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THURSDAY, FEBRUARY 24, 1870

THE MINISTER OF PUBLIC INSTRUCTION

ENGLISH politicians have peculiar ways of giving us what we want. For some time every one has felt, as it were, by a sort of instinct, that we shall at last have what all other civilised nations have long known to be indispensable,—a Minister of Public Instruction. Out of Parliament this has been on all sides spoken of as a matter of course. But in Parliament it is different. When the inevitable time comes for it to be spoken of there, it must be carefully avoided, or only coyly glanced at, by those who have thoroughly determined to give it, for etiquette imposes on them the necessity of appearing to yield to external importunity. A very pretty example of this parliamentary coquetry occurred last week on the first reading of the new Education Bill. Mr. Forster displayed no little ingenuity in avoiding the slightest allusion to a provision which everyone of his hearers knew to be absolutely essential to the success of the great measure he was introducing. He said, "The first thing that would suggest itself probably to the minds of all hon. members would be a system of organisation throughout the country," knowing perfectly well that "the first thing" that must suggest itself would be a central authority to create and direct that organisation. But the suggestion must not come from a Minister. It was not long, however, in coming from other quarters. Sir John Pakington, as a leading member of the Opposition, and other speakers, promptly supplied the deficiency, and the proposal was received with "ministerial cheers." All men know that the meaning of this amusing, and no doubt most necessary, little comedy is, that we may now hope soon to have a Minister of Public Instruction. That being the case it may not be amiss therefore that the probable scope of the duties connected with such an office should be briefly considered.

Such a Minister should, we think, take charge of the whole range of natural knowledge in all matters in which the State in any way intervenes to advance such knowledge. We understand the comprehensive term natural knowledge to include Education, Science, the Fine Arts, and Music. Towards the promotion of all these the Government at present, more or less contributes its direct influence. In order to ensure continuity of system and avoid its interruption when the head of the department vacates office with the change of Ministers, it will, we believe, be found necessary to place over each of these subdivisions a permanent, that is unparliamentary, Under Secretary of State; unless indeed the two last, Fine Arts and Music, may be found capable of being united under one head. But Education, Science, and Fine Arts, with Music, should certainly be kept distinct, not only with a view to division of labour, but to the special efficiency of each.

The Education branch would include the national system of compulsory primary education about to be established, public schools, universities, and agencies of all kinds aided by the State, which have for their object the training of youth. It would include also general literature in so far as that is recognised or subsidised in any way by the State.

The Science branch would include all establishments, in receipt of Government assistance, in which Science is taught as a special study; all those in which scientific observations or investigations are conducted under State auspices, and all museums in which natural objects are displayed for scientific purposes.

The Fine Arts branch would include all national collections of paintings, sculpture, and decorative works, national schools of design, the national buildings and monuments, whether of the present or the future.

The Music branch would include subsidies for cultivation of music, and rewards for pre-eminence in the art.

Such, it can hardly be doubted, will be the broad classification. No doubt a debateable ground will be found to exist between the different subdivisions, through which it will be difficult to trace a clear line of separation. For instance, between Science and Fine Arts there will be points of contact requiring perhaps careful re-adjustment; as in the cases of the British and South Kensington Museums, in each of which both Art and Science collections are under one roof. The opportunity presented by the institution of the new Minister is perhaps the last that will ever be afforded us of deciding whether collections differing so entirely in their characters and objects as those of Art and Science should be in juxtaposition. It is certain that the classes who visit collections of Natural History, *bonâ fide* for instruction, are not at all aided, and but little interested, by the immediate presence of antiquities and objects of *vertu*. The classes who only visit museums for amusement, are doubtless interested in both. But in creating a consistent system, which of these two classes should be chiefly considered? If the latter, is there not considerable danger of our falling into the error of making such collections mere places of amusement? Again, the minds best qualified to organise and maintain Art collections do not usually possess equal knowledge of, or feel equal interest in, Science, and *vice versâ*. It seems desirable then that the question of separating the two of kinds collections should now be settled once for all.

Coincident with the re-classifying of our national collections should be an endeavour to engraft on some of them, in which the want has long been felt, arrangements for facilitating the study of their contents.

Assuming that the scope of the new Minister's jurisdiction will be generally such as we have indicated, we may be at ease with respect to the Education branch. This has received so much attention of late, and the whole subject has been so thoroughly mastered by Mr. Forster, who will, let us hope, be our first Minister of Public Instruction, that he will enter with confidence on this part of his duties. But with Science it is far different. Our present very meagre and partial scientific arrangements are dislocated and scattered over every existing department of the State, as if with the express object of putting consistency, system, and efficiency out of the question. The first step towards organising this branch of knowledge must be by collecting facts and opinions relating to it. This step can only be taken through the agency of a Royal Commission instructed to give the widest possible scope to its inquiries into everything relating to both Instruction and Investigation in Science.

PROTOPLASM

As regards Protoplasm, in relation to Professor Huxley's Essay on the Physical Basis of Life. Pp. 68. By James Hutchinson Stirling, F.R.C.S., LL.D., Edinburgh. (Blackwood and Sons. 1869.)

WHEN one of the most powerful representatives of the Transcendental school of philosophy, himself possessing a knowledge of biological science, consents to do battle against the modern doctrines concerning Life and its assumed material substratum, *Protoplasm*, we may expect, at least, that the strongest arguments which can be adduced will be brought to bear against the obnoxious theories and their supposed materialistic tendencies. Still more especially must we prepare ourselves for battle *à outrance*, when the champion that steps forward is one who has already grappled so manfully with the "Secret of Hegel" and is otherwise so distinguished a leader amongst the adverse school of thinkers.

Although differing altogether from Prof. Huxley on the main subject of his essay, we find that in one respect the opinions expressed therein are perfectly in accordance with those of Mr. Stirling. With reference to the views enunciated concerning Comte and his writings, Mr. Stirling says: "I acknowledge in Mr. Huxley's every word the ring of a genuine experience." Mr. Stirling is even more severe on the hitherto much-lauded Positive philosopher than Prof. Huxley himself. He says:—

> There is not a sentence in his book that, in the hollow elaboration and windy pretentiousness of its build, is not an exact type of its own constructor. On the whole, indeed, when we consider the little to which he attained, the empty inflation of his claims, the monstrous and maniacal self-conceit into which he was *exalted*, it may appear, perhaps, that charity to M. Comte himself —to say nothing of the world—should induce us to wish that both his name and his works were buried in oblivion.

When the phrase "Physical Basis of Life" was employed, this was intended to convey the notion that there was one kind of matter, that is, one genus—including an almost infinite number of specifically different, though closely related, kinds—common to all living beings: this matter being named Protoplasm. In connection with this doctrine, though based upon other facts, there was a further inference. Life was believed to be a function of living *matter* rather than of living *form*—whether cellular or other. No secondary visible organisation, at all events, was supposed to be needed, though a primary but invisible *molecular* organisation was deemed all essential for the display of vital manifestations. Such a doctrine being once admitted, there is, as Mr. Stirling points out, no logical halting-place short of Prof. Huxley's further conclusion, "*that all vital action whatever, intellectual included, is but the result of the molecular forces of the protoplasm which displays it.*"

Let us see, then, what is to be said in favour of a *matter* of Life, and how far Mr. Stirling's objections to this doctrine are valid.

At the time when the doctrines of Schleiden and Schwann —that the 'cell' was the ultimate morphological unit capable of displaying 'vital' manifestations—were announced, and long after, the simplest independent living things, whether animal or vegetable, were supposed to be unicellular organisms. *Form* and visible organisation were, therefore, regarded by most as necessary for the manifestation of Life: it was supposed that a more or less spherical structure was needed, possessing a distinct cell-wall, with a nucleus, and other cell contents. But, of late years, increasing knowledge and faithful investigation has necessitated much change of doctrine, in regard to the nature of those simplest parts of complex organisms which are capable of displaying a vitality of their own, and in regard to the nature of the simplest independent living things.

Some of the principal modifications in the 'cell' doctrine of organisation are thus sketched by Mr. Stirling himself:—

> The first step taken in resolution of this theory was completed by Max Schultze, preceded by Leydig. This was the elimination of an investing membrane. Such membrane may, and does ultimately form; but, in the first instance, it appears the cell is naked. The second step in the resolution belongs, perhaps, to Brücke, though preceded by Bergman, and though Max Schultze, Kühne, Haeckel, and others ought to be mentioned in the same connection. This step was the elimination, or at least subordination, of the nucleus. The nucleus, we are to understand now, is necessary neither to the division nor to the existence of the cell.
>
> Thus, then, stripped of its membrane, relieved of its nucleus, what now remains of the cell? Why, nothing but what *was* the contained matter, the intracellular matrix, and *is*—Protoplasm.

Thus, then, we seem to reach our elementary life-stuff—our living *matter*. But Mr. Stirling would warn us against coming to any such conclusion—he will not so easily yield. He tells us we are quite wrong if we think we have got rid of the cell and have reduced ourselves to a simple matter of Life. And why? "Because," as he says, "all the great German histologists still hold by the cell, and can hardly open their mouths without mention of it." But, if this be so, after what Mr. Stirling has himself told us, to what else can we ascribe the practice, save to a seeming reality of that reputed fondness in the German mind for courtesy or conventional titles? Some such excuse there may be, but assuredly no other. We would ask Mr. Stirling to reconsider the bearing of the statements which he has himself adduced. In place of the old morphological vital unit—the cell—with its definite characters, we are reduced to a mere naked, non-nucleated bit of protoplasm, as the simplest material substratum adequate to display all those vital manifestations, previously considered to be the essential attributes of the formed elements above mentioned. The power of displaying vital manifestations has, in fact, been transferred from definitely formed morphological units, to utterly indefinite and formless masses of Protoplasm. Instead, therefore, of an obvious *form* of Life, we are reduced to a *matter* of Life, presenting no appreciable morphological characters. It becomes evident, moreover, that if the old term "cell" is still applied to these mere bits of living stuff or protoplasm—not because they are morphological units, but simply because biologists have been compelled to transfer the power of manifesting vital characteristics to such indefinite protoplasmic masses—then this term, thus employed, must be seen to have so entirely lost its old signification, that it can be regarded only as a mere courtesy title. Vital power has obviously been transferred from a definite morphological unit—the cell—to mere living matter, and if any people do persist in still calling a portion of such mere matter by the name of the morphological unit, simply because this was of old also *assumed* to be the vital unit, *we* must not allow such mere confusion in language to confuse us as to the real facts and inferences.

There is another point of view, also, to which Mr. Stirling does not seem to have given an adequate attention. The old doctrine did well enough at a time when the lowest known living things were "unicellular organisms," closely approximating in their characters to those morphological units of which the higher plants and animals are built up. But, since our knowledge has increased—since we have become more familiar with the various living things constituting the lowest groups of Professor Haeckel's third organic kingdom—PROTISTA—the maintenance of such doctrines has become im possible. Do we not now know that although the *Protoplasta* are amœboid animals possessing the old cell characters—that is, having a distinct nucleus, and a definite bounding membrane—there are, nevertheless, adult animals, leading an entirely independent existence, composing the lowest group *Monera*, some of wh ich have no bounding membrane, though they have a nucleus, whilst others, simpler still, are mere bits of protoplasm—naked, non-nucleated, structureless? Yet, such minute, homogeneous, and altogether indefinite bits of protoplasm, are as capable of displaying the fundame ntal characteristics of life, as are the more definite unicellular organisms to which such attributes were formerly supposed to be restricted. Without visible structure, they nevertheless assimilate materials from their environment, and grow; they constantly vary their shape, and are capable of executing slow movements; though possessing no nucleus, they are able to divide and reproduce their kind.

It seems only fair to mention in this place, that so far back as 1853, before the doctrine as to the constitution of the "cell" had undergone such a modification, or rather, as we should have said, before it had been generally acknowledged that vital manifestations could be displayed by mere bits of protoplasm lacking the supposed necessary elements of *form*, Professor Huxley had put forth a powerful remonstrance against the then all-prevalent "cell theory" of organisation.* His opinions were announced even five years before Virchow, the last great champion of the doctrine, issued his celebrated "Cellular Pathologie." Following, in the main, the doctrine of Wolff and Von Baer, Prof. Huxley contended that the primitive organic substance is a homogeneous plasma, in which a certain differentiation takes place, but that there is no evidence whatever to show that the molecular forces of this living matter ("vital forces" of most modern writers) are, by this differentiation, localised in any one part rather than in any other part—be it cell, or be it intercellular tissue. "Neither is there any evidence," he says, "that any alteration or other influence is exercised by the one over the other; the changes which each subsequently undergoes—though they are in harmony—having no causal connection with one another, but each proceeding, as it would seem, in accordance with the general determining laws of the organism." Whilst believing that the *periplast*—corresponding with the cell-wall and intercellular tissue of other writers—is the seat of all the most important metamorphic processes out of which the various tissues are produced, he also believes that this differentiation is not brought about by any mysterious action on the part of the cell or nucleus, but that it is rather the result of intimate *molecular* changes taking place in the plastic matter itself, after a definitely successive though inexplicable fashion. Prof. Huxley's fundamental position was, in fact, that "the primary differentiation is not a *necessary* preliminary to further organisation—that the cells are not machines by which alone further development can take place; they are rather mere indications of accustomed modes of evolution." This main position he has further illustrated by saying: "We have tried to show that they [the cells] are not instruments, but indications—that they are no more the producers of the vital phenomena than the shells *scattered in orderly lines* along the sea-beach, are the instruments by which the gravitative force of the moon acts upon the ocean. Like these, the cell only marks where the vital tides have been, and how they have acted."

* Brit. and For. Med. Chir. Review, 1853, p. 306.

Certainly the essence of this doctrine is, that the vital forces are "molecular forces"—that they are not dependent upon morphological forms or "cells" and, therefore, that essentially vital manifestations may take place in mere formless living matter—Protoplasm, if you will. As we have just seen, this is precisely the doctrine to which so many other distinguished biologists have now given in their adhesion. They too—Max Schultze, Haeckel, Kühne, and others—have gradually recognised that a something of definite form is no longer necessary: that there are independent living things, even lower in the scale than the old "unicellular organisims": that to constitute one of these, or to constitute a vital unit of one of the higher living things, all that is needed is mere formless, indefinite Protoplasm—or, as Mr. Stirling contemptuously expresses it, a mere "shred" of the matter of Life.

Much of what immediately follows, in Mr. Stirling's essay, we consider to be somewhat irrelevant. We think it has been written under the influence of a misconception as to Professor Huxley's real meaning. Mr. Stirling argues against Protoplasm, on the assumption of its being a substance definite in kind—as definite, we may say, as chloride of sodium—whilst apparently, Professor Huxley's meaning was rather that Protoplasm was the name of a genus of matter, or else of a species including almost innumerable varieties: that it was a proteinacious substance, in fact, of which there might be as many hundreds of isomeric modifications, as there are similar varieties of protein itself.

We regret that we are unable to follow Mr. Stirling into this second part of his essay. It seems to us to be the most interesting part of it, and we recommend our readers to study it for themselves. Much, however, of its reasoning, is, for us, deprived of its seeming cogency, because we cannot agree with Mr. Stirling in his previous conclusion as to the non-existence of a matter of Life. This, as he fully admits, is the really fundamental question about which the difference of opinion exists. And, if we cannot agree with him upon this first point, it is useless for us to follow him into his subsequent reasonings. We cannot, however, but admire his candour when he says:

It is to be acknowledged . . . that Mr. Huxley would be very much assisted in his identification of differences, were but the theories of the molecularists* on the one hand, and of Mr. Darwin on the other, once for all established. The three modes of theorising indicated, indeed, are not without a tendency to approach one another; and it is precisely their union that would secure a definitive triumph for the doctrine of materialism. Mr. Huxley, as we have seen—though what he desiderates is an autoplastic living *matter* that, produced by ordinary chemical processes, is yet capable of continuing and developing itself into new and higher forms—still begins with the egg. Now, the theory of the molecularists would, for its part, remove all the difficulties that, for materialism, are involved in this beginning; *it would place protoplasm undeniably at length on a merely chemical level; and would fairly enable Mr. Darwin, supplemented by such a life-stuff, to account by natural means for everything like an idea or thought that appears in creation.*

Nothing could be more outspoken and candid than this utterance of Mr. Stirling. He evidently believes that such doctrines of the "molecularists" concerning the new evolution of living things will have long to "await the proof"; but we, on the contrary, firmly believe the time to be not far distant when this will be as much an accredited dictum of science as are the other doctrines of the Correlation of the Physical Forces, and of the Correlation of the Vital and Physical Forces which have been its necessary predecessors. We would ask the Transcendentalists, at least, to speculate upon the possibility of this. Let them learn in the meantime how they may best readjust their doctrines, so that when the time comes in which such change will be absolutely necessary—if their views are to be in accordance with the established truths of science—there may be no sudden bewilderment, no feeling as if the very ground were being swept from underneath their feet. To such a thinker as Mr. Stirling, we should imagine the necessary modification of doctrine would not prove difficult. For, after all, the acceptation, to the fullest extent, of the doctrines concerning Life to which we have been alluding, involves, even from the Transcendentalists, only a somewhat different point of view. So long as Matter, and Force or Spirit, are but two aspects of a something one and indivisible, there is still room for opposite philosophical systems. The old questions may be discussed as earnestly as ever by those who have the leisure and the taste for such ontological inquiries. And, if perchance lured into such discussions, it would often be found that he who was most vehemently charged with Materialism would, from an ontological point of view, prefer to rank himself amongst those who professed the principles of a pure Idealism.

H. CHARLTON BASTIAN

AGRICULTURAL CHEMISTRY

How Crops Grow: A Treatise on the Chemical Composition, Structure, and Life of the Plant, for Agricultural Students. By Samuel W. Johnson, M.A., of Yale College, U.S. Revised, with numerous additions, and adapted for English use, by A. H. Church and W. T. Thiselton Dyer. 1 vol. 8vo., pp. 399. (London, 1869.)

THIS revised edition of an excellent American work ought to find its way into the hands of a very numerous class of youths whose future avocations will require a special acquaintance with the phenomena concerned in the growth of plants—either naturally or under the influence of cultivation—some familiarity with the general structure of plants, with the functions of their several organs and the nature of the various materials which it is the work of vegetation to produce for the food of animals and for numerous other purposes.

It is surprising that in a country where the practice of agriculture is one of the chief sources of wealth and is, directly or indirectly, the means of employing a vast amount of capital and labour, very little should have been done towards the scientific elucidation of agricultural practice so as to ensure improvement of the art. Yet such is still the case in this country with some rare exceptions like the experimental farm at Rothamsted, where Messrs. Lawes and Gilbert have done so much to aid the farmer in applying manures and cattle-feeding materials to the best advantage. The Royal Agricultural Society, the Highland Agricultural Society, and the Agricultural College at Cirencester, have also contributed towards the attainment of the same object; but, as a rule, agriculture is practised almost exclusively under the guidance of mere traditional principles and habitual routine, without those engaged in this business having any appreciation of the phenomena and natural laws which govern the growth of plants, even so far as they are known to science. This fact is, in part, no doubt a consequence of the general disregard of scientific teaching in this country; but it is also referable in some degree to the absence of any organised scientific investigation of plant-life in relation to agriculture, such as that carried on of late years in Germany by the aid of the various governments and with the hearty support of farmers. It is indeed strange that in a country like ours, where agriculture is no longer a mere

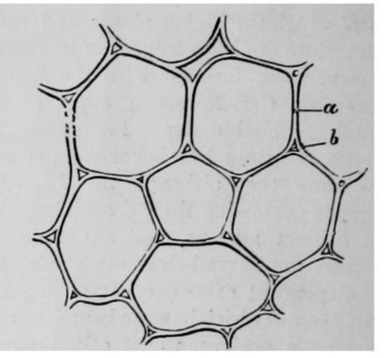


Fig. 1.—Section of cells in a cabbage stem, shewing at *a* the union of the cell-walls and the intercellular spaces at *b*.

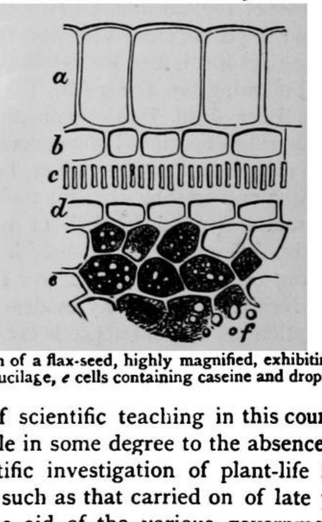


Fig. 2.—Section of a flax-seed, highly magnified, exhibiting *a* cells containing mucilage, *e* cells containing caseine and drops of oil *f*.

* Mr. Stirling so designates those who believe in the possibility of an evolution of living things; or, in other words, those who believe in the possibility of a so-called "spontaneous generation."

gathering of the natural produce of the earth, but has become, more thoroughly than elsewhere, the art of manufacturing food, there should not be an activity in the teaching and prosecution of agricultural science proportionate to that existing where agriculture is in a more primitive state.

However, a beginning is being made in this direction, and it is to aid students in acquiring a knowledge, both technical and scientific, of the less obvious aspects of plant-life, that the volume mentioned above has been written. The classification of the subjects treated of is

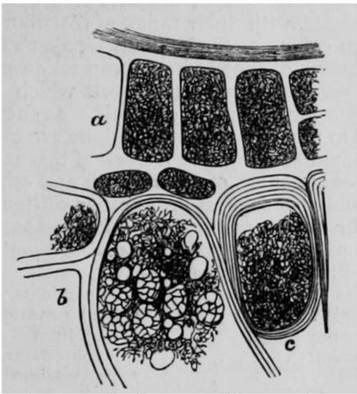


Fig. 3.—Section of outer cells of an oat grain; *a* and *c* containing chiefly caseine, and *b* starch granules.

adapted both to this object and to the wants of the lecture-room, while the illustrations of particular phenomena are in all cases chosen with reference to agricultural practice.

The first division is devoted to the chemical composition of the plant. Here the elementary substances of which plant organs and their materials consist are briefly described, chemical force is defined and the technical language of chemistry explained; then follows an account of the characters and composition of substances which are common products of vegetation in all its forms, of others which are more or less peculiar to certain plants; and lastly, the important subject of the ash of plants is discussed in regard to its general and special character, the extent to which its constituent parts are accidental or essential, and the functions it or its several parts may perform in the growth and development of the plant.

The special organized forms in which the common materials of plants present themselves are well illustrated

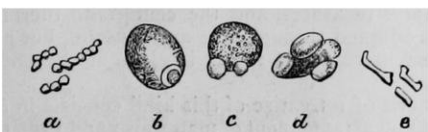


Fig. 4.—Forms of various albuminoid grains; *a* from vetch seed; *b* from the castor bean; *c* from flax seed; *d* from *Myrica Cerifera*; and *e* from the aril of the nutmeg.

by numerous woodcuts, of which figs. 1, 2, 3, and 4 will serve as examples.

Copious references to special investigations and monographs are given throughout this division of the book; but unless we have credit for doing even less than has been done in this country, it would seem that the references are too exclusively to German authorities. Only in one instance are the elaborate observations of Lawes and Gilbert mentioned (p. 316), and French experimenters are also but rarely referred to.

One excellent feature of this division consists in the tabulated statements of the amounts of particular constituents contained in various plants, in a form that will be not only instructive to the student, but very useful for reference.

The second division deals with the structure of the plant, the offices of its organs, the nature of the plant cell, the organs of nutrition and of reproduction, with their several functions.

The third division treats generally of the life of the

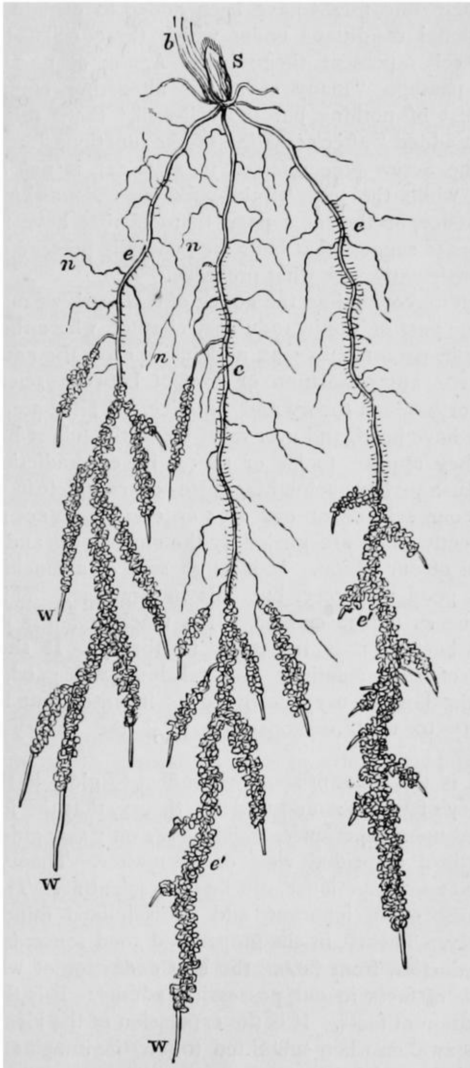


Fig. 5.—Young wheat plant with earth adhering to roots at *ė*, while the root tips W, and the other parts of the primary roots *e*, and of the secondary roots *n* are bare. *S* is the seed and *b* the blade.

plant, describes how the seed germinates, what is the food of plants, how it is taken up, the nature and the motion of the sap and the various forces by which this is influenced, the mode of reproduction, and, lastly, the death of the plant.

Then follows an appendix, with copious tables of the composition of agricultural plants and products, cattle feeding materials, &c., which cannot fail to be useful.

The first and third divisions of this work seem unexceptionable; but the more purely botanical portion might be improved by revision, thus, for instance, at p. 178, cystoliths and crystallised concretions in plant-cells are treated of in a manner which suggests want of familiarity with the microscopic examination of plants, and is calculated to make a beginner suppose all the crystalline concretions of plants are cystoliths of some sort.

At p. 216, it is stated that "In some cases, cells consist only of protoplasm and nucleus, being destitute of cell-walls during a portion or the whole of their existence," and a single line might have been added to explain the exceptional conditions under which these cells—that are not cells—present themselves. Again, at p. 218, after the passage "many cells are altogether empty, and consist of nothing but the cell-wall," there might have been added "after cessation of the functions."

The Screw pine referred to at p. 227 is not a palm; and while the term "imbricated buds" is awkward, the reference, on the same page, to roots that have no buds seems to suggest that buds are generally present on roots inconsistently with what precedes.

Before concluding the notice of this work we must refer to one part of the introductory chapter, where the author very justly condemns as a delusive error the notion that there is any opposition or conflict between science and art, or between theory and practice. "They are, as they ever have been, and ever must be, in the fullest harmony. If they appear to jar or stand in contradiction, it is because we have something false or incomplete in what we call our science or our art; or else we do not perceive correctly; but are misled by the narrowness and aberrations of our vision. It is often said of a machine, that it is good in theory, but fails in practice. This is as untrue as untrue can be. If a machine fail in practice, it is because it is imperfect in theory. It should be said of such a failure—the machine was good, judged by the best theory known to its inventor, but its incapacity to work demonstrates that the theory had a flaw."

It is the boast of some who affect to glory in the sufficiency of practice and to decry theory, that the former is based upon experience, which is the only safe guide. But this is a one-sided view of the matter. Theory is also based upon experience, if it be truly scientific. The vague surmise of an ignorant and undisciplined mind is not theory. Theory, in the proper and good sense, is always a deduction from facts—the best deduction of which the stock of facts in our possession admits. It is the interpretation of facts. It is the expression of the ideas which facts awaken when submitted to a fertile imagination and well-balanced judgment.

If the appreciation of these views were at all equal to their truth, and if the importance of their bearing on the advancement of agriculture were at all adequately recognised, there would probably be little reason to lament the want of attention either on the part of the farmer or the statesman, to the scientific aspects of that pursuit, and less scope for that blatant obstructive, the "practical man," who shuts his eyes and ears against everything his grandfather did not know of, believes only in the folly of wisdom, and is supremely happy in his own ignorance.

OUR BOOK SHELF

A Geographical Handbook of all known Ferns, with Tables to show their Distribution. By K. M. Lyell. (Murray, 1870.)

THIS useful and unpretending, but elegant little volume, consists of two parts. In the first, the genera and species of ferns are enumerated under a number of geographical divisions and subdivisions, which appear to have been judiciously selected. The stations, habitats, and geographical range of each species, are given with much care, and the authorities fully quoted. It thus forms a series of fern catalogues for eighteen divisions of the globe. The second part consists of a systematic list of all the species, with their range of distribution indicated in eighteen columns. Sir William Hooker's arrangements and limitations of species have been followed throughout, and this gives a unity to the work which has its value. But as ferns have generally so wide a range that genera restricted to any one part of the globe are exceptional, we think it would be as well in a work of this nature, to adopt the additional genera of John Smith and others.

We would also suggest for another edition, that a summary of the genera and species might be usefully given at the head of each geographical subdivision. Thus for "Europe Proper" we should have:

1.	Woodsia	2 species	13.	Asplenium	17 species
2.	Dicksonia	1 ,,	14.	Scolopendrium	2 ,,
3.	Hymenophyllum	1 ,,	15.	Aspidium	2 ,,
4.	Trichomanes	1 ,,	16.	Nephrodium	6 ,,
5.	Davallia	1 ,,	17.	Polypodium	4 ,,
6.	Cystopteris	4 ,,	18.	Nolhochlæna	2 ,,
7.	Adiantum	2 ,,	19.	Gymnogramme	2 ,,
8.	Cheilanthes	3 ,,	20.	Osmunda	1 ,,
9.	Cryptogramme	1 ,,	21.	Ophioglossum	2 ,,
10.	Pteris	4 ,,	22.	Botrychium	5 ,,
11.	Lomaria	1 ,,			—
12.	Woodwardia	1 ,,			55 species

Such summaries would offer useful materials for comparison, and show at a glance what genera were abundant, rare, or wanting, in a given district. We also think the specific names should have been printed with some difference of type, so as more readily to catch the eye; but these are small matters in so useful a work, which must have been a labour of love to its author, and which no lover of ferns should be without. A. R. W.

Agricultural Analysis.—*Agricultural Qualitative and Quantitative Chemical Analysis.* After E. Wolff, Fresenius, Krocker, and others. Edited by G. C. Caldwell. Pp. vi. and 307. 8vo. (New York: Judd. London: Trübner, 1869.)

MR. CALDWELL (Professor of Agricultural Chemistry in the Cornell University) prepared this compilation for the use of his own pupils and agricultural students generally. Many of the chapters consist of translations from Wolff's "Anleitung," and much more is taken from Fresenius's well-known works on analysis. The metric system of weights and measures, and the centigrade thermometric scale are adopted throughout; and a useful, but not sufficiently extensive set of tables is given at the end of the work.

The merit of a treatise of this kind consists in a proper selection and arrangement of materials; and Prof. Caldwell seems to have performed his task satisfactorily, though, as he admits, somewhat hastily. It is hardly necessary to add, that the book would be quite as much out of place in the hands of an unassisted student as any of those of which it is an adaptation. Under the guidance of a teacher, however, it would undoubtedly be of much service in an agricultural laboratory.

Jahrbuch der Erfindungen. H. Hirzel und H. Gretschel. (Leipzig: Quandt und Händel, 1869.)

THIS is one of a type of books which is not published in this country, either because our publishers are not sufficiently energetic, or because our public has not as yet sufficient interest in science. We may hope that in the yearly volumes of a periodical like NATURE, the general reader will find everything brought down to the latest date, with a sufficiently clear account of all important discoveries. There is needed, however, a sort of "bird's-eye view" book of the year's work, in which a great deal that emerges every week and is of little permanent importance, may be left out, and really considerable discoveries alone given. It is necessary, besides, that the narrative of these discoveries should not be written from the point of view of the man of science, who knows at once where to place them in relation to his previous knowledge. A brief account must be added of that part of what has been accepted or known which the new acquisition illustrates, supplements, or contradicts.

Thus in the description of Wüllner's interesting investigations, confirming those of Frankland and Lockyer as to the alterations which take place in gas spectra when the pressure of the gas is altered, it is necessary to give some account of the previously accepted doctrine of Plücker's "spectra of the first order," and "spectra of the second order." Again, it is useless to describe discoveries relating to circular polarisation without recalling to the general reader the meaning of the term.

We need not attempt in this place to give any account of the new discoveries which are explained in the *Jahrbuch*. It is a closely-printed volume of 416 pages, with 43 illustrations, and its execution appears to us as excellent as its plan. There is a full account of the acquisitions in solar and stellar physics, which have made the year 1868 memorable in science. In molecular physics we have Graham's discussion of the absorption of hydrogen by palladium. In acoustics we have a careful compendium of Regnault's recent laborious and excellent work on the velocity of sound propagation and of the less gigantic experiments by Kundt, which have since been followed out by Schneebeli and Seebeck with promise of bringing us to results more interesting and important from a theoretical point of view than those of Regnault. In heat there is a full account of the investigations, especially with regard to dark heat by Magnus and Dessains. We have all the modifications and improvements in "influence machines," like that of Holtz, which have been realised in the year. For domestic readers there are 25 pages on the sewing machine—single stitch, double stitch, and lock stitch, and 7 more on the knitting machine. Finally, there is a mass of detail, much of it extremely interesting, on the latest chemical discoveries, appropriately introduced by an account of Bunsen's method of washing and filtering precipitates under the pressure of a column of water. W. J.

Mohr's Titrirmethode.—*Lehrbuch der Chemisch-Analytischen Titrirmethode.* Von Dr. Friedrich Mohr. Third and improved edition. Part I. (Brunswick: 1870.)

WE are glad to see that Dr. Mohr's well-known work on volumetric analysis has reached a third edition, and that the author, who well deserves to be called the foster-parent of this branch of chemistry, has taken advantage of the opportunity to recast it entirely. We cannot do better than state his own account of its contents :—

"The work proffers, in the first place, an introduction to the manipulation and use of instrumental appliances, of which the best forms are completely described. Then follows Alkalimetry, under which are comprised all analyses that terminate with the saturation of acids and alkalies. The determination of potash, soda, ammonia, earths, and free acids generally is here described.

"The third section embraces analyses by reduction and oxidation, and especially their subdivision, according as permanganate, dichromate, or iodine solution is added in the final stage. It will be found to contain the determination of chlorine, iodine, bromine, chromic acid, all peroxides, and generally all substances which evolve or combine with oxygen, chlorine, cyanogen, &c. The next section includes analyses by precipitation, the estimation of silver, chlorine, cyanogen, copper, lead, &c.—where a precipitation begins or ends the process.

"The conclusion of the work" (which has not yet reached us) "is the practical part, which teaches the application of individual methods to the entire course of an analysis. Complete methods are stated for the analysis of alkalies, salt-cake, mineral waters, soils, guanos, and the ores of copper, zinc, and iron."

Those of our readers who are acquainted with the previous edition will perceive, from the above account of the contents of the present one, that Dr. Mohr has very much extended his original plan. Accordingly the new *Lehrbuch* contains six additional sheets, and thirteen fresh woodcuts. We need hardly say that the author has executed his work with the detailed care and enthusiasm which are known to characterise him; and the numerous illustrations, for which Messrs. Vieweg are responsible, are as remarkable for sharpness and portraiture as those in most English manuals are deficient in these respects. We shall feel much interested in reading the conclusion of the book, which will supply a want long felt in certain departments of manufacture. For the sake of English experimenters who are not familiar with German, a translation of the entire work, adapted to the prevailing notation, would be very desirable.

THE TRIAL OF THE PYX

THE trial of the Pyx is the formal testing of the coin of the realm, to ensure its being of the requisite weight and fineness. The name is derived from the Pyx, or chest, in which the coins selected for the purpose are contained. The first trial of the Pyx took place in the ninth and tenth years of Edward I. And as the last observance of this ancient ceremony was held during the past week, a few brief notes may not be without interest.

The authority under which the trials were made varied considerably. First, the members of the King's Council, then the Barons of the Exchequer constituted the court, King James I. presiding at one trial. The court now consists of several members of the Privy Council, under the presidency of the Lord High Chancellor and a jury selected from the Hon. Company of Goldsmiths.

Last week the high officers of the Mint assembled at the Treasury, and in their presence the Lord Chancellor charged the jury to examine the coin of the late Master of the Mint, Thomas Graham, F.R.S., and to ascertain whether it was within the latitude of "remedy" allowed by law.

This remedy amounts to 12 grains on each troy pound of gold coin, or to 0·257 grain on each sovereign; and 24 grains on each pound troy of silver coin. Portions cut from standard test-plates were handed to the jury who adjourned to Goldsmiths' Hall. They then opened the Pyx-chest and tested the coin by weight; having done this, a certain number of gold coins were melted into an ingot, which was then assayed; the same process being adopted with the silver coin. In the present instance the Pyx represented a coinage of 14 millions gold and 1 million of silver coin; the verdict of the jury being, that the coin both as to weight and fineness was within the remedy allowed by law. The details, however, were most favourable to the late illustrious Master who has so lately passed away.

An adverse verdict would probably have been followed by no more serious penalty than the forfeiture of the Master's sureties, but it is interesting to note that in the reign of Henry I. the money was so debased as to call for the exemplary punishment of the "Moneyers," while in Anglo-Saxon times the chief officer or Reeve would have been punished by the loss of his hand should he fail to clear himself of the charge of producing false coinage.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Red-Necked Grebe

A FINE specimen of the Red-necked Grebe, picked up alive, but wounded, near Bedford, on the 11th of February, has been sent to me dead, and is being stuffed. It is a female, in winter plumage.

Taunton, Feb. 16 W. TUCKWELL

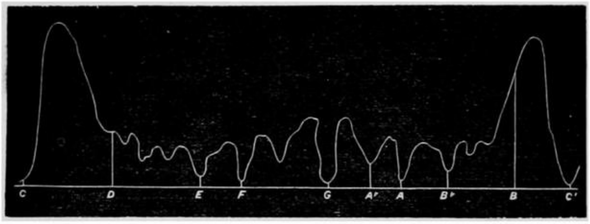
Professor Listing's Amplifier

IN reference to your report of the Boston Natural History Society in NATURE of 27th January, nothing more is requisite to amplify the power of a microscope than to cut off the rims of two or three eye-pieces and insert them in pairs into the ends of a draw tube. Any degree of *amplification* can be obtained whilst the achromatism is preserved. The Huyghenian eye-piece has generally been preferred because the dust accumulating on the inverted eye lens of a positive eye-piece is inconveniently magnified, and obscures the field of view. To those who are desirous of trying Professor Listing's plan, described in your last number, this simple method of mounting may be acceptable.

Lansdowne Crescent, W. ROYSTON PIGOTT

Analogy of Colour and Music

IT appears to me that in the discussion raised by Mr. Barrett's letter in your columns, too little attention has been paid to differences between harmony in music and harmony in colour, which are sufficiently great to show that the coincidences pointed out cannot be regarded as more than *numerical*. Your correspondents have hitherto regarded the subject rather from an optical than a musical point of view. I propose, with your permission, to make a few remarks from the latter stand-point. It is well known that to get a good concord, exact tuning is requisite—*i.e.*, that a slight deviation from the right pitch is sufficient to make a concord into a discord. Moreover, the better the concord, the more intolerable is any appreciable variation of its pitch. Thus unisons, fifths, and octaves are the most sensitive to defective tuning, while the intervals adjacent to them, such as minor seconds, sharp fourths, and sevenths are the harshest discords in the scale. The degree to which this holds will be seen at once by the following diagram, roughly copied from Helmholtz's "Ton-empfindungen." The ordinates of the curve represent the amount of "roughness"—*i.e.*, discordancy corresponding to the intervals marked on the line of abscissæ. The *quality* of tone selected is that of the violin.



A glance at the figure will show that the sounds which produce the most discordant effects with the key-note lie in the immediate neighbourhood of the unison, the octave and the fifth; and further, that a very slight departure from accurate pitch in any concord will provoke a harsh dissonance.

If we now turn to the spectrum, the state of things is widely different. The various colours shade off insensibly one into the other, and in any one colour there is very little, if any, change of tint, except close to its extremities. Thus, then, as far as mere colour goes, any part of any one colour-division produces an equally harmonious effect on the eye, and has an equal claim to be compared to a concord of the gamut.

Mr. Barrett, by taking the *central point* of each band of the spectrum as the basis of his comparison, has left this important circumstance out of sight. If we take it into consideration, the result will, I think, be to deprive Mr. Barrett's comparison of any serious importance.

In order to fix our ideas, let us assume that the *mean* red of the spectrum corresponds to *middle C* of the pianoforte with 264 vibrations per second. Taking the limiting and mean wave-lengths as quoted from Prof. Listing by Mr. Barrett, and calculating the number of corresponding sound-vibrations per second, we get the following table :—

	Wave-lengths in 10 millionths of a millimetre.	Number of sound vibrations per second.	Corresponding positions on gamut, with number of vibrations.	
	7234	250	247·5	B
Red	6853	264	264	C
	6472	280 −.	282 −	C♯
Orange	6164	293·5.		
	5856	309 −	297	D
Yellow	5601·5.	323 −.	317 −	D♯
	5347	338 +.	330	E
Green	5133	352·5.	352	F
	4919	368 −		
			376	F♯
Blue	4737	382 −		
			396	G
	4555	397 +.		
Indigo	4398	411 +		
	4241	427 −	422	G♯
Violet	4104	441 −		
			440	A
	3967	456 +.		
			469 +	A♯

(+ or − means that the number of vibrations given is a fraction less than ½ above or below the actual amount. In the last column are given the vibration-numbers of the sounds of the chromatic scale.)

By comparing the last two columns it will be seen that Red covers very nearly the portion of the scale between B natural and C sharp: Orange that from C♯ to half-way between D and D♯, and so on. The whole relation is exhibited in the following figure :—

Red Orange Yellow Green Blue Indigo Violet

It will be seen that each colour, on the whole, corresponds to *very discordant* intervals in the scale of pitch, and that only at one point in each (and that by no means always the middle point of each band) can it answer to a concord.

What has been said is probably enough to show how little real "correlation" Mr. Barrett's letter has succeeded in establishing. I will now notice the observations on Newton's rings contained in that signed "W. S. Okely." The diameters of these rings vary in lights of different refrangibility, as the *square root* of the wave-lengths,* and therefore they give a spectrum unfit for the purpose to which Mr. Okely applies them. Indeed this is otherwise obvious from the lengths of these diameters given in his letter from a work by Prof. Zannotti. They are as 1, $\sqrt[3]{\frac{8}{9}}$, $\sqrt[3]{\frac{5}{6}}$, $\sqrt[3]{\frac{3}{4}}$, $\sqrt[3]{\frac{2}{3}}$, $\sqrt[3]{\frac{3}{5}}$, $\sqrt[3]{\frac{9}{16}}$, $\sqrt[3]{\frac{1}{2}}$, and therefore, of course, the intervals between them are not as Mr. Okely makes them, $\frac{9}{8}$, $\frac{16}{15}$, $\frac{10}{9}$, $\frac{9}{8}$, $\frac{10}{9}$, $\frac{16}{15}$, $\frac{9}{8}$, but the *cube roots* of these fractions. The interval from red to red $\sqrt[3]{\frac{1}{2}}$, comes out rather less than 1·26, which lies between a major third and a fourth. Mr. Okely's view, which requires the interval from red to red to correspond to an octave, must therefore fall to the ground at once.

Mr. Deas' conception of "millions" of violinists performing "every conceivable sound within" the octave, with a view to the production of the "purest and most ethereal of sounds," seems to me the wildest delusion. Let us suppose the comparatively modest number of thirty-three at work on the interval from middle C to D, so that the first produces 264 vibrations per second, the second 265, the third 266, and so on up to the last, with 297 vibrations. We should obtain charming variety of *beats*, one a second, two a second, three a second, &c., up to thirty-three a second, those near the higher limit adapted to produce the very worst barking dissonance attainable. Mr. Deas' million-fiddler-power sound, so far from being "pure" or "ethereal," would not be a musical sound at all, but a mere bewildering chaos of noises, likely to drive the inventor himself,

* See Airy's Tracts or Lloyd's Wave Theory of Light.

after one trial of this *spectral* music, to take refuge in the awful solitudes described by Dante:—

" —— dove *il Sol tace.*"

Trinity College, Cambridge SEDLEY TAYLOR

Solar Spots Visible to the Naked Eye

OWING to the smoke and clouds which generally obscure the sky at Glasgow, opportunities to observe the phenomena of the heavens are very rare.

When gazing at the sun this morning (February 16th), I observed on its disc a dark line on the upper half of the disc. In order to convince myself that it was not a delusion, I directed a small pocket telescope (magnifying power about six times linear) and observed several spots. The principal one, as near as I could guess, was 5′ long by 1′ broad.

I would be glad to hear the extent of it from any one of your correspondents, who has measured it, as it must have been of enormous dimensions.

Argyle Street, Glasgow ROBERT M'CLURE

[The dimensions of this spot have been taken by M. Tremischini, who communicated his observations to the French Academy of Sciences at the meeting on the 14th inst., as will be seen from our report of the proceedings further on.—ED.]

Flight of Birds

IN reply to J. H.'s query respecting the flight of the albatross mentioned in a paper of mine on the flight of birds, read at the November meeting of the Norfolk and Norwich Naturalists' Society, I beg to assure him that no bird is able to fly without flapping its wings.

The birds observed by your correspondent's brother were performing one of the most beautiful feats of "wingmanship"—a feat which can only be indulged in, to any extent, by birds possessed of a superabundance of wing-power. The albatross is the great master of this style of flight. Having by repeated flappings of the wings raised itself into the air, and acquired a certain degree of velocity, it brings its body and outstretched wings to such an angle that the pressure of the breeze against its surface is sufficient, or nearly so, to neutralise the force of gravity; it can then "sail" on as long as the momentum lasts. It has been known to sail in this way, with the wings and body perfectly motionless, for more than an hour (though this is an unusually long time), and when the momentum becomes exhausted, a few strokes of the wing are sufficient to restore it. From its frequent indulgence in this sailing flight, the albatross may be said seldom to flap its wings, but certainly cannot be said *never* to do so.

Inserting this explanation may be what J. H. requires.

Norwich, February 7 T. SOUTHWELL

Relations of the State to Scientific Research

As an old worker in science, and as one who, had Nature not been unkind, might have been eminent, I desire to say a few words on the relations of the State to Scientific Research, a matter likely, I understand, to be the subject of a "Commission."

I take it for granted that it is a natural and proper function of the State to assist and develop labours, the results of which are of national importance, though their market value cannot be satisfactorily ascertained at the time they are being carried on, and therefore they can seldom be immediately remunerative. Of the seed sown to-day, the nation will reap in years to come, long after the sower is dead and gone. It is only right that the nation should help in the sowing. To continue, as of old, merely to reap where others have sown, may seem good in the sight of temporising politicians; but it will not seem so when there comes to be a scanty harvest by reason of the sowers having been feeble and few. It was bad political philosophy when the rulers of the great city overlooked the poor wise man.

But what I wish more particularly to deal with now, is the manner in which the State can best perform this acknowledged duty. In what way can Government most beneficially interfere with the spontaneous energy of original scientific labourers? And this I confess is a matter of no little difficulty. Let us suppose that a certain large sum of money should be set aside, in order to enable a large body of elect men to prosecute original inquiries undisturbed by the bark of the wolf at the door; in other words, let us suppose that Government pays directly for simple scientific investigation. In that case, such elect men will either have to work by the piece, being paid for and by their results when they have brought them forward, or they will have to receive a salary,—to be paid beforehand for work which they will be expected to do. The former plan is, in the first place, impracticable, for the simple reason that the value of the work cannot be satisfactorily gauged,—in the second place it would be most pernicious, and inevitably bring about a deluge of delusions. It would be a gigantic system of prize essays, and we all know that nothing but lies and nonsense proceed out of the mouths of prize essays.

The second plan flies in the face of a fundamental law of human nature. Suppose a hundred men to receive each, say, seven hundred a year, paid quarterly, in order that they may devote themselves to original research. How much of the divine afflatus would list to come into the minds of ninety and nine of them? The morning after they had received their quarter's salary, they would take up their apparatus and sit down by the side of the pool waiting till the waters should be stirred. But the stirring would never come. They would always be paulo-post-futurists; they would ever be writing title-pages of books that would never be seen. They would become admirable critics, keenly sensitive of the follies and errors of the pushing, squabbling, busy, outside mob of unpaid workers; but they, the ninety and nine, would not produce. As they grew old they would ask permission to retain their salaries while they went to live in a land in "which it always seemed afternoon." And when they, the first batch, died, those who succeeded them would boldly declare, as I am told the Fellows of the old Universities do, that they were paid not for the work of which their ability gave promise, but as a reward for having shown themselves worthy of filling the posts. The one man who would do any work at all would be the man who would find the greatest difficulty of getting into the guild, and he, most probably, would only get in by accident after all.

There may be a little exaggeration in the above. As an old man I am prone to be garrulous; but of this I feel above all things assured, that in all the higher functions of the scientific man, in all work that is not mechanical, help from Government or from elsewhere must be given—not directly and in exchange for actual scientific work, but indirectly for some other tasks that do not demand original thought—and given in such a way that active private research may comfortably be carried on at the same time.

In the good old times when the ties which bound together State and Church were not such ticklish ties as now, they used to reward abstract unremunerative learning indirectly by bestowing on it the rich offices of religion. Greek and philosophy took the bishoprics which rightly belonged to piety.

It is possible for science to copy the indirectness and yet to avoid the injustice of this old method; to retain the good while rejecting the evil of such a method of payment *not* by results. How such a plan may be carried out, I will venture with your permission, Sir, to trace in a succeeding letter.

IN SICCO

NOTES

IN the last number of the *Revue des Cours Scientifiques*, M. Alglave again announces further subscriptions to the Sars Fund amounting to 40*l.*, half this sum being a prize awarded by the Zoological Society of Paris in recognition of Sars's works.

M. STAS has been elected director of the Classe des Sciences in the Royal Academy of Belgium.

A DEPUTATION consisting of Earl Fortescue, the Right Hon. C. B. Adderley, Dr. Farr, and others, had an interview with Mr. Shaw-Lefevre at the Board of Trade on Saturday to recommend the legalisation of metric weights and measures in the Post Office, and the legal substitution of metric weights for the troy weight which the Standard Commissioners propose to abolish.

ALL who are interested in the science of ethnology in this country, and their number is daily on the increase, will be glad to learn that the Council of the Royal College of Surgeons are in treaty with Dr. Nicolucci of Nola di Sora, for the purchase of his fine collection of Italian and Greek skulls. This collection, comprising 165 specimens of ancient and modern crania, upon which the celebrated Italian ethnologist's well-known researches into the history of the races of Southern Europe have been mainly founded, will prove a valuable acquisition to the already extensive series in the Hunterian Museum.

MR. SCOTT, the Director of the Meteorological Office, has requested us to state that the French minister of the marine department has made arrangements for hoisting the "drum" signal at all semaphore stations on the French coast, between Dunkerque and Nantes, on receipt of telegraphic intelligence from the Meteorological Office. This signal will, therefore, have the same significance at those ports as on the Elbe and at our own stations. Herr von Freeden in the report of the Norddeutsche Seewarte states, that out of thirty telegrams which might be considered real storm-warnings, there were thirteen instances when the storm followed the same evening or next day; four when it was the previous day (in three of these instances a Sunday intervened and no telegram could be sent), six instances when the weather proved squally, and seven when it remained fine. On two occasions no telegram was received; in one of them on account of an interruption of the wires. As the result of observation, Herr von Freeden considers that the N.W. gales take a southerly direction from Ireland towards the Bay of Biscay, and therefore, do not affect the mouth of the Elbe. He also thinks further investigation would show a connection between them and S.W. winds, further westward, which have been blowing hard before reaching the Channel and have veered to N.W., though not enough to exclude the British Isles from their effect, while in North Germany the compensating current comes from S.E. These facts are of value in showing the utility of the system of "Weather telegraphy" instituted by the Meteorological Committee of the Royal Society.

WE have just received from Herr F. von Hauer the three first sheets of his geological map of the Austro-Hungarian Empire, compiled from the Survey of the Geological Institute; also a map by Mr. Foetterle, showing the occurrence, production, and distribution of coal in Austria. The subsequent sheets of the geological map are in the press and will appear in a few weeks.

AT the dinner of the Foremen Engineers on Saturday, Sir J. Whitworth referring to the depressed state of trade and the signs of improvement that are visible, observed that the progress made during the last forty years in the construction of self-acting machinery has been very remarkable. Twelve shillings a foot was formerly paid for the labour of chipping and filing iron surfaces and it was now done by the planing-machine for a penny. Mr. Bessemer's method of making steel has reduced the cost of some kinds of steel to one-half or one-third what it was. The consumption of coal for manufactures has been reduced more than one-half. The saving on English railways last year by using coal instead of coke was 1,200,000*l*. Mechanical and civil engineers, chemists, and other scientific men, are continually finding out new modes of producing wealth, and the owners of self-acting machinery generally go on improving and increasing their productions, from which those who have fixed incomes derive great advantage. The full employment of such machinery required a free exchange of the produce of all countries. Engineers have so reduced the cost and time of transit that when we have that free exchange, England will probably be the cheapest country in the world to live in. Sir Joseph went on to say, that looking to the immediate future, we may congratulate ourselves on the great opportunity arising for the development of engineering enterprise. The cultivation of land by steam power is greatly on the increase. In regard to the use of horse tramways now being urgently pressed forward, Sir Joseph protested that they were not suited to the present time. He considered that if toll gates were abolished, and roads kept in good order, engineers would soon produce a small, light locomotive that would do its work quietly and efficiently. The consumption of fuel per horse-power is now so small that road locomotives could be employed at far less cost than horses.

WE learn from *L'Institut* that M. Tchibatcheff has published the eighth and last volume of his work on the physical geography, climatology, botany, geology, and palæontology of Asia Minor. The fossils described in this volume belong chiefly to the Devonian tertiary and quaternary series. The Jurassic rocks are represented by only four ammonites found near Angora; the cretaceous rocks by twenty-seven different species, the Devonian by seventy-nine, and the carboniferous by fourteen. The remainder of the 604 species in Asia Minor belong to the tertiary or quaternary rocks. An appendix contains descriptions of fossils found in Devonian strata near Constantinople by Colonel Abdullah Bey. The knowledge of the fossils of Asia Minor, furnished by this work, does not introduce any change in the views previously entertained. The succession of organisms at different epochs has been the same in Asia Minor as in other countries already studied in this respect.

THE *Athenæum* states that the Rev. A. E. Eaton, of Trinity College, Cambridge, is preparing a monograph on the Ephemeridæ, or May-flies, in two parts. Part I. (which will treat of their generic and special nomenclature) is to contain a chronological catalogue of authorities and a synonymic alphabetical index to their works, descriptions of the known genera and species, figures of some organs characteristic of the genera and drawings of many of the species. Part II. will be occupied with an account of the anatomy and development of one or more characteristic British species.

THE Melbourne correspondent of the *Times* remarks, that as a partial set-off against the rabbit and sparrow scourges resulting from ill-considered introductions of European animals to Australia, we now and then light on what seems a new fact in natural history. Among other importations is the ostrich, and being a strong, long-legged bird of uncertain temper, it was deemed unsafe company for children and nurses in the Park. The Acclimatisation Society, therefore, fixed an inquisitive and zoological squatter with the flock of ostriches up country, where he was to look after the birds. Mr. S. Wilson, writing from his station at Longeranong, informs the Society that twelve young birds have been hatched in one nest, and "are getting on nicely." Referring to a common notion, derived from such books as "Goldsmith's Natural History," that the ostrich lays her eggs in the sand, leaving them to be hatched by the heat of the sun, Mr. Wilson says that during the period of incubation —about six weeks—the male and female sit on the nest by turns, both being seldom absent at the same time. As it is not to be assumed that the ostrich departs from its natural domestic arrangements in a new country, we must believe that the female has been hitherto commonly maligned, and the peculiar virtues of her husband altogether overlooked. Another nest of eleven eggs came to no good, as they were laid too early in the winter. The nest is "in a sandy hollow, without grass or rubbish, and the eggs are entirely without any cover."

AT the meeting of the Linnean Society, held Feb. 17, a paper was read by Mr. C. B. Clark on the *Commylenaceæ* of Bengal, of which order he proposes a new system of classification, based on characters of the capsules and the seeds; a paper on the Tree-Ferns of British Sikkim by Mr. Scott, which comprise eight indigenous species belonging to the genera *Cyathea*, *Hemitelia*, and *Alsophila*, an intoxicating drink being obtained by the natives from three different species; and an interesting letter addressed to Dr. Hooker by Dr. Hunce, from Whampoa near Canton, on the Flora of some little investigated districts in that neighbourhood.

WE have to record the decease of Mr. J. E. Sowerby, so well-known in connection with the illustration of botanical works, especially the new edition of the English Flora, edited by Mr. J. Boswell-Syme, now nearly completed.

THE Professorship of Medical Jurisprudence in the Royal College of Surgeons, Ireland, is vacant by the death of Dr. Geoghegan. The election by the Council was to take place on the 17th inst.

DURING the recent discussion on Easter Island at the Royal Geographical Society, it was stated that the layers of guano could be traced and the deposit of each twenty-four hours distinguished. It was calculated that it must have taken 4,000 years to form the 20 feet deposits on the Chincha Islands.

THE town of Dantzic having given a concession of the sewage to Messrs. Aird of Berlin and granted them land for its utilisation, preparations are now being made for carrying out the work under the direction of Mr. Baldwin Latham.

WE have received a report of the Birmingham Natural History and Microscopical Society, which appears to be actively at work, and we notice that Mr. Fiddian read a paper, of which he has sent us a copy, on a "Screw Motion" to remedy the defects in the adjustment of compound microscopes.

THE *Bulletin de la Société d'Acclimatation* contains an article on the use of the skins of the Kangaroo for glove-making, which seems to promise a successful result in this respect, and as furnishing a new source of animal food, as these animals thrive well in Europe.

THE Warsop aero-steam engine has lately been much talked of, and was the other day prominently described in the *Times*. The invention consists essentially in mixing heated air with the steam before its admission into the engine, and results have been obtained which show considerable advantages in the case of certain engines and boilers. *Engineering* referring to the subject remarks, that it is to be regretted that those interested in the invention should not have afforded means of judging whether its application to engines of an economical class would furnish results as satisfactory as those obtained with the defective boiler of the *Fox*.

A CORRESPONDENT in the *Society of Arts Journal* suggests the necessity of a new house for that body, with sufficient room for an extensive library and museum, and adds, that if a hundred of our trade magnates were to set down their names for 1,000*l*. each, the object would be accomplished, and they would be the Greshams of a New Exchange, where art and science would incessantly supplement and extend an ever-growing commerce, and lessen the sphere of human want.

THE *British Medical Journal*, referring to the general impression that the mortality attending the larger operations performed in hospitals has diminished during the last two or three years, suggests that if this be due to really improved plans of treatment, the introduction and free use of carbolic acid may have been one of the measures most influential, and recommends the use of this material in hospitals for preventing accidental contagion.

WE have received, from the Editor, the "Year Book of Photography, and Photographic News Almanac for 1870," containing a number of interesting and useful papers and memoranda.

THE *Journal of the Society of Arts* states that application has been made by the French Government for plans and statements relating to the organisation of the South Kensington Museum and Schools. Schemes based on the same principle and method have, it is understood, been proposed in New York and Boston.

CAPTAIN HANS BUSK has issued a circular in which he expresses his conviction that much may yet be done to diminish loss of life by shipwreck, and suggesting the construction of an experimental steam life-ship of 80 to 100 tons and 50 horse-power, capable of keeping the sea in any weather. An institute has been established for this purpose, and Captain Busk offers a donation of 200*l*. on condition that 1,400*l*. be contributed by the public towards the cost of the vessel during the present month. Upwards of 700*l*. has already been received. Subscriptions are to be sent to the Hon. Secretary, Major Wallace Campbell, or Captain Busk, 3, Garden Court, Temple, or to Messrs. Coutts' Bank.

WE have received from the publishers the January number of the *North British Review*, in which we notice an admirable series of notices of contemporary literature, in which there are among the scientific works referred to Ångström's "Researches on the Solar Spectrum," Ladenburg's "History of Chemistry," Micé's "Report on the Progress of Chemistry," Odling's "Outlines," Foster's "Physical Geography of the Mississippi Valley," Sir J. Lubbock's "Pre-Historic Times," and Newman's "British Moths."

A METHOD of protecting iron from atmospheric influences has been proposed by Messrs. Macmillan and Macgregor, of Dumbarton and Glasgow. They bring melted sulphur into contact with the cold metallic surface to be coated. The sulphur chills and sets into a hard, thin, protecting covering.

MR. RUGGLES states in his report to the International Statistical Congress held at the Hague that, in 1868, the extent of land under wheat, rye, barley, oats, buckwheat, and maize in the United States was upwards of sixty-six million acres, the cereal crop amounting to more than four times as much as was needed for the population of the country.

THE Secretary of State for India announces, at the request of the Governor-General of India, that the Government of India offers a prize of 5,000*l*. for machinery or a method suitable for the separation of the fibre and bark of the Rheea or China-grass from the stem and for separating the fibre from the bark. Dried stems and specimens of the fibre will be suppled on application to the Secretary to the Government of India in the Home Department.

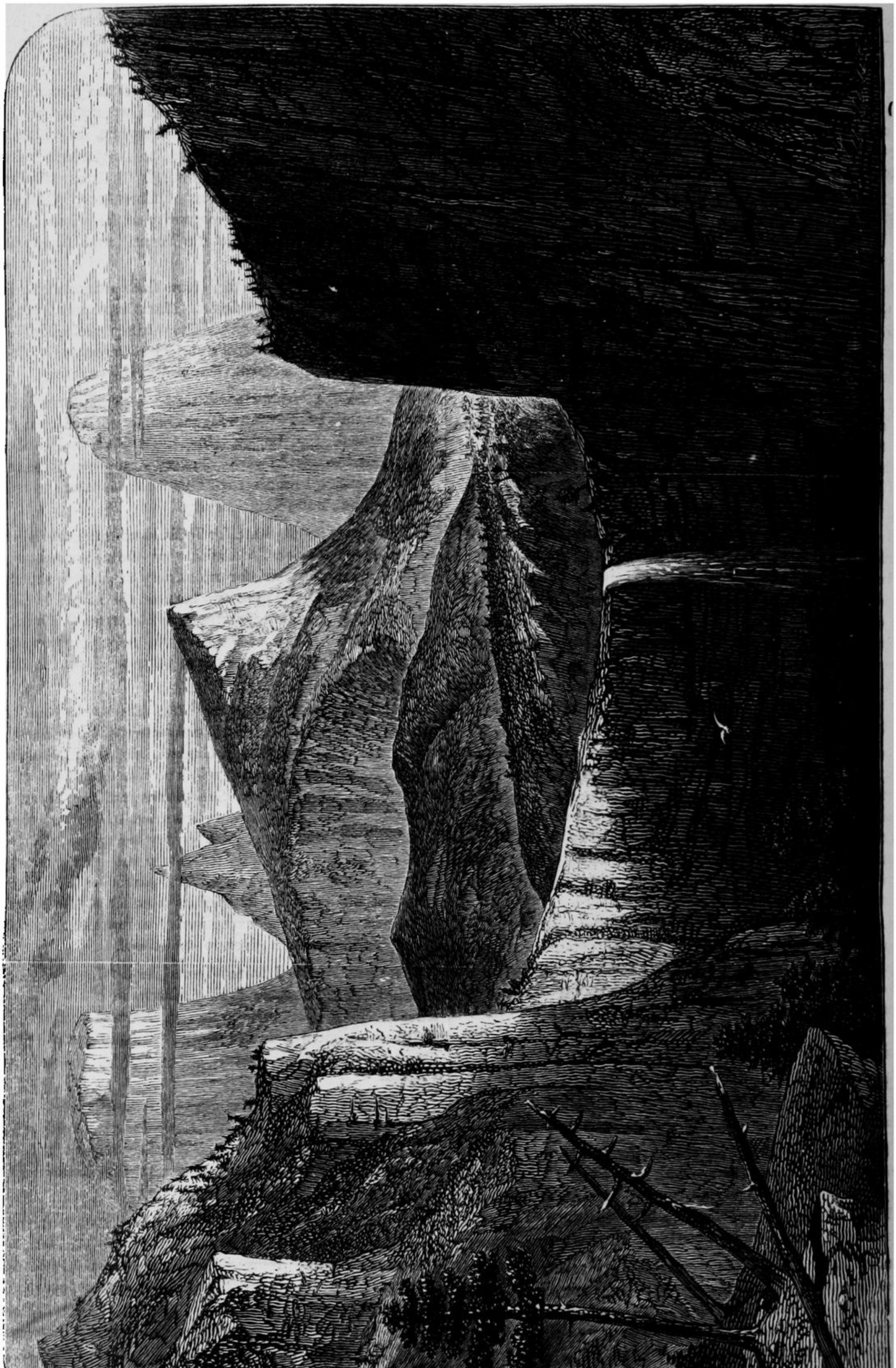
WE learn from a paper by Mr. Bartley in the *Society of Arts Journal*, that Science classes, attended by artizans, were in active operation last year in Chelsea, Hampton, Knightsbridge, Nine Elms, and Wandsworth, the total number of students being 223. The Chelsea School has just been enlarged by the students themselves, working from six to eleven at night. It will accommodate 200, the outlay being 50*l*. for material. This school has now 206 students in all. They are taught by Mr. Bickerton—formerly a cabinet-maker in Gloucestershire—who has distinguished himself as a pupil at the Royal School of Mines. With the exception of a few prizes, no local aid of any kind has been given to the school.

WE have received from Vienna the catalogue of a book-sale which is to take place during March, in which we notice the titles of many valuable works on Natural Science, History, Political Economy, Mathematics, &c., in various languages. Commissions for purchases are received by Messrs. Trübner in London.

A COURSE of eight lectures on English History, from the accession of Edward III. to the date of the Council of Constance, is now being delivered in the Pimlico Rooms, by Archibald Milman, Esq., under the auspices of the Committee for the higher education of women.

A DEPUTATION from the Society of Arts waited upon Earl Grey and Mr. Forster at the Privy Council Office on Friday, to present a memorial representing in reference to the system of State aid to science classes, the hardship of the new orders which are looked upon by teachers as a breach of faith by the department. Lord De Grey promised to consider the matter carefully.

IN consequence of the appointment of Mr. Dyer to the chair of Botany in the College of Science, Dublin, the Professorship of Natural History at the Royal Agricultural College at Cirencester is vacant.

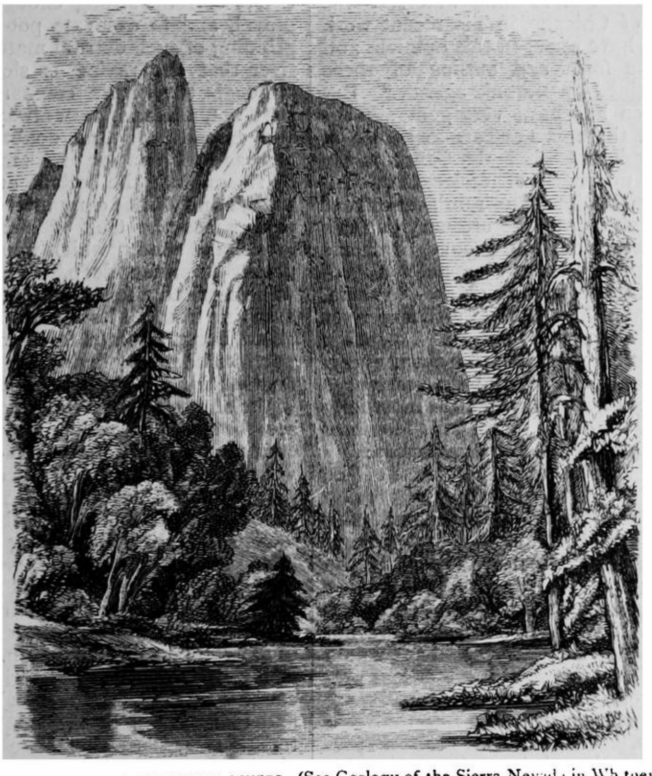


VIEW OF PART OF THE MERCED CAÑON WITH THE VERNAL FALL, YOSEMITE VALLEY, CALIFORNIA
(From Report on Geological Survey of California by C. J. D. Whitney, State-Geologist)

CAÑONS

TO the south of Salt Lake and the Mormon Territory lies a dreary series of plateaux traversed by the Colorado river and its tributaries, which bear their burthen of waters into the Gulf of California. Though this region possesses many considerable streams, it is over large areas a kind of desolate wilderness, for instead of irrigating the ground these streams flow in profound gorges, which serve as natural drains to carry off the water which may fall upon the tablelands. Many fabulous tales have been told of these regions, their natural marvels receiving many amplifications as they came to be rehearsed by Indians, trappers, and adventurous wanderers into the far west. In 1857 the Government of the United States despatched an expedition to explore that little known portion of the Continent, and the report published by the expedition in 1861 gave the first trustworthy and detailed account of the Colorado region. The truth turned out to be almost stranger than the fiction. A vast territory was found to be intersected by ravines leading into the main line of gorges of the Colorado. These ravines, or cañons as they are termed, meander over the table-land as rivers do over alluvial meadows; but they are thousands of feet deep—hundreds of miles long, and so numerous that the country traversed by them is said to be impassable, save to the fowls of the air.

The longest and deepest gorge is the Grand Cañon of the Colorado. Its length was set down by Dr. Newberry as about 300 miles; and its walls were described as rising steeply, sometimes vertically, from the margin of the river which filled the bottom of the ravine, to a height of from 3,000 to 6,000 feet—a line of precipice or natural section which has not yet found its equal on any other part of the globe.* Attention has lately been again called to this remarkable gorge by the interesting narrative in Mr. Bell's "New Tracks in North America," and by the fuller details, as yet only partially published, obtained by an exploring party under Colonel Powell, of the United States army. By successive travellers and Government expeditions the gorges of the Colorado had been reached here and there. The surveying party of 1857-58 mapped them out and gave many admirable drawings of them, but declared the river not to be navigable above the Black Cañon. Profiting by previous failures, and by all the information which he could receive from Indians and others, Colonel Powell conceived the bold idea of attempting the descent of the Colorado in boats. After months of toil and danger, he succeeded in forcing the passage of these forbidding gorges, and emerging safely at their further end. From his survey it appears that the Grand Cañon is 238 miles long, and from 2,500 to 4,000 feet deep. But though this is the longest, there are other ravines of hardly inferior dimensions. On the Green River, Col. Powell's party navigated a series 190 miles long. From where the Green River joins the Colorado, they passed through a succession of cañons for a distance of 256 miles before they came to the Grand Cañon.



HEAD OF MERCED AND TUOLUMNE RIVERS (See Geology of the Sierra Nevada in Whitney's Geological Survey of California, pp. 415—419)

Each cañon has tributary cañons: these again have often also their tributaries. In some places the lateral gorges crowd so closely together where they join the main one, that they are divided by perpendicular walls of rock, which seem so narrow at top as hardly to furnish footing for a man, though in reality large enough to support cathedrals. And these walls shoot 2,000 or 3,000 feet above the river, "while rocks and crags and peaks rise still higher, away back from the river, until they reach an altitude of nearly 5,000 feet." They consist to a large extent of brown, grey, and orange-coloured sandstones, gently inclined or horizontal, beneath which marble and granite in some places have been deeply trenched. In some places

* See Dr. Newberry's section of this gorge in NATURE, No. 6, p 163.

the walls are so absolutely vertical, that it is impossible to find a pathway between their base and the water. But where, owing to rapids, some portage was necessary, the explorers usually succeeded in carrying their stores, and sometimes even their boats, along the base of the cliffs.

The water of the Colorado River is red and muddy. It receives some tributary streams of clear water, but others are very turbid, particularly one which the expedition appropriately marked as the Dirty Devil. Moreover, after every heavy shower of rain, "cascades of red mud pour over the walls from the red sandstone above, with a fall of hundreds of feet." We await with interest the detailed report which Colonel Powell will furnish of these features of the river.

Dr. Newberry, who described this territory in the report of the former Exploring Expedition above referred to, declared his opinion that, notwithstanding the stupendous scale on which these cañons or ravines had been formed, they were all nevertheless true river-gorges, excavated by the erosive action of running water. Some geologists, as Dr. Foster of Chicago, in his recent work on the Mississippi Valley, have opposed this opinion, and have suggested that "the form and outline of these chasms were first determined by plutonic agency." But Dr. Newberry's explanation has been very generally accepted. He showed that there is nowhere any trace of fracture or disturbance, and that when the Cañon is dry its rocky bottom shows no mark of dislocation. Indeed, when we consider the intricate ramifications of these cañons, so precisely similar to the ordinary outlines of a drainage system over a low flat ground, it seems impossible to conceive of any agency capable of producing such ravines save the streams which flow in them.

But if cañons are merely the results of ordinary river erosion, why do they not occur everywhere? To such a question we may reply that river-ravines do occur everywhere, but it is only where the special circumstances which favour the formation of such ravines are most fully developed that they grow into the depth and length of cañons. What then are these special circumstances?

If we watch what takes place along the course of the rivers of this country, we can mark two kinds of erosion distinctly at work. First there is the river, grinding down the sides and bottom of its channel by sweeping along sand and shingle; and, secondly, there is the action of rain, springs, and frosts perpetually loosening the sides of the water-course, and sending the débris into the river which sweeps it away. If the river were not interfered with by these other subaërial agents, it would in time dig out for itself a gorge with more or less precipitous sides. But in proportion as these agents come into play, the ravine-like character passes into that of a valley with sloping sides. Where river erosion predominates we have ravines, where it is modified by rains and springs, but especially by frosts, we have valleys. Many of our rivers run both through gorges and along valleys, the changes in the nature of their banks being determined by corresponding changes in the nature and grouping of the rocks of which these banks consist, and the greater or less facility with which the rocks have been worn away by the one form of denudation or the other. The conditions needful for the formation of cañons, therefore, appear at present to be chiefly these:—1st. The erosive power of the streams must be greatly in excess of that of the other forms of atmospheric denudation. The rainfall must be small, or, at least, so equally distributed over the year as to reduce pluvial action to a minimum. Frosts must be equally rare and unimportant. The main streams drawing their supplies of water from a distance, either from melted snow or abundant rainfall in the upper parts of their basins, must be maintained in sufficient volume to keep their channels full, either for the whole, or a good part of the year. 2nd. There must be a considerable uniformity in the character of the rock which the stream has first to cut through. It is not necessary that the rock should be soft, but it should preserve for a long distance, and present to the erosive action of the river, the same kind of geological texture and structure. Hence, horizontal or gently undulating strata, as of sandstone, or limestone, offer the greatest facilities for the erosion of cañons, as we know they do in our own country for the formation of ordinary river-ravines. When once the river has excavated its channel so deep that it cannot quit it, the nature of the rock may vary indefinitely without materially altering the aspect of the cañon. Hence on the Colorado, while the upper and chief part of the cañon has been cut through flat sandstone, limestone, and other strata, the lower portion has been excavated in marble and even in granite. 3rd. The country must be sufficiently elevated above the sea, either originally or by subsequent upheaval, to permit of a considerable declivity in its river-channels. The slope must be sufficient, not merely to let the water run off, but to give rise to currents strong enough to sweep along sand and gravel, and to excavate pot-holes. It is by the ceaseless grinding of such detrital material along the bottom of the river that the ravine is slowly deepened. Geologists, although they have constantly recognised this action, have not, perhaps, been always fully aware of its rapidity and extent, partly, no doubt, from the want of reliable data as to the nature and amount of the detritus pushed by rivers along the bottom of their beds. Messrs. Humphreys and Abbot computed that the Mississippi annually pushes into the Gulf of Mexico 750,000,000 cubic feet of gravel and sand, "which would cover a square mile about twenty-seven feet deep." The writer of the present paper was surprised a few years ago to find that the Rhine, after escaping from all its ravines and entering the low country about Bonn, retained force enough to drive along shingle upon its bed. By laying the ear to the bottom of a boat floating down mid-channel, it was easy to hear the grating of the stones as they rolled over each other. Hence we see that a river, which may be perfectly navigable by steamers, may yet have rapidity enough to scour its bed with coarse shingle. The scour will, of course, be greater in proportion to the narrowing of the breadth of the stream and the increase of the slope.

It is mainly this eroding action which, so far as we know at present, has carved out the cañons of the Colorado. These wonderful ravines, meandering as ordinary rivers do, have sunk inch by inch into the country, retaining their original curves and windings, though continually increasing in depth. Unassisted, or aided but feebly, by the other subaërial agents, which, in such a country as ours, tend to break down the walls of ravines; and undisturbed by the inequalities of surface so characteristic of regions that have been under the influence of glacier-ice,* the rivers, probably once much fuller than now, have been allowed to dig out their gorges through the table-lands of the Colorado, and to convert a tract of country, originally, perhaps, green and well-watered, into a dreary desert, intersected by a network of profound impassable ravines.

ARCH. GEIKIE

SCIENTIFIC SERIAL

Revue des Cours Scientifiques, February 19.—This number contains a list of subscribers to the Sars Fund; also a lecture delivered at the Sorbonne by M. A. Gazin, on "Motive Power," in which are described the laws obtaining in regard to those natural forces which are already made available as sources of motive-power and the application of some other forces which may probably be turned to account in the same way as science progresses; for instance, the application by M. Mouchot and Ericsson of solar heat for working a steam engine is especially mentioned as worthy of consideration; and the application of the force of tides suggested by M. Tommasi.

* The absence of any trace of glacial action on the Pacific slope is noted by Whitney (Proc. Acad. Nat. Sciences, California, iii. 272), and by Foster ("Mississippi Valley," p. 338).

ON THE PROGRESS OF PALÆONTOLOGY

ANNIVERSARY ADDRESS DELIVERED BEFORE THE GEOLOGICAL SOCIETY

It is now eight years since, in the absence of the late Mr. Leonard Horner, who then presided over us, it fell to my lot, as one of the secretaries of this society, to draw up the customary Annual Address. I availed myself of the opportunity to endeavour to "take stock" of that portion of the science of biology which is commonly called "palæontology," as it then existed; and discussing one after another the doctrines held by palæontologists, I put before you the results of my attempts to sift the well-established from the hypothetical or the doubtful. Permit me briefly to recall to your minds what those results were.

1. The living population of all parts of the earth's surface which have yet been examined, has undergone a succession of changes which, upon the whole, have been of a slow and gradual character.

2. When the fossil remains which are the evidences of these successive changes, as they have occurred in any two more or less distant parts of the surface of the earth, are compared, they exhibit a certain broad and general parallelism. In other words, certain forms of life in one locality occur in the same general order of succession as, or are *homotaxial* with, similar forms in the other locality.

3. Homotaxis is not to be held identical with synchronism without independent evidence. It is possible that similar, or even identical, faunæ and floræ in two different localities may be of extremely different ages, if the term "age" is used in its proper chronological sense. I stated that "geographical provinces or zones may have been as distinctly marked in the Palæozoic epoch as at present; and those seemingly sudden appearances of new genera and species, which we ascribe to new creation, may be simple results of migration."

4. The opinion that the oldest known fossils are the earliest forms of life, has no solid foundation.

5. If we confine ourselves to positively ascertained facts, the total amount of change in the forms of animal and vegetable life since the existence of such forms is recorded, is small. When compared with the lapse of time since the first appearance of these forms, the amount of change is wonderfully small. Moreover, in each great group of the animal and vegetable kingdoms, there are certain forms which I termed PERSISTENT TYPES, which have remained, with but very little apparent change, from their first appearance to the present time.

7. In answer to the question "What then does an impartial survey of the positively ascertained truths of palæontology testify, in relation to the common doctrines of progressive modification, which suppose that modification to have taken place by a necessary progress from more to less embryonic forms, from more to less generalised types, within the limits of the period represented by the fossiliferous rocks?" I reply, "It negatives these doctrines, for it either shows us no evidence of such modification, or demonstrates such modification as has occurred to have been very slight; and, as to the nature of that modification, it yields no evidence whatsoever that the earlier members of any long continued group were more generalised in structure than the later ones."

I think I cannot employ my last opportunity of addressing you, officially, more properly—I may say more dutifully—than in revising these old judgments with such help as further knowledge and reflection, and an extreme desire to get at the truth, may afford me.

1. With respect to the first proposition, I may remark that whatever may be the case among physical geologists, catastrophic palæontologists are practically extinct. It is now no part of recognised geological doctrine that the species of one formation all died out and were replaced by a bran-new set in the next formation. On the contrary, it is generally, if not universally, agreed that the succession of life has been the result of a slow and gradual replacement of species by species; and that all appearances of abruptness of change are due to breaks in the series of deposits, or other changes in physical conditions. The continuity of living forms has been unbroken from the earliest times to the present day.

2, 3. The use of the word "homotaxis" instead of "synchronism" has not, so far as I know, found much favour in the eyes of geologists. I hope, therefore, that it is a love for scientific caution, and not mere personal affection for a bantling of my own, which leads me still to think that the change of phrase is of importance; and, that the sooner it is made, the sooner shall we get rid of a number of pitfalls which beset the reasoner upon the facts and theories of geology.

One of the latest pieces of foreign intelligence which has reached us is the information that the Austrian geologists have, at last, succumbed to the weighty evidence which M. Barrande has accumulated, and have admitted the doctrine of colonies. But the admission of the doctrine of colonies implies the further admission that even identity of organic remains is no proof of the synchronism of the deposits which contain them.

4. The discussions touching the *Eozoon* which commenced in 1864, have abundantly justified the fourth proposition. In 1862, the oldest record of life was in the Cambrian Rocks; but if the *Eozoon* be, as Principal Dawson and Dr. Carpenter have shown so much reason for believing, the remains of a living being, the discovery of its true nature carried life back to a period which, as Sir William Logan has observed, is as remote from that during which the Cambrian Rocks were deposited, as the Cambrian epoch itself is from the tertiaries. In other words, the ascertained duration of life upon the globe was nearly doubled, at a stroke.

5. The significance of persistent types, and of the small amount of change which has taken place even in those forms which can be shown to have been modified, becomes greater and greater in my eyes, the longer I occupy myself with the biology of the past.

Consider how long a time has elapsed since the Miocene epoch. Yet, at that time, there is reason to believe that every important group in every order of the *Mammalia* was represented. Even the comparatively scanty Eocene fauna yields examples of the orders *Cheiroptera*, *Insectivora*, *Rodentia*, and *Perissodactyla*; of *Artiodactyla* under both the Ruminant and the Porcine modifications; of *Carnivora*, *Cetacea*, and *Marsupialia*.

Or, if we go back to the older half of the Mesozoic epoch, how truly surprising it is to find every order of the *Reptilia*, except the *Ophidia*, represented; while some groups, such as the *Ornithoscelida* and the *Pterosauria*, more specialised than any which now exist, abounded.

There is one division of the *Amphibia* which offers especially important evidence upon this point, inasmuch as it bridges over the gap between the Mesozoic and the Palæozoic formations, often supposed to be of such prodigious magnitude, extending, as it does, from the bottom of the Carboniferous series to the top of the Trias, if not into the Lias. I refer to the Labyrinthodonts. As the address of 1862 was passing through the press, I was able to mention, in a note, the discovery of a large Labyrinthodont, with well-ossified vertebræ, from the Edinburgh coal-field. Since that time eight or ten distinct genera of Labyrinthodonts have been discovered in the carboniferous rocks of England, Scotland, and Ireland, not to mention the American forms described by Principal Dawson and Professor Cope. So that, at the present time, the Labyrinthodont Fauna of the Carboniferous rocks is more extensive and diversified than that of the Trias, while its chief types, so far as osteology enables us to judge, are quite as highly organised. Thus it is certain tha ta comparatively highly organised vertebrate type, such as that of the Labyrinthodonts, is capable of persisting, with no considerable change, through the period represented by the vast deposits which constitute the Carboniferous, the Permian, and the Triassic formations.

The very remarkable results which have been brought to light by the sounding and dredging operations, which have been carried on with such remarkable success by the expeditions sent out by our own, the American, and the Swedish Governments, under the supervision of able naturalists, have a bearing in the same direction. These investigations have demonstrated the existence, at great depths in the ocean, of living animals in some cases identical with, in others very similar to, those which are found fossilised in the white chalk. The *Globigerinæ*, Coccoliths, Coccospheres, Discoliths, in the one are absolutely identical with those in the other; there are identical, or closely analogous, species of Sponges, Echinoderms, and Brachiopods. Off the coast of Portugal, there now lives a species of *Beryx*, which, doubtless, leaves its bones and scales here and there in the Atlantic ooze, as its predecessor left its spoils in the mud of the sea of the Cretaceous epoch.

Many years ago* I ventured to speak of the Atlantic mud as "modern chalk," and I know of no fact inconsistent with the view which Professor Wyville Thomson has advocated, that the modern chalk is not only the lineal descendant of the ancient

* *Saturday Review*, 1858, "Chalk, Ancient and Modern."

chalk, but that it remains, so to speak, in the possession of the ancestral estate; and that from the cretaceous period (if not much earlier) to the present day, the deep sea has covered a large part of what is now the area of the Atlantic. But if *Globigerina*, and *Terebratula caput-serpentis* and *Beryx*, not to mention other forms of animals and of plants, thus bridge over the interval between the present and the Mesozoic periods, is it possible that the majority of other living things underwent a "sea change into something new and strange" all at once?

7. Thus far I have endeavoured to expand, and to enforce by fresh arguments, but not to modify in any important respect, the ideas submitted to you on a former occasion. But when I come to the propositions touching progressive modification, it appears to me, with the help of the new light which has broken from various quarters, that there is much ground for softening the somewhat Brutus-like severity with which I have dealt with a doctrine, for the truth of which I should have been glad enough to be able to find a good foundation, in 1862. So far indeed as the *Invertebrata* and the lower *Vertebrata* are concerned, the facts and the conclusions which are to be drawn from them appear to me to remain what they were. For anything that, as yet, appears to the contrary, the earliest known Marsupials may have been as highly organised as their living congeners; the Permian lizards show no signs of inferiority to those of the present day; the Labyrinthodonts cannot be be placed below the living Salamander and Triton; the Devonian Ganoids are closely related to *Polypterus* and to *Lepidosiren*.

But when we turn to the higher *Vertebrata*, the results of recent investigations, however we may sift and criticise them, seem to me to leave a clear balance in favour of the doctrine of the evolution of living forms one from another. In discussing this question, however, it is very necessary to discriminate carefully between the different kinds of evidence from fossil remains, which are brought forward in favour of evolution.

Every such fossil which takes an intermediate place between forms of life already known, may be said, so far as it is intermediate, to be evidence in favour of evolution, inasmuch as it shows a possible road by which evolution may have taken place. But the mere discovery of such a form does not, in itself, prove that evolution took place by and through it, nor does it constitute more than presumptive evidence in favour of evolution in general. Suppose A, B, C to be three forms, of which B is intermediate in structure between A and C. Then the doctrine of evolution offers four possible alternatives. A may have become C by way of B; or C may have become A by way of B; or A and C may be independent modifications of B; or A, B, and C may be independent modifications of some unknown D. Take the case of the Pigs, the *Anoplotheridæ* and the Ruminants. The *Anoplotheridæ* are intermediate between the first and the last; but this does not tell us whether Ruminants have come from the pigs, or pigs from Ruminants, or both from *Anoplotheridæ*, or whether pigs, Ruminants, and *Anoplotheridæ* alike may not have diverged from some common stock.

But, if it can be shown that A, B, and C exhibit successive stages in the degree of modification, or specialisation, of the same type; and if, further, it can be proved that they occur in successively newer deposits, A being in the oldest, and C in the newest, then the intermediate character of B has quite another importance, and I should accept it without hesitation as a link in the genealogy of C. I should consider the burden of proof to be thrown upon any one who denied C to have been derived from A by way of B; or in some closely analogous fashion. For it is always probable that one may not hit upon the exact line of filiation, and, in dealing with fossils, may mistake uncles and nephews for fathers and sons.

I think it necessary to distinguish between the former and the latter classes of intermediate forms, as *intercalary types* and *linear types*. When I apply the former term I merely mean to say, that as a matter of fact, the form B, so named, is intermediate between the others, in the sense in which the *Anoplotherium* is intermediate between the Pigs and the Ruminants—without either affirming, or denying, any direct genetic relation between the three forms involved. When I apply the latter term, on the other hand, I mean to express the opinion that the forms A, B, and C constitute a line of descent, and that B is thus part of the lineage of C.

From the time when Cuvier's wonderful researches upon the extinct Mammals of the Paris gypsum first made intercalary types known, and caused them to be recognised as such, the number of such forms has steadily increased among the higher Mammalia. Not only do we now know numerous intercalary forms of *Ungulata*, but M. Gaudry's great monograph upon the fossils of Pikermi (which strikes me as one of the most perfect pieces of palæontological work I have seen for a long time) shows us, among the *Primates*, *Mesopithecus* as an intercalary form between the *Semnopitheci* and the *Macaci*; and among the *Carnivora*, *Hyænictis*, and *Ictitherium* as intercalary, or, perhaps, linear, types between the *Viverridæ* and the *Hyænidæ*.

Hardly any order of the higher Mammalia stands so apparently separate and isolated from the rest as that of the *Cetacea*, though a careful consideration of the structure of the fissipede *Carnivora*, or seals, shows in them many an approximation towards the still more completely marine mammals. The extinct *Zeuglodon*, however, presents us with an intercalary form between the type of the seals and that of the whales.

The skull of this great Eocene sea monster, in fact, shows, by the narrow and prolonged interorbital region; the extensive union of the parietal bones in a sagittal suture; the well-developed nasal bones; the distinct and large incisors implanted in premaxillary bones, which take a full share in bounding the fore part of the gape; the two-fanged molar teeth with triangular and serrated crowns, not exceeding five on each side in each jaw; and the existence of a deciduous dentition—its close relation with the seals. While, on the other hand, the produced, rostral form of the snout, the long symphysis and the low coronary process of the mandible, are approximations to the cetacean form of those parts.

The scapula resembles that of the cetacean *Hyperoodon*, but the supra-spinous fossa is larger and more seal-like; as is the humerus, which differs from that of the *Cetacea* in presenting true articular surfaces for the free jointing of the bones of the fore-arm. In the apparently complete absence of hinder limbs, and in the characters of the vertebral column, the *Zeuglodon* lies on the cetacean side of the boundary line; so that, upon the whole, the Zeuglodonts, transitional as they are, are conveniently retained in the cetacean order. And the publication, in 1864, of M. Van Beneden's memoir on the miocene and pliocene *Squalodon*, furnished much better means than anatomists previously possessed, of fitting in another link of the chain which connects the existing *Cetacea* with *Zeuglodon*. The teeth are much more numerous, although the molars exhibit the zeuglodont double fang; the nasal bones are very short, and the upper surface of the rostrum presents the groove, filled up during life by the prolongation of the ethmoidal cartilage, which is so characteristic of the majority of the *Cetacea*.

It appears to me that, just as among the existing *Carnivora*, the walruses and the eared seals are intercalary forms between the fissipede *Carnivora* and the ordinary seals; so the Zeuglodons are intercalary between the *Carnivora*, as a whole, and the *Cetacea*. Whether the Zeuglodonts are also linear types in their relation to these two groups cannot be ascertained, until we have more definite knowledge than we possess at present, respecting the relations in time of the *Carnivora* and the *Cetacea*.

Thus far, we have been concerned with the intercalary types which occupy the intervals between families, or orders, of the same class. But the investigations which have been carried on by Prof. Gegenbaur, Prof. Cope, and myself, into the structure and relations of the extinct reptilian forms of *Dinosauria* and *Compsognatha*, have brought to light the existence of intercalary forms between what have hitherto been always regarded as very distinct classes of the vertebrate sub-kingdom, namely, *Reptilia* and *Aves*. Whatever inferences may, or may not, be drawn from the fact, it is now an established truth that, in many of these *Ornithoscelida*, the hind limbs and the pelvis are much more similar to those of birds than they are to those of reptiles, and that these Bird-reptiles, or Reptile-birds, were more or less completely bipedal.

When I addressed you in 1862, I should have been bold indeed had I suggested that palæontology would before long show us the possibility of a direct transition from the type of the lizard to that of the ostrich. At the present moment we have, in the *Ornithoscelida*, the intercalary type, which proves that transition to be something more than a possibility. But it is very doubtful whether any of the genera of *Ornithoscelida* with which we are at present acquainted are the actual linear types by which the transition from the lizard to the bird was effected. These are, very probably, still hidden from us in the older formations.

Let us now endeavour to find some cases of true linear types, or forms which are intermediate between others because they stand in a direct genetic relation to them. It is no easy matter to find clear and unmistakeable evidence of filiation among fossil animals. For, in order that such evidence should be quite satisfactory, it is necessary that we should be acquainted with all the most important features of the organization of the animals which are supposed to be thus related; and not merely with the fragments upon which the genera and species of the palæontologist are so often based. M. Gaudry has arranged the species of *Hyænidæ*, *Proboscidea*, *Rhinocerotidæ*, and *Equidæ* in their order of filiation from their earliest appearance in the Miocene epoch to the present time, and Professor Rütimeyer has drawn up similar schemes for the Oxen—with what I am disposed to think is a fair and probable approximation to the order of Nature. But as no one is better aware than these two learned, acute, and philosophical biologists, all such arrangements must be regarded as provisional, except in those cases in which, by a fortunate accident, large series of remains are obtainable from a thick and wide-spread series of deposits. It is easy to accumulate probabilities—hard to make out some particular case in such a way that it will stand rigorous criticism.

After much search, however, I think that such a case is to be made out in favour of the pedigree of the Horses.

The genus *Equus* is represented as far back as the latter part of the Miocene epoch; but, in deposits belonging to the middle of that epoch, its place is taken by two other genera, *Hipparion* and *Hipparitherium* (or *Anchitherium*); and, in the lowest Miocene and upper Eocene only the last genus occurs. A species of *Hipparitherium* was referred by Cuvier to the *Palæotheria* under the name of *P. Aurelianense*. The grinding teeth are in fact very similar in shape and in pattern, and in the absence of cement, to those of some species of *Palæotherium*, especially Cuvier's, *Palæotherium minus*, which has been formed into a separate genus, *Plagiolophus*, by Pomel.

But in the fact that there are six full-sized grinders, the first premolar being very small; that the anterior grinders are as large as, or rather larger than, the posterior ones; that the second premolar has an anterior prolongation; and that the posterior molar of the lower jaw has, as Cuvier pointed out, a posterior lobe of much smaller size and different form, the dentition of *Hipparitherium* departs from the type of the *Palæotherium*, and approaches that of the horse.

Again, the skeleton of *Hipparitherium* is extremely equine. M. Christol, who founded the genus, goes so far as to say that the descriptions of the bones of the horse, or the ass, current in veterinary works, would fit those of *Hipparitherium*. And, in a general way, this may be true enough, but there are some most important differences, which, indeed, are justly indicated by the same careful observer. Thus the ulna is complete throughout, and its shaft is not a mere rudiment, fused into one bone with the radius. There are three toes, one large in the middle, and one small on each side. The femur is quite like that of a horse, and has the characteristic fossa above the external condyle. In the British Museum, there is a most instructive specimen of the leg bones, showing that the fibula was represented by the external malleolus and by a flat tongue of bone, which extends up from it on the outer side of the tibia, and is closely ankylosed with the latter bone. The hind toes are three, like those of the fore leg; and the middle metatarsal bone is much less compressed from side to side than in the horse.

In the *Hipparion* the teeth nearly resemble those of the Horses, though the crowns of the grinders are not so long; like those of the Horses they are abundantly coated with cement. The shaft of the ulna is reduced to a mere style ankylosed throughout nearly its whole length with the radius, and appearing to be little more than a ridge on the surface of the latter bone until it is carefully examined. The front toes are still three, but the outer ones are more slender than in *Hipparitherium*, and their hoofs smaller in proportion to that of the middle toe. In the leg, the distal end of the fibula is so completely united with the tibia that it appears to be a mere process of the latter bone, as in the Horses.

In the Horses, finally, the crowns of the grinding teeth become longer, and their patterns are slightly modified; the middle of the shaft of the ulna vanishes, and its proximal and distal ends ankylose with the radius. The phalanges of the two outer toes in each foot disappear, their metacarpal and metatarsal bones being left as the "splints."

The *Hipparion* has large depressions on the face in front of the orbits, like those for the "larmiers" of many ruminants; but traces of these are to be seen in some of the fossil horses from the Sewalik Hills.

When we consider these facts, and the further circumstance that the Hipparions, the remains of which have been collected in immense numbers, were subject, as M. Gaudry and others have pointed out, to a great range of variation, it appears to me impossible to resist the conclusion that the types of the *Hipparitherium*, of the *Hipparion*, and of the ancient Horses constitute the lineage of the modern Horses, the *Hipparion* being the intermediate stage between the other two, and answering to B in my former illustration.

The nature of the process by which the *Hipparitherium* has been converted into the horse is one of specialisation or of more and more complete deviation from what might be called the average form of an ungulate mammal. In the Horses, the reduction of some parts of the limbs, together with the special modification of those which are left, is carried to a greater extent than in any other hoofed mammals. The reduction is less, and the specialisation is less in the *Hipparion*, and still less in the *Hipparitherium*; but yet as compared with other mammals, the reduction and specialisation of parts in the *Hipparitherium* remains great.

Is it not probable, then, that, just as in the Miocene epoch we find an ancestral equine form less modified than the horse, so, if we go back to the Eocene epoch we shall find some quadruped related to the *Hipparitherium*, as *Hipparion* is related to *Equus*, and consequently departing less from the average form?

I think that this desideratum is very nearly, if not quite, supplied by *Plagiolophus*, remains of which occur abundantly in some parts of the upper and middle Eocene formations. The patterns of the grinding teeth of *Plagiolophus* are similar to those of *Hipparitherium*, and they are similarly deficient in cement; but the grinders diminish in size forwards, and the last lower molar has a large hind lobe, convex outwards and concave inwards, as in *Palæotherium*. The ulna is complete and much larger than in any of the *Equidæ*, while it is more slender than in most of the *Palæotheria*. It is fixedly united, but not ankylosed with the radius. There are three toes in the fore-limb, the outer ones being slender, but less attenuated than in the *Equidæ*. The femur is more like that of the *Palæotheria* than that of the horse, and has only a small depression above its outer condyle in the place of the great fossa which is so obvious in the *Equidæ*. The fibula is distinct, but very slender, and its distal end is ankylosed with the tibia. There are three toes on the hind-foot having similar proportions to those on the fore-foot. The principal metacarpal and metatarsal bones are flatter than they are in any of the *Equidæ*.

In its general form, *Plagiolophus* resembles a very small and slender horse, and totally unlike the reluctant, pig-like creature depicted in Cuvier's restoration of his *Palæotherium minus* in the Ossemeus Fossiles.

It would be hazardous to say that *Plagiolophus* is the exact radical form of the Equine quadrupeds; but I do not think there can be any reasonable doubt the latter animals have resulted from the modification of some quadruped similar to *Plagiolophus*.

We have thus arrived at the Middle Eocene formation, and yet have traced back the Horses only to a three-toed stock. But these three-toed forms, no less than the Equine quadrupeds themselves, present rudiments of the two other toes which appertain to what I have termed the "average" quadruped. If the expectation raised by the splints of the horse that, in some ancestor of the horse, these splints would be found to be complete digits, has been verified, we are furnished with very strong reasons for looking for a no less complete verification of the expectation that the three-toed *Plagiolophus*-like "avus" of the horse must have had a five-toed "atavus" at some earlier period.

No such five-toed "atavus," however, has yet made its appearance among the few middle and older Eocene Mammalia which are known.

Another series of closely-affiliated forms, though the evidence they afford is perhaps less complete than that of the Equine series, is presented to us by the *Dichobune* of the Eocene epoch, the *Cainotherium* of the Miocene, and the *Tragulidæ*, or so-called "Muskdeer" of the present day.

The *Tragulidæ* have no incisors in the upper jaw, and only six grinding teeth on each side of each jaw, while the canine is moved up to the outer incisor, and there is a diastema in the lower jaw. There are four complete toes on the hind-foot, but the middle metatarsals usually become, sooner or later, ankylosed into a cannon bone. The navicular and the cuboid unite, and the distal end of the fibula is ankylosed with the tibia.

In *Cainotherium* and *Dichibune* the upper incisors are fully developed. There are seven grinders; the teeth form a continuous series without diastema. The metatarsals, the navicular and cuboid, and the distal end of the fibula, remain free. The *Cainotherium*, also the second metacarpal, is developed, but is much shorter than the third, while the fifth is absent or rudimentary. In this respect it resembles *Anoplotherium secundarium*. This circumstance, and the peculiar pattern of the upper molars in *Cainotherium*, lead me to hesitate in considering it as the actual ancestor of the modern *Tragulidæ*. If *Dichobune* has a four-toed front foot (though I am inclined to suspect it resembles *Cainotherium*) it will be a better representative of the oldest form of the Traguline series. But *Dichobune* occurs in the middle Eocene, and is, in fact, the oldest known artiodactyle mammal. Where, then, must we look for its five-toed ancestor?

If we follow down other lines of recent and tertiary *Ungulata*, the same question presents itself. The pigs are traceable back through the Miocene epoch to the upper Eocene, where they appear in the two well-marked forms of *Hyopotamus* and *Chœropotamus*. But *Hyopotamus* appears to have had only two toes.

Again, all the great groups of the Ruminants, the *Bovidæ*, *Antelopidæ*, *Cameleopardalidæ*, and *Cervidæ*, are represented in the Miocene epoch, and so are the camels. The upper Eocene *Anoplotherium*, which is intercalary between the pigs and the *Tragulidæ*, has only two or, at most, three toes. Among the scanty mammals of the lower Eocene formation we have the perissodactyle *Ungulata* represented by *Coryphodon*, *Hyracotherium*, and *Pliolophus*. Suppose for a moment, for the sake of following out the argument, that *Pliolophus* represents the primary stock of the perissodactyles, and *Dichobune* that of the Artiodactyles (though I am far from saying that such is the case) then, we find in the earliest fauna of the Eocene epoch, to which our investigations carry us, the two divisions of the *Ungulata* completely differentiated, and no trace of any common stock of both or five-toed predecessors to either. With the case of the horse before us, justifying a belief in the production of new animal forms by modification of old ones, I see no escape from the necessity of seeking for these ancestors of the *Ungulata* beyond the limits of the tertiary formations.

I could as soon admit special creation, at once, as suppose that the perissodactyles and artiodactyles had no five-toed ancestors. And when we consider how large a portion of the tertiary period elapsed before *Hipparitherium* was converted into *Equus*, it is difficult to escape the conclusion that a large proportion of time anterior to the tertiary must have been expended in converting the common stock of the *Ungulata* into perissodactyles and artiodactyles.

The same moral is inculcated by the study of every other order of tertiary monodelphius *Mammalia*. Each of these orders is represented in the Miocene epoch:—the Eocene formation, as I have already said, contains *Cheiroptera*, *Insectivora*, *Rodentia*, *Ungulata*, *Carnivora*, and *Cetacea*. But the *Cheiroptera* are extreme modifications of the *Insectivora*; just as the *Cetacea* are extreme modifications of the Carnivorous type; and therefore it is to my mind incredible that monodelphous *Insectivora* and *Carnivora* should not have been abundantly developed along with *Ungulata* in the Mesozoic epoch. But, if this be the case, how much farther back must we go to find the common stock of the monodelphous *Mammalia*? As to the *Didelphia*, if we may trust the evidence which seems to be afforded by their very scanty remains, that a Hypsiprymnoid form existed at the epoch of the Trias, side by side with a carnivorous form. At the epoch of the Trias, therefore, the *Marsupialia* must have already existed long enough to have become differentiated into carnivorous and herbivorous forms. But the *Monotremata* are lower forms than the *Didelphia*, which last are intercalary between the *Ornithodelphia* and the *Monodelphia*. To what point of the palæozoic epoch then must we, upon any rational estimate, relegate the origin of the *Monotremata*?

The investigation of the occurrence of the classes and of the orders of the *Sauropsida* in time, points in exactly the same direction. If, as there is great reason to believe, true Birds existed in the Triassic epoch, the ornithoscelidous forms by which Reptiles passed into Birds must have preceded them. In fact, there is even, at present, considerable ground for suspecting the existence of *Dinosauria* in the Permian formations. But in that case Lizards must be of still earlier date. And if the very small differences which are observable between the *Crocodilia* of the older mesozoic formations and those of the present day, furnish any sort of approximation towards an estimate of the average rate of change among the *Sauropsida*; it is almost appalling to reflect how far back in palæozoic times we must go, before we can hope to arrive at that common stock from which the *Crocodilia*, *Lacertilia*, *Ornithoscelida*, and *Plesiosauria*, which had attained so great a development in the Triassic epoch must have been derived.

The *Amphibia* and *Pisces* tell the same story. There is not a single class of vertebrated animals, which, when it first appears, is represented by analogues of the lowest known members of the same class. Therefore, if there is any truth in the doctrine of evolution, every class must be vastly older than the first record of its appearance upon the surface of the globe. But if considerations of this kind compel us to place the origin of vertebrated animals at a period sufficiently distant from the upper Silurian, in which the first Elasmobranchs and Ganoids occur, to allow of the evolution of such fishes as these from a Vertebrate as simple as the *Amphioxus*; I can only repeat that it is appalling to speculate upon the extent to which that origin must have preceded the epoch of the first recorded appearance of vertebrate life.

Such is the further commentary which I have to offer upon the statement of the chief results of palæontology, which I formerly ventured to lay before you.

But the growth of knowledge in the interval makes me conscious of an omission of considerable moment in that statement, inasmuch as it contains no reference to the bearings of palæontology upon the theory of the distribution of life; or takes note of the remarkable manner in which the facts of distribution, in present and past times, accord with the doctrine of evolution —especially in regard to land animals.

That connection between palæontology and geology on the one hand, and the present distribution of terrestrial animals, which so strikingly impressed Mr. Darwin thirty years ago, as to lead him to speak of a "law of succession of types"; and of the wonderful relationship on the same continent between the dead and the living, has recently received much elucidation from the researches of Gaudry, of Rütimeyer, of Leidig, and of Alphonse Milne-Edwards, taken in connection with the earlier labours of our lamented colleague Falconer. And it has been instructively discussed in the thoughtful and ingenious work of Mr. Andrew Murray "On the geographical distribution of mammals."

I propose to lay before you, as briefly as I can, the ideas to which a long consideration of the subject has given rise in my own mind.

If the doctrine of evolution is sound, one of its immediate consequences clearly is, that the present distribution of life upon the globe is the product of two factors: the one being the distribution which obtained in the immediately preceding epoch; and the other, the character and the extent of the changes which have taken place in physical geography between the one epoch and the other. Or, to put the matter in another way—the Fauna and Flora of any given area, in any given epoch, can consist only of such forms of life as are directly descended from those which constituted the Fauna and Flora of the same area, in the immediately preceding epoch; unless the physical geography (under which I include climatal conditions) of the area has been so altered as to give rise to immigration of living forms from some other area.

The evolutionist therefore is bound to grapple with the following problem whenever it is clearly put before him:—Here are the Faunæ of the same area during successive epochs. Show good cause for believing, either that these Faunæ have been derived from one another by gradual modification, or that the Faunæ have reached the area in question by migration from some area in which they have undergone their development.

I propose to attempt to deal with this problem so far as it is exemplified by the distribution of the terrestrial Vertebrata, and I shall endeavour to show you that it is capable of solution in a sense entirely favourable to the doctrine of evolution.

I have, elsewhere,* stated, at length, the reasons which lead me to recognise four primary distributional provinces for the terrestrial Vertebrata in the present world; namely, firstly, the *Novozelanian*, or New Zealand province: secondly, the *Australian* province, including Australia, Tasmania, and the Negrito Islands: thirdly, *Austro-Columbia*, or South America plus North America as far as Mexico; and fourthly, the rest of the world or *Arctogæa*, in which province America, north of Mexico, constitutes one sub-province; Africa, south of the Sahara, a second; Hindostan a third; and the remainder of the old world a fourth.

* "On the classification and distribution of the Alectoromorphæ." Proceedings of the Zoological Society, 1868.

Now the truth which Mr. Darwin perceived and promulgated as "the law of the succession of types" is, that in all these provinces the animals found in Pliocene or Pleistocene deposits are closely affined to those which now inhabit the same provinces, and that conversely, the forms characteristic of other provinces are absent. North and South America, perhaps, present one or two exceptions to the last rule, but they are readily susceptible of explanation. Thus, in Australia, the later tertiary Mammals are Marsupials (possibly with exception of the Dog and a Rodent or two, as at present). In Austro-Columbia the later tertiary Fauna exhibits numerous and varied forms of Platyrhine apes, Rodents, Cats, Dogs, Stags, *Edentata*, and Opossums; but, as at present, no Catarhine apes, no Lemurs, no *Insectivora*, Oxen, Antelopes, Rhinoceroses or *Didelphia* other than opossums. And, in the widespread Arctogæal province, the Pliocene and Pleistocene Mammals belong to the same groups as those which now exist in the province. The law of succession of types, therefore, holds good for the present epoch as compared with its predecessor. Does it equally well apply to the Pliocene Fauna when we compare it with that of the Miocene epoch? By great good fortune an extensive Mammalian Fauna of this epoch has now become known, in four very distant portions of the Arctogæal province which do not differ greatly in latitude. Thus Falconer and Cautley have made known the Fauna of the sub-Himalayas and the Perim Islands; Gaudry that of Attica; many observers that of Central Europe and France; Leidig, that of Nebraska on the eastern flank of the Rocky Mountains. The results are very striking. The total Miocene Fauna comprises many genera and species of Catarhine apes, of Bats, of *Insectivora*, of Arctogæal types of *Rodentia*, of *Proboscidea*, of Equine Rhinocenti, and Tapirine quadrupeds; of cameline, bovine, antelopine, cervine, and traguline Ruminants; of Pigs and Hippopotamuses; of *Viverridæ* and *Hyænidæ* among other *Carnivora;* with *Edentata* allied to the Arctogæal *Orycteropus* and *Manis*, and not to the Austro-Columbian Edentates. The only type present in the Miocene, but absent in the existing, Fauna of Eastern Arctogæa is that of the *Didelphidæ*, which, however, remains in North America.

But it is very remarkable, that while the Miocene Fauna of the Arctogæal Province, as a whole, is of the same character as the existing Fauna of the same province as a whole, the component elements of the Fauna were differently associated. In the Miocene epoch, North America possessed Elephants, Horses, Rhinoceroses, and a great number and variety of Ruminants and Pigs which are absent in the present indigenous Fauna. Europe had its Apes, Elephants, Rhinoceroses, Tapirs, Musk-deer, Giraffes, Hyenas, great Cats, Edentates, and opossum-like Marsupials, which have equally vanished from its present Fauna. And in Northern India, the African types of Hippopotamuses, Giraffes, and Elephants were mixed up with what are now the Asiatic types of the latter and with Camels, Semnopithecine and Pithecine apes of no less distinctly Asiatic forms.

In fact, the Miocene Mammalian Fauna of Europe and the Himalayan regions contains associated together the types which are now separately located in the South African and Indian sub-provinces of Arctogæa. Now there is every reason to believe, on other grounds, that both Hindostan, south of the Ganges, and Africa, south of the Sahara, were separated by a wide sea from Europe and North Asia, during the middle and upper Eocene epochs. Hence it becomes highly probable that the well-known similarities and no less remarkable differences between the present Faunæ of India and South Africa have arisen in some such fashion as the following. Sometime during the Miocene epoch, possibly when the Himalayan chain was elevated, the bottom of the nummulitic sea was upheaved and converted into dry land, in the direction of a line extending from Abyssinia to the mouth of the Ganges. By this means, the Dekhan on the one hand, and South Africa on the other, became connected with the Miocene dry land and with one another. The Miocene Mammals spread gradually over this intermediate dry land, and if the condition of its eastern and western ends offered as wide contrasts as the valleys of the Ganges and Arabia do now, many forms which made their way into Africa must have been different from those which reached the Dekhan, while others might pass into both these sub-provinces.

That there was a continuity of dry land between Europe and North America during the Miocene epoch, appears to me to be a necessary consequence; the fact that many genera or terrestrial Mammals such as *Castor, Hystrix, Elephas, Mastodon, Equus, Hipparion, Hipparitherium, Rhinoceros, Cervus, Amphicyon, Hyænarctos*, and *Machairodus*, are common to the Miocene formations of the two areas, and have as yet been found (except perhaps *Hipparitherium*) in no deposit of earlier age. Whether this connection took place by the east, or by the west, or by both sides of the old world, there is at present no certain evidence, and the question is immaterial to the present argument; but, as there are good grounds for the belief that the Australian province and the Indian and South African sub-provinces were separated by sea from the rest of Arctogæa before the Miocene epoch, so it has been rendered no less probable by the investigations of Mr. Carrick Moore and Prof. Duncan that Austro-Columbia was separated by sea from North America, during a large part of the Miocene epoch.

It is unfortunate that we have no knowledge of the Miocene Mammalian Fauna of the Australian and Austro-Columbian provinces. But seeing that not a trace of a Platyrhine ape, of a Procyonine carnivore, of a characteristically South American Rodent, of a Sloth, an Armadillo, or an Ant-eater, has yet been found in Miocene deposits of Arctogæa, I cannot doubt that they already existed in the Miocene Austro-Columbian province.

Nor is it less probable that the characteristic types of Australian Mammalia were already developed in that region in Miocene times.

But Austro-Columbia presents difficulties from which Australia is free—*Camelidæ* and *Tapiridæ* are now indigenous in South America as they are in Arctogæa, and among the Pliocene Austro-Columbian mammals, the Austro-Columbian genera *Equus, Mastodon*, and *Machairodus* are numbered. Are these post-Miocene immigrants, or præ-Miocene natives?

Still more perplexing are the strange and interesting forms *Toxodon, Macrauchenia*, and *Typotherium;* and a new Anoplotheriod mammal (*Omalodotherium*) which Dr. Cunningham sent over to me some time ago from Patagonia. I confess I am strongly inclined to surmise that these last, at any rate, are remnants of the population of Austro-Columbia before the Miocene epoch, and were not derived from Arctogæa by way of the north and east.

The fact that this immense Fauna of Miocene Arctogæa is now fully and richly represented only in India and South Africa, while it is shrunk and depauperised in North Asia, Europe, and North America, becomes at once intelligible, if we suppose that India and South Africa had but a scanty mammalian population before the Miocene immigration, while the conditions were highly favourable to the new comers. It is to be supposed that these new regions offered themselves to the Miocene Ungulates as South America and Australia offered themselves to the cattle, sheep, and horses of modern colonists. But after these great areas were thus peopled came the Glacial epoch, during which the excessive cold, to say nothing of depression and ice-covering, must have almost depopulated all the northern parts of Arctogæa, destroying all the higher mammalian forms except those which, like the elephant and rhinoceros, could adjust their coats to the altered condition. Even these must have been driven away from the greater part of the area. Only those Miocene mammals which had passed into Hindostan and into South Africa would escape decimation by these changes in the physical geography of Arctogæa. And when the northern hemisphere passed into its present condition, these lost tribes of the Miocene Fauna were hemmed by the Himalayas, the Sahara, the Red Sea, and the Arabian deserts, within their present boundaries.

Now, on the hypothesis of evolution, there is no sort of difficulty in admitting that the differences between the Miocene forms of the Mammalian Fauna and those which exist now, are the results of gradual modification; and since such differences in distribution as obtain are readily explained by the changes which have taken place in the physical geography of the world since the Miocene epoch, it is clear that the result of the comparison of the Miocene and present Faunæ is distinctly in favour of evolution. Indeed, I may go further. I may say that the hypothesis of evolution explains the facts of Miocene, Pliocene, and Recent distribution; and that no other supposition even pretends to account for them. It is, indeed, a conceivable supposition that every species of Rhinoceros and every species of Hyæna, in the long succession of forms between the Miocene and the present species, was separately constructed out of dust, or out of nothing, by supernatural power. But until I receive distinct evidence of the fact, I refuse to run the risk of insulting any sane man by supposing that he seriously holds such a notion.

Let us now take a step further back in time, and inquire into the relations between the Miocene Fauna and its predecessor of the upper Eocene formation.

Here it is to be regretted that our materials for forming a judgment are nothing to be compared in point of extent, or variety, with those which are yielded by the Miocene strata. However, what we do know of this upper Eocene Fauna of Europe, gives sufficient positive information to enable us to draw some tolerably safe inferences. It has yielded representatives of *Insectivora*, of *Cheiroptera*, of *Rodentia*, of *Carnivora*, of Artiodactyle, and Perissodactyle *Ungulata* and of opossum-like Marsupials. No Australian type of marsupial has been discovered in the upper Eocene, nor any Edentate mammal. The genera (except in the case perhaps of some of the *Insectivora*, *Cheiroptera*, and *Rodentia*) are different from those of the Miocene epoch, but present remarkable general similarity to the Miocene and recent genera. In several cases, as I have already shown, it has now been clearly made out that the relation between the Eocene and Miocene forms is such that the Eocene form is the least specialised; while its Miocene ally is more so, and the specialisation reaches its maximum in the recent forms of the same type.

So far as the Upper Eocene and the Miocene Mammalian Faunæ are comparable, their relations are such as in no way to oppose the hypothesis that the older are the progenitors of the more recent forms, while, in some cases, they distinctly favour that hypothesis. The period in time and the changes in physical geography, represented by the nummulitic deposits, are undoubtedly very great, while the remains of middle Eocene and older Eocene Mammals are comparatively few. The general facies of the middle Eocene Fauna, however, is quite that of the upper.

The older Eocene, pre-nummulitic mammalian Fauna, contains Bats, two genera of *Carnivora*, three genera of *Ungulata* (probably all perissodactyle), and a didelphid marsupial. All these forms, except perhaps the Bat and the Opossum, belong to genera which are not known to occur out of the lower Eocene. The *Coryphodon*, however, appears to have been allied to the Miocene and later Tapirs; while *Phiolophus*, in its skull and dentition, curiously partakes of both artiodactyle and perissodactyle characters. The third trochanter upon its femur, and its three-toed hind foot, however, appear definitely to fix its position in the latter division.

There is nothing, then, in what is known of the older Eocene mammals of the Arctogæal province to forbid the supposition that they stood in an ancestral relation to those of the Calcaire Grossier and the Gypsum of the Paris basin; and that our present fauna, therefore, is directly derived from that which already existed in Arctogæa at the commencement of the Tertiary period. But if we now cross the frontier between the Cainozoic and the Mesozoic Faunæ, as they are preserved within the Arctogæal area, we meet with an astounding change, and what appears to be a complete and unmistakeable break in the line of biological continuity.

Among the twelve or fourteen species of Mammalia which are said to have been found in the Purbecks, not one is a member of the orders *Cheiroptera*, *Rodentia*, *Ungulata*, or *Carnivora*, which are so well represented in the Tertiaries. No *Insectivora* are certainly known, nor any opossum-like Marsupials. Thus there is a vast negative difference between the Cainozoic and the Mesozoic mammalian Faunæ of Europe. But there is a still more important positive difference, inasmuch as all these Mammalia appear to be Marsupials belonging to Australian groups; and thus appertaining to a different distributional province from the Eocene and Miocene marsupials, which are Austro-Columbian. So far as the imperfect materials which exist enable a judgment to be formed, the same law appear to have held good for all the earlier mesozoic Mammalia. Of the Stonesfield slate mammals, one, *Amphitherium*, has a definitely Australian character; one, *Phaseolotherium*, may be either Dasyurid or Didelphine; of a third, *Stereognathus*, nothing can at present be said. The two mammals of the Trias, also, appear to belong to Australian groups.

Everyone is aware of the many curious points of resemblance between the marine Fauna of the European Mesozoic rocks and that which now exists in Australia. But if there was this Australian facies about both the terrestrial and the marine Faunæ of Mesozoic Europe, and if there is this unaccountable and immense break between the Fauna of Mesozoic and that of Tertiary Europe, is it not a very obvious suggestion that, in the Mesozoic epoch, the Australian province included Europe, and that the Arctogæal province was contained within other limits? The Arctogæal province is at present enormous, while the Australian is relatively small. Why should not these proportions have been different during the Mesozoic epoch?

Thus, I am led to think that by far the simplest and most rational mode of accounting for the great change which took place in the living inhabitants of the European area at the end of the Mesozoic epoch, is the supposition that it arose from a great change in the physical geography of the globe, whereby an area long tenanted by Cainozoic forms was brought into such relations with the European area, that migration from the one to the other became possible, and took place on a great scale.

This supposition relieves us, at once, from the difficulty in which we were left, some time ago, by the arguments which I used to demonstrate the necessity of the existence of all the great types of the Eocene epoch in some antecedent period.

It is this Mesozoic continent (which may well have lain in the neighbourhood of what are now the shores of the North Pacific Ocean), which I suppose to have been occupied by the Mesozoic *Monodelphia*; and it is in this region that I conceive they must have gone through the long series of changes by which they were specialised into the forms which we refer to different orders. I think it very probable that what is now South America may have received the characteristic elements of its Mammalian Fauna during the Mesozoic epoch; and there can be little doubt that the general nature of the change which took place at the end of the Mesozoic epoch in Europe, was the upheaval of the eastern and northern regions of the Mesozoic sea bottom into a westward extension of the Mesozoic continent, over which the Mammalian Fauna, by which it was already peopled, gradually spread. This invasion of the land was prefaced by a previous invasion of the Cretaceous sea by modern forms of mollusca and fish.

It is easy to imagine how an analogous change might come about in the existing world. There is, at present, a great difference between the Fauna of the Polynesian Islands and that of the west coast of America. The animals which are leaving their spoils in the deposits now forming in these localites are widely different. Hence, if a gradual shifting of the deep sea, which at present bars migration, between the easternmost of these islands and America took place to the westward, while the American side of the sea-bottom was gradually upheaved, the palæontologist of the future would find, over the Pacific area, exactly such a change as I am supposing to have occurred in the North Atlantic area at the close of the Mesozoic period. An Australian Fauna would be found underlying an American Fauna, and the transition from the one to the other would be as abrupt as that between the Chalk and lower Tertiaries. And as the drainage area of the newly-formed extension of the American continent gave rise to rivers and lakes, the mammals mired in their mud would differ from those of like deposits on the Australian side just as the Eocene mammals differ from those of the Purbecks.

How do similar reasonings apply to the other great change of life—that which took place at the end of the Palæozoic period?

In the Triassic epoch, the distribution of the dry land and of terrestrial vertebrate life appears to have been, generally, similar to that which existed in the Miocene epoch; so that the Triassic continents and their Faunæ seem to be related to the Mesozoic lands and their Faunæ, just as those of the Miocene epoch are related to those of the present day.

In fact, as I have recently endeavoured to prove to the Society, there was an Arctogæal continent and an Arctogæal province of distribution in Triassic times as there is now. And the *Sauropsida* and *Marsupialia* which constituted that fauna were, I doubt not, the progenitors of the *Sauropsida* and *Marsupialia* of the whole Mesozoic epoch.

Looking at the present terrestrial fauna of Australia, it appears to me to be very probable that it is essentially a remnant of the Fauna of the Triassic, or even of an earlier, age; in which case Australia must at that time have been in continuity with the Arctogæal continent.

But now comes the further inquiry, Where was the highly-differentiated Sauropsidan Fauna of the Trias in Palæozoic times? The supposition that the Dinosaurian, Crocodilian, Dicynodontian, and Plesiosaurian types were suddenly created at the end of the Permian epoch may be dismissed, without further consideration, as a monstrous and unwarranted assumption. The supposition that all the types were rapidly differentiated out of *Lacertilia*, in the time represented by the passage from the

Palæozoic to the Mesozoic formation, appears to me to be hardly more credible; to say nothing of the indications of the existence of Dinosaurian forms in the Permian rocks, which have already been obtained.

For my part I entertain no sort of doubt that the reptiles, birds, and mammals of the Trias are the direct descendants of reptiles, birds, and mammals which existed in the latter part of the Palæozoic epoch, but not in any area of the present dry land which has yet been explored by the geologist.

This may seem a bold assumption, but it will not appear unwarrantable to those who reflect upon the very small extent of the earth surface which has hitherto exhibited the remains of the great Mammalian Fauna of the Eocene times. In this respect the Permian land vertebrate Fauna appears to me to be related to the Triassic, much as the Eocene is to the Miocene. Terrestrial reptiles have been found in Permian rocks only in three localities: in some spots of France and recently of England, and over a more extensive area in Germany. Who can suppose that the few fossils yet found in these regions give any sufficient representation of the Permian Fauna?

It may be said that the Carboniferous formations demonstrate the existence of a vast extent of dry land in the present dry land area; and that the supposed terrestrial Palæozoic vertebrate Fauna ought to have left its remains in the coal measures, especially as there is now reason to believe that much of the coal was formed on dry land. But if we consider the matter more closely, I think that this apparent objection loses its force. It is clear that during the Carboniferous epoch, the vast area of land which is now covered by coal measures must have been undergoing a gradual depression. The dry land thus depressed must, therefore, have existed, as such, before the Carboniferous epoch—in other words, the Devonian times—and its terrestrial population may never have been other than such as existed during the Devonian, or some previous epoch, although much higher forms may have been developed elsewhere.

Again, let me say that I am making no gratuitous assumption of inconceivable changes. It is clear that the enormous area of Polynesia is, on the whole, an area over which depression has taken place to an immense extent. Consequently a great continent, or assemblage of sub-continental masses of land, must have existed at some former time, and that at a recent period, geologically speaking, in the area of the Pacific. But if that continent had contained mammals, some of them must have remained to tell the tale; and as it is well known that these islands have no indigenous *Mammalia*, it is safe to assume that none existed. Thus, midway between Australia and South America, each of which possesses an abundant and diversified Mammalian Fauna, a mass of land, which may have been as large as both put together, must have existed without a Mammalian inhabitant. Suppose that the shores of this great land were fringed, as those of tropical Australia are now, with belts of mangroves which would extend landwards on the one side, and be buried beneath littoral deposits on the other side, as depression went on; and great beds of mangrove lignite might accumulate over the sinking land. Let upheaval of the whole now take place, in such a manner as to bring the newly emerging land into continuity with the South American, or Australian, continent; and, in course of time, it would be peopled by an extension of the Fauna of one of these two regions—just as I imagine the European Permian dry land to have been peopled.

I see nothing whatever against the supposition that distributional provinces of terrestrial life existed in the Devonian epoch, inasmuch as M. Barrande has proved that they existed much earlier. I am aware of no reason for doubting that, as regards the grades of terrestrial life contained in them, one of these may have been related to another as New Zealand is to Australia, or as Australia is to India, at present. Analogy seems to me to be rather in favour of, than against, the supposition that while only Ganoid fishes inhabited the fresh waters of our Devonian land, *Amphibia* and *Reptilia*, or even higher forms, may have existed, though we have not yet found them. The earliest Carboniferous *Amphibia* now known, such as *Anthracosaurus*, are so highly specialised, that I can by no means conceive that they have been developed out of piscine forms in the interval between the Devonian and the Carboniferous periods, considerable as that is. And I take refuge in one of two alternatives. Either they existed in our own area during the Devonian epoch and we have simply not yet found them; or, they formed part of the population of some other distributional province of that day; and only entered our area by migration, at the end of the Devonian epoch. Whether *Reptilia* and *Mammalia* existed along with them is to me, at present, a perfectly open question, which is just as likely to receive an affirmative, as a negative, answer from future inquirers.

Let me now gather together the threads of my argumentation into the form of a connected hypothetical view of the manner in which the distribution of living and extinct animals has been brought about.

I conceive that distinct provinces of the distribution of terrestrial life have existed since the earliest period at which that life is recorded, and, possibly, much earlier; and I suppose, with Mr. Darwin, that the progress of modification of terrestrial forms is more rapid in areas of elevation than in areas of depression. I take it to be certain that Labyrinthodont *Amphibia* existed in the distributional province which included the dry land depressed during the Carboniferous epoch: and I conceive that, in some other distributional provinces of that day, which remained in the condition of stationary, or of increasing dry land, the various types of the terrestrial *Sauropsida* and of the *Mammalia* were gradually developing.

The Permian epoch marks the commencement of a new movement of upheaval in our area, which attained its maximum in the Triassic epoch when dry land existed in North America, Europe, Asia, and Africa as it does now. Into this great new continental area the mammals, birds, and reptiles, developed during the Palæozoic epoch, spread, and formed the great Triassic Arctogæal province. But, at the end of the Triassic period, the movement of depression recommenced in our area, though it was doubtless balanced by elevation elsewhere; modification and development, checked in the one province, went on in that elsewhere; and the chief forms of mammals, birds, and reptiles, as we now know them, were evolved, and peopled the Mesozoic continent, from which I conceive Australia to have become separated as early as the end of the Triassic epoch, or not much later. This Mesozoic continent must, I conceive, have lain to the east, about the shores of the North Pacific and Indian Oceans; and I am inclined to believe that it continued along the eastern side of the Pacific area to what is now the province of Austro-Columbia, the characteristic Fauna of which is probably a remnant of the population of the latter part of this period.

Towards the latter part of the Mesozoic period, the movement of upheaval around the shores of the Atlantic once more recommenced, and was very probably accompanied by a depression around those of the Pacific. The Vertebrate Fauna elaborated in the Mesozoic continent, moved westward and took possession of the new lands which gradually increased in extent up to, and in some directions after, the Miocene epoch.

It is in favour of this hypothesis, I think, that it is consistent with the persistence of a general uniformity of the directions of the great masses of land and water. From the Devonian period, or earlier, to the present day, the four great oceans, Atlantic, Pacific, Arctic, and Antarctic, may have occupied their present positions, and only their coasts and channels of communication have undergone an incessant alteration. And, finally, the hypothesis I have put before you requires no supposition that the rate of change in organic life has been either greater, or less, in ancient times than it is now; nor any assumption, either physical or biological, which has not its justification in analogous phenomena of existing nature.

I have now only to discharge the last duty of my office, which is to thank you, not only for the patient attention with which you have listened to me so long to-day; but also for the uniform kindness with which, for the past two years, you have rendered my endeavours to perform the important, and often laborious, functions of your President, a pleasure, instead of a burden.

T. H. HUXLEY

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Feb. 17.—The following papers were read: "Account of the Great Melbourne Telescope from April 1868, to its commencement of operations in Australia in 1869." By Albert le Sueur. The author stated that the building in which the telescope is placed is rectangular, 80 feet long meridionally by 25 wide, with walls 11 feet high. Of the meridional length, the telescope-room occupies the north 40 feet; the next 12 feet are appropriated to the polishing machine, crane, and engine; the remaining 28 feet are divided into two rooms, one of which is at present used as an office, the other, 25 by 14, is intended for a laboratory. The moveable roof is 40 feet long, and runs on rails laid the whole length of the walls; the telescope room may therefore be completely covered in, and as completely uncovered when required, the roof in the latter case resting on the south building, which on that account has a very low permanent roof. The telescope, when housed, lies meridionally on the east side of the pier, and nearly in a horizontal direction, provision having been made to prevent the tube being lowered beyond a certain small inclination. Some trouble was experienced in removing the varnish from the specula, and they would require repolishing. Of work done, the author could not yet speak with any satisfaction since it became at all practicable to use the telescope; the history which he had to relate was a long chapter of weary heart-breaking watchings, with an occasional half hour's work. η Argûs was the first object observed for purpose of delineation; after the first night's work little (and that by snatches) was done towards it; a new inroad of workmen and a long course of extremely unfavourable weather having carried the nebula out of convenient reach. The search, which was reluctantly given up, will, however, be again soon resumed. The horseshoe nebula is a grand object, conspicuous and with shape even in the finder. It appears, however, to present no marked difference (with perhaps one exception) which may not be accounted for by the difference of aperture used. This exception is the presence of a small but conspicuous double star at the s p angle of the knot which lies between the ϑ and the bright streak; the experiment has not been tried of cutting down the aperture to approximate to an 18-inch Herschelian, but the intrinsic brightness of the principal star, and the presence in the C G H of stars not more bright (No. 3 of Herschel's catalogue is certainly less bright) go far to show, without this experiment, that the star did not exist as such with its present brilliancy at the time of the C G H and P T 33 observations. The important position of the star, and the careful scrutiny which the knot and its neighbourhood must have repeatedly undergone, forbid the assumption that it was simply overlooked by Sir John Herschel. The star β is conspicuously and beautifully double, the companion of considerable brilliancy, about 15 mag.; with its present brilliancy and elongation, the author thinks, it should be within reach of an 18-inch. The appearance of the knot is sparkling, though no discrete stars can be seen, except perhaps a second faint one, which is suspected at the s f angle; part of the streak near to the knot is also sparkling, but not in so marked a manner; the other portions appear of the ordinary milky nebulosity. The fainter nebulosity (S) of the bright streak pretty well marks out the borders of the almost vacuous lane which leads up to and past the knot; on receding from the lane it becomes very faint; nor is this faintness uniform, but the appearances are so fugitive that, after repeated and painful effort, they could not be caught. The borders, however, stretching to the stars are occasionally pretty well seen. On one or two occasions the author suspected the existence of a link between the nebulosity about the star No. 10 and the lower portion of the ϑ; this, however, requires verification. At the f end, the upper and smaller semicircle is plainly marked, the lower and larger very faintly; its exact figure is, therefore, uncertain. 3570 is a small but beautiful spiral. The two brighter knots are resolvable. Of work out of the regular course, amongst other things, Neptune has been observed on some five or six occasions for figure and a second satellite, with only negative results. In the absence of a photographic apparatus to be used at the uninterrupted focus of large mirror, attempts have been made to utilise the second or Cassegrain image; an average exposure of near ten minutes on an eight-day moon produced pictures which (by no means good) were of sufficient promise to make it worth while to resume the attempt under more favourable conditions. The time of exposure is somewhat surprising, and would seem to indicate a great loss of chemical rays by a second perpendicular reflexion; but perhaps the inactivity was mainly due to absorption at the surface of the large mirror, which was then very yellow. The spectroscope arrived some time ago, but has not been much used; it is thought that for *star* work of any value some modification will be required, principally the exchange of the present collimator for one of longer focal length; a greater dispersion, moreover, seems desirable. The spectroscope was mainly designed for nebula work, is handy and compact, and will be of much service. For spectroscopic work on objects having a sensible diameter, the great telescope itself labours under some disadvantages; the enormous focal length and consequent magnification of the image is a serious inconvenience in the case of faint objects, and may be only partially remedied by a suitable condenser. This magnifying of the image may, however, in some cases be advantageous, from the possibility thereby afforded of viewing small definite portions of moderately bright objects, though unfortunately that will be seldom. Of nebulæ, Orion has been examined for purpose of practice; the three lines are plainly and conspicuously seen; the hydrogen line is comparatively much fainter than was anticipated, disappearing in the fainter portions of the nebula. 30 Doradûs shows the nitrogen line with facility, the second line certainly, but not in all positions, and always with difficulty; the hydrogen line is suspected only. No trace of a continuous spectrum could be seen. η Argûs has been observed on only one unfavourable morning; the nitrogen line was seen over a considerable space; the presence or absence of others, or of a continuous spectrum, could not be stated with certainty. With respect to future operations, it is intended that at first the routine work shall consist of a detailed delineation of the objects figured by Sir John Herschel, or any others which may prove interesting; this will take some time; for even without the impediment of cloudy weather, the delineation with any degree of satisfactory correctness, of a moderately large nebula, requires a considerable amount of work with careful and frequent scrutiny. It is hoped, however, that this work will by practice be found less painfully difficult than it is at present. The spectroscope will be used as much as possible, the moon photographed, and attempts made to photograph the nebulæ, when a photographic apparatus has beenpr ocured, and staging, photographic room, &c., added to the building. It is moreover hoped that before long a refractor, of some nine inches aperture, may be procured, to be mounted with the reflector, or, preferably, as a separate instrument. This telescope, besides being of much general use, will find much and valuable employment in determining micromctrically the chief points in the nebulæ under examination with the reflector, with more expedition and accuracy than at present; for spectroscopic work this telescope would be a valuable adjunct, especially if it be constructed of such comparatively short focal length as seems now to be practicable.

"On a distinct form of Transient Hemiopsia," by Hubert Airy. From a comparison of the different accounts of "Hemiopsia," "Half-vision," or "Half-blindness," by Wollaston, M. Arago, Brewster, the Astronomer Royal, Dufour, Sir John Herschel, Sir Charles Wheatstone, and Mr. Tyrrell, the author considers that, irrespective of the wide primary distinction between the transient and permanent forms of Hemiopsia, there are different forms of transient Hemiopsia which have all been included under the same name; Wollaston, Arago, Brewster, and Tyrrell, describing one form of the transient affection, while Sir John Herschel, Sir Charles Wheatstone, the Astronomer Royal, Professor Dufour, and the author agree in describing another. As to the actual seat of the visual derangement, the author considers that the exact agreement of the two eyes in the nature, extent, and degree of their affection, proves (assuming the semidecussation of the optic nerves at the chiasma) the seat of the affection to be at some point behind the chiasma of these nerves. All the causes that are found to lead to transient half-blindness, point to the brain as the seat of disturbance. Still clearer is the evidence given by the loss of speech and of memory, the derangement of hearing, and the partial paralysis which sometimes follow an attack of teichopsia. Such cases as Sir John Herschel's, where the cloud passed over the whole field from left to right, can only be explained by supposing the disturbance to lie in some region of the brain where the opposite halves are in contact. The mischief may possibly be seated in the corpora quadrigemina or geniculata, or even in the cerebellum itself. The phenomena are so definite and so localised, and their course is so regular, that we can hardly avoid the conviction that their cause is equally definite and equally localised; and it is difficult to admit so vague an agent as nervous sympathy with gastric derangement, except as acting through the medium of some secondary local manifestation in the brain.

Chemical Society, February 17.—Prof. Williamson, F.R.S., president, in the chair. The following gentlemen were elected fellows:—R. T. Atcherley, T. W. Axe, A. H. Bateman, E. Francis, A. Prangley, W. Pritchard, L. B. Ross, T. G.

Rylands, T. Wills, and P. Wright. An account was given by Prof. Tyndall of his researches on "the action of light on gases and vapours," illustrated by a series of beautiful experiments. Dr. Tyndall began by remarking that it had, for the last ten years, been his endeavour to make radiant heat a means of getting an insight into the working of the atomic forces, or, in other words, into the state which is called chemical combination. Whilst pursuing his experiments with luminous waves on matter in a finely divided state, he was forced to imagine molecules and atoms; in fact, his belief in the existence of atoms is founded more upon those physical evidences than upon the considerations which are current in the chemical world. If he had to give up the notion of atoms, and to replace that conception by the abstract idea of multiple proportions, he would feel completely at a loss how to account for changes in the physical properties of matter. After these introductory remarks, the lecturer proceeded to the main subject. The apparatus which served to illustrate the statements, consisted of a glass tube about 3 feet in length, and 3 inches internal diameter, closed at each end by glass discs. This tube, after having been exhausted by an air-pump, was partially filled with dry air which had been permitted to bubble through the liquid whose vapours were to be examined. The condensed beam of an electric lamp was caused to pass through the tube from end to end. Since the aim of these experiments is to render visible the chemical action of light upon vapours, substances have been chosen, one at least of whose products of decomposion by light has so high a boiling point that as soon as it is formed it is precipitated. Nitrous oxide gas, the vapours of Alleylic Iodide, Amylic Nitrite, Benzole, &c., mixed with some air which had passed through hydric nitrate or hydric chloride, were found well suited for this purpose. In all cases, no matter what the nature of the vapours was, if it was only employed in a sufficiently attenuated state, the visible action commenced with the formation of a blue cloud, which in some instances was of the deepest azure tinge, rivalling the colour of the purest Italian sky. When a cell containing some of the liquid whose vapours are to be examined was inserted between the lamp and the tube, no clouds were formed within the tube; the luminous waves traversing the liquid had been deprived of their acting power. When polarised light was sent through the tube the blue cloud was visible only in one direction—the direction varying according to the position of the Nicol's prison; when the short diagonal of the Nicol was vertical, the blue cloud was seen when the specator's eye looked horizontally upon the tube, not otherwise; as soon as the prism was turned round its axis, the blue cloud was only seen when the line of vision fell vertically upon the experimental tube.

After concluding his account of this highly interesting subject, Prof. Tyndall showed some of the experiments bearing on his researches upon Dust, quite recently communicated at the Royal Institution.

Anthropological Society, February 15.—Dr. Berthold Seemann, V. P., in the chair. C. W. Eddy, Esq., M.A., and E. Schiemann, Esq., were elected Fellows. The following papers were read:—No. 1. "On the Aborigines of the Chatham Islands," by Dr. Barnard Davis and Mr. E. A. Welch. Mr. Welch, after discussing the history and discovery of those islands, described their conquest by the Maories and the ultimate fate of the Aborigines. Dr. Barnard Davis gave the results of a particular examination of the characters presented by the skulls and skeletons of many of the inhabitants. In three cases the cephalic indices of the skulls were stated to be ·74, ·74 and ·87. The stature of the Moriories, or Chatham Islanders, appeared to indicate a race shorter and stouter than the inhabitants of New Zealand. "On polygamy: its influence in determining the sex of our race and its effects on the growth of population." By Dr. J. Campbell. The author, who had been many years resident in Siam, gave minute details of the relative proportions of female to male births in the harems of the King and other important Siamese dignitaries. The result seemed to be that the proportions of males and females born were, as in the case of Monogamist marriages, entirely equal.—Mr. Ralph Tate described an inscribed rock on the banks of the Iguana, a tributary of the Orinoco. This presented an incised marking which the author considered to be more ancient than the present inhabitants of the district.

Royal Microscopical Society, February 9.—Annual meeting; the Rev. J. B. Reade, president, in the chair. The following gentlemen were elected officers for the ensuring year:—President, Rev. J. B. Reade; Vice-Presidents, Charles Brooke, L. S. Beale, James Glaisher, F. H. Wenham; Treasurer, Richard Mestayer; Secretaries, H. J. Slack, Jabez Hogg. The president read an address and announced his intention of presenting to the Society a copy of the Philosophical Transactions —60 vols. *in extenso*, from 1665 to 1812, and the parts from 1813 to the present time, as issued by the Royal Society.—Mrs. Holland presented the silver medal of the Society of Arts, with certificates awarded to her late husband for his "Microscopic Triplet," 1832. A micrometer ruled on silver by Mr. Barton, with some specimens of beads and bead lenses made by the late Mr. Holland, &c.—A vote of thanks was given to the president and Mrs. Holland for their presents.

Institution of Civil Engineers, February 1.—Mr. Charles B. Vignoles, F.R.S., president, in the chair. "On the statistics of railway expenditure and income, and their bearing on future railway policy and management." Mr. John Thornhill Harrison, M. Inst. C.E. From returns now made to the Board of Trade supplying definite information on most points of interest, the author had prepared a synopsis of this information for twenty of the principal railways in England and Scotland, representing about 85 per cent. of the entire capital expended in the United Kingdom. The original cost of railways, the working expenses, revenue, &c., were described, and it was shown that while the National Debt, amounting to 750 millions sterling, with a return of 26½ millions per annum, or 3½ per cent., was a burden on the industry and capital of the country, the capital expended on railways, amounting to 500 millions sterling, gave a return of 20 millions, or 4 per cent. per annum; whilst a sum nearly equal to the interest on the National Debt was annually expended in labour and materials.

PARIS

Academy of Sciences, February 14.—The greater part of the proceedings at this meeting had reference to subjects connected with mathematics and mechanics. M. de Saint-Venant presented a second part of his memoir on the determination of the pressure of incoherent soils upon walls; M. Morin an elaborate report upon a memoir by M. Tresca on the effects of punching and stamping, and on the mechanical theory of the deformation of solid bodies; M. de Saint-Venant appended to the latter a theoretical proof of the equality of the two coefficients of resistance to cutting with shears and to extension or compression in the continuous movement of deformation of ductile solids beyond the limits of their elasticity; and a report on five memoirs by M. F. Lucas, entitled investigations upon the mechanics of atoms.—M. C. Jordan communicated a paper on a new combination of the twenty-seven right lines of a surface of the third order; M. Pellet a note on the functions irreducible by means of one module, and one modular function, and M. A. Ribacour a note on the deformation of surfaces.—An extract from a letter or M. De la Rire to M. Jamin was read, referring to the observations of M. Trève on the action of magnetism upon rarefied gases.—M. Zaliwski read a note on a battery with three liquids regarded by him as superior to Bunsen's battery. The battery consists of an inner porous vessel, containing nitric acid and a plate of carbon, and an outer porous vessel containing sulphuric acid, the whole placed in a vessel containing a solution of hydroclorate of ammonia and a plate of zinc.—In a short note M. J. M. Séguin communicated some interesting observations on the accidental images of white objects.—M. A. Demoget described a new electro-magnetic apparatus, which, by an increase in the number of coils and alteration in their shape, can be made to produce a greater evolution of electricity, with less rapid revolution, than Siemens' apparatus.—M. P. Volpicelli communicated a paper containing an account of a photographic barometer, and some historical details upon lunar radiation. He noticed some modifications which he has introduced into the construction of a barometer for the Observatory at Rome, and with regard to the second subject, after citing several old writers, such as Aristotle, Thomas Aquinas, Pico della Mirandola, and Cardan as having admitted the production of calorific effects by the lunar radiation, maintains that the first experimental and incontestable demonstration of the phenomenon was given by Melloni in 1843.—A short note, illustrated with a figure, upon two solar spots now visible with the naked eye, by M. Tremeschini, was presented. The penumbras of these spots measure 0° 3′ 45″ and 0° 4′ 50″ in their greatest diameter; one of them contains a nucleus measuring 0° 0′ 45″11 in its greatest diameter.—M. J. Girard presented a note upon double crystals of snow, the formation of which he ascribes to the splitting of a drop of water too large to form a single crystal; the twin crystals are united by a small stay of a hexagonal prismatic form, having each of its edges corresponding exactly to the origin of one of the six regular branches of the two crystals.—A note by M. P. Bert on the influence of green light on the Sensitive plant, was presented by M. Claude Bernard. The author placed several young Sensitive plants in lanterns filled with variously coloured glass, and found that those exposed to green light lost their sensibility and died almost as quickly as those placed in perfect darkness.—M. A. Milne-Edwards communicated a note on some Mammalia from Eastern Thibet, which notwithstanding the rigour of the climate, is inhabited by two species of monkeys, a *Macacus* and a *Semnopithecus*.—The author noticed two forms of Insectivorous mammals forming new genera—one, which he calls *Nectogale elegans*, being intermediate between the Desmans and the Shrews; the other very nearly related to the latter, and called *Anourosorex* on account of the rudimentary state of its tail.—A singular animal, resembling a bear in general appearance, was noticed as forming a new genus, *Ailuropoda*, allied to the pandas and raccoons.—A long-snouted mole and a new flying squirrel were also mentioned.—The following notes and memoirs were communicated, but no particulars of their contents are given.—On the statistics of the therapeutical properties of the mineral waters of Baréges, Amélie-les-Bains, Vichy, and Bourbonne, by M. Champouillon; on some questions which may be referred to the theory of permanent isothermal lines, by M. E. Combescure; on staphyloraphie, and on the action of hydrate of chloral, by Mr. Lawson Tait; on the cause of the oscillatory movement of molecular granules by M. Lerique de Mouchy; on an accumulation of heat by the concentration of radiant heat through convex lènses of rock salt and the application of this heat to the production of a current of air giving rise to a continuous movement, by M. Vernier; on the production of the electric light by induction coils, by M. Delaurier; and a note on the trisection of the angle, by M. L. Vezzia.

BERLIN

German Chemical Society, February 14.—A. W. Hofmann gave a new instance of the aid science derives from industry. The manufacture of chloral yields as a secondary product chloride of ethyle mixed with other chlorinated liquids. By treating this mixture with alcoholic ammonia in Frankland's digester, large quantities of the chlorides of ethylated ammonia bases are formed, while sal-ammoniac separates. This appears now to be the most reasonable method for producing ethyl-amines. The chlorinated compounds mixed with the chloride of ethyl are not acted upon by NH_3. They remain behind. These liquids commence to boil at 30° C., and seem to consist partly of dichlorinated marsh gas.—O. Liebreich reported on Suewern's disinfecting process. The bulk of the substance employed consists of lime disguised by the presence of chloride of magnesium and tar. Its utility proved to be very limited. Amongst the details given by Mr. Liebreich he mentioned that the canal water operated upon contained nitrogenous matter in extremely variable quantities according to the temperature of the air. For 59 parts of N. found in it during warm days, it contained 2 parts only while the weather was cold.—V. Meyer described a new and ingenious synthetical method for producing organic acids. This method is founded on the observation that formiate of sodium, when heated, splits into H and the group COO Na. Thus, when heated alone, the formiate yields oxalate of sodium and H_2. When heated with the potassium salt of a sulpho-organic acid such as phenyl-sulphurous acid $HKSO_3$, acid sulphate of potassium separates and COO Na takes the place occupied by the group KSO_3. In the instance mentioned benzoate of sodium is formed. Sulphobenzoate of potassium similarly treated yields isophtalic acid. The same chemist made some interesting remarks on the constitution of camphor and of camphoric acid.—Mr. Franck, the discoverer of potassium salts in Stassfurth, and the originator of the important industry founded on this occurrence, gave some details of the manufacture of bromine from the mother liquors. After describing an apparatus for pouring bromine from one vessel to another, he described his process for purifying this substance by re-distillation. This he effects by allowing the vapour to pass through a solution of bromide of iron before it passes into the condenser. The chlorine mixed with the bromine is thus retained in the shape of chloride of iron. Bromide of iron is the best material for the production of potassium. It is also well suited for transport; and a large quantity of the bromine shipped to America, goes there in the form of dry bromide of iron. Parchment-paper and clay mixed with colza oil, serve to unite the vessels used in the distilling process of bromine. The retorts are made of sandstone, and are lined with tar inside. The bromine acts on the tar, entailing the inevitable loss of sixty or eighty pounds of bromine in a new retort, and the production of brominated organic products, boiling between 60° and 400° C., but containing no bromoform. The bromine manufactured at Stassfurth contains no trace of iodine.

DIARY

THURSDAY, February 24.

Royal Society, at 8.30.—Note on Certain Lichens: Dr. Stenhouse.—Successive Action of Sodium and Iodide of Ethyle upon Acetic Ether: Dr. Frankland and Mr. Duppa.
Zoological Society,
Society of Antiquaries, at 8.30.—On the Guilds at Wymondham, Norfolk: Mr. G. A. Carthew.
Royal Institution, at 3.—Chemistry: Prof. Odling.
London Institution, at 7.30.

FRIDAY, February 25.

Quekett Microscopical Club, at 8.
Royal Institution, at 9.—On the Results of the Ordnance Survey of Sinai: Captain Wilson.

SATURDAY, February 26.

Royal Institution, at 3.—Science of Religion: Prof. Max Müller.
Royal Botanic Society, at 3.45.

MONDAY, February 28.

Geographical Society, at 8.30.
Institue of British Architects, at 8.
Institute of Actuaries, at 7.—On the Proper Method of Loading the Premiums required for the Grant of Life Annuities and Assurances: Mr. W. M. Makeham.
London Institution, at 4.

TUESDAY, March 1.

Medical and Chirurgical Society, at 8.30.
Anthropological Society, at 8.—On the Circassian Slaves and the Sultan's Harem: Major F. Millingen.
Institution of Civil Engineers, at 8.—Discussion upon The Mhow-Ke-Mullee Viaduct, and The Pennair Bridge.—The Wolf Rock Lighthouse: Mr. J. N. Douglass.
Syro-Egyptian Society, at 7.30.
Royal Institution, at 3.—Plant Life: Dr. Masters.

WEDNESDAY, March 2.

Obstetrical Society, at 8.—Anniversary.
Horticultural Society, at 1 30.
Society of Arts, at 8.

THURSDAY, March 3.

Royal Institution, at 3.—Chemistry: Prof. Odling.
Royal Society, at
Linnean Society, at 8.—On Hybridism among Cinchonæ: Mr. J. Broughton.

CONTENTS

Errata.—Page 409, first column sixth line: for "Fern Deal," read "Ferndene."—Page 410, first column, fourth line: for "29," read "19."—Page 410, second column, fortieth line: for "50," read "40."—Page 406, second column, twenty-fourth line: for "x," read "ϕ."

Professor Jevons requests us to state that the subject of the paper read by him at the Manchester Philosophical Society, though similar to that read at the Royal Society, was not the same as was inadvertently stated in our report at p. 393.