission admitted the use of experiments on animals.

The testimony of the late Professor Claude Bernard has been often adduced against that of all other physiologists because he once wrote, "Nous venons les mains vides, mais la bouche pleine de promesses légitimes." This phrase occurs in an elaborate exposition of the necessity of experiments on living animals not only for knowledge but for use. Bernard well understood the bearing of experiments upon medicine, but he foresaw future developments of scientific treatment, in comparison with which his own eminent services would appear insignificant. The following quotation shows that his evidence on the whole question did not differ from that of other competent witnesses:

"On voit que la physiologie, ou médecine scientifique, comprend à la fois ce qu'on a artificiellement séparé sous les noms de physiologie normale, de physiologie pathologique, et de thérapeutique. Au point de vue pratique, c'est certainement la thérapeutique qui intéresse au plus haut degré le médecin; or, c'est précisément la thérapeutique que doit le plus de progrès à la physiologie expérimentale."—*Leçons de Physiologie Opératoire*, p. 20.

[2] On the other hand, it is almost as clear that no serious obstacle would be put in the way of even painful experiments in the cause of science, if all their opponents were convinced of their utility, and were acquainted with the methods of science in general, or the facts of medical science in particular. This seems to follow from the very moderate opposition to, or tacit acquiescence in, the infliction of pain for desirable objects which obviously cannot be otherwise attained, such as more delicate food, more docile horses, increased wealth and comfort, or the pleasurable excitement of chasing and killing animals.

even in England, until comparatively lately, the torture of harmless animals was thought an innocent pastime. Men of science have not always risen above the average humanity and moral enlightenment of their age and country. But speaking of this country, and of modern times, it may safely be said that no charge of wanton, needless, or excessive sacrifice of animals can be, or indeed has been, seriously alleged against the small number of experimental physiologists and pathologists at work in the three kingdoms.[1] Science has herself provided the means by which pain is reduced to a minimum. The beneficent discovery of anæsthetics is one cause of the great difference between the sufferings inflicted by Harvey, Boyle, Hales, Haller, Hunter, Magendie and Bell, and the generally painless experiments of a modern laboratory. These may be classified as follows, with reference to the suffering inflicted:—

(1) Many physiological experiments are entirely unaccompanied by pain, and can therefore be performed, according to convenience, either upon animals or upon man himself. Such are many experiments upon vision, taste, smell and touch; experiments on the value of different kinds of food, experiments on the effect of exercise, temperature, and other conditions on the excretions; many experiments on bodily heat, on the pulse, and on respiration.

(2) In still more numerous cases, observations and experiments can be made on the tissues and organs after the death of an animal: *e.g.*, the relative tenacity of the different textures, the mechanical effects of violence upon the bones, the action of the heart (which in cold-blooded creatures continues long after their death) and the whole of a long and important series of experiments on the functions of muscles and nerves, which cause no pain, since they are performed on the tissues of a dead organism.

(3) Next, but far less in number, comes a third class of experiments which are performed on animals rendered insensible by various anæsthetic agents. These can be, and were, by the practice of physiologists long before legislative sanction was added, carried out without any pain or even discomfort to the animal, which being killed before awakening, is deprived of life in probably the most painless manner possible.

(4) There are, however, certain observations, for which it is necessary to allow an animal to recover from insensibility, and to live for a longer or shorter time. In such cases the severest pain, that of the operation, is abolished, and the subsequent suffering is sometimes quite insignificant, usually that of a healing wound, and occasionally that of inflammation, colic, or fever. In many of these experiments the initial pain is so trifling that it would be absurd to give an anæsthetic; such are acupuncture and inoculation. It would be unreasonable to give a rabbit chloroform for such observations as bleeding, vaccination, or pricking with the needle of a subcutaneous syringe, for which no human being would take it.

(5) There remain a small number of experiments in which anæsthetics would be impracticable. These are chiefly the experimental production of various diseases, such as tubercle, glanders, cattle-plague, where the pain is that of the subsequent

[1] The following extract is taken from the Report of the Royal Commission, which was drawn up after a prolonged and patient examination of witnesses and documents, and was signed by all the Commission—Lord Cardwell (Chairman), Lord Winmarleigh, the Rt. Hon. W. E. Forster, the late Sir John Karslake, Professor Huxley, Mr. Erichsen, and Mr. R. H. Hutton:—

"That the abuse of the practice by inhuman or unskilful persons, in short, the infliction upon animals of any unnecessary pain, is justly abhorrent to the moral sense of your Majesty's subjects generally, not least so of the most distinguished physiologists and the most eminent surgeons and physicians."

The imputation of cruelty which has always been indignantly repudiated, has not been substantiated by a single authentic instance. In their evidence, given before the Royal Commission, the Royal Society for the Prevention of Cruelty to Animals state, through their Secretary, that they do not know a single case of wanton cruelty.

On the occasion of the present Act (39 and 40 Vict. cap. 77) being passed, all teachers of physiology, in a memorial addressed to the House of Commons, said:

"We repeat the statement which most of us have made before the Commission, that within our personal knowledge, the abuses in connection with scientific investigation, against which in this Bill it is proposed to legislate, do not exist, and never have existed in this country." Signed by the late Prof. Sharpey (University College, London); Dr. William Carpenter, C.B. (formerly Lecturer on Physiology at the London Hospital); Professor G. Humphry (Cambridge); Professor Rutherford (Edinburgh); Dr. Pavy (Guy's Hospital); Dr. M. Foster (Trinity College, Cambridge); Dr. Burdon Sanderson (University College, London); Dr. Robert McDonnell (Dublin); Prof. Redfern (Belfast); Prof. Cleland (Galway); Prof. Charles (Cork); Prof. McKendrick (now of Glasgow); Dr. Pye-Smith (Guy's Hospital); Prof. Yeo (King's College, London); Mr. Charles Yule (Magdalen College, Oxford); Prof. Gamgee (Owens College, Manchester).

disease, and more justly described as discomfort than as torture; and the trial of certain modes of treatment, as inoculation, and of various drugs, where the suffering produced is less than the familiar effects of corresponding remedies in human beings. Probably the most painful scientific experiments ever performed have not been vivisections at all. Such are those of ascertaining the effect of starvation, carried out abroad many years ago; observations of great value and importance, but happily not needing repetition.

Vivisections in the popular sense of the word, experiments comparable to surgical operations, involving cutting and irritation of sensitive parts, can, with few exceptions, be performed without the slightest pain. Hence the results of acutely painful experiments, comparable with the pain endured by rabbits and weasels caught in ordinary traps, by young animals being gelded, by wounded birds, or by rats poisoned with strychnine or phosphorus, are not to be found in our physiological laboratories.

That the utmost possible limitation of the infliction of pain has always been the object and practice of scientific workers in England,[1] is sufficiently proved by a Report which was drawn up by a Committee of the Physiological Section of the British Association for the Advancement of Science, in 1871, several years before the appointment of the Royal Commission.

While the suffering caused to animals by scientific experiments has been enormously exaggerated, both absolutely and relatively, no one denies that both pain and death are and must be inflicted thereby. Otherwise there would be no more reason for licensing and inspecting the physiologist's laboratory than that of the chemist. The whole question is one of justification for causing the pain or death of brutes. Few who compare the extent of suffering and of slaughter thus caused with that generally recognised as right in other cases by enlightened Christian morality, or who compare the objects for which animals commonly suffer pain and death (for food, for dress, for profit, for convenience, or for amusement) with those of the scientific observer (for advance of knowledge and for relief of human suffering) will hesitate to conclude that so long as the principles and practice of scientific men in this country continue what they now are, their investigations should rather be fostered than impeded.

But any possible danger of abuse is prevented by the Act passed in 1876, by which not only are all physiological laboratories placed under the inspection of the Home Office, and exist only by its license, but, in addition, no experiment involving pain can be performed without a special, elaborate, and carefully guarded certificate. Indeed, so stringently has the law been administered that more than one investigation of great practical value has been prevented, others have been injuriously hampered or delayed, and a serious check has been given to medical science in England. In two instances eminent members of the profession found it necessary to go abroad in order to carry out investigations of great importance. The object of one was to decide a question in relation to treatment of wounds; that of the other was to determine the action of certain new drugs.

This was certainly not the intention of the Royal Commission in recommending, or of Parliament in passing, an Act for the purpose of preventing possible abuses without hindering scientific and useful work. What is now needed is such an expression of opinion in Parliament as will permit the Act to be worked in the spirit in which it was framed and loyally accepted, and according to its strict provisions. It may be remarked that attempts have been made, by the same methods of agitation, to check physiological research by legislation in Germany, Denmark, Sweden, and the United States: but in each case the humane and enlightened judgment of the country has refused to impede researches of which the usefulness is beyond dispute.

§ 5. It has been imagined that students of medicine perform operations upon living animals in order to gain manual dexterity; such a practice would be as useless as it would be reprehensible, and has never, we believe, been thought of. For our veterinary surgeons it would be quite unnecessary, and they have always reprobated the practice.

It has also been supposed that students might, for amusement, perform physiological experiments upon living animals. This would be practically impossible, since not only are knowledge and skill necessary, but a properly equipped laboratory and suitable appliances.[1] If, however, any ill-disposed person without scientific object or training should be guilty of cruelty most alien from the practice and the training of the profession, there is no doubt that every member of it, teacher or student, would help to detect and punish such conduct.[2] The case has never arisen; if it did, it could be efficiently dealt with under the law known as "Martin's Act."

§ 6. The real objects of scientific experiment on living animals are briefly as follows:—

i. *To extend, correct, and define our knowledge of the functions of the living body.*

Even apart from ulterior advantage to medicine, physiology must be held to be a branch of science of at least equal importance with chemistry or geology; and to be successfully cultivated, it must be cultivated for its own sake, without perpetual or premature inquiry as to the immediate and material results which increased knowledge of the laws of Nature will bring. In physiology, as in other natural sciences, the investigator must have primarily in view the discovery of truth; for, in the words adopted by the Royal Commissioners, "if in the pursuit of science he seeks after immediate practical utility, he may generally rest assured that he will seek in vain." There must be, to quote the words of an older authority, "light-bearing," as well as "fruit-bearing experiments."

As examples of this first kind of experiment, and of their success in extending useful knowledge, we may refer to the following:—

(1) The great discovery of the circulation of the blood by Harvey, the firstfruits of the experimental method.[3] Upon this as the foundation depends all the subsequent progress in the surgical treatment of hæmorrhage and of aneurisms, and the recognition and treatment of diseases of the heart, the arteries, and the veins.

(2) The discovery of the effects of electricity on animals by Galvani and Volta, from which have resulted not only the development of one great branch of electrical science, but also important means of diagnosis and treatment in cases of paralysis.

(3) Artificial respiration, invented and improved in the case of animals with purely scientific objects by Vesalius, Hooke, Lower, and others, and long afterwards applied with complete success to resuscitation from drowning.

(4) The experiments of the Rev. Dr. Hales on pressure of the blood in the arteries.

(5) Those of Boyle, Hooke, Mayow, and other natural philosophers on respiration.

(6) Transfusion of blood from one animal to another, accomplished by Sir Christopher Wren and others of the early Fellows of the Royal Society in the seventeenth century, but only recently, owing to fresh physiological knowledge, applied with success to the saving of human life.

(7) Experiments by a Committee of Physicians at Dublin, in 1835; showing the way in which the sounds that attend the action of the heart are produced, and enabling physicians to judge of the condition of the organ by the alterations of the sounds.

(8) The discoveries of reflex action and of the separate endowments of motor and sensory nerves, on which much of our present knowledge of the functions and disorders of the nervous system is founded.

(9) The discovery of vasomotor nerves.

[1] The following quotation, from a Manual of Physiological Experiment by a well-known German physiologist, will serve to show that humane consideration for animals is not confined to this country:—"An experiment involving vivisection should never be performed, especially for purposes of demonstration, without previous consideration whether its object may not be otherwise attained;" and, as a second rule, "Insensibility by chloroform or other drugs should be produced whenever the nature of the experiment does not render this absolutely impossible."—Cyon, *Physiologische Methodik*, p. 9.

[1] It is obvious that this sound general principle admits of exceptions when the skilled person with suitable appliances must, from the nature of the case, carry out his researches on board ship, as for instance for investigation into the functions of jelly-fish, or the electric torpedo; or in the open fields, as in inquiries into means of protection from epidemic diseases of cattle.

[2] For the real sentiments of medical students, see Dr. Pavy's evidence before the Royal Commission, *Blue Book*, p. 114.

[3] Some persons have ventured to deny that Harvey's discoveries were due to vivisection, on the faith of a reported statement of his to the Hon. Robert Boyle (another eminent vivisector), and in contradiction to Harvey's express words. Others have denied that the circulation was proved by vivisection, because Harvey having proved all but one point by a series of experiments on living animals, Malpighi completed the demonstration by another experiment on another living animal. The full account of the matter is contained in Harvey's own treatise, "De Motu Cordis et Sanguinis." It is briefly referred to in the article *Harvey* of the "Encyclopædia Britannica," and in the evidence of Professor Turner, of Edinburgh, before the Royal Commission (Blue Book, pp. 157, 158); where also are given the account of the discovery by vivisection of the great system of lymphatic vessels, by Aselli and Pecquet, and of the discovery of motor and sensory nerves by the same means by Bell and Magendie.

ii. *To obtain direct and exact knowledge of the processes of disease.*

The following examples may be cited :—

(1) Experiments relating to the nutrition of the body and the maintenance of its constant temperature constitute the basis of the existing knowledge of fever.

(2) Experiments relating to the mechanism of the circulation, and to the influence of the nervous system thereon, have served to explain the nature and mode of origin of the various forms of dropsy.

(3) Experiments as to the effect of plugging arteries (Embolism) have afforded explanations of diseased processes previously not understood, and in particular of many obscure cases of sudden death.

(4) Experimental investigations of the functions of the liver and other secreting glands have materially advanced our knowledge of diabetes and of the affections known as Bright's disease.

(5) Knowledge gained from experiments relating to the mode of action of the muscles, and of the nervous system which regulates them, constitutes the basis of the pathology and diagnosis of convulsive and paralytic diseases.

(6) Experiments on animal grafting and as to the nature of the processes by which wounds are healed and injured parts restored. Among the best known are those which relate to the mode of repair of fractured or otherwise injured bones, particularly the researches of Duhamel (1740), Sir Astley Cooper (1820), and Syme (1831). In recent times such inquiries have been pursued much more completely by Ollier and others, and with practical results of ever-increasing value.

(7) The dangerous form of blood poisoning after operations has been investigated by strictly physiological experiments, with the result of almost complete protection from it.

(8) Researches into the origin and nature of inflammation, by Redfern, Cohnheim, Von Recklinghausen, and others, have been of necessity conducted by means of experiments on animals, and have proved of great practical value.

(9) Our recently extended knowledge of the locality of diseases of the brain, and of their accurate diagnosis and treatment, has been due, partly to clinical observations, partly to pathological investigations, but also, and not least, to direct experiments upon the lower animals.[1]

iii. *To test various remedial measures directly.*

The utility of the greater number of the older remedies and methods was first learnt empirically: but many of them were not applied to the best purpose until they have been investigated by observations on the lower animals. As regards the remedies and appliances of modern times, they have, in almost every instance, been investigated first and brought into use afterwards. For example :—

(1) Subcutaneous injection was used in the laboratory for years before it was applied in practice.

(2) The useful property of the well-known anodyne chloral hydrate was first investigated in the laboratory, and then introduced into practice.

(3) Pepsin and pancreatin were known for years as physiological agents before they were applied in practice.

(4) The action and mode of administration of such important new drugs as nitrate of amyl, physostigma, and the anæsthetic, methylene, were discovered entirely by physiological experiments.

(5) The better appreciation and more useful application of some of the most valuable remedies were gained by experiments, such as those by Traube on digitalis, by Magendie on strychnia, and by Moreau and others on saline purgatives.

(6) The application of various practically useful methods of checking hæmorrhage was tested upon animals before being tried on human beings, with the result of saving innumerable lives.[2]

(7) Similar preliminary trials of subcutaneous and other operations, especially those of tenotomy, have helped in the relief of numerous deformities; while the trial of such formidable operations as excision of the kidney and tentative improvements in ovariotomy have led to some of the most brilliant results of modern surgery.[3]

In cases where new drugs are to be introduced, or new operative methods tried, the first experiments must be made either upon living animals or upon living men. Where circumstances excluded the former alternative, members of our profession have not hesitated to make themselves the subject of often hazardous experiments: but happily, in most instances, the sacrifice of a few guinea-pigs or frogs will suffice to help in saving human life.

iv. *To ascertain the means of checking contagion, and preventing epidemic disease both in man and in brutes.*[1]

An experiment of this kind, inoculating the udder of a cow so as to produce a vaccine pustule, was one of the links in the great discovery of Jenner. Among more recent examples may be mentioned :—

(1) The experimental investigations of the last fifteen years, as to the origin and nature of the infective diseases which spring from wounds and injuries (pyæmia and septicæmia), the results of which constitute the basis of antiseptic surgery.[2]

(2) The discovery by experiments of the infective nature of tuberculosis (1868), of its relation to chronic inflammation, and finally of the dependence of its infectiveness on a living parasitic organism (1881).

(3) Discovery of the mode of origin, and consequently of the prevention, of various parasitic entozoa (hydatids, trichina) which infect the human body, by inference from investigation of their development in the bodies of animals.

Among diseases of animals may be mentioned :—

(1) Silkworm disease, which has been brought completely under control by the experimental discoveries of Pasteur.

(2) Small-pox of sheep, against which preventive inoculation has been long used.

(3) Cattle-plague, the prevention of which is entirely founded on the knowledge of its mode of spreading gained by experiment.

(4) Pleuro-pneumonia of cattle, and foot and mouth disease, of which, although experiment has not as yet yielded a satisfactory mode of prevention, it has furnished exact knowledge as to the method of its propagation.

(5) Splenic fever of cattle, and the analagous diseases of horses, sheep, and other animals, against which experiment has recently furnished a mode of prevention, now successfully used in countries in which this disease has most fatally prevailed, particularly in France.

(6) Farcy and glanders, the early detection and prevention of which has been greatly promoted by experiments.

v. *For instruction.*

It is not necessary to insist on the well-known difference between book-learning and demonstration. Like chemistry, physiology must be taught practically if it is to be taught well, and it is necessary that all students of medicine to whom the care of the human body will be intrusted should have a practical and thorough familiarity with the most important functions of that body. For this purpose no painful experiments are necessary, and none are performed in our medical schools and colleges. Most of the demonstrations of what is called "practical physiology" are demonstrations of the microscopical structure of the tissues, or of their chemical properties and processes, or of their physical endowments, and the remainder apply to the organs of insensible or recently killed animals. Whether the occasional repetition of an experiment of great importance, and involving very little pain, would be morally justifiable may admit of question; but, as a matter of fact, it is not and cannot be done. Apart from the provisions of the Act, this question was decided long before by the resolution quoted above.

vi *For the detection of poisons.*

The fact that some of the most subtle and dangerous poisons cannot be certainly identified by ordinary testing (*i.e.* by recognition of their physical and chemical properties), is well known. In such cases the physiological test, or the effect of the poison upon the lower animals, is the only means by which the guilt of murder can be brought home to a criminal, or the innocence of a wrongfully accused person established. This, like many other scientific facts, has been disputed by ill-informed persons: but it is beyond serious question.[3]

[1] See an article in NATURE, vol. xxv., p. 73.
[2] See an article in the *Nineteenth Century* for December 1881, p. 926.
[3] See a paper by Mr. Spencer Wells, *Trans. Internat. Med. Congr.* vol. ii. p. 226.

[1] For details on this part of the subject, see the Address by Mr. Simon, C.B., F.R.S., entitled "Experiments on Life as fundamental to the Science of Preventive Medicine." ("Transactions of the International Medical Congress. 1881.")
[2] For details, see a paper in the *Nineteenth Century* for March, 1882, by Mr. George Fleming, President of the Royal College of Veterinary Surgeons: "Vivisection and Diseases of Animals."
[3] See on this subject a paper by Prof. Gamgee, of Owens College, "The Utility of Physiological Tests in Medico-Legal Inquiries."

It was found necessary to insert a clause in the Act allowing a judge to order any needful experiments by a medical jurist. But this may cause, and has already caused, injurious delays, and it would be desirable for each person engaged in this department of scientific work to take out the necessary license beforehand.

§ 7. The above is only a brief enumeration of some of the more striking and illustrative cases in which the objects proposed by experiments on animals have been attained. In some of these success has been brilliant and complete, in others comparative and needing fuller development. In some the results have been the direct and exclusive consequence of the experiments, in others they have been due to these either as confirming or correcting previous conjectures, or as guiding clinical research, or as suggesting fruitful investigations by other methods.

Without exaggerating its extent and cogency, the evidence is ample to show, what no one conversant with the subject doubts, that the great strides made in the practice of medicine during the last fifty years have been chiefly due to the exact scientific experimental inquiries of this epoch. In fact, experience fully bears out what reason demonstrates and authority confirms, that medicine rests chiefly upon physiology, and that physiology cannot advance without experiments.

The prejudices excited by the account of long past or distant abuses of the right and duty of experiment will, it may be hoped, be dispelled (as in many cases they have been) by increased knowledge of the facts; while those which have been raised by reckless misstatement will subside on candid investigation. If any fear remain that evils which do not now exist may possibly arise in future, it may be dispelled by a consideration of the stringent regulations of the existing law, even if carried out with the utmost desire not to obstruct demonstrably useful scientific work.

But it is on the scientific investigator himself that the responsibility must ultimately rest of determining what is the best method of accomplishing a given scientific result, and by what means *the greatest possible result may be obtained at the least possible cost of suffering*. If restrictions are supposed to be necessary to control the conduct of careless individuals, let them be continued; but so long as scientific men exercise their responsibility in the humane spirit which has hitherto guided investigation in this country, they have a right to ask that no unnecessary obstacles should be placed in their way.

It is therefore hoped that such a decided and influential expression of opinion will be made in Parliament as will not only rebuke ill-advised attempts to totally abolish one of the most important methods of natural knowledge, and an indispensable method for the improvement of medicine; but will also strengthen the hands of the Government in administering the law, so as not to interfere with the just claims of science and with the paramount claims of human suffering.

THE BRITISH ASSOCIATION AND CANADA

THE following circular is being sent out by the British Association for the Advancement of Science:—

"22, *Albemarle Street, London, W., March* 19

"SIR,—We have been instructed by the Council of the British Association to communicate to you the accompanying letter from Sir A. T. Galt, G.C.M.G., High Commissioner for Canada. This letter was written in reply to one addressed by us to him, making certain inquiries with reference to the invitation to visit Montreal in 1884, which was accepted by the General Committee at the Southampton meeting last year. In that letter it was our endeavour to obtain information as accurate as possible concerning the probable expense of the journey to and from Montreal, including a stay of a fortnight or three weeks in Canada (in addition to the period of the meeting), and excursions to some of the more interesting localities. From the statements in Sir A. T. Galt's letter, the members will be able to form an opinion as to the probable cost of the expedition, the amount of which must obviously, to a considerable extent, depend upon the length of time which they are willing to devote to the visit.

"It is obviously most important to secure that the Montreal meeting should be attended by a strong and thoroughly representative body of members, so that the gathering may be both creditable to the Association and gratifying to our Canadian hosts. Further, many arrangements must be made prior to the meeting, and these must be settled considerably in advance of the usual dates. It will therefore greatly aid the Council and those who will have to carry out their instructions in detail, if you will be so good as to state your intention concerning the visit to Montreal by filling up the annexed form and returning it as addressed before April 14.

"We remain, sir, your obedient servants,
"C. W. SIEMENS, President
"A. W. WILLIAMSON, General Treasurer
"DOUGLAS GALTON, } General
"A. G. VERNON HARCOURT, } Secretaries
"T. G. BONNEY, Secretary"

"9, *Victoria Chambers, London, S.W., March* 3, 1883

"Dear Sir,—I have to refer you to your letter of November 28 on the subject of the visit of the British Association for the Advancement of Science to Montreal in 1884, in accordance with the decision of the general committee, at their meeting at Southampton on August 28 last, and to inform you that I have received a communication from the Chairman of the Montreal Invitation Committee (T. Sterry Hunt, M.A., LL.D., F.R.S.), containing some detailed information on the different matters you mentioned to me.

"It is my pleasant duty to state that the inhabitants of the city of Montreal received with satisfaction the intimation that the Association had decided to honour them with a visit, and much public spirit has already been manifested in the desire that everything should be done to make the occasion worthy of the illustrious body and of the country. Committees on invitation, on finance, and on conveyance have already been formed, and a guarantee fund opened very satisfactorily; while the Government of the Dominion, in view of the widespread interest which the matter has awakened, will ask Parliament during its present session to vote a considerable sum ($20,000) as a contribution to the funds that will be subscribed by the public. Montreal, I may add, is not without experience of the requirements of an important meeting of the kind, having twice been favoured with visits from the American Association, the last occasion being in 1882, when an attendance of more than 900 members and associates was registered, and the Association, with its nine sections, found ample accommodation in the buildings of McGill University.

"I propose to answer your questions in the same order as that in which they were placed in your communication, but it will not be possible for me to do so in such full detail as I should like so far in advance of the time of their application, especially in regard to the cost of conveyance and the various expeditions to be arranged. I trust, however, the following information will be sufficient for the purpose of giving to the members of your distinguished Association an idea of the probable expenses they may be called upon to defray during their stay in Canada.

"(I.) 'The cost of the journey to and from Montreal to one who makes it as a member of the Association or as the near relative of a member.'

"Dr. Sterry Hunt desires me to say that the committee will arrange fifty free passages for the conveyance of the officers of the Association whose attendance is indispensable at its annual meetings. The funds at the disposal of the committee will also enable it to negotiate with the steamship companies for the reduction of the ordinary ocean passages in favour of *bonâ fide* members of the Association. Two courses are open in which this can be done.

"(1) To arrange for a number of passages to be offered at the single rate for the double journey—say 15*l.* 10*s.*

"(2) For a general reduction, so far as the funds will permit.

"Either of these plans can be adopted, but the steamship companies, although fully disposed to entertain the matter, do not care to make any definite engagements so far in advance, which will, I am sure, be readily understood. I am to state, however, that the committee is prepared, with the aid of the Government grant, to devote 3000*l.* to these purposes alone.

"(II.) 'The cost of board and lodging per head per diem for the above during the week of the meeting at Montreal.'

"I cannot do better than quote a paragraph from Dr. Sterry Hunt's letter, in regard to this inquiry:—'In reply to Prof. Bonney's question as to the expenses of board and lodging for members of the British Association during the meeting in Montreal, the committee will give assurance that free entertainment will be provided for at least 150, and probably for all other members who may attend.'

"I may amplify this by stating for your information that the tariff of the Montreal hotels ranges from $2 50c. to $4 per day inclusive, and that private accommodation can be obtained at much lower prices than in England.

"(III.) 'A scheme of expeditions which would occupy from two to three weeks subsequent to the meeting, and the cost of each of them.'

"Dr. Sterry Hunt says:—'As to the proposed excursions, we are prepared to say that the Grand Trunk, the Canada Pacific, and the Intercolonial Railways will furnish free transportation over their lines throughout the dominion of Canada from Nova Scotia to the North-West. The Canada Pacific will also arrange an excursion to the Rocky Mountains, and the Grand Trunk one to the Great Lakes (note: this will include Niagara) and Chicago; while the South-Eastern Railway will do the same for the White Mountains and Portland and Boston. For an excursion of this kind, occupying three or four weeks, tourists should be provided with, say, 20*l*. in money for hotels, carriages, and other incidental expenses, though it is possible that a less sum than this would be needed.'

"I am inclosing a copy of a circular that has been prepared by the Montreal committee. It contains interesting information, and it will be seen that the arrangements are in the hands of representative and eminent men.

"I believe from the information that reaches me that the Association will receive the addition of a considerable number of associates in Canada, and that the visit will give an impetus to scientific research in the Dominion such as it has not experienced before. It is confidently anticipated also that the American Association will hold its meeting in 1884 at a convenient time and place, affording an opportunity for scientific intercourse that I imagine does not often occur.

"I will gladly supply any further information you may require if it is in my power to do so, and shall readily cooperate in any measures having for their object the success of the meeting of the British Association for the Advancement of Science at Montreal in 1884.

"I am, dear Sir, your obedient servant,
"A. T. GALT,
"High Commissioner for Canada, and Vice-Chairman of the Montreal Citizens' Committee
"Prof. T. G. Bonney, M.A., F.R.S., F.G.S., &c.,
"22, Albemarle Street, W."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Savilian Professorship of Geometry in the University is vacant, and an election to the office will be held before the end of Trinity Term (July 7, 1883). A Fellowship in New College is now annexed to the Professorship. The duty of the Professor is to lecture and give instruction in pure and analytical geometry. The combined emoluments of the office from both sources will be, for the present, 700*l*. a year, but may possibly hereafter be increased to an amount not exceeding 900*l*. a year. The qualifications required in candidates for the Savilian Professorships by the existing Statutes of the University are as follows:—"Hos Professores sive lectores, prout voluit fundator, statuimus et decernimus fore perpetuis temporibus eligendos ex hominibus bonæ famæ et conversationis honestæ, ex quacunque natione orbis Christiani, et cujuscunque ordinis sive professionis, qui in mathematicis instructissimi sint, et annos ad minimum sex et viginti nati; et, si Angli fuerint, sint ad minimum Artium Magistri." Candidates are requested to send to the Registrar of the University their applications, and any documents which they may wish to submit to the Electors, on or before Thursday, May 31.

VICTORIA UNIVERSITY.—At a meeting of the University Court on March 30, Vice-Chancellor Greenwood laid on the table the supplementary charter, dated March 20, 1883, enabling the University to confer degrees and distinctions in medicine and surgery. After some discussion it was resolved that the Council be empowered and instructed to appoint external examiners in medicine and surgery for a limited period, and to appoint certain lecturers of the University to act as University examiners; also to prepare, after a report from the General Board of Studies, a statute or statutes and regulations relating to degrees in medicine and surgery for the consideration of the Court, and also to report of the subsequent appointment of external examiners in medicine and surgery, in accordance with the recommendation of the University Council. The Council were instructed to ascertain whether the University charter would allow of the same facilities that had been given to Owens College students being extended to the students of other colleges when those colleges sought admission to the University. The Council were of opinion that such facilities should certainly be given.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, March 15.—Frank Crisp, treasurer and vice-president, in the chair.—Dr. T. S. Cobbold read a paper on *Simondsia paradoxa*, and on its probable affinity with *Sphærularia bombi*. Thirty years ago Prof. Simonds discovered a remarkable parasite within cysts in the stomach of a wild boar which died in the Zoological Gardens, London. Prof. Simonds regarded the worm as a species of *Strongylus*, but Dr. Cobbold in 1864 suggested its affinities might probably be nearer the genus *Spiroptera*, then naming it *Simondsia*. The original drawings unfortunately were lost, and only quite lately, along with the specimens, they have turned up and have enabled Dr. Cobbold to investigate them more closely. He arrives at the conclusion that *Simondsia* is a genus of endoparasitic nematodes in which the female is encysted and furnished with an external and much enlarged uterus, whose walls expand into branches terminating in cæca. The male is ½ inch and the female $\frac{6}{10}$ inch long. Moreover, it is now found that what was at first regarded as the head turns out to be the tail, so that supposed Strongyloid character is incorrect. Taking into account what is known of *Sphærularia bombi* as interpreted by Schneider, and whose views are universally accepted, it appears that *Simondsia*, though unique, yet approaches towards *Sphærularia* in respect of the enormously developed female reproductive organ, which in both lies outside the body proper. Until Sir J. Lubbock's memoir on *Sphærularia* appeared, the so-called male had never been indicated; but, judged by Schneider's interpretation of that genus, the male is still unknown. Dr. Cobbold points out that the so-called rosette in *Simondsia* is morphologically a prolapsed uterus furnished with two egg-containing branches; he regards the external branched processes as homologous with the sphærules of *Sphærularia*, whilst the ultimate cæcal capsules have nothing comparable to them in nature.—A paper was read on the moths of the family Urapteridæ in the British Museum, by Arthur G. Butler. The author, basing distinctions on wing neuration and other characters, redistributes the family, and indicates the following new genera:—*Tristrophis*, *Gonorthus*, *Sirinpteris*, *Nepheloleuca*, *Thinopteryx*, *Xeropteryx*, and *Æschropteryx*.—The eighteenth contribution to the mollusca of the *Challenger* Expedition, by the Rev. R. Boog-Watson was read, in which the author treats of the family Tornatellidæ, therein describing six new species of the genus *Actæon*.

Geological Society, March 7.—J. W. Hulke, F.R.S., president, in the chair.—Messrs. Thomas Gustav Hawley, Richard Lydekker, and J. O'Donoghue were elected Fellows, and M. F. L. Cornet, of Mons, a Foreign Correspondent of the Society.—The following communications were read:—On Gray and Milne's seismographic apparatus, by Thomas Gray, B.Sc., F.R.S.E. Communicated by the President. This apparatus was stated to have for its object the registration of the time of occurrence, the duration, and the nature, magnitude, and period of the motions of the earth during an earthquake. The instrument was made by Mr. James White, Glasgow, and is to be used by Prof. John Milne in his investigations in Japan. In this apparatus two mutually rectangular components of the horizontal motion of the earth are recorded on a sheet of smoked paper wound round a drum, kept continuously in motion by clockwork, by means of two conical pendulum-seismographs. The vertical motion is recorded on the same sheet of paper by means of a compensated-spring seismograph. In details these instruments differ considerably from those described in the *Philosophical Magazine* for September, 1881, but the principle is the same. The time of occurrence of an earthquake is determined by causing the circuit of two electromagnets to be closed by the shaking. One of these magnets relieves a mechanism, forming part of a time-keeper, which causes the dial of the timepiece to come suddenly forward on the hands and then move back to its original position. The hands are provided with ink-pads, which mark their positions on the dial, thus indicating the hour, minute, and second when the circuit was closed. The second electromagnet causes a pointer to make a mark on the paper receiving the record of the motion. This mark indicates the part of the earthquake at which the circuit was closed. The duration of the earthquake is estimated from the length of the record on the smoked paper and the rate of motion of the drum. The nature and period of the different movements are obtained from the curves drawn on the paper.—Notes on some fossils, chiefly Mollusca, from the Inferior Oolite, by the Rev. G. F. Whidborne, M.A., F.G.S.—On some fossil sponges from the Inferior Oolite, by Prof. W. J. Sollas, M.A., F.G.S. Some fossil sponges have been described from the Inferior Oolite of the Continent, but hitherto none have appeared in the lists of fossils from this formation in British localities. The collection of sponges described by the author was made by the Rev. G. F. Whidborne. The author described eleven species (six of which he identified with those already described from Continental localities) belonging to nine genera, and concluded his paper with some general remarks. These sponges are calcareous, but are considered by the author to have been originally siliceous, replacement of the one mineral by the other having taken place as already noticed by him. The beds in which these sponges are found bear all the appearance of being comparatively shallow-water deposits.—On the Dinosaurs from the Maastricht beds, by Prof. H. G. Seeley, F.R.S., F.G.S.

EDINBURGH

Royal Society, March 5.—The Right Hon. Lord Moncrieff, president, in the chair.—Prof. Turner, in a paper on bicipital ribs, described two examples which he had recently come across in the human subject. In both of these cases, one of which closely resembled a specimen in the Anatomical Museum of the University which Knox had explained as due to the fusion of a cervical with a thoracic rib, the real cause was the union of the two first thoracic ribs. That the former explanation was the true one in certain instances was demonstrated by other specimens; and the distinctive peculiarities of each kind of fusion were pointed out.—Sir William Thomson read two papers on gyrostatics and on oscillations and waves in an adynamic gyrostatic system. The papers were in great part experimental illustrations of the theorems regarding gyrostatic stability which are laid down in Thomson and Tait's "Natural Philosophy" (second edition, vol. i. part i. § 345). It was thus demonstrated to the eye that a system when under gyrostatic domination is stable in positions for which, statically considered, the system is unstable as regards an *even* number of degrees of freedom; so that, to take a particular case, a gyrostat which is unstable, because statically unstable as regards one mode, is rendered stable by making it statically unstable as regards two modes. Hence also an ordinary spinning top is stable because it is statically unstable in two of its degrees of freedom. The curious behaviour of a gyrostat resting horizontally on gimbals with its axis of rotation vertical was also shown, viz. its instability as soon as the framework on which it rested was moved in the opposite rotational sense to the spin of the gyrostat. The author then proceeded to point out that all phenomena of elasticity which are ordinarily treated by assuming forces of attraction or repulsion between parts or stresses through connections can be as readily explained by the assumption of connecting links subject only to gyrostatic domination. The gyrostatic hypothesis led to other consequences which the ordinary dynamic assumption did not involve; but it had not been found as yet that elasticity had properties corresponding to these.—Sir William Thomson also communicated a paper on the dynamical theory of dispersion, which was virtually an application of the principle of forced vibrations to a molecular structure, each molecule forming the nucleus of a region whose density increases gradually from without inwards. As bearing upon the same kind of problem, a model was shown illustrating Prof. Stokes' dynamical theory of fluorescence, which is that, if the first of a connected chain of elements is disturbed by a periodic disturbance having no close relation to the free vibration periods of the chain, the disturbance does not pass along the chain, but has its energy stored up in the first few elements, to be given back again when occasion offers.

PARIS

Academy of Sciences, March 19.—President, M. E. Blanchard.—The following communications were read:—Summary description of a new system of equatorials and its installation at the Paris Observatory, by M. M. Lœwy.—Observations of the Swift-Brooks comet made at the Paris Observatory, by M. Périgaud.—Graphic proof of Euler's theorem on the partition of pentagonal numbers, by Prof. Sylvester.—Observations on blue milk (second part), by M. J. Reiset.—On the second edition of the "Pilot of Newfoundland," of Admiral Cloué, and on a question of atmospherical optics, by M. Faye.—Function of the lymphatic vessels in the production of certain pathological phenomena, by M. Alph. Guérin.—The following memoirs were presented:—On the possibility of increasing the irrigation waters of the Rhone, by means of reserves to be established in the lakes of Geneva, Bourget, and Annecy, by M. Ar. Dumont.—Determinations of longitudes effected at Chili, by the Transit of Venus Expedition, by M. de Bernardières.—On the number of the divisions of an entire number, by M. T. Q. Stieltjes.—On the equations to the partial derivatives, by M. G. Darboux.—On the application of the elliptic and ultra-elliptic intervals to the theory of unicursal curves, by M. Laguerre.—Table of reduced positive quaternary quadratic forms of which the determinant is equal or inferior to 20, by M. L. Charve.—Method of obtaining the formula giving the general integral of the differential equation—

$$x^n\frac{d^n y}{dx^n} + A_1 x^{n-1}\frac{d^{n-1}y}{dx^{n-1}} + A_2 x^{n-2}\frac{d^{n-2}y}{dx^{n-2}} + \ldots + A_n y = f(x),$$

by means of a definite multiple integral, by Abbé Aoust.—New equations relative to the transmission of force, by M. Marcel Deprez.—The transmission of force by batteries of electrical apparatus, by M. James Moser.—On the maximum yield which a steam motor may attain, by M. P. Charpentier.—Influence of tempering on the electrical resistance of glass, by M. G. Foussereau.—On a modification into the bichromate of potassium pile to adapt it for lighting, by M. Trouvé.—On the calories of combination of the glycolates, by M. D. Tommasi.—On mononitrosoresorcine, by M. A. Fèvre.—Contributions to a study of the plastering of wires, by M. P. Picard.—Physiological effects of coffee, by M. J. A. Fort.—On salmon-breeding in California, by MM. Raveret-Wattel and Bartel.—On the solenoconchal molluscs of the deep sea, by M. P. Fischer.—Ovogenesis among the Ascidians, by M. Ad. Sabatier.—Influence of the wind on meteorological phenomena, by M. E. Allard.—On the hailstorm of March 9 at the Hyères Salines, by M. Le Goarant de Tremelin.—The Alfianello meteorite, by M. Denza.

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